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"A maioria pensa com a sensibilidade, eu sinto com o pensamento. Para o homem vulgar, sentir é viver e pensar é saber viver. Para mim, pensar é viver e sentir não é mais que o alimento de pensar." ***Fernando Pessoa, Livro do Desassossego, 1982.***

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Introduction

This dissertation consists of three self-contained studies that employ empirical methodologies to shed light on important topics. The initial chapter presents a comprehensive analysis of the impacts of government spending on the Eurozone economy, including transmission mechanisms, regional spill-overs, sectoral diversity, and state dependencies. The second chapter investigates the political implications of fiscal austerity by assessing its effects on the voting outcomes of the extreme right and left parties, political fragmentation, and trust in the incumbent government. In the third chapter, I revisit the classical inquiry into the relationship between wage inflation and unemployment through a historical lens and document a time-varying Phillips curve since 1870 for a set of advanced economies. I contend that the wage inflation-unemployment tradeoff is weaker in a low-price inflation environment, as a consequence of a stronger unemployment response to monetary policy in such a scenario.

Chapter 1: The Effects of Government Spending in the Eurozone

In recent years, the impact of fiscal policy on the Eurozone economy has become a topic of increasing interest among academics and policymakers. With the European Central Bank's main policy interest rate having reached its lower bound, there have been calls for more fiscal actions to stimulate economic growth. The Covid-19 rescue package implemented by the European Commission has also highlighted the need for a deeper understanding of the effects of government spending on the Eurozone economy. Despite this heightened attention, the literature still lacks a comprehensive analysis that can address these important questions.

The goal of this chapter is to fill this gap by providing new empirical evidence on the economic impact of fiscal policy in the Eurozone. Specifically, we focus on regional data and examine the transmission mechanism of fiscal policy. To do this, we use ARDECO database which offers data series on output, private investment, employment, hours worked, and wages at different regional aggregations and sectoral divisions.

Our approach uses a Bartik-type instrument for identification. This instrument relates changes in regional government spending to the differential regional exposure to changes in national government spending. We use this instrument in an instrumental variable local projections setting to estimate fiscal multipliers and impulse responses.

Our baseline estimates reveal that changes in regional government spending have a significant impact on the Eurozone economy. We find a government spending relative output multiplier of 2.2 and an employment multiplier of 1.4. Our estimates suggest that a €1 million increase in government spending creates 33 new jobs four years after the shock materialized, at a cost per job created of about €30,000.

To better understand the underlying fiscal transmission mechanism, we will examine the responses of several interesting variables to the regional fiscal shock. Our evidence points towards strong positive supply-side effects of government spending changes, with a crowding-in of private investment and a rise in labor productivity and total factor productivity. We also find significant effects on durables consumption, real wages, labor share, and markup.

Our findings reveal strong heterogeneities across economic sectors, states of the economy, and member states, and only small fiscal spillovers. These results raise important questions about the widely shared belief among policy circles of positive and sizable fiscal spillovers. Overall, this chapter offers new empirical evidence and insights into the effects of fiscal policy on the Eurozone economy, which can inform policymakers and academics alike.

Chapter 2: The Political Costs of Austerity

The rise of anti-establishment and EU-skeptic parties has been notable since the Great Recession and subsequent European Sovereign Debt Crisis. As a result, there has been an increase in partisan conflict and more fragmented parliaments. This polarized political environment is concerning as it can lead to higher policy uncertainty and lower economic growth. It is interesting to note that the rise in support for extreme parties occurred during a time of significant fiscal policy interventions. Our paper investigates the link between fiscal consolidations and rising polarization and explores the political costs of fiscal austerity.

We collected a unique regional dataset on election results and used party classifications to identify far-right and far-left political parties. To identify exogenous changes in regional public spending, we used a Bartik-type instrument that combined regional sensitivities with national government expenditures and a national consolidation measure. We found that fiscal consolidations have significant political costs: a 1% reduction in regional public spending leads to an increase in extreme parties' vote share of around 3 percentage points. The increase in the vote share captured by extreme parties can be explained by a fall in voter turnout and an increase in total votes for these parties. This suggests that in response to fiscal consolidations, fewer people vote, and those who do have a higher tendency to vote for extreme parties. Additionally, austerity increases fragmentation, leading to negative economic impacts on the polarized political environment.

We also examined whether austerity-driven recessions yield different political outcomes than general economic downturns do. Our findings indicate that recessions that coincide with fiscal consolidations lead to a significantly larger increase in the vote share for extreme parties than those that are unrelated to austerity. This could be attributed to the trust channel of fiscal consolidations, as people's trust in the government deteriorates more strongly during austerity recessions compared to non-austerity recessions. This suggests a potential "doom loop" between distrust in the political system and more extreme voting following fiscal consolidations.

Chapter 3: Monetary Policy and the Wage Inflation-Unemployment Tradeoff

I use newly assembled data for 18 advanced economies between 1870 and 2020 to study how monetary policy affects wage inflation and unemployment and document two key findings regarding their tradeoff. First, the wage Phillips curve has always been "alive and well" and its recent flattening is not unique to the last 150 years. In fact, the Phillips curve displays a time-varying slope. Second, the tradeoff becomes weaker in low-price inflation environments due to a more pronounced unemployment response to monetary policy, which is consistent with the New Keynesian model's predictions.

The results of the study suggest that policymakers' ability to explore the wage inflation-unemployment tradeoff is impaired in a low-price inflation environment. The findings emphasize the importance of using historical data to better understand the relationship between wage inflation and unemployment rates and suggest that changes in the price inflation environment shape the wage inflation-unemployment tradeoff.

Chapter 1

The Effects of Government Spending in the Eurozone

Joint with Ana Sofia Pessoa and Mathias Klein

1.1 Introduction

How does fiscal policy affect the Eurozone economy? Over the last decade, this topic has gained renewed attention among academics and policymakers alike. As the main policy interest rate of the European Central Bank (ECB) reached its lower bound, economic commentators have frequently asked for more fiscal actions to stimulate the

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economy. Indeed, in one of his last press conferences, parting ECB President Mario Draghi stated that "...now it's high time I think for the fiscal policy to take charge" (Draghi, 2019). Moreover, motivated by the close trade linkages among member states of the European single market, there is particular interest in how fiscal interventions spill over from one region to another (Blanchard, Erceg, and Lindé, 2016). More recently, the Covid-19 rescue package implemented by the European Commission has re-emphasized the need for more precise knowledge concerning the impact and scope of government spending on the Eurozone economy.[1]

Despite the increased interest in the effects of fiscal policy in the Eurozone, the literature still lacks a thorough analysis that is able to address these important questions. In this paper, we aim to fill this gap by providing new empirical evidence on the economic impact of fiscal policy and its transmission mechanism in the Eurozone focusing on regional data. In particular, we follow recent studies on the U.S. economy (Chodorow-Reich, Feiveson, Liscow, and Woolston, 2012; Nakamura and Steinsson, 2014; Bernardini, Schryder, and Peersman, 2020) and use regional variation in government spending to estimate how fiscal policy shapes the economy. Our contribution is threefold. First, we compute regional output and employment multipliers for the Eurozone. Second, we analyze the underlying fiscal transmission mechanism in detail by providing novel evidence on how changes in regional government spending affect key variables including investment, productivity, wages, and hours worked. Thirdly, we investigate the significance of regional fiscal spillovers and test for heterogeneous effects across economic sectors, states of the economy, and member states.

Our empirical analysis employs a newly assembled rich dataset, ARDECO, which offers series on output, private investment, employment, hours worked, and wages at different regional aggregations and sectoral divisions. We use the sum of gross value added and intermediate consumption of the non-market sector as a measure of regional government spending. To justify this choice, we show that our measure and government spending are closely linked by definition and that both series' statistical properties are very similar at the national level. For identification, we use a Bartik type instrument, which identifies the effect of government spending on economic activity by relating the changes in *regional* government spending to the differential regional exposure to changes in *national* government spending (Bartik, 1991). We combine the Bartik instrument with instrumental variable local projections to estimate fiscal multipliers and impulse responses.

Using regional variation in government spending to map the impact of fiscal policy offers several advantages over studies focusing on the national level. First, because all regions are part of the monetary union, they are all subject to the ECB's

1. For a general discussion on the current challenges for fiscal policy in the Eurozone, see, e.g., Pappa (2020) or Bilbiie, Monacelli, and Perotti (2021).

monetary policy. Thus, by including time fixed effects in our regressions, we can control for confounding effects of monetary policy interventions, which is a common challenge when studying the effects of government spending at the national level. Second, our analysis at the regional level substantially increases the number of observations such that potential state-dependencies and heterogeneous effects across economic sectors can be estimated more accurately. Thirdly, the significant differences in intra-regional trade flows permit a highly detailed investigation into the size of fiscal spillovers. Similar to Nakamura and Steinsson (2014), our results show relative effects, that is, we estimate the impact of an increase in government spending in one region of the Eurozone relative to another on relative economic activity, the "open economy relative multiplier".

Our baseline estimates reveal a government spending relative output multiplier of 2.2, which implies a €1.2 increase (decrease) in relative private sector production for every €1 increase (decrease) in relative government production. Moreover, we find an employment multiplier of 1.4, which shows that changes in regional government spending have sizeable effects on local labor markets. In particular, our estimates imply that a €1 million increase in government spending creates 33 new jobs four years after the shock materialized or, in other words, a cost per job created of about €30,000. These results are robust to several modifications of the baseline model, like different constructions of the Bartik instrument, changes in the sample, and controlling for national tax policies and sovereign risk premia. Furthermore, to account for potential anticipation concerns, we show that the results remain when constructing the Bartik instrument by only using variations in national government spending that are orthogonal to past economic conditions, due to changes in national military spending or professional forecast errors.

