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Employment to output elasticities and reforms towards flexicurity: Evidence from OECD countries

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Abstract

Labor market reforms in the direction of "flexicurity" have been widely endorsed as a means to increase an economy's ability to adjust to negative shocks while offering adequate social safety nets. This paper empirically examines how such reforms influence employment's responsiveness to output fluctuations (employment–output elasticity). To address this question, we employ a single equation error correction model with policy interactions on a panel of OECD countries, which also incorporates the period of the Great Recession, and distinguish between passive and active labor market policy types. Flexicurity is represented by three policy measures: unemployment benefit generosity, the flexibility of hiring and firing rules, and spending on active labor market policies. We find that the effects of any single policy change are shaped by the broader existing policy mix within which it takes place. A hypothetical flexicurity reform towards the policy mix of Denmark, a well-known example of the flexicurity regime, is found to increase or leave unchanged countries' short-run employment–output elasticities, depending on the initial policy mix. These results are robust to accounting for a large set of additional labor market institutions.

KEYWORDS
employment–output elasticity, flexicurity, labor market policy, welfare state

JEL CLASSIFICATION
E24, E32, J21, J65

1 | INTRODUCTION

The response of employment to external shocks is a central concern to policy-makers. Crivelli et al. (2012) show that unemployment rose substantially in advanced countries after 2008, while this did not happen in Latin American or African economies. In particular, they find that the employment responses to output shocks (output elasticity of employment) differ significantly among advanced countries— point estimates for the long-run elasticity range from 0.64 for Western Europe to 0.81 for North America. A number of papers looked at the role of institutional differences in determining these intercountry variations in the employment responsiveness to output shocks (Bassanini, 2012; Blanchard & Wolfers, 2000; Crivelli et al., 2012).[1]

A high degree of intercountry heterogeneity in the types of institutions governing the labor market persists in the European Union (EU) and indeed in the OECD. However, in recent years the flexicurity system— combining fairly generous unemployment support systems with flexible hiring and firing rules and high levels of active labor market policies to foster participation and employability— has been widely endorsed as a means to increasing an economy's ability to adjust to negative shocks while offering adequate social safety nets.[2] This support has in part been driven by the observation that, in the aftermath of the Great Recession, while many countries experienced massive increases in unemployment and output contractions, a flexicurity country such as Denmark suffered relatively minor unemployment consequences (Andersen, 2015). Surprisingly, however, empirical research that sheds light on the aggregate impact of flexicurity on labor market outcomes is limited, with extant studies focusing either on qualitative assessments (e.g., Andersen and Svarer, 2007) or on the effect of specific aspects of the flexicurity package (e.g., Card et al., 2010, Faccini & Bondibene, 2012, Kluve, 2010). This is an important limitation since the relevance of the interaction between different policy instruments has been forcefully demonstrated theoretically— e.g., Davoine and Keuschnigg (2015), Dabusinskas et al. (2016), and Molana et al. (2021)— and, thus, matters for any country that aims to adopt the prescribed labor market reforms by institutions such as the International Monetary Fund (IMF) and the European Commission.

[1] At a theoretical level, the response of employment-to-output fluctuations may be driven by a number of factors including variations in the degree of capital mobility (Azariadis & Pissarides, 2007; Molana et al., 2021), degree of openness to international trade (Cacciatore, 2014), the nature of the distribution of firm-size (Görg et al., 2017), the structure of labor market institutions and active labor market policies characterizing different economies (Molana et al., 2018).

[2] Flexicurity policies are central to the European 2020 employment strategy and have, more broadly, been supported by international institutions such as the IMF (see, e.g., Blanchard et al., 2014).

The first objective of this paper is to offer more up-to-date evidence, including the time of the Great Recession, on the employment–output elasticity and its changes— thus capturing the adjustments in employment and their level of persistence over time. Specifically, one of our paper' s primary contributions is to take a step towards overcoming the relative lack of empirical evidence on the aggregate labor market effects of flexicurity reforms by focusing on their effects on the employment–output elasticity— which is an important indicator of how an economy responds to shocks. In addition, and most importantly, it will not only pay particular attention to the role of passive and active labor market policies (PLMP and ALMP, respectively) but also to their interactions in driving outcomes. The conjectures that guide our approach are based on the theoretical results of Molana et al. (2018) who demonstrate that different combinations of policy instruments that characterize flexicurity yield different degrees of volatility in employment/unemployment outcomes and highlight the importance of the initial conditions.

Our analysis covers both the Great Recession and the recovery period in a panel of 20 OECD countries. In particular, we consider two different time spans: from 1960 to 2012 and from 1986 to 2012; the latter enables us to explore the individual and combined effects of policy reforms. We first estimate the employment-to-output elasticity and find that, on average, the response of employment-to-output fluctuations in the short run increased substantially in the 1980s and 1990s but has stalled since then. In contrast, the long-run elasticity has remained stable over time.

We then characterize labor market regimes by three policy measures that capture two typical PLMPs (i.e., the generosity of unemployment protection and the flexibility of the labor market with respect to hiring and firing) and an ALMP such as the expenditure on training programs to enhance employability. When examined individually, we find that some specific policies have had substantial effects: both the generosity of unemployment benefit and higher degrees of flexibility in hiring and firing regulations induce greater short-run output elasticity of employment. The combined impact of these two policies is also found to have a sizeable offsetting effect on the responsiveness of employment-to-output fluctuations, implying the existence of complementarity of policy outcomes.

To further explore this complementarity effect, we consider three forms of labor market regimes— *average*, *liberal*, and *interventionist*. The first is a hypothetical system that we obtain by setting the policy variables to their sample average values. The *liberal* regime, such as a liberal welfare state, is characterized by a high flexibility index (at the 75th percentile of the sample distribution) and relatively low levels of the other two policy variables (unemployment benefit and training expenditure), at their 25th percentiles. The *interventionist* regime, in contrast, is characterized by a relatively low flexibility index (at its 25th percentile) and relatively high values of the other two instruments (at their 75th percentiles). Our interest is to investigate the effects on the responsiveness of employment in the different regimes of implementing reforms in the direction of flexicurity. Consistent with the theoretical predictions of Molana et al. (2018, 2021), we find that policy complementarities matter when considering the impact of changes to individual policy instruments. For example, our estimations reveal that, ceteris paribus, raising the generosity of unemployment benefit in a *liberal* regime is not associated with higher short-run employment–output elasticity. In an *interventionist* regime, a reform towards more labor market flexibility is not accompanied by greater short-run employment responsiveness.

The above results suggest that reform "packages" that shift the policy mix characterizing each regime towards the Danish *flexicurity* model are not likely to have the same impact on the responsiveness of employment-to-output variations across all benchmark systems. In particular, further estimations reveal that for countries such as the United States and Canada with liberal regimes, the flexicurity reform does not have statistically significant effects on employment's responsiveness. However, we find that labor market reforms towards flexicurity would imply greater short-run employment–output elasticities in, for example, Australia, Japan, Germany, and Great Britain in the order of approximately 0.2–0.25 percentage points. Interestingly, while Germany is both less interventionist and less liberal compared to Denmark (our benchmark) and has high levels of expenditure on training programs, Great Britain is much less interventionist and more liberal and has a lower level of active labor market policies. A key message of this paper, therefore, is that a flexicurity reform, as widely advocated, may result in very different aggregate effects depending on the characteristics of the initial regimes, especially in the short run, resulting in greater employment volatility by amplifying the magnitude of the response to output fluctuations.

This paper contributes to the empirical literature that investigates the role of labor market institutions and policies on labor market outcomes (Bassanini, 2012; Bertola et al., 2002, Blanchard & Wolfers, 2000; Crivelli et al., 2012, Faccini & Bondibene, 2012; Nickell et al., 2005). However, some of these works only look at the long-run impact of these policies on the labor market— with the exception of Bassanini (2012) and Faccini and Bondibene (2012) who consider the short-run dynamic implications. We look at both the short-run and the long-run elasticities but, unlike all of these papers, we also examine the complementarities of PLMPs and ALMPs from the perspective of a flexicurity reform using recent data evidence.

This study is also related to papers evaluating the macroeconomic effects of labor market policies. Gnocchi et al. (2015) and Abbritti and Weber (2010, 2018) examine the effects of labor market institutions in driving business cycle fluctuations. Fonseca et al. (2010) also explore the relationship between labor market institutions and business cycles, focusing on international comovements. The effects of institutions on the dynamic adjustment of inflation have been studied by Bowdler and Nunziata (2007), while Rumler and Scharler (2009) examine how institutions affect the volatility of output and inflation. We differ in our focus (and thus contribute to the debate) on the role of flexicurity reforms on aggregate labor market outcomes. From a broader context, a number of papers have assessed the benefits (or the effectiveness) of flexicurity reforms, but these studies are mostly qualitative: for example, Andersen and Svarer (2007) and Andersen (2015) provide an assessment of the Danish experience, focusing, respectively, on workfare policies and long-term unemployment. Qualitative assessments of different welfare state reforms in selected countries in the EU have also been documented by Eichhorst and Konle-Seidl (2006) and Räisänen et al. (2012). By contrast, Kluve (2010) provides a quantitative assessment of the effectiveness of ALMP measures, concluding that training programs have had mild effects on employment outcomes. Shahidi et al. (2016) assess the health-related impact of temporary contracts that may result from the less stringent hiring and firing regulations associated with flexicurity reforms.

In the next section, we present our empirical approach. Section 3 focuses on the description of the data and its time series properties. Section 4 obtains the baseline elasticity estimates and presents temporal changes and cross-country heterogeneity in the short-run elasticity estimate. Section 5 continues with a presentation of the selected labor market policies. Section 6 reports the estimates with the policy variables and discusses the effects of policy changes under different labor market regimes. This is followed by the evaluation of the flexicurity reform in Section 7. Finally, Section 8 presents some robustness checks, and Section 9 concludes.

2 | EMPIRICAL APPROACH

Taking advantage of the relatively long-time dimension of our data, we follow standard time series methodology and describe the dynamic relationship between employment and output in a single equation error correction model (Hendry, 1995). This one-step estimation approach has also been popularized recently by Rao et al. (2010) and Kumar and Rao (2012) for panel data applications.[3] An advantage of the approach is that it encompasses the comovement of employment and output in both the short and long term and thus allows for the joint estimation of the short- and long-run employment-to-output elasticities. Related empirical literature typically estimates either short- or long-run elasticities only. Bassanini (2012), for instance, estimates elasticities for the short run by relying on cyclical fluctuations of the variables. In contrast, Kapsos (2006) or Crivelli et al. (2012) estimate long-run elasticities by regressing the level of employment on the level of output.

The error correction model between employment (e) and output (y), which we apply in this paper, is

$$\Delta e_{i,t} = \alpha\left[e_{i,t-1} + \beta y_{i,t-1} + \delta t\right] + \gamma \Delta y_{i,t} + \epsilon_{i,t}. \tag{1}$$

Equation (1) implies that the change in employment (annual and logarithmic) in country i and year t is driven by the change in output ($\Delta y_{i,t}$) in that country and year, the deviation of employment from its long-run equilibrium in $t-1$ (term in brackets) and a random error term ($\epsilon_{i,t}$) capturing contemporaneous employment shocks. The term in the square bracket is the long-run equilibrium relationship between employment and output, where β is assumed to be negative and it measures the long-run employment-to-output elasticity. The long-run relationship also allows for a deterministic linear time trend (δt), which may represent long-run technological or demographic changes that are common across countries. The short-run employment–output elasticity is captured by γ.

The model assumes that the time series of both employment and output are first-order integrated so that their annual changes are stationary. It also anticipates that employment and output form a long-run cointegrating relationship of the form captured in the bracket. Empirically, this cointegrating relationship may or may not exist. If it exists, then $\alpha < 0$ in the model, which implies that any deviation in employment from its long-run equilibrium will result in an error-correction adjustment, where the speed of adjustment is captured by α.

This empirical approach, which does not rely on estimating aggregate labor demand or labor supply equations separately, enables us to investigate how employment fluctuates in response to output variations over time, regardless of whether this response originates from demand-side or supply-side shocks and of how the movements in output propagate through the labor market to affect employment. Our approach is deliberately atheoretical and can be perceived as a generalization of Okun's law— a purely empirical relationship. Of course, as with Okun's law, the relationship between output and employment may also depend on other factors, such as labor productivity growth, changes in working hours per employee, or the size of the labor force. To account for these forces, we will present several robustness checks to our baseline empirical model.

We reformulate Equation (1) into the following regression equation:

$$\Delta e_{i,t} = \alpha e_{i,t-1} + \beta^* y_{i,t-1} + \gamma \Delta y_{i,t} + \delta_t + \delta_i + \varepsilon_{i,t}, \tag{2}$$

[3] Gnocchi et al. (2015) employ a similar approach to examine the direct relationship between labor market policy instruments and the business cycle. A similar empirical approach is taken for the estimation of the employment–output elasticity for the Gulf countries and for Turkey, respectively, by Behar (2017) and by Sahin et al. (2015) and for estimating the effects of short-time work schemes on various labor market outcomes by Hijzen and Martin (2013).

where α remains the speed of adjustment parameter, γ is the short-run elasticity, while the long-run elasticity is obtained as $-\frac{\beta^*}{\alpha}$. Estimating this equation instead of (1) allows for greater flexibility in the treatment of the common time trend, as it assumes year-specific fixed effects (δ_t) instead of a linear time trend. Moreover, the equation accounts for country-specific constants (δ_i). As discussed later in this paper, as part of robustness checks, we experiment with alternative specifications— for example, we add country-specific linear time trends to account for the possibility that secular trends,for example, in terms of demography, productivity, or technology, differ from country to country. As a more direct test for the role of demographic forces, in another specification, we estimate Equation (2) with both employment and output divided by the size of the working-age population, a specification which directly accounts for changes in the size of the labor force. Furthermore, in yet another specification, we measure employment with full-time equivalent employment instead of the number of employees, thereby allowing for employment adjustments both at the extensive and intensive margins. The conclusions based on (1) remain robust regardless of the alternative specifications.

In the above framework, it is possible to assess the existence of a cointegrating relationship by testing whether α is significantly smaller than zero (Ericsson & MacKinnon, 2002). Should the estimated value of α be non-negative, then the cointegrating relationship would be considered non-existent, in which case Equation (2) would simplify to

$$\Delta e_{i,t} = \gamma \Delta y_{i,t} + \delta_t + \delta_i + \varepsilon_{i,t}, \tag{3}$$

and the short- and long-run elasticities would both be γ. In what follows, we refer to Equation (2) as the error correction (EC) specification and Equation (3) as the first-difference (FD) specification.

To incorporate the role of policies, we follow on the previous literature (e.g. Blanchard & Wolfers, 2000; Crivelli et al., 2012; Bassanini, 2012; Hijzen & Martin, 2013; Abbritti & Weber, 2018) and include interactions of the right-hand side variables with the policy variables in the regression equation. The FD specification will then become

$$\Delta e_{i,t} = \gamma_1 \Delta y_{i,t} + \gamma_2 \Delta y_{i,t} \tilde{P}_{i,t-1} + \eta \tilde{P}_{i,t-1} + \delta_t + \delta_i + \varepsilon_{i,t}, \tag{4}$$

where $\tilde{P}_{i,t-1}$ denotes the vector of policy variables (and possibly their interactions with each other), all lagged by 1 year. For ease of interpretation, the policy variables are "centered" within each year, that is, expressed as deviations from their country means as $\tilde{P}_{i,t} = P_{i,t} - \frac{1}{N}\sum_{j=1}^{N} P_{j,t}$. The short-run elasticity of any given country can thus be interpreted as an average elasticity plus country deviations originating from policy differences at any given point in time,

$$\mathrm{sre}_i = \gamma_1 + \sum_k \gamma_2^k \tilde{P}_i^k, \tag{5}$$

where k is the index for the different policy measures and— possibly— their interactions. The estimate for γ_1 will always give the short-run elasticity for the average country.[4] The estimated γ_2 will in turn measure how the policy variables (or their interactions) correlate with this elasticity or, to put it differently, what change in the elasticity associates with a one-unit increase in a given policy variable.

[4] This is ensured by centering the policy variables.

The EC specification (2) can similarly be extended to incorporate the policy variables. Specifically, doing so will result in estimating an equation such as (4) but augmented with the terms $\alpha_1 e_{i,t-1} + \alpha_2 e_{i,t-1}\tilde{P}_{i,t-1}$ and $\beta_1 y_{i,t-1} + \beta_2 y_{i,t-1}\tilde{P}_{i,t-1}$. This specification has the advantage of enabling us to also see how the policy variables influence the long-run elasticity or the speed of adjustment to the equilibrium. Similar to the short-run one, the long-run elasticity can be expressed as a function of parameters common to all countries and policy-induced individual deviations, more formally,

$$\mathrm{lre}_i = -\frac{\beta_1 + \sum_k \beta_2^k \tilde{P}_i^k}{\alpha_1 + \sum_k \alpha_2^k \tilde{P}_i^k}. \tag{6}$$

Policy variables can, of course, relate endogenously to simultaneous employment dynamics. It is natural to assume that, in some cases, national governments adjust their labor market policies to employment shocks. In order to alleviate such reverse causality issues, we use the first lag of the policy variables in our regressions.[5] Moreover, in order to lessen endogeneity concerns from omitted variables, we control for several country-time-specific confounders in the regressions. Possible confounders are, for example, other labor market indicators, such as union density or the tax wedge, but also the degree of openness to international trade or the size of the government. These can explain cross-country differences in the employment–output relationship and, at the same time, correlate with the labor market policy mix. Of course, to the extent that our observable variables, and our one-period lag, do not capture all possible endogeneity issues, our results may be somewhat biased. This needs to be kept in mind in the interpretation of results. Some recent empirical studies on the effect of certain labor market policies on employment use more refined identification strategies in that they identify from major reform events or rely on industry-level variation in employment responsiveness (e.g., Duval et al., 2020; Ciminelli et al., 2020) or use instrumental variable estimation techniques (e.g., Hijzen & Martin, 2013). While these strategies are undoubtedly better able to infer causal relationships, the studies mentioned above focus only on individual policies. Our interest in the labor market policy *mix* as well as our focus on both the short- and long-run horizons limit the possibilities for such estimation strategies.

3 | DATA AND TIME SERIES PROPERTIES

Our database is an annual panel of 20 OECD countries. We start with a balanced panel of employment and output for 53 years (1960–2012). As a result of the subsequent inclusion of the policy variables in the analysis, the time span reduces to 27 years (1986–2012) and the panel becomes slightly unbalanced. The set of countries consists of 13 pre-2004 European Union members (not including Luxembourg and Greece[6]), Australia, Canada, Japan, New Zealand, Norway, Switzerland, and the United States. We source the time series for employment and output from the Penn World Table (PWT, version 10.0).[7] Output is measured by the real GDP at constant 2017 prices

[5] Abbritti and Weber (2018) also use the lags of the policy variables in their interacted panel Vector-Autoregression (VAR) model to partially correct for possible endogeneity issues.

[6] Luxembourg and Greece are excluded because important labor market indicators are either not available for them or only available for a limited time span.

[7] The Penn World Table we use is described in Feenstra et al. (2015). The data are available for download at www.ggdc.net/pwt.

(in million US dollars), while employment is captured either by the number of persons employed or, in an alternative specification, the full-time equivalent (FTE) employment. We calculate FTE employment as the number of employees multiplied by the average annual hours worked per employed person and divided by the total number of hours a full-time employee works in a year (40 hours × 52 weeks). We relegate the description of the labor market policy variables to Section 5, while further definitions and data sources are summarized in Table A.1 in the Supporting Information Appendix.

Before we delve into the econometric analysis, we consider the time series properties of employment and output. In particular, we carry out unit root tests on the panel of 53 years both country-by-country (Augmented Dickey-Fuller test (ADF), Kwiatkowski-Phillips-Schmidt-Shin test (KPSS)) and panelwise (Levin–Lin–Chu, Fisher-ADF, and Hadri). The results of these tests are available in Tables A.2 and A.3 for the country-by-country tests and in Tables A.4 and A.5 for the panel in the Supporting Information Appendix. Because the results are qualitatively similar for the two employment measures, we only report the results for the number of employees series. Based on these test results, we conclude that, in levels, both employment and output display unit roots against being trend stationary, while their first differences are stationary for the majority of the countries. A notable exception is Spain, where the results cast doubt on whether the first difference in employment can be considered stationary.