To shed light on the underlying fiscal transmission mechanism, we estimate the responses of several interesting variables to the regional fiscal shock. We find that an increase in regional government spending leads to a significant increase in private investment. This crowding-in of private investment can be rationalized by a strong and persistent rise in labor productivity and total factor productivity. Thus, our evidence points towards strong positive supply side effects of government spending changes, in line with recent U.S. evidence by Auerbach, Gorodnichenko, and Murphy (2020b) and Jørgensen and Ravn (2022). Furthermore, the fiscal stimulus induces a significant rise in durable consumption (measured by the number of motor vehicles) together with higher real wages and an increase (decrease) in the labor share (markup). We also take a closer look at the effects on regional labor markets and find that higher regional government spending induces a considerable increase in total hours worked. Interestingly, the bulk of this increase is accounted for by the extensive margin (total number of employees), whereas the intensive margin (hours per employee) barely responds to the regional fiscal shock.

Using the full level of detail in the dataset, we determine which economic sectors are responsible for the significant crowding-in of private economic activity and employment by estimating sector-specific fiscal multipliers. We find that the industry and (non-financial) services sectors account for the lion's share of the increase in private demand. While both sectors together make up for less than 60% of total private activity, they contribute to more than 75% of the fiscal policy induced private economic expansion. In light of the disproportional amplification effects of these two sectors, policymakers should target them specifically when designing stabilization measures. Although the close trade linkages across European regions within the European single market might suggest strong spillover effects, our estimates reveal only small (and mostly insignificant) fiscal spillovers which raises questions about the widely shared belief among policy circles of positive and sizable fiscal spillovers (e.g., In't Veld (2016)). Moreover, in light of these findings, recommendations to jump-start the European economy by increasing public spending in regions with fiscal capacity should be interpreted with caution since the positive spillover effects might be more limited than is conventionally thought.

Finally, we detect significant state dependencies. First, fiscal multipliers are significantly larger in economic recessions than in economic booms. Second, fiscal policy is significantly more effective in core countries of the Eurozone compared to periphery countries. However, in contrast to recent studies at the national level (Born, D'Ascanio, Müller, and Pfeifer, 2019; Barnichon, Debortoli, and Matthes, 2020), we find no evidence that the sign of the fiscal intervention considerably affects the size of the multiplier. Government spending stimulus-multipliers do not significantly differ from consolidation-multipliers.

What do our regional estimates imply for national multipliers? As argued by Chodorow-Reich (2019), cross-sectional multipliers provide a lower bound for the closed economy, deficit-financed, no-monetary-policy-response multiplier. Thus, our results would suggest a national output multiplier of above but close to 2 which is slightly larger than the multiplier of 1.7 proposed by Chodorow-Reich (2019) for the U.S. economy. While this value might appear large at first glance, one must keep in mind that our estimates abstract from any endogenous monetary policy tightening in response to the fiscal expansion, which according to standard theory, dampens the size of the multiplier. Against this background, time-series studies which estimate fiscal multipliers at the zero lower bound find multiplier magnitudes that are much more in line with our cross-sectional evidence (Miyamoto, Nguyen, and Sergeyev, 2018; Ramey and Zubairy, 2018; Klein and Winkler, 2021).

Our paper contributes to the recent and fast-growing literature that uses subnational data to estimate the impact of fiscal policy (Becker, Egger, and Von Ehrlich, 2010; Becker, Egger, and Von Ehrlich, 2013; Nakamura and Steinsson, 2014; Dupor and Guerrero, 2017; Auerbach, Gorodnichenko, and Murphy, 2020b; Bernardini, Schryder, and Peersman, 2020). So far, this literature mainly focuses on the U.S.

economy, with only limited evidence for the Eurozone.[2] In general, one could expect that fiscal multipliers differ between the U.S. and the European economy due to non-trivial differences in institutional constraints and characteristics of financial services, goods markets, and labor mobility, for example.

Some previous papers rely on sub-national data to study the economic effects of regional funds from the European Union (EU) (Becker, Egger, and Von Ehrlich, 2010, 2013; Coelho, 2019; Canova and Pappa, 2021). While these structural funds typically face a significant implementation lag and primarily intend to foster long-run growth of lagging regions, our analysis focuses on discretionary fiscal policy and our identification relies on variation in government spending and economic activity in the Eurozone.

Brueckner, Valentinyi, and Pappa (2019) use a similar dataset and investigate how the size of the fiscal spending multiplier depends on the degree of local autonomy across European regions. In contrast to their paper and much of the existing U.S. evidence, we take on a more general perspective and provide new insights into several important aspects of the fiscal transmission mechanism in the Eurozone. In other words, the dataset's level of detail enables us to zoom into a wide range of fiscal policy effects. In particular, the underlying drivers of our fiscal multiplier estimates, like the influence of fiscal policy on investment, productivity, (public and private) employment, or earnings can be considered carefully. Moreover, the dataset enables us to conduct a thorough investigation into regional fiscal spillovers and heterogeneous effects across economic sectors, states of the economy, and member states. Overall, we believe that our new insights have the potential to contribute to discussions among academics and policymakers about the gains and limitations of fiscal policy in the Eurozone.

The remainder of the paper is organized as follows. Section 3.2 describes the data we use. Section 2.3 presents the methodology. Section 3.4 shows our empirical results. Finally, Section 3.5 concludes.

1.2 Data

We use data from the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy (ARDECO), which is maintained and updated by the Joint Research Centre.[3] It is a highly disaggregated dataset across both sectoral and regional dimensions. The database contains a set of vari-

2. Studies on the Italian and Portuguese economies, respectively, are Acconcia, Corsetti, and Simonelli (2014) and Carvalho, Franco, and Peralta (2020). We refer the reader to Chodorow-Reich (2019) for an extensive survey on the cross-sectional evidence on fiscal stimulus using subnational data.

3. It can be found online here.

ous long time-series indicators for EU regions at several statistical scales. It expands the Cambridge Econometrics Dataset used by much of the literature on European regional dynamics (e.g., Badinger, Müller, and Tondl (2004)).

The database provides regional measures for output (gross domestic product (GDP) and gross value added (GVA)), investment, earnings, hours worked and employment for different economic sectors like industry, construction, financial, non-financial, and non-market services. The dataset is an annual unbalanced panel covering the period 1980–2017 for the European Union (EU) and some European Free Trade Association (EFTA) and candidate countries. By construction, ARDECO's regional data is consistent with the commonly used national accounts data (see Lequiller and Blades (2006) and Lequiller and Blades (2014) for more details on the construction of the national accounts data). In particular, the regional ARDECO time series are constructed in such a way that the country aggregates equal the corresponding time series in the National Accounts reported in the AMECO dataset.[4]

The data are divided into NUTS (Nomenclature of Territorial Units for Statistics) regions. NUTS is a geocode standard for referencing the subdivisions of countries for statistical purposes. The hierarchy of three NUTS levels (NUTS 1, 2, 3) is established by Eurostat in agreement with each member state, and for most countries the respective NUTS level corresponds to a specific administrative division within the country. ARDECO provides all data series at these regional disaggregation levels except for the NUTS 3, for which it reports only population, employment, GDP, and GVA.

Our baseline Eurozone sample covers 12 countries, namely the first Euro adopters Austria, Belgium, Finland, France, Ireland, Italy, Luxembourg, Germany, Greece, the Netherlands, Portugal, and Spain. We exploit NUTS 2 level data from 1999 (when the Euro was introduced) until 2017 for all countries except Greece, which joined the Euro in 2001. Therefore, we only use Greek data from 2001 onwards.[5] Our sample thus consists of regions that are part of a monetary union with a common policy interest rate set by the ECB. As the policy interest rate is the same for all regions of the Eurozone, our approach of estimating regional fiscal multipliers has the advantage that we can directly control for confounding monetary policy reactions, which is a common challenge for estimates at the country level (Nakamura and Steinsson, 2014). In total, our sample consists of 167 European regions which generates a much larger cross-sectional variation compared to previous studies on the U.S. states level (Nakamura and Steinsson, 2014; Bernardini, Schryder, and Peersman, 2020).

4. See Appendix 2.A.1 for more information.

5. See Table 2.A.1 for more details on the NUTS 2 classification for the countries used in the sample.

For our main analysis, we use data on demography (total population), labor markets (employment, employee compensation, total hours worked), capital formation (gross fixed capital formation) and output (GDP and GVA).[6]

1.2.1 Regional Government Spending Data

Official data on final consumption expenditure of the general government (henceforth, government spending) is not available at the European regional level. Hereinafter, in the spirit of Brueckner, Valentinyi, and Pappa (2019), we use the sum of GVA and intermediate consumption of the non-market sector as a proxy for government spending. GVA of the non-market sector is computed as the sum of compensation to employees (including social contributions), consumption of fixed capital (which measures the decline in value of fixed assets owned as a result of normal wear and tear and obsolescence), and taxes less subsidies on production.[7] [8] Because GVA of the non-market sector does not include intermediate consumption, which is, however, one of the main components of government spending, we use input-output (IO) tables from the PBL EUREGIO database to calculate regional intermediate consumption shares of the non-market sector which we then add to the GVA of the non-market sector.

Our regional measure (GVA plus intermediate consumption of the non-market sector) is a valid proxy for government spending for several reasons. First, as previously mentioned, ARDECO's regional data is consistent with the national accounts data by construction. By definition, there exists a close link between government spending and the GVA of the non-market sector. In particular, even though the non-market sector includes other institutional units, the general government is the main actor responsible for changes in the non-market GVA. The non-market sector consists of six sub-sectors from which the three largest are also closely linked to the general government in the national accounts. Taking the example of Finland, the only country in our sample which publishes the required detailed information, on average, 86% of the GVA of the three largest sub-sectors (public administration and defense, education, human health and social work activities) was booked by the general gov-

6. The construction of all variables used in the paper is described in the appendix, see Table 2.A.2.

7. For more details, see the Manual on Regional Accounts from Eurostat. Importantly, net taxes on production does not include neither consumption nor corporate taxes.