More specifically, the country ADF tests cannot reject the null hypothesis of a unit root for the levels (except for output in Switzerland and Spain), while they always reject it for the first differences (except for employment in Spain). The KPSS test rejects the null of stationarity for the levels in most of the cases, while the results for the first differences are somewhat mixed, as they reject stationarity in quite a few cases. Because the KPSS test is known for rejecting too often, that is having a high rate of type I error, we rely here more on the ADF test. The panel unit root tests largely confirm that both series have a unit root in levels. As for the first differences, the Levin–Lin–Chu and Fisher-ADF tests clearly reject the null of a unit root. The Hadri test rejects the null hypothesis, indicating that, for some countries, the first differences cannot be considered stationary against an alternative hypothesis of some panels having unit roots. These results are similar if we run the tests on a panel excluding Spain.

With these results in mind, we continue with the assumption that output and employment have unit roots, while their first differences are stationary. However, to account for the mixed results of the unit root tests for Spain, most results in this paper are reported both with and without Spain in the sample.

Next, to assess the existence of cointegrating relationships between employment and output, we apply two types of tests. First, we perform the Johansen cointegrating rank test on each country (Johansen, 1991), while allowing for a time trend in the cointegrating equation. The resulting trace and maximum-eigenvalue statistics are reported in Table A.6, together with the relevant 5% critical values at the bottom of the table. For the majority of the countries, we find a cointegrating equation between employment and output. When the number of cointegrating equations is chosen by minimizing the HQIC (Hannan and Quinn Information Criterion), we find cointegration for all the 20 countries. Second, in the next section, we test for the presence of cointegration by examining the statistical significance of the speed of adjustment parameter when estimating Equation (2) on the pooled data. A negative and significant estimate would confirm the existence of cointegration. Because the conventional critical values for statistical significance do not apply under the null hypothesis of no cointegration, we use the critical values produced by Ericsson and MacKinnon (2002) and refined by MacKinnon (2010). As the results in Table 1 in the next section and Table A.7 show, the t-statistics for the speed of adjustment

TABLE 1 Elasticity estimates

	Number of employees				FTE employment			
Depvar: Δe	**1960–2012**		**1986–2012**		**1960–2012**		**1986–2012**	
Δy (SR elasticity)	0.317***	0.343***	0.521***	0.515***	0.408***	0.428***	0.590***	0.594***
	(0.028)	(0.027)	(0.045)	(0.041)	(0.031)	(0.031)	(0.054)	(0.050)
e_{-1} (Speed of adjustment)		−0.034***		−0.084***		−0.035***		−0.083***
		(0.004)		(0.015)		(0.004)		(0.013)
		[−8.26]		[−5.71]		[−8.63]		[−6.32]
y_{-1}		0.027***		0.061***		0.020***		0.046***
		(0.004)		(0.012)		(0.006)		(0.015)
Observations	1040	1040	540	540	1028	1028	540	540
R-squared	0.477	0.517	0.621	0.655	0.495	0.525	0.627	0.660
Long Run (LR) elasticity		0.781***		0.726***		0.584***		0.550***
		(0.134)		(0.101)		(0.151)		(0.136)
Cointegration (CI) test 5% critical values		−3.97		−4.15		−3.97		−4.15

Note: Estimation of models (2) and (3) are based on OLS, where employment is either the number of employees or fulltime equivalent (FTE) employment. Country and year dummies are included in all regressions. Parentheses include robust standard errors and brackets t-statistics. Standard errors for the long-run elasticity are obtained with the delta method. The CI test is the Ericsson–MacKinnon cointegration test, with the null hypothesis of no cointegration. Critical values (obtained from MacKinnon, 2010) must be compared with the t-statistic for the speed of adjustment parameter estimate. We can reject the null of no cointegration. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

parameter estimate (in squared brackets) lie outside the 5% critical values, indicating the existence of cointegration.[8]

4 | ELASTICITY ESTIMATES

How responsive was employment-to-output fluctuations in the past decades in developed OECD countries? To what extent does this relationship vary over time or across individual countries? Assuming a permanent output change, does an immediate response differ substantially from the long-run effect? To answer these questions, this section presents estimates of the short-run and long-run employment-to-output elasticities and explores time and country variations therein.

4.1 | Baseline estimates

We estimate the average value of the employment-to-output elasticity for our panel of OECD countries. Using the empirical model (2), we can identify both the short-run and the long-run estimates in one step and test for the existence of a cointegrating relationship. For robustness and for reasons discussed above, we also report the elasticity estimates from model (3). We consider both the full-time span (1960–2012) as well as a shorter panel (1986–2012), which corresponds to the time period for which labor market policy variables are observed. Further, we report results both with the number of employees and the FTE employment as employment variables. All regressions include a full set of country and year-fixed effects and are estimated with ordinary least squares (OLS) and robust standard errors. Table 1 reports the results.

The short-run employment–output elasticity of the average country in our sample is estimated to be 0.3–0.4 on the long panel and 0.5–0.6 on the shorter one. An estimate of 0.5, for example, indicates that a 1% increase in the volume of output is accompanied by a 0.5% instantaneous increase in employment. The fact that our estimate on the shorter— and more recent— panel is larger than the estimate on the full panel shows that, for the average country, employment has become more responsive to output shocks over the recent decades. The estimate of the long-run employment–output elasticity, obtained from the regression with the number of employees and using the EC specification, is around 0.8 for the average country. This number implies that if output permanently increases by 1%, the number of employees will rise by 0.8% in the long run, ceteris paribus.[9] We note, however, that the long-run elasticity is estimated with large uncertainty, as indicated by its 95% confidence interval ranging between 0.5 and 1. When FTE employment is used, we obtain point estimates that are somewhat higher for the short-run elasticity and lower

[8] Note that both methods we use to test for cointegration are developed for single country time series. Although there exist cointegration tests for panel data (Pedroni, 1999; Westerlund, 2007), these have been developed for large-T (and at least medium-N) panels and their reliability on panels of limited size in both dimensions is not yet proven (Behar & Hodge, 2008). Based on the Pedroni and the Westerlund panel tests, we fail to reject the null of no cointegration. With this consideration in mind, we proceed reporting estimates for both the EC and the FD models and indicate whenever the presumption of cointegration leads to qualitatively different results.

[9] Figure A.1 in the Supporting Information Appendix, which was produced by making dynamic forecasts of our estimated model with and without a 1% shock to output and taking the difference of the two forecast paths, illustrates this dynamic adjustment of employment to a 1% permanent output shock over time. As is evident from the figure, the instantaneous impact is greater in the shorter panel than in the longer one. However, the adjustment of employment as the impact of the increase in output dies out is the same for both panels, essentially resulting in similar long-run elasticities.

for the long-run elasticity. Notice, though, that the confidence intervals around these estimates overlap, meaning that the two types of estimates are not different statistically. Nevertheless, the direction of the differences is economically sensible. In the short run, some of the employment adjustment is likely to take place on the intensive margin because adjustment on the extensive margin (hiring/firing) is more costly. If there is a negative output shock, employees may temporarily work reduced hours. Similarly, if the output shock is positive, existing employees may initially be required to work overtime before new ones are hired. In the longer run, however, if the shock turns out to be permanent, extensive margin adjustment will also take place. In fact, the long-run adjustment on the extensive margin may be larger than for FTE employment if part-time employment becomes more widespread over time, which has been the case in the labor markets of most OECD countries in recent decades.

Our elasticity estimates are roughly consistent with those found for developed countries in the previous literature (e.g., Kapsos, 2006; Crivelli et al., 2012), despite the different estimation methodologies and time series length. These studies also look at a larger set of countries, including less developed economies and show that developed countries typically have larger employment–output elasticities than less developed ones.

Results reported thus far are based on the sample including Spain. Results excluding Spain are reported in Table A.7 in the Supporting Information Appendix; they are largely comparable to the estimates reported here.

4.2 | Estimates by period and country

Next, we investigate more closely how the short-run elasticity changes over time. To this end, we interact the output change variable on the right-hand side of Equation (2) with dummies for 5-year periods (except for the last period of 8 years) and estimate it on the long panel. The resulting period-specific elasticity estimates for both employment measures are plotted in Figure 1 for the sample of 20 countries and in Figure 2 for a sample without Spain.[10] The time pattern of the short-run elasticity is very similar across the different charts. It shows a clear upward trend until around the end-1990s, rising rapidly from around 0.2 (0.3 if FTE employment is considered) in the first two decades of the sample to around 0.6. After the end of the 1990s, however, the increase seems to have stalled or even reversed slightly.

Although there may be several factors underlying the observed time pattern of the short-run elasticity, labor market institutions and policies are certainly among the important ones. For example, as suggested by Blanchard et al. (2006), the sharp increase at around the middle of the sample period may be related to the introduction of more generous unemployment insurance systems in many OECD countries, as a response to large increases in the unemployment rate during the early 1980s. Similarly, the spread of the dual labor market across developed countries could have contributed to the upward trend in elasticity, as temporary employment contracts subject to less protective regulations enable stronger employment responses to output shocks (e.g., Bentolila et al., 2010). Furthermore, a labor market policy that could potentially contribute to the recent stabilization/decline of the short-run elasticity is a short-time work scheme, which has become a popular policy tool in OECD countries during the Great Recession (Hijzen & Martin, 2013). Short-time work programs intend to preserve jobs by encouraging work sharing and by

[10] Estimating the FD specification (3) results in very similar graphs.

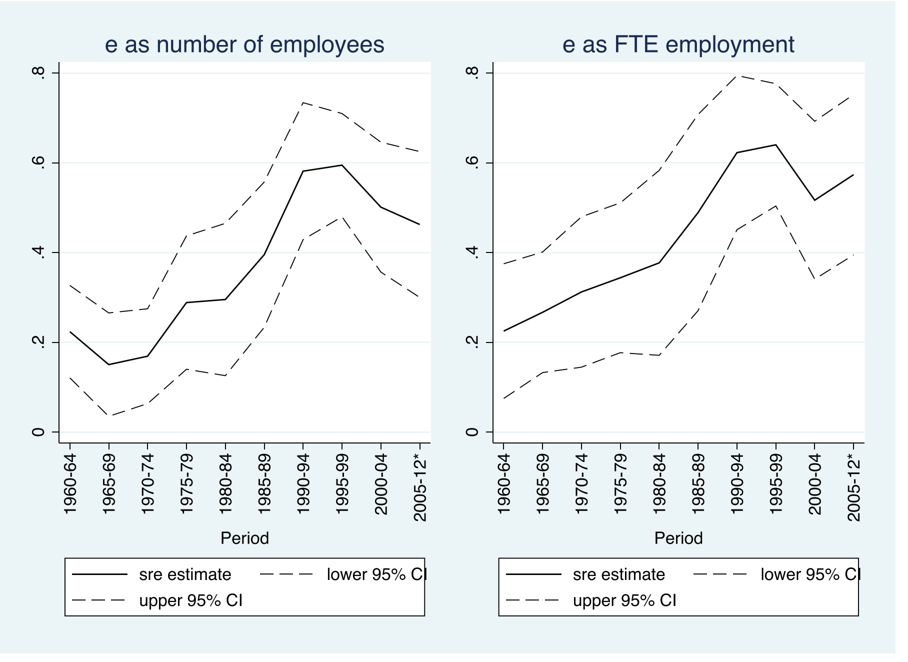

FIGURE 1 Short-run elasticity estimates over time [Colour figure can be viewed at wileyonlinelibrary.com]

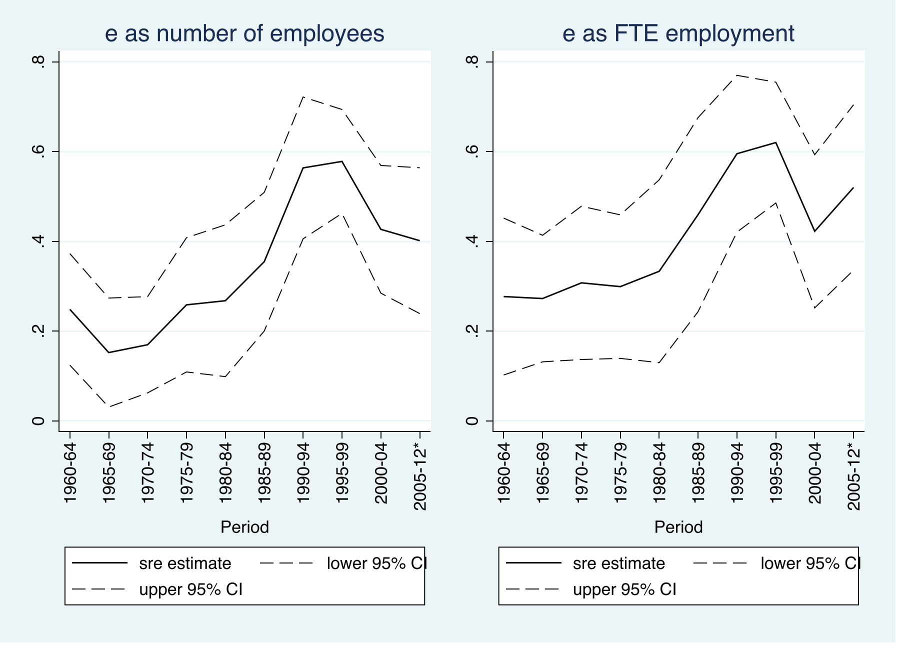

FIGURE 2 Short-run elasticity estimates over time (without Spain) [Colour figure can be viewed at wileyonlinelibrary.com]

providing income support to workers whose hours are reduced, enabling a more muted response of employment to negative shocks.

Beyond a general trend in labor market institutions, the nature of the output shock and the type of countries affected by these shocks may also influence the strength of employment response. In particular, employment may respond to a positive output shock differently from a negative one because the rigidities in the labor market usually have an asymmetric nature (e.g., downward nominal wage rigidities). And since the degree of labor market rigidities varies from country to country, it also matters which country is affected by a shock. The literature on business cycle asymmetry documents that asymmetric labor market rigidities can amplify the business cycle in downturns, meaning that employment and output rise slowly in the expansionary and fall fast in the contractionary phases of the cycle (Abbritti & Fahr, 2013; McKay & Reis, 2008). It is also shown that in most European countries the degree of this asymmetry is larger for employment than for output, implying that the employment-to-output elasticity of these countries is larger during economic downturns than during expansions (Abbritti & Fahr, 2013; Kolasa et al., 2021).[11]

We use our empirical model to carry out a simple test for the presence of such asymmetries. First, we identify periods of economic slowdown for each country in our sample, where we rely on the literature on turning point analysis but with the important distinction that we have annual data.[12] Second, we create a binary variable for the slowdown observations and include this variable and its interaction with output growth in the baseline regression. We run regressions for the whole sample as well as for a restricted sample of European countries, in order to check if the degree of asymmetry is indeed greater for Europe. The regression results reported in Table 2 suggest the existence of some degree of asymmetry. The point estimates for the interaction term are positive in all specifications— but are only statistically significant for the European subsample. Assessing the relative frequency of slowdowns in our sample by decade in Table A.8 we found that contractionary years were most common during the 1980s and 1990s. These findings indicate that some of the documented rise in the estimated short-run elasticity over these decades may be due to more frequent economic slowdowns causing larger employment responses in many of our sample countries.

Finally, we explore the extent of intercountry heterogeneity in the short-run employment-to-output elasticity. Specifically, we estimate model (2) by interacting the output change variable with country dummies to obtain country-specific elasticities. Figure 3 reports the short-run elasticities by country with 95% confidence intervals, estimated either on the long panel or the short panel and plotted in descending order of the elasticity obtained from the long panel. Clearly, a

[11] The asymmetry found in Europe is consistent with Kolasa et al. (2021), who find that— against the conventional view— hours per worker in European countries are much less procyclical than in the United States, and in some economies even comove negatively with output. This means that hours per worker may increase in Europe in times of negative shocks, aggravating the upward pressure on unemployment.

[12] We generate the GDP growth cycle for each country with the band-pass filter of Baxter and King (1999) and identify slowdown periods as years on the contractionary phases of the cycle. Contractionary phases are the years from each peak until the next trough. Harding and Pagan (2002) developed an algorithm to find peaks and troughs for quarterly data. We apply their method— using the user-written Stata command sbbq— but with altered parameters because of the annual frequency. We set the window over which local minima and maxima are computed as well as the minimum phase length to 1 year (instead of two quarters optimal for quarterly data), while the minimum cycle length is set to 2 years (instead of five quarters). We are aware of the problem that higher frequency information is lost when annual data are used. As it turns out, the solution provided by the Harding–Pagan algorithm under the above parameter values is trivial. The years of slowdown that we identify are exactly the same as if we simply take the years in which the GDP cycle declines relative to the previous year. Figure A.2 indicates periods of economic slowdown by country with gray color.

TABLE 2 Business cycle asymmetry of the elasticity

Depvar: Δe	Number of employees				FTE employment			
	All countries		Europe only		All countries		Europe only	
Δy	0.331***	0.366***	0.355***	0.357***	0.392***	0.422***	0.369***	0.369***
	(0.040)	(0.039)	(0.045)	(0.043)	(0.046)	(0.046)	(0.051)	(0.051)
Slowdown	0.002	0.002	0.001	0.000	−0.001	0.000	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$\Delta y\times$ slowdown	0.049	0.049	0.118**	0.109**	0.088	0.087	0.152**	0.143**
	(0.053)	(0.049)	(0.057)	(0.054)	(0.056)	(0.053)	(0.063)	(0.061)
e_{-1}		−0.035***		−0.032***		−0.036***		−0.034***
		(0.004)		(0.008)		(0.004)		(0.008)
y_{-1}		0.027***		0.027***		0.021***		0.020***
		(0.004)		(0.005)		(0.006)		(0.007)
Observations	1040	1040	780	780	1028	1028	778	778
R-squared	0.483	0.524	0.517	0.542	0.499	0.530	0.504	0.520

Note: Estimation of models (2) and (3) are based on OLS, where employment is either the number of employees or fulltime equivalent (FTE) employment. Slowdown is a binary variable which is 1 for country-years during the contractionary phase of the growth cycle. The cycle is computed using a band-pass filter and peaks and troughs are selected based on Harding and Pagan (2002) but using annual series. Country and year dummies are included in all regressions. Parentheses include robust standard errors. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

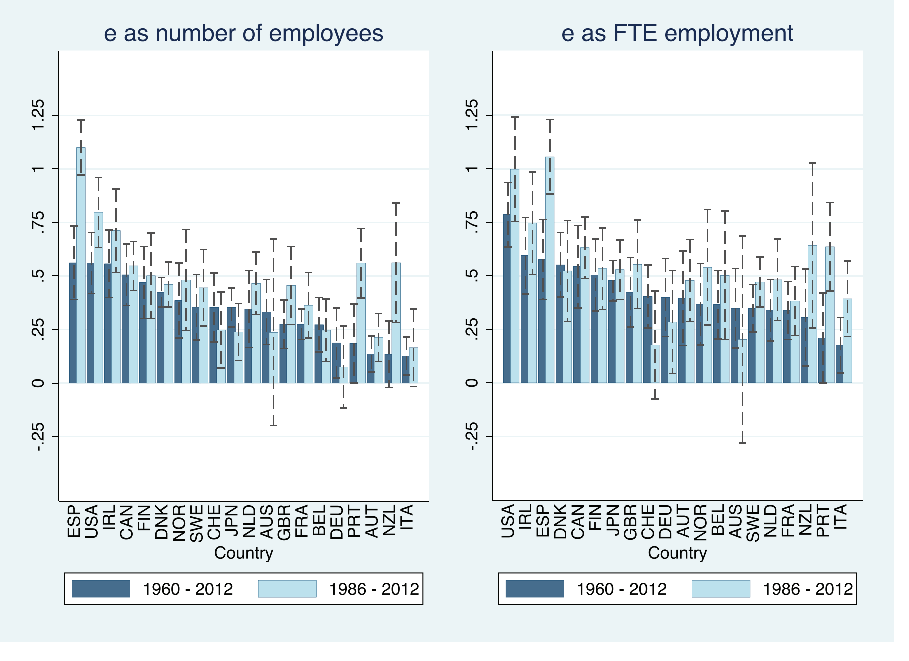

FIGURE 3 Estimated short-run elasticities by country [Colour figure can be viewed at wileyonlinelibrary.com]

considerable intercountry variation exists in the responsiveness of employment-to-output. Countries with the largest elasticities on the left-hand side of the graph (Spain, the United States, Ireland) have at least three times higher employment responsiveness than countries on the right-hand side (e.g., Italy, Austria). The point estimates typically get larger as the sample is shortened to the more recent decades; for most countries, however, these changes are not statistically significant, as the overlapping confidence intervals indicate. Exceptions are Spain and Portugal, whose short-run elasticities are significantly larger when estimated on the short (more recent) panel than on the long panel.[13] Spain is in a special position among the countries in that its estimated employment-to-output elasticity is among the highest irrespective of the length of the sample or how we measure employment. Moreover, the responsiveness of the number of employees to output shocks, as estimated on the more recent panel, is significantly higher than the elasticity of any other country in our sample. Bentolila et al. (2010) investigate the specificities of the Spanish labor market (in comparison to France) and explain the high responsiveness of employment-to-output fluctuations with the exceptionally high share of temporary employment contracts in Spain and the large existing gap between the dismissal costs of permanent versus temporary workers.[14]