8. Data from PBL EUREGIO indicate that, for the regions in our sample and the period of 2000-2010, GVA of the non-market sector is composed on average of 67% compensation to employees, 30% consumption of fixed capital, and 3% net taxes on production. The PBL EUREGIO database is discussed in more detail in Appendix 1.A.3.

ernment during our sample period.[9] Consequently, almost the entire variation in the GVA of the non-market sector refers to activities by the general government.

Second, government spending and our proxy measure show very similar statistical properties. When running regressions at the national and regional level, we find a strong and significant relationship between both measures with estimated coefficients close to 1. We will thus refer to our regional proxy as government spending throughout the rest of the paper. More details on the series, data sources, and justification of our proxy choice are given in Appendix 1.A.2.[10]

1.3 Methodology

In estimating the effects of a regional government spending shock, we closely follow Bernardini, Schryder, and Peersman (2020). Particularly, we study the impact of regional government spending in the Eurozone by first examining the dynamics of the cumulative GDP and employment multipliers. To that end, we use local projections (Jordà, 2005) and estimate for each horizon $h = 0, ..., 4$, the following equation:

$$\sum_{m=0}^{h} z_{i,t+m} = \beta_h \sum_{m=0}^{h} \frac{G_{i,t+m} - G_{i,t-1}}{Y_{i,t-1}} + \gamma_h(L) X_{i,t-k} + \alpha_{i,h} + \delta_{t,h} + \varepsilon_{i,t+m}, \tag{1.3.1}$$

where $z_{i,t+m}$ is either the change in real per capita GDP, $\frac{Y_{i,t+m}-Y_{i,t-1}}{Y_{i,t-1}}$, or the change in the employment rate, $\frac{E_{i,t+m}-E_{i,t-1}}{E_{i,t-1}}$, in region i between time $t-1$ and time $t+m$. Following Nakamura and Steinsson (2014), the employment multiplier is measured in terms of the employment ratio. $\frac{G_{i,t+m}-G_{i,t-1}}{Y_{i,t-1}}$ is the change in real per capita government spending in region i between time $t-1$ and time $t+m$, relative to real per capita GDP in $t-1$.[11] When $z_{i,t+m}$ indicates the change in real GDP, as government spending and GDP are in the same units, β_h directly yields, for each horizon h, the output multiplier. In the case of employment, β_h measures the employment multiplier as the change in the employment rate in response to a one percent increase in government spending relative to GDP.

9. Data for other countries not considered in our sample confirm this pattern. For example, for Estonia in 2018, 89% of public administration and defense, education, human health and social work activities GVA was booked by the general government, comparable to the 89% for Lithuania in 2019, 90% for Latvia in 2010, and 95% for Ukraine in 2015.

10. One should keep in mind that our regional government spending measure does not include investment expenditure and thus, does not account for procurement contracts related to fixed capital formation. Thus, our estimates have to be interpreted as government consumption multipliers.

11. Weighing by population is important to obtain more representative population average treatment effect estimates (Chodorow-Reich, 2020).

$(L)X_{i,t-k}$ is a vector of control variables with $k = 2$, and $\alpha_{i,h}$ and $\delta_{t,h}$ are respectively region and time fixed effects, which are included in the regressions to control for region-specific characteristics and common aggregate changes like, for example global shocks, and shocks that originate in another country and spill over to the Eurozone. Importantly, the time fixed effects absorb any endogenous monetary policy reaction by the ECB in response to an increase in government spending. Thus, our approach of using regional data to trace out the dynamic effects of a government spending shock does not face the problem of properly controlling for changes in the monetary policy stance, which is a common challenge for fiscal policy analyses at the national level. The vector of control variables includes two lags of the variable of interest (GDP growth or the growth rate in the employment ratio) and the growth rate in real per capita government spending. We use Driscoll and Kraay (1998) standard errors, which take into account the potential residual correlation across regions, as well as serial correlation and heteroskedasticity among the residuals over time.[12]

For identification, we follow, among others, Nekarda and Ramey (2011), Dupor and Guerrero (2017), and Perotti, Reis, and Ramey (2007) and instrument the change in government spending with a Bartik-type instrument (Bartik, 1991). We compute the instrument as

$$Bartik_{i,t} \;=\; s_i \times \frac{(G_{I,t} - G_{I,t-1})}{Y_{I,t-1}}, \tag{1.3.2}$$

where $s_i = \dfrac{\overline{G_i}}{\overline{G_I}}$ and $\overline{G_i}$ and $\overline{G_I}$ are averages of per capita government spending in region i and country I, respectively, in the five years preceding country I's Eurozone accession. In order to compute these averages, we use data from 1994 to 1998 for all countries in the sample except Greece, which joined the Eurozone in 2001 and for which we use 1996 to 2000. Intuitively, if s_i is above 1, region i spends more per capita than the national average. This implies that a disproportionate amount is spent in this region compared to other regions in the country. Figure 2.C.1 in the appendix shows a heat map depicting the share s_i for the considered NUTS 2 regions. There is considerable cross-sectional variation in this measure, ranging from 0.38 to 2.27. We calculate the lowest shares for Mayotte (France, 0.38), Peloponnese (Greece, 0.70), and Andalucia (Spain, 0.70), and the highest shares for Melilla (Spain, 2.27), Ceuta (Spain 2.16), and Brussels Capital District (Belgium, 2.10).[13] There is only small variation in the shares over time. When calculating time-varying

12. In the appendix, we show that our main results change marginally when dropping additional control variables and only including region and time fixed effects in the regressions (see Panel C of Table 1.C.3).

13. We show that our results change little when, instead of using per capita values, the regional shares are constructed using absolute values. In this case, the shares indicate a scaling factor and add up to one at the country level. We choose the per capita specification of the Bartik instrument as the

shares for each region, we find that the average standard deviation is 0.03. This low time variation justifies our choice of constant regional shares.[14]

The idea of the Bartik instrument is to scale national government spending such that spending varies more in regions with a larger predetermined share of national government spending. Moreover, as the predetermined share of average spending measures the differential exposure in regions to common national government spending changes, it helps to avoid confounding effects as argued by Goldsmith-Pinkham, Sorkin, and Swift (2020).[15]

More precisely, our identifying assumption is that central governments do not change spending because regions that receive a disproportionate amount of government spending are doing poorly relative to other regions. Intuitively, this assumption might be violated when focusing on high aggregation levels with only few regions within a country because politically and economically important regions could directly influence central government decisions. There is evidence that our analysis at the NUTS 2 level is not subject to this concern. In particular, for each region we construct a measure of the relative stance of the business cycle defined as the difference between the regions annual GDP growth rate and the average annual growth rate of all other regions within the same country. We regress the growth rate of national government spending on this regional business cycle indicator interacted with the regional shares, s_i. A negative coefficient would indicate a violation of our identifying assumption in the sense that national government spending would increase when regions spending relatively more are doing poorly compared to other regions. However, we find a small positive and insignificant coefficient suggesting that our identification strategy is valid.[16] Notably, we also conduct an additional robustness check where we show that the main results remain when going to the NUTS 3 level (with 922 regions in total), where direct influence of some regions on the central government should not be a severe concern after all.

Another potential concern with our estimation strategy would arise if regions receiving large amounts of national spending were more cyclically sensitive than

baseline because it provides a higher F-statistic compared to the absolute level specification. We drop the region of Guadeloupe from our entire analysis because it shows an extremely high government spending share (above 100).

14. Nevertheless, in a robustness exercise, following Nekarda and Ramey (2011) and acknowledging that there might have been structural changes throughout the sample, we use the full Eurozone sample to compute the share s_i instead of the five years preceding the Eurozone accession. The main findings remain unchanged.

15. Figure 1.B.2 shows the evolution of $\frac{G_{i,t}}{G_{I,t}}$ over time for four selected regions. It reassures that the relationship between regional and national government spending per capita is very stable during the sample period.

16. We find similar results when running the regression only for the regions with the top 5%, top10%, or top 20% highest shares.

other regions. We use the standard deviation of output growth to compare the cyclical sensitivity of regions that receive large and small amounts of national spending. The standard deviations are almost identical in regions with above median national spending shares and in regions with below median national spending shares (0.028 versus 0.029), indicating that a difference in overall cyclical sensitivity does not bias our approach.

Notably, we demonstrate that our main findings are robust to replacing national government spending in the construction of the Bartik instrument by different measures of unexpected changes in national government expenditure. For this purpose, we will use the residual of an estimated fiscal spending rule, military spending changes, and the forecast error of government spending.

Besides computing output and employment multipliers, we further estimate impulse response functions for other important variables as

$$w_{i,t+m} = \beta_h \frac{G_{i,t} - G_{i,t-1}}{Y_{i,t-1}} + \gamma_h(L)X_{i,t-k} + \alpha_{i,h} + \delta_{t,h} + \varepsilon_{i,t+m}, \qquad (1.3.3)$$

where $w_{i,t+m}$ is the growth rate of the variable of interest, $\frac{W_{i,t+m}-W_{i,t-1}}{W_{i,t-1}}$, for all variables except the labor share, for which we consider $w_{i,t+m}$ to be the difference in levels, $W_{i,t+m} - W_{i,t-1}$. $(L)X_{i,t-k}$ is a vector of control variables and $\alpha_{i,h}$ and $\delta_{t,h}$ are again region and time fixed effects, respectively. The vector of control variables now includes two lags of the respective variable of interest and real per capita government spending growth. β_h directly yields the response of the variable of interest to a one percent increase in government spending relative to GDP instrumented by the Bartik measure. One important difference between equations (2.D.1) and (2.3.1) is that equation (2.D.1) estimates the cumulated response to the cumulated government spending increase, whereas equation (2.3.1) estimates the cumulated response to a one-year change in government spending.