5 | LABOR MARKET POLICIES IN OECD COUNTRIES

The remaining part of this paper investigates how labor market institutions and policies influence the employment–output elasticity. In particular, we focus on three policies which we identify as key elements of the flexicurity regime: (i) the generosity of unemployment benefits, (ii) the flexibility of the labor market with respect to hiring and firing, and (iii) the expenditure on training programs to enhance employability. The first two are considered passive labor market policies, while the latter is a prime example of active labor market measures. Our interest in this set of labor market policy instruments is based on the fact that they reflect the key pillars of *flexicurity* — which has been widely endorsed as a means to reconciling the need for the flexibility required to adjust to negative shocks with that for adequate social safety nets (see, e.g., Blanchard et al., 2014). The descriptive statistics of the three policy variables— together with additional labor market indicators that we discuss later— are reported in Table 3.

The generosity of the unemployment benefit system is proxied by the gross replacement rate (*grr*), which is the ratio of the gross unemployment benefit level to the previous gross earnings of the average production worker in a country.[15] *grr* is a summary measure, calculated by the OECD as an average for two earnings levels, three family situations, and three durations of unemployment. The series are available for every odd year between 1961 and 2011. To complete the time series, we fill in the even years with linear interpolation.[16] The benefit replacement rate of

[13] These findings are largely unchanged (though with some changes in the country ranking) when estimating the first-difference specification.

[14] According to OECD data, no country included in our sample has ever had a higher share of temporary employment than Spain. For the average of the period 1990–2019, for which data on temporary employment is available, this share is 30% for Spain and only 10% for the rest of the countries.

[15] We are aware that the net replacement rate, the ratio of net benefits to net earnings, would be a better measure for our purposes. The net rate is, however, produced by the OECD only since 2001 and hence would dramatically shorten the time series dimension of our study.

[16] The indicator was originally constructed for the OECD Jobs Study (OECD, 1994) and calculated for the average production worker. This series is available until 2005. Numbers for the more recent years are calculated for the average worker

TABLE 3 Policy variables and their descriptive statistics

Variable	Full name	*N*	Mean	Std. Dev.	Min	Max
The three pillars of flexicurity						
grr	Gross unemployment benefit replacement rate	529	0.311	0.120	0.025	0.652
flex	Labor market flexibility index	529	4.055	0.965	1.677	5.746
train	Training expenditures per unemployed (log)	529	7.060	1.250	3.150	9.252
Additional policy variables						
uden	Trade union density	529	0.366	0.207	0.078	0.874
cove	Adjusted bargaining/union coverage rate	529	0.678	0.269	0.130	1.000
cent	centralization of collective bargaining index	529	2.480	1.047	0.875	5.000
taxw	Tax wedge	529	0.516	0.125	0.298	0.856
part	Prevalence of part-time employment	529	0.181	0.077	0.000	0.333
temp	Temporary employment share	365	0.121	0.064	0.037	0.350

Note: Own calculations based on data sources listed in Table A.1. The tax wedge is from Abbritti and Weber (2018). The descriptive statistics refer to the period 1985–2011 and 20 countries including Spain. Training expenditures are adjusted for purchasing power parity (PPP) differences and expressed in the natural logarithm of PPP US dollars.

the average country in our sample is approximately 0.3, implying that the unemployment benefit is close to one-third of the previous gross earnings. The degree of benefit generosity varies considerably across countries: for example, Denmark, the Netherlands, and Norway have higher-than-average replacement rates, while Canada, Japan, Great Britain, and the United States provide weaker unemployment protection (Figure A.3). The variation over time is also nonnegligible, with Italy having increased its replacement rate considerably in the first half of the sample and with Denmark and the Netherlands decreasing it somewhat in the more recent years.

Finding an explicit measure of labor market flexibility for both hiring and firing is empirically challenging. However— as is well demonstrated in the theoretical literature (see, for instance, Cacciatore et al., 2016; Zanetti, 2011— employment protection affects both job creation and job destruction. Hence, greater restrictions that are aimed at protecting workers from job losses, also lead to heightened disincentives to hire these workers. Thus, to capture flexibility in this paper, we adopt the inverse of the OECD employment protection index (EPL), available for the years 1985–2013. The EPL is a composite index, calculated as a weighted average of the employment protection subindices for the dismissals from regular contracts and the use of temporary contracts.[17] Our *flex* variable is the inverse of the EPL index ($flex = 6 - EPL$), ranging from 0 to 6, and showing how easy it is for employers to lay off regular workers (firing) or to employ them on temporary work contracts (hiring), as laid down by a country's regulatory system. Hence, a higher value of *flex* means a more flexible labor market, which entails both lower costs of hiring and firing. According to *flex*, the least flexible labor markets can be found in Southern Europe, while the most flexible ones are in the United States, Canada, Great Britain, and Ireland. Time series graphs

(both production and nonproduction) using the OECD tax-benefit models. To minimize the impact of this methodological break, we rely on the former series until 2005 and extend it until 2011 by using the corresponding time changes in the latter series. This simple extrapolation naturally assumes that the dynamics of the two series are similar. Comparing the time changes of the two series for the years where both are available (2001–2005) we find that they indeed correlate strongly, with a correlation coefficient around 0.9.

[17] Following the OECD practice, $\mathrm{EPL} = \frac{7}{12}\mathrm{EPRC} + \frac{5}{12}\mathrm{EPT}$, where EPRC is the subindex for dismissals from regular contracts and EPT is the subindex for the use of temporary contracts. A detailed description is provided in OECD (2013).

in Figure A.4 also reveal a tendency among countries with less flexible labor markets to increase flexibility over time, which especially applies to Germany, Sweden, Spain, Italy, and Portugal. Interestingly, Spain belongs to the least flexible countries in our sample based on *flex*, which is clearly at odds with the observed large flows of temporary workers into and out of employment in Spain (Bentolila et al., 2010). The reason is that the EPL index does not reflect the large gap between the dismissal costs between permanent and temporary workers. Therefore, we conclude that, despite its attractiveness and widespread use in the literature, the EPL index— hence *flex*— is an inadequate indicator for the Spanish labor market.

We capture the importance of active labor market policies with the ALMP expenditures on training programs per unemployed person (*train*). This category of training is defined to only include programs (both institutional and workplace based) that are *targeted* at unemployed persons or employees at high risk of becoming unemployed. Training programs which are part of general education are excluded from this definition. Although the full spectrum of ALMPs includes a variety of different measures such as job creation, rehabilitation, or job sharing, we concentrate on training for two main reasons. First, we believe it best represents the motive behind the flexicurity idea of actively promoting workers' employability. Second, training makes up for the largest share of ALMP expenditures, totaling up to 45% for the average country-year in our sample. To make our *train* variable comparable across countries, we convert it to a common currency (US dollar) and adjust for PPP differences. Finally, we express it in a natural logarithm. All data for the calculation of this variable are sourced from the OECD, with 1985 as the starting year. The sample average of *train* is 7.06, that is, 1164 PPP US dollars per unemployed person annually. As for cross-country differences, training expenditures turn out to be higher (and increasing) in European countries, as compared with non-European OECD members, which suggests the relatively high importance of ALMPs within Europe (Figure A.5).

The above three labor market policy measures (*grr*, *flex*, and *train*) are obviously not applied in isolation but form parts of a policy regime. Cross-country policy correlations in Table A.9 reveal that countries with generous unemployment benefits tend to spend more on training schemes. This positive relationship is highly statistically significant and has become stronger over time. One possible reason for this is that generous unemployment benefit schemes need to be complemented with active labor market policies in order to sufficiently motivate the unemployed to seek work (Blanchard et al., 2014; Nickell et al., 2005). Furthermore, the data also reveal that the generous systems tend to coexist with less flexible labor markets, though this relationship becomes statistically significant only in the more recent years of our sample period. In fact, along these three labor market measures, most countries in our sample can be broadly categorized into two regimes, one with a flexible labor market and low generosity (which we term "liberal") and one with generous programs and less flexibility ("interventionist"). The *liberal* regime mostly exists in Anglo-Saxon countries (especially in Canada, Great Britain, and the United States), the *interventionist* in most countries of continental Europe and Scandinavia.

Besides the three labor market policies that characterize the flexicurity aspect, our analysis also accounts for the role of further labor market institutions in influencing the employment-to-output elasticity. We aim at a relatively comprehensive representation of labor market institutions while acknowledging limitations due to data availability. Motivated by recent literature on the business cycle implications of labor market institutions (Abbritti & Weber, 2010, 2018; Gnocchi et al., 2015), we consider the following additional policy variables: the trade union density; the collective wage agreement coverage rate among employees; the degree of centralization of the wage-bargaining process (a summary index ranging between 0 and 5); and the tax wedge which measures the difference between the employer's labor costs and the net salary received by the employee.[18] Furthermore, we also account for the varying importance of non-full-time and non-permanent employment across countries and over time. Because official OECD data on the share of part-time and temporary contracts are of limited availability, we experiment with an outcome-based measure. Using data on the average annual hours worked per employee (avh) from the Penn World Table and assuming that full-time annual working hours equal 2080 (52 weeks × 40 hours per week) we generate a variable for the prevalence of part-time employment as $1 - \text{avh}/2080$. Note that this measure does capture not only the importance of part-time employment but also some temporary employment, namely employment shorter than a year (seasonal work). Finally, in a robustness check, we also consider OECD data on the share of temporary employment contracts, which however considerably reduces our sample size. Table 3 presents summary statistics of all these policy variables, while Table A.1 lists the data sources.