1.4 Results

1.4.1 Output and Employment Multipliers

We start by presenting the estimates of the output and employment multiplier of the baseline model. The main results are shown in Figure 2.D.1. Panels 1.4.1a and 1.4.1b show the cumulative GDP and employment multipliers estimated according to Equation (2.D.1). The solid line shows the point estimate β_h over a horizon of four years. Panels 1.4.1c, 1.4.1d, and 1.4.1e plot respectively the cumulated impulse responses of GDP, employment ratio, and government spending estimated according to Equation (2.3.1). The dark and light shadings are 68% and 95% Driscoll and Kraay (1998) adjusted confidence bands. Finally, Panel 1.4.1f depicts the F-Statistic test of weak instruments for the first-stage regression of the output multiplier.[17] For just-identified specifications, it is equivalent to the Olea and Pflueger (2013) F-Statistic and the threshold is 23.1 for the 5% critical value. For easier visual comparison, we set an upper bound of 200 on the reported F-Statistic.

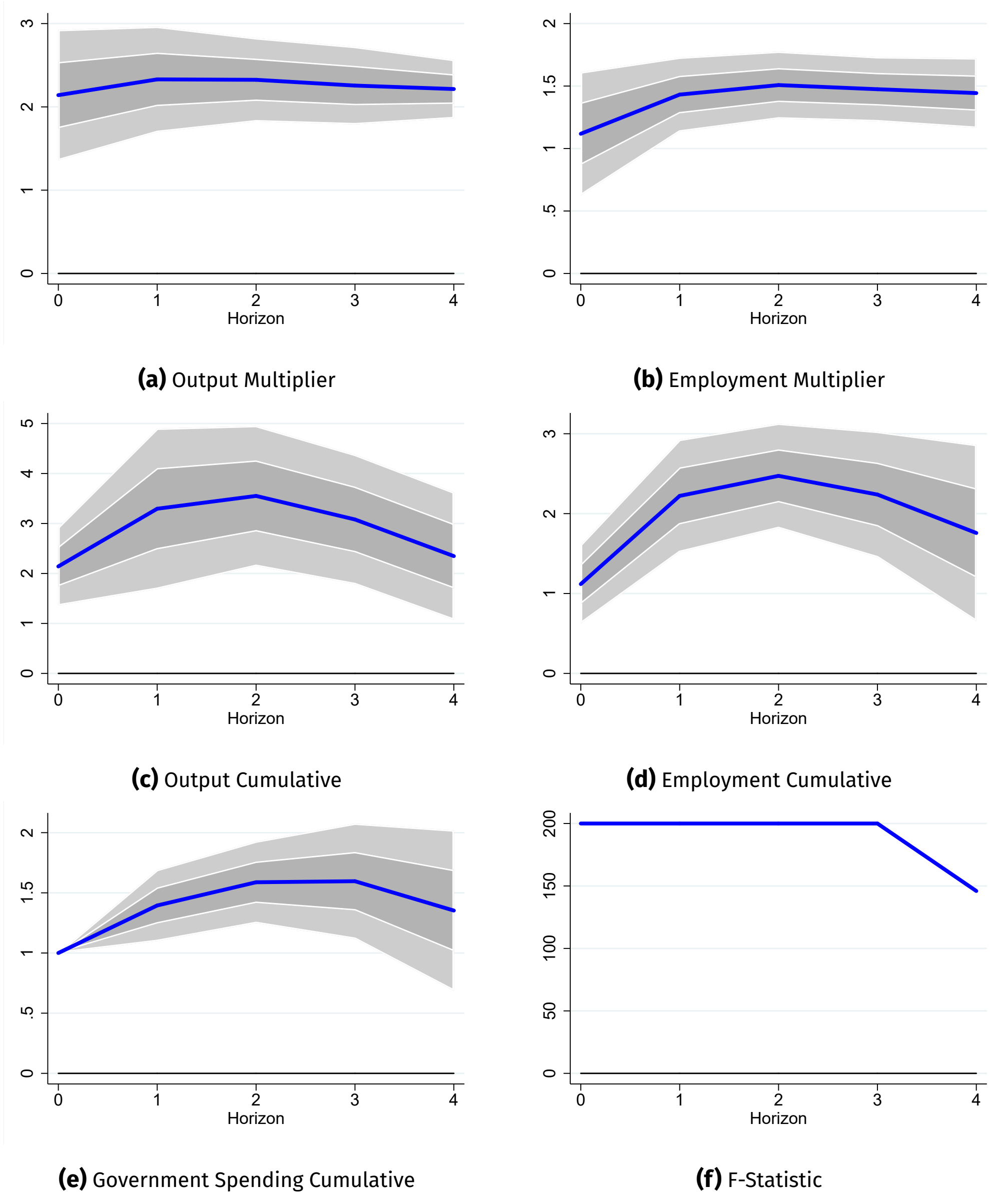
As Panel 1.4.1f shows, the Bartik measure is a strong instrument for regional government spending for all years of the forecast horizon. The computed F-Statistic is well above the threshold value of 23.1, suggesting that weak instruments are unlikely to be a concern for our analysis.

Our baseline estimates reveal an output multiplier of 2.14 on impact, which slowly increases to 2.21 four years after the shock materialized. This implies that a €1 increase (decrease) in relative government production leads to a €1.2 increase (decrease) in relative private sector production. The four-year multiplier is estimated relatively precisely with the 95% confidence band ranging from 1.86 to 2.56. Panels 1.4.1c and 1.4.1e show that the fairly stable output multiplier is due to similar hump-shaped responses in output and government spending. Government spending continuously increases up until three years after the shock and then converges back to steady state. Output shows a similar pattern, although the decline starts already in year 2. Importantly, GDP rises persistently by more than €2, which leads to the reported multiplier.

The employment multiplier as reported in Panel 1.4.1b behaves similarly to the output multiplier. On impact, we estimate an employment multiplier of 1.12, which then rises slightly to 1.44 at the end of the forecast horizon. Thus, besides boosting real economic activity, changes in regional government spending also have sizeable effects on local labor markets. Again, the estimates are highly significant and the 95% confidence band of the four-year employment multiplier ranges from 1.17 to 1.71. As shown in Panel 1.4.1d, employment significantly increases on impact and then rises for two years after the shock before slowly decreasing.

When decomposing the employment multiplier into employment in the private and public sectors, we find that both contribute to the positive impact of government spending on total employment. Figure 1.B.3 in the appendix shows the private and public employment multipliers. On average, private employment accounts for more than 2/3 of the total employment multiplier. Thus, the lion's share of the positive labor market effect of regional fiscal stimulus is due to employment changes in the private sector. Taken together, an increase in relative government spending leads to a strong and significant rise in relative private economic activity and employment implying that discretionary fiscal policy constitutes a powerful tool to stimulate regional economies in the Eurozone.

17. The F-Statistic for the first-stage regression of the employment multiplier is very similar to the one in Panel 1.4.1f since the only difference is the lagged control variables (GDP for the output multiplier and the employment ratio for the employment multiplier).



(a) Output Multiplier

(b) Employment Multiplier

(c) Output Cumulative

(d) Employment Cumulative

(e) Government Spending Cumulative

(f) F-Statistic

Figure 1.4.1. Output and Employment Multipliers. Panels 1.4.1a and 1.4.1b show the cumulative relative fiscal and employment multipliers estimated according to Equation (2.D.1). Panels 1.4.1c and 1.4.1d depict the underlying impulse responses of GDP and employment rate to the cumulative change in government spending which is plotted in Panel 1.4.1e and estimated according to Equation (2.3.1). Panel 1.4.1f shows the related first-stage F-Statistics over a four-year horizon. Shaded areas are 68% (dark) and 95% (light) confidence intervals.

Our estimates are comparable to previous regional fiscal multipliers documented for both Europe and the U.S.. Coelho (2019) finds an impact multiplier of 1.8 for European regions relying on structural funds distributed by the European Commission. Using provincial expenditure cuts in Italy, Acconcia, Corsetti, and Simonelli (2014) report a multiplier of 1.55 on impact and of 1.95 for the 1-year cumulative multiplier. For the U.S., Nakamura and Steinsson (2014) use regional variation in military buildups to compute 2-year output multipliers ranging from 1.4 in their baseline specification up to 2.5 when applying a Bartik instument approach as we do, and Bernardini, Schryder, and Peersman (2020) estimate an impact output multiplier of around 2 when applying a Bartik instrument and of 1.3 when using a Blanchard and Perotti (2002) recursive identification.[18] Regarding employment multipliers, evidence in the literature is more mixed. Contrarily to us, Becker, Egger, and Von Ehrlich (2010) and Coelho (2019) find no significant responses of employment to fiscal spending for European regions. For Portugal, using changes in government spending prior to local elections, Carvalho, Franco, and Peralta (2020) find an employment multiplier of 1.5. For the U.S., Nakamura and Steinsson (2014) find regional employment multipliers ranging from 1.3 in their preferred specification to 1.8 when applying a Bartik instument approach.

1.4.2 Robustness

Our main Eurozone multiplier estimates are robust to several modifications of the baseline model. We report a battery of robustness checks in Appendix 1.C. The estimates change only little when applying alternative ways to construct the Bartik instrument and using different ways to extract unexpected variation in national government spending. Our findings are also robust to changes in the sample and to additionally controlling for national tax policies and sovereign spreads. We further demonstrate that the estimates are not prone to dynamic and cross-sectional heterogeneity. The baseline multiplier estimates are also robust when following closely Nakamura and Steinsson (2014) and using national military spending interacted with region fixed effects as instrument. Furthermore, the results do not change much when not including lagged control variables in the regressions or excluding regions that spend disproportionately more per capita than the national average. We also re-estimated the baseline model when excluding intermediate consumption from our proxy regional government spending series. Then, regional government spending is measured by the GVA of the non-market sector. As expected, the multipliers increase because the shock size (1% of GDP per capita) becomes larger relative to the baseline proxy used. To assess how important any individual country is for the results, we re-estimate the baseline regressions by sequentially dropping one country at a time. The obtained results are comparable to the baseline in every case. Finally, we

18. For a survey on regional fiscal multiplier estimates, see Chodorow-Reich (2019).

use a Bayesian approach and estimate multipliers by means of Bayesian local projections. The results are similar to our baseline estimates which implies that Bayesian local projections do not deliver a significant improvement for our analysis.

To sum up, our baseline findings are robust to several modifications. In the following, we will thus rely on our baseline specification to produce additional new insights into the fiscal transmission mechanism in the Eurozone.

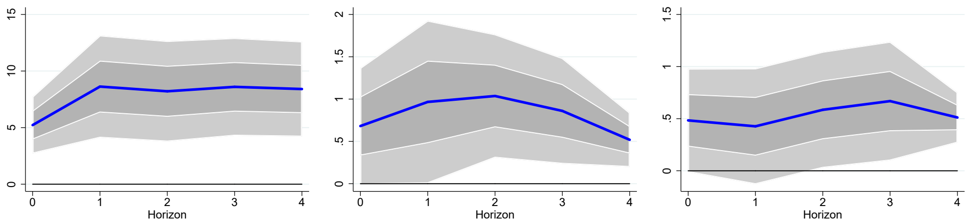
1.4.3 Impulse Response Analysis

To get a better understanding of the fiscal transmission mechanism in the Eurozone, this section presents additional impulse responses to a regional fiscal spending shock. More precisely, we report responses to a one percent increase in regional government spending relative to regional GDP, calculated based on Equation (2.3.1). The solid lines in Figure 2.4.5 show point estimates and the dark and light shadings again indicate 68% and 95% Driscoll and Kraay (1998) confidence bands. All responses are expressed in percent changes (growth rates) with the exception of the labor share response, which is presented as a percentage point change.

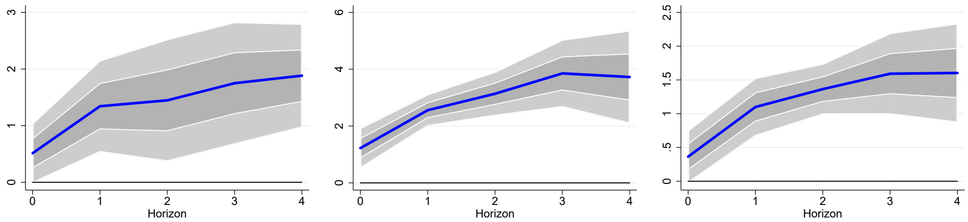
Our estimated regional output multiplier speaks in favor of a strong crowding-in of private demand following the regional fiscal expansion. Panel 2.4.5c of Figure 2.4.5 shows that a substantial component of the increase in private demand is due to an increase in private investment. The fiscal expansion leads to a significant and persistent increase in regional private investment expenditures. On impact, private investment increases by around 5%, which is roughly twice as large as the output response reported in Figure 2.D.1. Investment further increases in the first year after the shock and then slowly converges back to its pre-shock level. A complementary metric to quantify the investment response is the investment multiplier, which can be estimated in close analogy with the output multiplier described in Equation (2.D.1). The estimated private investment multiplier is presented in Figure 1.B.4 in the appendix, and we find that it is about half the size of the output multiplier. On impact, the investment multiplier is estimated to be around 1, slightly increasing to 1.1 four years after the spending increase. This finding supports the evidence reported in other studies which also find a rise in private investment following a fiscal spending stimulus at the national U.S. level (D'Alessandro, Fella, and Melosi, 2019) and at the regional level as a response to an increase in EU structural funds (Becker, Egger, and Von Ehrlich, 2013).

Panels 1.4.2b and 1.4.2c of Figure 2.4.5 provide a rationale for the strong private investment response. We find that productivity significantly increases in response to higher regional government expenditures. This is true when measuring productivity by total factor productivity (TFP) or labor productivity.[19] The maximum increase in TFP is slightly larger than the maximum rise in labor productivity and the peak

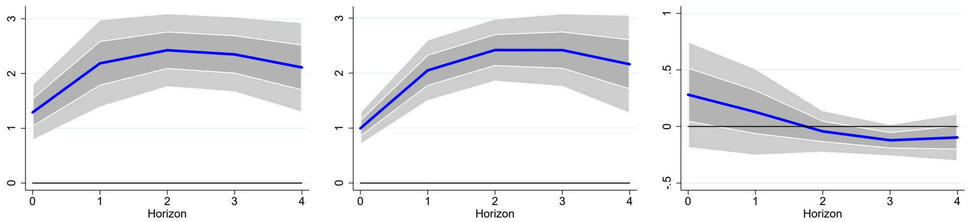
19. More details on the construction of our TFP variable can be found in Appendix 1.A.5.



(a) Investment **(b)** TFP **(c)** Labor Productivity



(d) Motor Vehicles **(e)** Hourly Wage **(f)** Labor share



(g) Total Hours **(h)** Employment (# employees) **(i)** Hours per Worker

Figure 1.4.2. Impulse Responses. These figures plot the response of a one percent increase of per capita government spending relative to per capita GDP. All responses are expressed in percent changes (growth rates) with the exception of the labor share variable, which is presented as a percentage point change (its difference). Shaded areas are 68% (dark) and 95% (light) confidence intervals.

response of TFP occurs somewhat earlier than the peak of labor productivity. The positive labor productivity response is in line with the regional U.S. evidence by Nekarda and Ramey (2011) and Auerbach, Gorodnichenko, and Murphy (2020b). In addition, Jørgensen and Ravn (2022) find that an aggregate government spending shock leads to a rise in (utilization-adjusted) TFP. To reconcile these positive supply side effects of fiscal policy, Auerbach, Gorodnichenko, and Murphy (2020b) propose a model with endogenous firm entry in which increasing government spending leads to a rise in the number of firms together with higher labor productivity. By introducing variable technology utilization into an otherwise standard New Keynesian model, Jørgensen and Ravn (2022) demonstrate that productivity and investment increase after a fiscal expansion.[20] By making a rise in productivity an endogenous response to a government spending shock, these model extensions produce a crowding-in of private demand, which ultimately increases the government spending output multiplier. Our regional Eurozone evidence on a significant crowding in of private investment coupled with higher productivity following a fiscal spending shock reinforces these modeling choices and points towards an important role of fiscal policy in driving movements in productivity. Another important indicator for supply side effects following a government spending shock is the price response. In a standard New Keynesian model, higher government spending raises aggregate demand and pushes up prices. However, this price increase can be overturned when allowing for endogenous productivity (Jørgensen and Ravn, 2022). In the Appendix (Figure 1.B.5), we present the impulse response of inflation to the regional government spending shock. Inflation is measured as the growth rate of the regional consumer price index (CPI), which was retrieved from various national sources but is available only for a subset of the baseline sample.[21] An increase in regional public spending leads to a significant fall in inflation in the impact period and one year after the fiscal intervention. Thereafter the inflation response turns positive but is mostly insignificant. This evidence further highlights the importance of supply side effect for understanding the transmission mechanism of government spending shocks.

Official data for private consumption expenditure, the second-most important component of private demand, are not available at the regional European level. Nonetheless, we rely on a common proxy for durable consumption to learn more about households' consumption decisions following a regional fiscal expansion. We follow a related literature and use the per capita number of motor vehicles as a measure for durable consumption (Mian, Rao, and Sufi, 2013; Demyanyk, Loutskina, and Murphy, 2019).[22] Figure 2.4.5d shows that the number of vehicles rises significantly after a fiscal expansion. On impact, there is an increase of around 0.6%, which then persistently builds up to almost 2% at the end of the forecast horizon. Thus, higher public spending crowds in consumption expenditure and, in particular, durable purchases in line with the U.S. evidence by Demyanyk, Loutskina, and Murphy (2019) and Auerbach, Gorodnichenko, and Murphy (2020b).

Households' consumption expenditure should be closely linked to their disposable income stream in the sense that an increase in income might well lead to higher

20. A model with variable capital utilization can also generate a productivity increase following a fiscal spending expansion. However, as shown by Jørgensen and Ravn (2022), the required substantial increase in capital utilization is not supported by the data.

21. Regional CPI data at the NUTS 2 level is available for Austria (since 2010), Finland, Italy, Portugal, and Spain (since 2002). Data for Germany is available only at the NUTS1 level.

22. Data on the per capita number of motor vehicles are taken from Eurostat. For details, see Table 2.A.2 in the appendix.

(durable) consumption spending. Panel 2.4.5e indeed supports this hypothesis. Here, we report the real wage response expressed as average real compensation per hour worked. Wages increase significantly and persistently in response to the fiscal stimulus. On impact, wages rise by more than 1% and continue to increase until the end of the forecast horizon. The wage response to an aggregate government spending shock is the subject of a considerable debate with different results emerging from different identification schemes (Galı, López-Salido, and Vallés, 2007; Ramey, 2011). At the regional level, the results are also mixed. While Auerbach, Gorodnichenko, and Murphy (2020b) find a positive earnings response, Nekarda and Ramey (2011) report a fall in wages following higher government spending. Our finding of a significant increase in real wages is in line with standard New Keynesian models, where a positive government spending shock lowers the markup of price over marginal costs and thus leads to a rise in real wages.[23]

The labor share response as shown in Panel 2.4.5f further supports this line of reasoning.[24] The labor share significantly increases in response to the regional fiscal expansion. Four years after the fiscal shock, the labor share is around 1.6 percentage points higher. In accordance with our evidence, Cantore and Freund (2021) find that an aggregate government spending shock leads to a rise in the labor share, whereas Auerbach, Gorodnichenko, and Murphy (2020b) estimate an acyclical labor share response.[25] The inverse of the labor share is commonly used as a measure for the price-cost markup (Auerbach, Gorodnichenko, and Murphy, 2020b; Nekarda and Ramey, 2020).[26] When following this argument, our evidence implies that a government spending shock lowers the markup, thus giving rise to a countercyclical markup behavior. While other studies also report evidence in favor of a countercyclical markup at the aggregate U.S. level (Bils, 1987; Rotemberg and Woodford, 1999), Nekarda and Ramey (2020) show that an increase in government spending increases output and leads to a rise in the markup.