6 | ELASTICITIES AND THE LABOR MARKET POLICY MIX

In what follows, we investigate how individual policies and the policy mix relate to the responsiveness of employment-to-output shocks. Due to the aforementioned peculiarities of the Spanish labor market, we have decided to exclude Spain from the baseline estimation, but conduct a separate discussion and robustness check on it.

As our baseline, we estimate the EC specification while including the three flexicurity policy variables and their interactions with output growth and the lagged values of employment and output (Δy_{it}, y_{it-1}, e_{it-1}). For robustness, we also estimate the FD specification as in Equation (4). The estimation results are reported in Table A.11 for the EC and Table A.12 for the FD specification. All the presented regressions include country-fixed effects, common year effects, and a small set of country-time-specific control variables that can potentially affect employment elasticities. The latter includes the size of the working-age population, openness to international trade, and the size of the government (see a more detailed description in Table A.1).[19] Though not reported, these control variables turn out to be significant predictors of employment growth in most regressions— while population growth and the size of the government associates positively with employment, the coefficient for trade openness is negative, though not quantitatively large. Furthermore, in the last two regressions, we also account for the role of additional labor market indicators and their interactions with output growth and the lagged values of employment and output. We chose to include the first five of the six additional Labor market indicators (LMIs) listed in Table 3, which is the largest possible selection without compromising the sample size. Similar to the three flexicurity policy variables, the LMI variables are centered and lagged by 1 year.

The estimates in Table A.11 can be interpreted as follows. The first row shows the short-run employment–output elasticity estimate for the average country, which is around 0.5 and robust across model specifications. The coefficients for the interactions of output growth with the policies capture how this elasticity changes if a country deviates in a policy measure from the average. The estimate of 0.843 (significant at 1% level) in the second row of column 4, for example, means that,

[18] We are grateful to Mirco Abbritti and Sebastian Weber for generously providing us with their tax wedge variable used in Abbritti and Weber (2018).

[19] Panel unit root tests indicate that trade openness and government size are first-order integrated while the working-age population is second-order integrated (Table A.10). This motivates us to include in the regressions the annual changes of trade openness and government size and the change in the annual change (second-order difference) of the working-age population.

all things being equal, a country with an unemployment replacement rate that is higher than the average by 0.1 (10 percentage points) tends to have a 0.0843 higher-than-average short-run elasticity. Furthermore, a one-point higher flexibility index corresponds to a 0.113 higher elasticity (estimate of 0.113*** in column 4). These results show that greater unemployment benefit and flexibility in the labor market tend to amplify the short-run effects of output changes on employment and, as we will discuss below, they are consistent with theoretical predictions. In contrast, training expenditure is not found to have a significant explanatory power for the short-run elasticity; this is likely due to the fact that training takes longer to have effects on workers' productivity. Note that the coefficient estimates in the different columns of the table are reasonably stable. Nevertheless, the inclusion of the additional LMIs and their interactions (20 additional regressors) in the last two columns is accompanied by a substantial loss of degrees of freedom in an already demanding empirical specification and causes some of the key estimates to lose their statistical significance.

Columns 5 and 7 include the results for all possible interactions between the flexicurity policy variables ($grr \times train$, $grr \times flex$, $train \times flex$, and the triple interaction $grr \times train \times flex$). Broadly, the results show that policies do not only matter individually but also in combination. In particular, we find that although when taken individually, higher benefit generosity and more labor market flexibility are both associated with a higher employment responsiveness, there is an offsetting effect when the two are considered jointly. Testing the joint significance of the terms with policy interactions (test results are reported at the bottom of the table) also confirms that accounting for policy complementarities significantly improves the model fit. We conclude that the existence of such complementary effects among the policy variables indicates that the consequences of any single policy change are affected by the broader policy mix within which it takes place. Finally, the above results are largely unchanged when we estimate the FD specification (Table A.12).

The results above are likely to reflect the complex interactions and the feedback effects of policies on output–employment dynamics. Our findings are consistent with existing theoretical predictions. From the perspective of a search and matching framework, a higher generosity of unemployment benefit increases the responsiveness of employment to shocks (Molana et al., 2018; Shimer, 2005; Zanetti, 2011) as our estimates reveal. The driving force is that a higher benefit raises the workers' outside option and their threat point in wage bargaining, which reduces match profitability. This in turn makes firms more "sensitive" and implies that a shock to productivity will result in greater movement (in percentage terms) in terms of vacancy creation and employment. This result has been shown to hold whether job separation is exogenous (e.g., Hagedorn & Manovskii, 2008) or endogenous (Zanetti, 2011). The effects of greater labor market flexibility are, at a theoretical level, more nuanced. On the one hand, greater flexibility in hiring (e.g., lower vacancy creation costs) has been shown to induce higher employment responsiveness. On the other hand, increased flexibility in terms of firing rules can have a dampening effect on employment fluctuations, since it raises job match surplus, making firms less sensitive to a productivity shock. Thus, when both policies are implemented jointly (i.e., via increases in both hiring and firing flexibility), the impact on employment fluctuations will depend on which effect dominates. The positive impact on the short-run elasticity obtained in this paper suggests that empirically the flexibility in hiring regulation may have been the dominant force behind employment fluctuations. This is consistent with the fact that the strictness of firing regulation has been found empirically to have a mild (and even ambiguous) effect on firms job creation activities and thus employment (e.g., OECD, 2013). The offsetting effects of unemployment generosity and flexibility also produce an interesting result, which can also be explained from a theoretical perspective. First, as already established, the effects of higher hiring flexibility on employment elasticity are driven by the reduction in hiring frictions. Second, when there is lower job creation, firms have incentives to create more jobs. But this effect can be exactly (or more than) offset by increases in unemployment benefits that result in higher wages and thus reduce job match profitability.

A less-studied aspect of labor market policies is the impact of ALMPs such as training programs. When training expenditure is assumed to be a mere cost to firms (as theoretically modeled by, for example, Onwordi, 2016; Pissarides, 2009; Stähler & Thomas, 2012), it leads to lower job match surplus and profitability to the firms. In this instance, even a small productivity shock can lead to large fluctuations in employment. However, as argued by Molana et al. (2018), training is likely to be productivity enhancing; allowing for this, they show that training expenditure can have a moderating effect on employment fluctuations in response to productivity shocks. We, however, fail to find empirical support for these predictions since our results show mostly statistically insignificant effects.

To better appreciate the impact of these policies, we study their complementarities more systematically. Particularly, we examine the effects of, ceteris paribus, a one-standard-deviation upward move in each policy variable under three labor market regimes: *average*, *liberal*, and *interventionist*. The policy regimes differ in the values that the nonchanging policy variables take. The hypothetical *average* regime sets the policy variables at their sample average values. Because the policy variables in the regressions are centered, their average value is zero. The noncentered averages are 0.309 for *grr*, 7.116 for *train*, and 4.114 for *flex* for the sample that excludes Spain (Table 3 reports the noncentered averages with Spain). The *liberal* regime assumes a flexibility index at the 75th percentile of its sample distribution (*flex* = 4.955, noncentered) and the other two policy variables at their 25th percentiles (*grr* = 0.219, *train* = 6.626, both noncentered). The *interventionist* regime, in contrast, is characterized by a flexibility index at its 25th percentile (*flex* = 3.425) and the other two at their 75th percentiles (*grr* = 0.388, *train* = 7.919). These interquartile differences roughly coincide with the average differences between the typical liberal and interventionist countries in our sample.

We rely on expression (5) to calculate the effects on the short-run elasticity, using the estimated coefficients in Table A.11, reported either in column 4 (without policy interactions) or in column 5 (with policy interactions). Without policy interactions, the effect on the short-run elasticity of a one-standard-deviation upward move in the gross replacement rate, for instance, is obtained as $\hat{\gamma}_2^{\tilde{grr}} \times \hat{\sigma}^{grr} = 0.843 \times 0.122 = 0.103$, where $\hat{\gamma}_2^{\tilde{grr}}$ is the estimated coefficient for the interaction of Δy with the gross replacement rate in column 4, and $\hat{\sigma}^{grr}$ is the sample standard deviation of the gross replacement rate. With policy interactions, the other policy variables will also matter and the change in the short-run elasticity becomes

$$sre\text{ change} = \left(\hat{\gamma}_2^{\tilde{grr}} + \hat{\gamma}_2^{\tilde{grr}\times\tilde{train}} \times \tilde{train} + \hat{\gamma}_2^{\tilde{grr}\times\tilde{flex}} \times \tilde{flex} + \hat{\gamma}_2^{\tilde{grr}\times\tilde{train}\text{ x }\tilde{flex}} \times \tilde{train} \times \tilde{flex}\right) \times \hat{\sigma}^{grr},$$

where the $\hat{\gamma}_2$s are the coefficients for the corresponding policy interactions with Δy in column 5 of Table A.11 and the policy variables $\tilde{train}$ and $\tilde{flex}$ are set at their regime-specific "centered" values. The results of this exercise are reported on the left-hand side of Table 4, together with standard errors which reflect parameter uncertainty. For robustness, the right-hand side of the table also reports the estimates based on the regressions with additional LMIs in columns 6 and 7 of Table A.11.

When policy complementarities are not allowed for, the estimated effects do not depend on the policy regime (first column): a move towards more unemployment benefit generosity and a more flexible labor market both correspond to larger short-run employment–output elasticities

TABLE 4 Effects on the short-run elasticity of σ-increases in the policy variables

	Baseline				With additional LMIs			
Policy	Without	With interactions			Without	With interactions		
variable	Interactions	Average	Liberal	Interventionist	interactions	Average	Liberal	Interventionist
grr	0.103***	0.122***	0.039	0.156**	0.131***	0.138***	0.096	0.155**
	(0.040)	(0.043)	(0.052)	(0.063)	(0.044)	(0.051)	(0.077)	(0.061)
train	0.037	0.077	−0.001	0.041	0.040	0.039	0.161	0.019
	(0.032)	(0.050)	(0.166)	(0.046)	(0.031)	(0.049)	(0.191)	(0.048)
flex	0.108***	0.111***	0.186***	0.073	0.062*	0.063	0.122**	0.010
	(0.033)	(0.035)	(0.049)	(0.054)	(0.037)	(0.040)	(0.060)	(0.056)

Note: The figures show the effects of a one-standard-deviation increase in a policy variable on the short-run employment–output elasticity. Baseline estimates are based on columns 4 (without interactions) and 5 (with interactions), those with additional LMIs on columns 6 and 7 of Table A.11. Average, liberal, and interventionist regimes differ in the values the nonchanging policy variables are assumed to take. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(both roughly by 0.1). When, however, policy complementarities are taken into account, it matters considerably in which labor market regime a certain policy change takes effect. Specifically, our estimation reveals that in *liberal* regimes, making the unemployment benefit more generous does not result in a significantly higher short-run elasticity. In a similar vein, a reform towards more labor market flexibility does not lead to a significantly higher short-run employment responsiveness under an *interventionist* policy mix. Indeed, as noted previously, it is possible that the effects of the two policies can exactly offset each other, which may explain the nonstatistically significant effects. In particular, as argued by, for example, Hornstein et al. (2005) in a paper that theoretically assesses labor market fluctuations, the increase in the generosity of unemployment benefit needs to be sufficiently high in order to induce large fluctuations in employment. This would be the case especially if accompanied by greater hiring and firing flexibility. An important implication of these results is, therefore, that the impact of a specific policy on employment fluctuations depends on the initial policy mix and levels.

These findings are qualitatively robust to the inclusion of additional LMIs in the regression (right-hand side of Table 4) or to estimate the FD specification of the model (Table A.13), although these changes make the impact of a move towards more flexibility qualitatively smaller. In contrast, the results for the *flex* indicator are not robust to the inclusion of Spain in the sample, which is shown on the right-hand side of Table A.14 in the Supporting Information Appendix. This is consistent with employment in Spain being very responsive to output shocks, which is however at odds with the comparatively low value of *flex* for Spain. As said, this contradiction is due to the fact that in Spain there is a very high share of temporary work contracts, where employment protection is significantly lower than for regular contracts (Bentolila et al., 2010), a feature not captured by our empirical model. To account for this, we reproduce the estimation with additional LMIs (and their interactions with output growth), where the set of LMIs also includes a variable for the share of temporary work contracts (temp). These results are shown on the right-hand side of Table A.14. Because variable temp is observed only for two-thirds of the observations in our sample, the results are not directly comparable to our baseline estimates. It is however notable that our earlier significant positive coefficient for *flex* is restored once the varying importance of temporary contracts across countries is accounted for.

Further, using the estimates from the EC regressions, we also calculate the corresponding changes in the long-run elasticity, which we report for the baseline case in Table A.15.[20] The numbers suggest that the effects of these policies found on the short-run elasticity mostly fade out in the long run. Interestingly, the point estimates for training turn negative in the long run (though being not statistically significant), which could suggest that training has a lock-in effect, reflecting a greater propensity of firms to hold on to trained employees in response to shocks (Molana et al., 2018).[21]

7 | REFORMING TOWARDS FLEXICURITY

In light of the results obtained so far, in this section, we consider the effects of a flexicurity reform on the employment–output elasticity. A flexicurity regime is characterized by a combination of generous unemployment insurance and training programs and a relatively flexible labor market. Our specific objective is to examine the effects, in each country, of "reforming" its existing policy mix in 2010 towards the policy mix characterizing the Danish system, a well-known example of a flexicurity regime (e.g., as discussed in Koster et al., 2011; Räisänen et al., 2012).

Denmark has one of the highest unemployment benefit replacement rates and training expenditures among the countries in our sample, while it scores close to the average in the flexibility index. Table A.16 in the Supporting Information Appendix displays the countries' policy distances from Denmark in 2010. Clearly, these figures imply that a reform in the direction of flexicurity would require most countries within our sample to increase their benefit generosity and raise their expenditures on training, while, simultaneously adjusting the flexibility of their labor markets towards an average level. In light of this, our priors are that a reform in the direction of flexicurity is likely to increase or leave the short-run elasticity of all countries roughly unchanged. The main reason, as suggested by our data, is that this reform would make each country's regime more generous or leave them almost as generous as they were. Moreover, for most countries with regimes characterized by a generosity similar to Denmark, the reform would mean an increase in labor market flexibility. Less straightforward is the case of countries with *liberal* regimes, where the parallel moves towards less flexibility and more generosity work in opposite directions, as revealed in the previous section.

We report the estimated changes in the short-run elasticity resulting from the policy shift described above in Figure 4, in descending order, together with the 95% confidence intervals. The computation was based on expression (5) and the estimated coefficients in Table A.11, column 5, and performed as

$$sre\ \text{change}_i = \sum_k \hat{\gamma}_2^k \left[\tilde{P}^k_{\text{DNK},t=2010} - \tilde{P}^k_{i,t=2010} \right],$$

where $\tilde{P}^k$ denote the policy variables and their interactions and DNK stands for Denmark.

[20] This we do by evaluating expression (6) before and after the one-standard-deviation increase in a given policy variable and taking the difference. Corresponding standard errors are obtained with the Delta method. For the calculation "without interactions," we assume that policy variables take their average values. Taking other values does not influence the results qualitatively.

[21] In a search and matching open economy model, Molana et al. (2018) show that increases in training expenditure can reduce employment volatility in response to exogenous shocks even in the presence of more generous unemployment insurance.

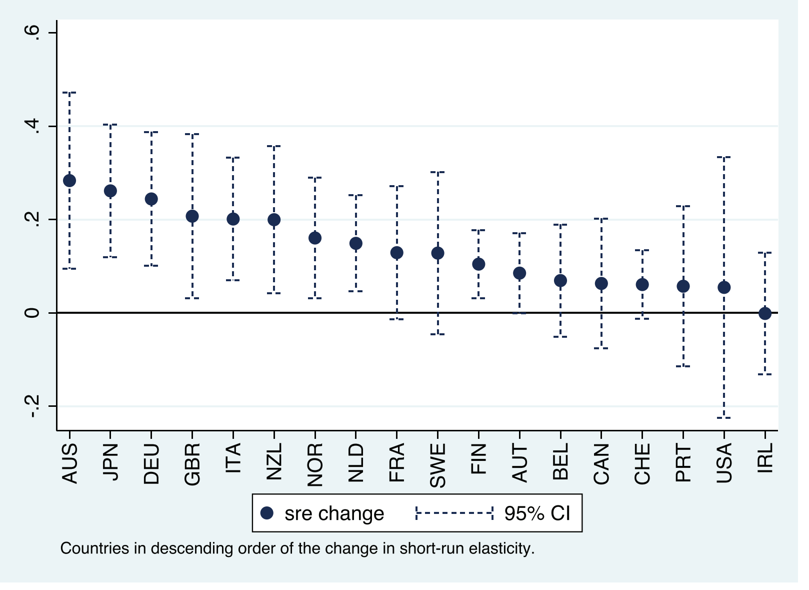

FIGURE 4 Impact of a flexicurity reform on the short-run elasticity [Colour figure can be viewed at wileyonlinelibrary.com]

Consistent with our priors, we find that a flexicurity reform would increase or leave unchanged the short-run employment–output elasticity in all of our sample countries. This result is in line with the philosophy underlying the flexicurity system, which does not aim to prevent employment separations (hence the high short-run elasticity), but to provide targeted incentives and support for the unemployed to return to work. The highest increases of above 0.2 are obtained for Australia, Japan, Germany, and Great Britain. Positive and significant effects are also obtained for Italy, New Zealand, Norway, the Netherlands, and Finland. At the other end, the estimated effects are small or not statistically significant for, among others, Ireland, the USA, Portugal, and Canada.

The group of countries with the highest expected effects includes those with considerably less generous systems than Denmark (e.g., Australia, Great Britain). An initially less generous system, in itself, however does not guarantee a positive effect. For two of the least generous countries, the United States and Canada, we do not find statistically significant effects. Under a flexicurity reform, these two countries would need to reduce the flexibility of their labor markets, which would counteract the effects of the reform package on output–employment dynamics.

The above implications of a flexicurity reform on the short-run elasticity are largely robust when relying on estimates from the FD specification, as can be seen by comparing the first columns of Tables A.17 and A.18. The groups of countries for which statistically significant increases in the short-run elasticity are predicted are almost identical under the baseline and the FD specifications, and the relative sizes of the country estimates also remain largely unchanged. We obtain smaller estimates for most countries however when additional LMIs are included in the EC model. Nevertheless, the predicted effects for the four most affected countries (Australia, Japan, Germany, and Great Britain) remain large and statistically significant even under this model.

These results indirectly lend further support to the importance of taking into account all—combined and complementary— effects when we evaluate a labor market reform.

TABLE 5 Robustness with country trends and per-capita transformation

Policy variable	Country trends: Without interactions	Country trends: With interactions: Average	Liberal	Interventionist	Per capita transformation: Without interactions	Per capita transformation: With interactions: Average	Liberal	Interventionist
grr	0.084**	0.077	0.049	0.124*	0.085*	0.091*	0.005	0.126*
	(0.041)	(0.047)	(0.056)	(0.067)	(0.046)	(0.048)	(0.054)	(0.066)
train	0.084**	0.069	0.156	0.077	0.044	0.064	0.056	−0.019
	(0.034)	(0.050)	(0.181)	(0.051)	(0.037)	(0.048)	(0.166)	(0.047)
flex	0.115***	0.085**	0.104**	0.051	0.077***	0.052	0.110**	0.045
	(0.034)	(0.036)	(0.051)	(0.059)	(0.030)	(0.032)	(0.045)	(0.052)

Note: The figures show the effects of a one-standard-deviation increase in a policy variable on the short-run employment–output elasticity based on regressions of the FD specification. The left part of the table reports results from regressions with linear country trends, the right part reports results from regressions with per-capita transformed employment and output. Additional LMIs are not included. The regressions with per-capita transformation exclude from the set of control variables the change in the working-age population growth. The sample excludes Spain. Average, liberal, and interventionist regimes differ in the values the nonchanging policy variables are assumed to take. Standard errors are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

As for the long-run elasticity, our model does not predict statistically significant changes (Figure A.6). Hence, no evidence is found that a flexicurity reform would have an impact on the responsiveness of employment-to-output in the long run.