Finally, we take a closer look at the labor market responses to the regional fiscal spending expansion. Our estimates reveal a significant and persistent increase

23. Figure 1.B.6 shows that disposable income also increases following the regional fiscal stimulus. Contrary to our hourly wage measure, disposable income is calculated after taxes and additionally includes capital income.

24. Here, the labor share is defined as the ratio between total private compensation and gross value added in the private sector.

25. Cantore and Freund (2021) rationalize the increase of the labor share following a government spending shock in a two-agent New Keynesian model populated by capitalists and workers. Capitalists do not supply labor, and, thus, workers make up the entire labor force. The combination of an increase in labor demand due to additional government expenditures combined with no labor supply response by capitalists implies that the labor share rises.

26. The inverse of the labor share is a valid measure of the markup when assuming a Cobb-Douglas production function and abstracting from overhead labor.

in total hours worked as shown in Panel 1.4.2g. On impact, hours worked rise by more than 1% and then increase to 2.5% two years after the shock before slowly converging back to equilibrium. To better understand the driving forces of the increase in hours, we decompose the response into the extensive margin (the total number of employees) and the intensive margin (the number of hours worked per employee). As Panels 1.4.2h and 1.4.2i indicate, we find that the bulk of the increase is accounted for by the extensive margin. The total number of employees responds in a very similar manner as hours worked. In contrast, hours per worker are barely affected by the regional fiscal spending shock. These findings reconcile with our baseline employment multiplier estimates, which imply that the fiscal stimulus is associated with a significant increase in the employment rate. These results support the evidence by Auerbach, Gorodnichenko, and Murphy (2020b) and Carvalho, Franco, and Peralta (2020), who also find that most of the change in hours worked in response to demand shocks is due to adjustments in the extensive margin. Moreover, Serrato and Wingender (2016), Corbi, Papaioannou, and Surico (2019), and Canova and Pappa (2021) also estimate that an increase in regional fiscal spending significantly boosts regional employment. Analogously, Monacelli, Perotti, and Trigari (2010) show that a positive aggregate government spending shock leads to a significant reduction in the unemployment rate.

To quantify how fiscal spending materializes in jobs created, we do a back-of-the-envelope calculation using the estimated coefficients from the employment impulse response function and the average employment and output series in the sample. Our estimates imply that, if the government increases spending by €1 million, it creates 15 additional jobs in the year of the shock, of which 12 are in the private sector and 3 in the public sector, which is consistent with the low estimates for the public sector by Adelino, Cunha, and Ferreira (2017). Because the build-up in employment is very persistent, the stimulus of €1 million produces a total of 33 new jobs after four years, of which 22 are in the private sector and 11 in the public sector.[27] This corresponds to a cost per job created of approximately €30,000, in line with the U.S. estimates that range from roughly $25K to $125K as argued in Chodorow-Reich (2019) and in line with European estimates, for example, a cost per job of €24,000 estimated by Carvalho, Franco, and Peralta (2020).

Taken together, our impulse response analysis has presented several important insights into the fiscal transmission mechanism in the Eurozone. Higher regional government spending i) crowds in private investment through positive supply side effects (increasing productivity), ii) boosts (durable) consumption expenditure, iii)

27. To calculate the job costs across sectors, we re-estimate the employment response for the private and public sector, respectively. Then we apply a similar back-of-the-envelope calculation as done for total employment.

raises real wages while increasing (lowering) the labor share (markup), and iv) expands hours worked, which is mainly driven by increasing the number of employees.

1.4.4 Sectoral Analysis

Our main results show that an increase in regional government spending causes a significant crowding-in of private economic activity and employment. The richness of our dataset and, in particular, its sectoral division allows to get a better understanding of which sectors mainly contribute to these strong effects. In doing so, we first re-estimate the baseline multiplier regressions in Equation (2.D.1) but replace regional GDP and employment by GVA and employment of the private sector.[28] Second, we decompose these private sector multipliers into the specific components coming from different economic sectors, namely, agriculture, industry, construction, services, and finance.[29]

Table 1.4.1 presents the results. While Panel A presents the aggregate multipliers across all sectors, Panel B displays the multipliers for each economic sector separately. On impact, the industry and services sectors mainly contribute to the strong increase in private economic activity. Out of the €1.68 increase in private economic activity, the industry sector contributes with 70 cents and the services sector with 69 cents. Thus, taken together both sectors account for more than 82% of the on-impact increase in private economic activity which is much larger than their combined average share in total private activity (roughly 56%). Higher production in the construction sector adds 27 cents to the total effect and the finance sector only contributes with 5 cents. For all years, the contribution of the agriculture sector is estimated to be negative and increasing over time, reducing the total effect by 14 cents four years after the shock. While the contributions of the industry and services sectors are relatively stable over time, the finance (construction) sector gains (looses) importance in the medium run. At the end of the forecast horizon, the finance sector contributes with 40 cents to the aggregate effects. The stronger impact of the finance sector over time might be explained by a higher credit demand by private firms and households due to the expansionary effects of the fiscal expansion which looses borrowing constraints and reinforces a feedback loop between higher private demand, more credit, and increasing investment and productivity. Moreover, while the fiscal stimulus strongly favors the construction sector in the short-run it stimulates high-productive sectors relatively more over the medium run. In fact, with the

28. We use GVA as the output measure because GDP is not available at the sectoral level. We still normalize the responses such that, on impact, government spending increases by one percent of per capita GDP.

29. These sectors account on average for 2.7%, 25.7%, 8.2%, 30.6%, and 32.8% of the private economy's regional GVA, respectively.

exception of agriculture, the construction sector shows, on average, the lowest labor productivity level in our sample.

Table 1.4.1. Output and Employment Multipliers: Decomposition by Economic Sectors

	GVA Multiplier					Employment Multiplier				
	Impact	1 Year	2 Years	3 Years	4 Years	Impact	1 Year	2 Years	3 Years	4 Years
Panel A: Baseline specification for the private sector										
Multiplier	1.68***	1.87***	1.88***	1.81***	1.72***	1.18***	1.52***	1.56***	1.52***	1.43***
	(0.51)	(0.42)	(0.32)	(0.29)	(0.24)	(0.33)	(0.26)	(0.24)	(0.22)	(0.23)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel B: Multipliers by economic sectors										
Agriculture	-0.04	-0.04	-0.04	-0.09**	-0.14***	-0.04	0.01	0.01	0.02	0.04
	(0.07)	(0.08)	(0.08)	(0.04)	(0.03)	(0.10)	(0.07)	(0.06)	(0.05)	(0.05)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Industry	0.70**	0.66**	0.67***	0.67***	0.66***	0.28***	0.36***	0.39***	0.37***	0.38***
	(0.29)	(0.26)	(0.20)	(0.17)	(0.20)	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Construction	0.27**	0.23***	0.23***	0.19***	0.17***	0.33***	0.39***	0.41***	0.35***	0.33***
	(0.11)	(0.06)	(0.05)	(0.05)	(0.04)	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Services	0.69***	0.84***	0.82***	0.75***	0.65***	0.49***	0.63***	0.67***	0.67***	0.60***
	(0.17)	(0.12)	(0.10)	(0.08)	(0.08)	(0.11)	(0.09)	(0.10)	(0.09)	(0.07)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Finance	0.05	0.18	0.19	0.29***	0.40***	0.12*	0.12*	0.08	0.09*	0.08
	(0.21)	(0.13)	(0.13)	(0.10)	(0.07)	(0.07)	(0.07)	(0.07)	(0.05)	(0.06)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963

Notes: Industry includes all industry with the exception of construction. Services combine wholesale, retail, transport, accommodation and food services, information and communication. Finance refers to financial and business services. Here, all estimated multipliers are expressed in terms of GVA because output series are not available at the sectoral level. Therefore, the total multiplier (including all sectors) shows minor differences compared to the baseline output (GDP) multiplier. Additionally, we also exclude GVA of non-market sector as we want to analyze the private sector response.

In terms of the employment multiplier, the picture slightly differs. The services sector is the single most dominant contributor to the increase in aggregate private employment. This finding makes intuitive sense because the services sector includes particular labor-intensive work like hospitality or food services. The industry and construction sectors contribute by a similar amount, whereas the agriculture and finance sectors display the smallest contributions.

Investigating the sector-specific responses of additional variables further highlights the heterogeneous impact of fiscal policy across economic sectors in the Eurozone. In the appendix, we report the responses of investment, wages, and total hours (see Figures 1.B.7, 1.B.8, and 1.B.9). Investment increases particularly in the industry, services, and with some delay also in the finance sectors, which are the strongest contributors to the aggregate output multiplier as well. While investment in the construction sector also rises in the first two years after the fiscal expansion, the response becomes negative thereafter. These results thus help understanding the different output responses across sectors.

Wages increase in all sectors with the most pronounced increase in the industry and construction sectors. Interestingly, while the responses of output and investment in the construction sector fall over time, wages slowly increase over the forecast horizon which might be due to sluggish wage negotiations in this sector. The industry, construction, and services sectors experience the strongest increases in hours worked, whereas the rise is more limited in the finance sector and hours even fall in the agriculture sector.

Taken together, while the increase in aggregate private economic activity is mainly coming from the industry and services sectors (and to some extent from the finance sector), the services sector is the main contributor to the aggregate increase in private employment. The disproportionate amplification effects of the industry and services sectors might be taken into consideration by policymakers when thinking about adequate fiscal stabilization measures.

1.4.5 Regional Fiscal Spillovers

The deep regional integration within the European single market has raised particular interest in how fiscal stimuli spill over from one region to another. In particular, in the presence of positive spillover effects, regions with ample fiscal capacity could use additional fiscal stimuli to boost demand from regions facing substantial economic slack (Blanchard, Erceg, and Lindé, 2016).

Moreover, from an econometric standpoint, the existence of positive (negative) spillover effects of one region's spending on another's outcomes could lead to an overestimation (underestimation) of the true effect of the own regional government spending change. For example, relative output might shift if an increase in one region's output is associated with reducing activity in another region. Strong worker flows from relatively weak to relatively strong performing regions can lead to such relative output shifts. Moreover, while our multiplier estimations assume an increase in one region's spending, other states face the burden of financing the regional stimulus. These channels can lead to negative fiscal spillovers, which would imply that our estimated multipliers are an underestimation of the total effect of public spending on a region. On the contrary, close trade and financial linkages might well induce positive fiscal spillovers, which then result in an overestimation of the impact on local and aggregate economic activity. The conventional wisdom underlying several recommendations shared across policy circles is that fiscal spillovers in the EU are positive and strong (In't Veld, 2016). In the following, we show that regional fiscal spillovers in the Eurozone are relatively small.

Ideally, we would use inter-regional bilateral trade flows to assess the contribution of region j's government spending shock to the spillovers experienced in region i. Unfortunately, these data are not available at the regional European level. However, we use estimates from Thissen, Lankhuizen, Oort, Los, and Diodato (2018), who construct a social accounting matrix with the most likely trade flows between European regions consistent with national accounts.[30] This dataset is the closest proxy for a matrix of bilateral trade between European regions.[31] It contains information for each pair of sector-region on how much each sector in a specific region imported from each individual sector and region. We aggregate this information by region such that we estimate the most likely trade flow between regions in the Eurozone.[32]

We extend the baseline specification (2.D.1) to account for regional fiscal spillovers. First, for each region i and horizon $h = 0, ..., 4$, we compute a weighted sum of spillover fiscal shocks as follows:

$$\sum_{j \neq i} \bar{w}_{i,j}(G_{j,t+m} - G_{j,t-1}),$$

where $G_{j,t}$ is government spending in region j in period t and $j \neq i$. $\bar{w}_{i,j}$ is the average trade weight between both regions for the period 2000-2010.

$$\bar{w}_{i,j} \;=\; \sum_{t=2000}^{2010} w_{i,j,t} \times \frac{1}{11}, \quad \textit{where} \quad w_{i,j,t} \;=\; \frac{imports_{i,j,t}}{G_{j,t}}.$$

We follow Auerbach and Gorodnichenko (2013) and calculate $w_{i,j,t}$ as the ratio between imports in region j coming from region i and government spending in region j in year t. Hence, we account for both the spillovers from trade linkages and the size of the government in the importing regions. Because the trade data are only available for the period 2000–2010, we use the subsample to calculate average trade weights, $\bar{w}_{i,j}$, and hold them constant for the regressions on the entire sample. To assess the size of spillovers, we either use all trade partners, trade partners from the same country, or only i's top 10% of trade partners with regard to $\bar{w}_{i,j}$. Then, we estimate the own and spillover multipliers for each horizon $h = 0, ..., 4$:

30. Coelho (2019) uses the same dataset to study fiscal spillovers associated with structural funds financed by the European Commission.

31. The authors use a top-down approach to construct the time series of multiregional input-output tables, where national accounts in the format of national Supply, Use and Input-Output Tables are taken as given.

32. See Appendix 1.A.3 for more details and Table 1.A.6 for a visualization of our procedure.

$$\begin{aligned}\sum_{m=0}^{h} z_{i,t+m} =&\beta_h \sum_{m=0}^{h}\left(\frac{G_{i,t+m}-G_{i,t-1}}{Y_{i,t-1}}\right)\\ &+\phi_h \sum_{m=0}^{h}\left(\frac{\sum_{j\neq i}\bar{w}_{i,j}(G_{j,t+m}-G_{j,t-1})}{Y_{i,t-1}}\right)\\ &+\gamma_h(L)X_{i,t-k}+\alpha_{i,h}+\delta_{t,h}+\epsilon_{i,t+m}.\end{aligned} \tag{1.4.1}$$

For each horizon h, β_h directly yields the output or employment multiplier of a one percent increase in the own region government spending relative to GDP, and ϕ_h represents the spillover multipliers of a one percent change in trade partners' government spending. A positive (negative) ϕ_h implies that an increase in other regions' government spending raises (lowers) economic activity or employment in the own region. We again use Driscoll and Kraay (1998) standard errors to calculate confidence intervals.

Besides using the baseline instrument described in Equation (1.3.2) for the own regional government spending change, we now also construct an instrument for the regional spillover spending change. We compute this spillover Bartik instrument as:

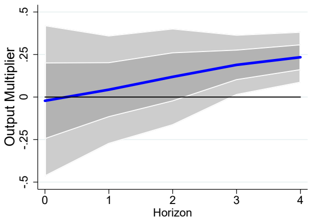
$$\frac{\sum_{j\neq i}\bar{w}_{i,j}\times(G_{J,t}-G_{J,t-1})\times s_j}{Y_{I,t-1}}, \tag{1.4.2}$$

where, similarly to s_i, s_j is the ratio between average per capita government spending in region j belonging to country J.

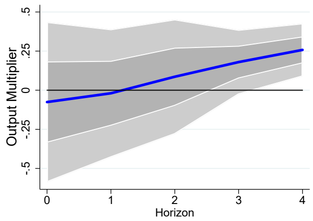
Figure 1.4.3 shows the estimated spillover multipliers. Panel 1.4.3a shows the output multiplier estimates using all trade partners. The estimated spillover multiplier is small and insignificant for most periods of the forecast horizon. Only at the end of the forecast horizon, the multiplier becomes significant but the point estimate remains to be small with a value below 0.25. This general picture of small and mostly insignificant fiscal spillovers holds for employment (Panel 1.4.3d), and also when moving from all regions to only the top 10% trade partners (Panels 1.4.3b and 1.4.3e) or when restricting to regions within the same country (Panels 1.4.3c and 1.4.3f).

To put the magnitude of the spillover multipliers into perspective, remember that our baseline own output and employment multipliers were estimated to be around 2.2 and 1.4. Now, the respective spillover multipliers take on values below 0.25 for output and below 0.15 for employment, which implies that only around 1/10 of the baseline multiplier estimates can be explained by fiscal spillover effects. This insight is further supported by the estimated own multipliers according to Equation (1.4.1) which we report in the appendix (see Figure 1.B.10). Because the own multiplier estimates barely change compared to the baseline results, fiscal spillovers do not affect our main findings.

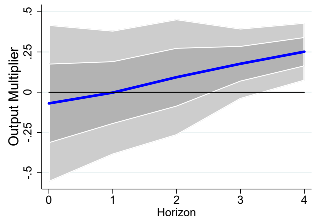
The finding of small fiscal spillovers also persist when looking at other variables than output and employment. In the appendix, we show the spillover responses



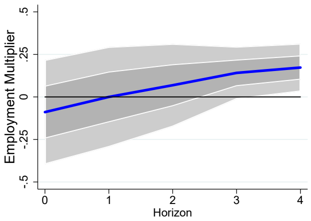
(a) All Regions



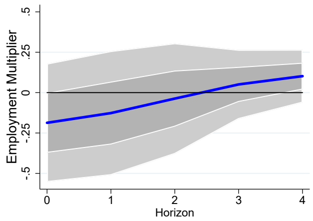
(b) Top Trade Partners



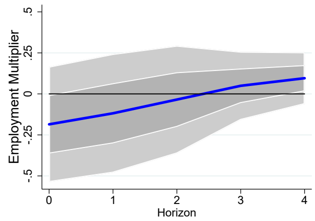
(c) Same Country



(d) All Regions



(e) Top Trade Partners



(f) Same Country

Figure 1.4.3. Output and Employment Spillover Multipliers. Panels 1.4.3a and 1.4.3d show the output and employment spillover multiplier taking into account the spillovers from all regions. Panels 1.4.3b and 1.4.3e consider only the spillovers from the main trade partners (top 10% of the weights). Panels 1.4.3c and 1.4.3f consider only trade partners from the same country. Shaded areas are 68% (dark) and 95% (light) Driscoll and Kraay (1998) confidence intervals.

when considering all regions for investment, consumption (again measured by the number of motor vehicles), and wages (see Figure 1.B.11).[33] For all these additional variables, the effects are limited even showing some evidence of negative spillovers, although the estimates are mostly insignificant. Thus, the results regarding the fiscal transmission mechanism documented in Section 1.4.3 should be interpreted as responses mainly originated by changes in government spending in the own region, whereas cross-regional spillovers contribute only to a very small extent to the detected induced dynamics.

Overall, these results reveal relatively small fiscal spillovers for the Eurozone and thus reinforce the existing results on the U.S. economy (Serrato and Wingender, 2016; Dupor and Guerrero, 2017; Auerbach, Gorodnichenko, and Murphy, 2020a; Bernardini, Schryder, and Peersman, 2020) and Italy (Acconcia, Corsetti, and Simonelli, 2014), but stand in some contrast to the sizeable spillovers reported by Coelho (2019) and McCrory (2020). In addition, our findings do not accord with the conventional policy narrative that government spending increases are thought to have large and positive spillover effects in the Eurozone (In't Veld, 2016). Relatedly,

33. Results are very similar when only considering the top 10% trade partners or when restricting to regions within the same country.

our insights imply that recommendations to jump-start the European economy by increasing public spending in regions with fiscal capacity should be interpreted with caution since the positive spillover effects might be limited despite the European single market.

1.4.6 State Dependent Multipliers

As a final exercise, we investigate whether regional fiscal multipliers in the Eurozone are characterized by significant state dependencies. In particular, we test whether fiscal multipliers depend on the state of the business cycle, on the sign of the fiscal intervention (consolidation versus expansion), and if they differ between core and periphery countries of the Eurozone.