8 ROBUSTNESS

8.1 Heterogeneous country trends

Our simple model accounts for country differences that are constant over time but cannot account for the possibility that secular trends in the employment–output relationship due to, for example, demography, productivity, or technology, differ from country to country. In one robustness exercise, we aim to allow for such heterogeneous trends by adding country-specific linear time trends to the model. First, we reproduce Tables 1 and A.7 while including country-specific linear time trends and find that the short-run elasticity estimates are robust to this change.[22] We also find evidence that the cointegrating relationship exists under the assumption of such trends. Nevertheless, for the short panel, the estimates for the speed of adjustment parameter and the long-run elasticity are significantly larger in absolute value than the baseline figures, and the long-run elasticity becomes larger than one and thus falls outside the conventional range of such estimates. This suggests that the inclusion of country trends in the EC regression on the short panel carries the risk of model overfitting. For this reason, we proceed with estimating the policy regressions only for the more parsimonious FD specification.

The left-hand side of Table 5 reports the estimated effects on the short-run elasticity of one-standard-error increases in the policy variables. It is based on regressions of the FD specification without additional LMIs such as the left-hand side of Table A.13 but with the inclusion of country-specific linear trends. The inclusion of trends leaves the qualitative finding on the regime-specific effects unchanged. More benefit generosity increases the short-run elasticity only in relatively interventionist regimes, while a higher flexibility index raises the elasticity only in more liberal

[22] To save space, these estimation tables are not reported but are available upon request.

regimes. Finally, the simulated reform towards flexicurity using the model with country trends leads to very similar results to those without such trends, which is evident from comparing columns (1) and (2) in Table A.18.

8.2 | Per capita measurement of employment and output

As a more direct test for the role of demographic forces, in one robustness check, we estimate Equation (2) with both employment and output divided by the size of the working age population (ages 20–64), a specification which directly accounts for changes in the size of the labor force. The per capita transformation, which essentially turns employment into employment rate and output into output per capita, largely retains the time series properties of the two variables. Reproducing Tables 1 and A.7 shows that the estimates for the short-run elasticity are remarkably robust to the per-capita transformation of employment and output. Nevertheless, as opposed to the baseline estimates, the Ericsson–MacKinnon cointegration test cannot reject the null of no cointegration. This makes us reproduce the policy regressions with the FD specification, which does not assume the existence of cointegration. Our main results on the regime-specific effects of policy changes on the short-run elasticity are robust to the per-capita transformation (right-hand side of Table 5). Consequently, the derived effects of a flexicurity reform in individual countries remain similar to the baseline results (column (3) in Table A.18).

8.3 | Broad measure of ALMP expenditures

In the main analysis, we represented the ALMP spendings of a country with its expenditures on ALMP-related training programs. However, ALMPs also incorporate elements beyond training. According to the OECD classification, the following categories of policies belong to ALMPs[23]: training (institutional, workplace, and integrated training, special support for apprenticeship); employment incentives (recruitment incentives, employment maintenance incentives, job rotation, and job sharing); sheltered and supported employment and rehabilitation; direct job creation; and start-up incentives. As discussed earlier, our focus on training is motivated by the fact that training expenditures constitute the highest share of total spending on ALMPs and that training is clearly targeted at improving employability, which may be less true for other types of ALMPs. For instance, direct job creation such as public works programs tends to be less effective in promoting employability, especially when they are not combined with training (e.g., Gehrke & Hartwig, 2018). This section tests the robustness of the results to using a broad ALMP measure, which combines all the categories listed above. Just as *train* in the main analysis, the broad ALMP variable is also expressed in PPP-adjusted US dollars and divided by the number of unemployed persons in the country. This broad ALMP has a sample mean of 7.52 with a standard deviation of 1.04 and it is very strongly correlated with *train* (correlation coefficient is 0.9).

Table 6 reports the estimated effects of σ-increases in the policy variables. The reported estimates are obtained from regressions identical to those behind Table 4 in all respects but the ALMP variable. We can conclude that the results are largely robust to this change. In particular,

[23] A detailed description of the categories can be found on the OECD's webpage here: https://www.oecd.org/els/emp/Coverage-and-classification-of-OECD-data-2015.pdf

TABLE 6 Robustness with broad ALMP

Policy variable	Without LMIs: Without interactions	Without LMIs: With interactions Average	Liberal	Interventionist	With LMIs: Without interactions	With LMIs: With interactions Average	Liberal	Interventionist
grr	0.118***	0.130***	0.066	0.099	0.140***	0.166***	0.115	0.149***
	(0.043)	(0.049)	(0.069)	(0.065)	(0.044)	(0.056)	(0.087)	(0.057)
almp	0.019	0.068	−0.163	0.016	0.011	0.060	−0.106	0.059
	(0.037)	(0.052)	(0.226)	(0.044)	(0.039)	(0.054)	(0.234)	(0.052)
flex	0.105***	0.098***	0.173***	0.103	0.066*	0.060	0.157***	0.017
	(0.033)	(0.035)	(0.053)	(0.063)	(0.039)	(0.039)	(0.058)	(0.059)

Note: The figures show the effects of a one-standard-deviation increase in a policy variable on the short-run employment–output elasticity. ALMPs are represented by a broad ALMP measure that also comprises training. The estimates are based on a regression of the EC specification either without or with additional LMIs. The sample excludes Spain. Average, liberal, and interventionist regimes differ in the values the nonchanging policy variables are assumed to take. Standard errors are in parentheses. ***$p < 0.01$, **$p < 0.05$,*$p < 0.1$.

the estimates associated with the increase in ALMP spending remain statistically zero under all policy regimes. The flexicurity reform exercise also yields qualitatively similar results if we base it on the regressions with the alternative ALMP variable (column (3) in Table A.17).

8.4 | Two-step estimation

Our empirical approach behind regression Equation (2) is to estimate the long-run cointegrating relationship and the dynamic adjustment in a single step— an approach that has recently been proposed by Rao et al. (2010) and Kumar and Rao (2012). Alternatively, one could follow a two-step approach, first estimating the long-run cointegrating relationship between e_{it} and y_{it} and then, given that there is cointegration, using the lagged first-stage residuals, that is, the lagged error correction (ECM) term, in the second-stage regression for the dynamic relationship. An advantage of the two-step approach is that it allows for alternative estimation methods that have been proposed to estimate cointegrating relationships: the fully modified OLS (FMOLS) introduced by Phillips and Hansen (1990) and the dynamic OLS (DOLS) proposed by Stock and Watson (1993). If the cointegrating equation error and the regressor innovations (in our case, innovations of output) are correlated, OLS estimates of the long-run cointegrating relationship can be substantially biased in small samples (albeit being superconsistent) and the standard error estimates become invalid. To soak up the possibly problematic correlation, the FMOLS uses a semiparametric correction while the DOLS augments the cointegrating regression with leads and lags of the first-differenced regressors. These two methods produce asymptotically unbiased and normally distributed coefficient estimates.

In this robustness check, we carry out two-step estimations, using either the FMOLS or the DOLS estimators in the first stage. Our aim is to reproduce the estimation results reported in Table 1 for the long panel and to show that the resulting estimates for the employment-to-output elasticity are close to the baseline estimates. In the first step, we estimate the cointegrating relationship between log employment (number of employees) and log output with FMOLS and DOLS, where we include as deterministic terms either a constant only or a constant and a linear time trend.[24] The upper part of Table A.19 presents the corresponding estimates for the long-run relationship between employment and output. Interestingly, the estimate is significantly smaller than our implied long-run elasticity from the baseline estimation, suggesting that the baseline estimate is upward biased. The presence of cointegration can be tested by checking whether the residuals obtained from these first-stage regressions are stationary. Conventional panel unit root tests with a null hypothesis of nonstationarity (Levin–Lin–Chu, Fisher-ADF) reject the null for all countries, while the Hadri tests with a null hypothesis of stationarity for *all* countries also reject the null. This is consistent with the results of the Johansen test performed earlier, where we found that cointegration exists for most— but not for all— countries in our sample. In the second step, we estimate the dynamic relationship in a regression similar to (2) but replace the variables in levels, $e_{i,t-1}$ and $y_{i,t-1}$, with the lagged ECM term. These estimates are reported in the lower part of Table A.19. Importantly, the short-run elasticity estimates are remarkably robust and remain at the level of the baseline estimate (0.3) regardless of the estimation method used in the first stage. Furthermore, the coefficient for the lagged residual, which corresponds to the speed of adjustment, is quantitatively very close to our baseline estimate and highly significant statistically.

9 | CONCLUSION

The variations in employment responses to external shock and differences in its recovery pattern seen in the aftermath of the Great Recession are partly blamed on the nature of labor market policies and institutions characterizing the individual countries within both the EU and the OECD. Consequently, the need to enhance the ability of the labor market to adjust to shocks has heightened the emphasis on *flexicurity* reforms. This paper examines the responsiveness of employment-to-output fluctuations in a panel of 20 OECD countries, paying particular attention to the role of the key pillars of flexicurity as well as their interactive effects in driving this outcome.

Interesting results emerge. For an average country, we find that there has been a significant increase in the responsiveness of employment-to-output fluctuations over the past decades, with labor market policies playing a crucial role in shaping this outcome. In particular, our results show that important complementarities exist between these policies, especially in determining the short-run employment–output dynamics, and that— depending on the initial policy mix— reforms in the direction of flexicurity may result in asymmetric outcomes for the individual economies. A clear conclusion that emerges from this study is that a one-size-fits-all policy may not yield the same results everywhere. Instead, in considering which reform to undertake, an economy should first consider its existing policy framework.

Of course, there may be other factors that remain unexplained in our estimation that may in fact be relevant in driving employment dynamics— for instance, the changes in the nature of job creation in different countries (e.g., different reliance on flexible short-term contracts)— which we leave for future research.

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[24] Contrary to the rest of the analysis, this estimation is done in EViews and not in Stata because EViews has better functionalities for FMOLS and DOLS on panel data. DOLS includes zero or one lead and lag, depending on the Akaike Information Criterion (IC). Default options are applied for the assumptions concerning the long-run variance.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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