There is an ongoing debate in the literature concerning business cycle-dependent effects of fiscal policy. While some studies indeed provide evidence that fiscal multipliers are larger in economic recessions than economic booms (Auerbach and Gorodnichenko, 2012; Nakamura and Steinsson, 2014), others do not find that fiscal multipliers vary across states of the business cycle (Ramey and Zubairy, 2018). Concerning the sign of the fiscal intervention, Barnichon, Debortoli, and Matthes (2020) show that, at the aggregate U.S. level, a reduction in government spending is associated with a larger fiscal multiplier when compared to an increase in government spending. Born, D'Ascanio, et al. (2019) find similar results for a panel of advanced and emerging market economies. Finally, the significant fiscal consolidation measures implemented in several European countries in the aftermath of the Great Recession and the dismal growth performances that followed have raised questions about the detrimental effects of austerity programs (Blanchard and Leigh, 2013). Thus, testing for a potential non-linearity between core and periphery countries is intended to provide information about significant country heterogeneities within the Eurozone.

To test for potential state dependencies, we extend our baseline specification (2.D.1). For each horizon $h = 0, \ldots, 4$, we estimate the regression:

$$\begin{aligned}\sum_{m=0}^{h} z_{i,t+m} =& I_{i,t}\left[\beta_h^A \sum_{m=0}^{h} \frac{G_{i,t+m} - G_{i,t-1}}{Y_{i,t-1}} + \gamma_h^A(L)X_{i,t-k}\right] \\ &+ (1 - I_{i,t})\left[\beta_h^B \sum_{m=0}^{h} \frac{G_{i,t+m} - G_{i,t-1}}{Y_{i,t-1}} + \gamma_h^B(L)X_{i,t-k}\right] \\ &+ \alpha_{i,h} + \delta_{t,h} + \varepsilon_{i,t+m},\end{aligned} \tag{1.4.3}$$

where $I_{i,t}$ is an indicator variable for the defined state in period t. We now instrument spending changes with the Bartik instrument but interacted with the state indicator. β_h^A and β_h^B directly yield, for each horizon h and states A and B, the fiscal output or employment multiplier, respectively. Here, we use Driscoll and Kraay

(1998) standard errors, and compute the Anderson and Rubin (1949) test and the heteroskedasticity and autocorrelation consistent (HAC) test to test for statistical differences in multipliers across states.

To investigate potential state dependencies across the business cycle, we closely follow Nakamura and Steinsson (2014) and define the indicator variable, $I_{i,t}$, based on regional unemployment fluctuations. More precisely, we define that a region is in an economic expansion (recession) in t if the unemployment rate in $t-1$ is below (above) the region's median. We define the state based on lagged unemployment to minimize contemporaneous correlations between fiscal shocks and the state of the business cycle.

Panel B in Table 1.4.2 presents the results, where the upper part reports our baseline (state-independent) estimates to allow for a direct comparison. For all years, the multiplier is estimated to be larger when the region experiences a recession compared to an economic boom. This is true for the output and employment multiplier alike. For the employment multiplier, the difference across business cycle states is also estimated to be significant, while for the output multiplier, the difference is borderline insignificant (especially at longer horizons). Thus, our evidence broadly supports the view that fiscal interventions have a larger effect on the economy during periods of economic slack, in line with the empirical evidence by Auerbach and Gorodnichenko (2012) and Nakamura and Steinsson (2014).

Next, we study whether the sign of the fiscal intervention affects the size of the fiscal multiplier. To differentiate between fiscal consolidations and fiscal expansions, we allow for different effects depending on the sign of our Bartik instrument. Whenever the change in national spending takes on a positive value, we treat the fiscal intervention as a spending expansion ($I_{i,t} = 1$), and whenever the instrument takes on a negative value, we assign a fiscal consolidation ($I_{i,t} = 0$).[34]

Panel C of Table 1.4.2 shows the estimated fiscal multipliers.[35] For the output multiplier, we do not find clear evidence that the sign of the fiscal intervention considerably influences the size of the multiplier. While for some years the output multiplier associated with a fiscal expansion is larger than the one associated with a fiscal consolidation, the picture flips in other years. For most years of the forecast horizon, the employment multiplier brought by a fiscal consolidation is larger than the one brought by a fiscal expansion. However, the differences are small such that multipliers do not significantly depend on the sign of the fiscal intervention.

Finally, we test for differences in fiscal multipliers between core and periphery countries. Greece, Ireland, Italy, Portugal, and Spain are considered periphery

34. This procedure implies that out of the 2,621 regional shocks considered, 2,207 shocks, or 84%, are treated as fiscal expansions, while the remaining 414 or 16% are treated as consolidations.

35. The multipliers are positive in both states because a fiscal consolidation is associated with a fall in government spending and a reduction in output (employment), whereas a fiscal expansion leads to an increase in government spending and a rise in output (employment).

Table 1.4.2. Output and Employment State Dependent Multipliers

	Output Multiplier					Employment Multiplier				
	Impact	1 Year	2 Years	3 Years	4 Years	Impact	1 Year	2 Years	3 Years	4 Years
Panel A: Baseline Specification										
Multiplier	2.14***	2.33***	2.33***	2.26***	2.21***	1.12***	1.43***	1.51***	1.47***	1.44***
	(0.40)	(0.32)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel B: Business Cycle Recessions versus Expansions										
Recessions	2.57***	2.69***	2.76***	2.74***	2.64***	1.44***	1.77***	1.92***	1.97***	1.92***
	(0.56)	(0.34)	(0.25)	(0.21)	(0.15)	(0.33)	(0.15)	(0.11)	(0.18)	(0.20)
Expansions	2.17***	2.45***	2.41***	2.35***	2.33***	0.94***	1.29***	1.38***	1.38***	1.33***
	(0.26)	(0.29)	(0.22)	(0.20)	(0.17)	(0.19)	(0.21)	(0.22)	(0.22)	(0.23)
HAC Test	0.33	0.36	0.16	0.10	0.14	0.01	0.04	0.05	0.10	0.09
AR Test	0.26	0.32	0.11	0.11	0.16	0.01	0.03	0.04	0.09	0.10
# Obs	2428	2266	2104	1943	1783	2428	2266	2104	1943	1783
Panel C: Fiscal Consolidation versus Fiscal Stimulus										
Consolidation	2.16***	2.55***	2.42***	2.33***	2.29***	1.09***	1.47***	1.37***	1.36***	1.32***
	(0.47)	(0.39)	(0.30)	(0.25)	(0.22)	(0.26)	(0.12)	(0.06)	(0.09)	(0.12)
Stimulus	2.33***	2.33***	2.45***	2.26***	2.36***	0.97**	1.25***	1.43***	1.18***	1.27***
	(0.68)	(0.59)	(0.51)	(0.40)	(0.29)	(0.44)	(0.40)	(0.44)	(0.29)	(0.27)
HAC Test	0.77	0.61	0.93	0.79	0.64	0.78	0.57	0.90	0.45	0.83
AR Test	0.78	0.60	0.93	0.79	0.67	0.78	0.57	0.90	0.46	0.82
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel D: Core versus Periphery										
Core	2.63***	2.66***	2.73***	2.92***	2.90***	1.34***	1.68***	1.80***	2.20***	2.28***
	(0.59)	(0.42)	(0.27)	(0.23)	(0.21)	(0.40)	(0.31)	(0.24)	(0.17)	(0.18)
Periphery	1.79***	2.06***	2.10***	2.01***	1.99***	1.04***	1.35***	1.43***	1.34***	1.32***
	(0.28)	(0.29)	(0.27)	(0.25)	(0.20)	(0.20)	(0.13)	(0.13)	(0.13)	(0.16)
HAC Test	0.11	0.09	0.02	0.00	0.00	0.29	0.23	0.13	0.00	0.00
AR Test	0.11	0.14	0.12	0.06	0.09	0.29	0.25	0.21	0.06	0.11
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963

Notes: In Panel B, we show the results for expansions and recessions. A given region is in the low unemployment state (expansion) if in the previous period the unemployment rate was below the region's median, and it is in high unemployment state (recession) if the rate was above or equal to the region's median. In Panel C, we show state dependencies for fiscal consolidations and stimuli. Precisely, we define fiscal consolidations (stimuli) whenever the Bartik instrument is negative (positive). In Panel D, we study differences between the core and periphery Eurozone countries. The PIIGS countries (Portugal, Ireland, Italy, Greece, and Spain) are considered periphery countries, while Austria, Belgium, Finland, France, Germany, Luxembourg, and the Netherlands belong to the core group. The AR Test presents the p-value of the difference between states using the Anderson and Rubin (1949) test, while the HAC Test indicates the HAC-robust p-values.

$(I_{i,t} = 1 \ \forall t)$, while Austria, Belgium, Finland, France, Germany, Luxembourg, and the Netherlands are treated as core countries $(I_{i,t} = 0 \ \forall t)$. In this case, the indicator variable is time invariant. Panel D of Table 1.4.2 shows that fiscal multipliers in the Eurozone display significant country heterogeneity. Both output and employment multipliers are considerably larger in core countries than in the periphery. Moreover, for most horizons considered, the difference between the multipliers is also estimated to be significant. Thus, specific country characteristics in the periphery seem to reduce the impact of fiscal interventions, whereas the opposite describes the situation in the core countries. The political and legal system, the labor market and pricing frictions or financial developments are all potentially responsible for differences in fiscal multipliers between core and periphery countries. As shown earlier, to a significant amount, fiscal policy in the Eurozone operates via a productivity channel through which higher government spending increases productivity and private investment. Because labor productivity and TFP are, on average, lower in the periphery than in the core countries, productivity differences across member states might rationalize differences in fiscal multipliers. However, understanding in more detail what drives these country heterogeneities could be an interesting avenue for future research.

1.5 Conclusion

The effectiveness of fiscal policy in the Eurozone is a central topic of ongoing debates among economists and policymakers alike. Using a newly assembled dataset at the regional level, this paper investigates the impact of fiscal policy in the Eurozone and provides new evidence on its transmission mechanism. In particular, our baseline estimates reveal a fiscal spending output (employment) multiplier of 2.2 (1.4). Moreover, the regional fiscal stimulus leads to a significant increase in private investment together with a rise in labor productivity and TFP. Furthermore, an increase in government spending causes higher wages and durable consumption expenditure and a rise (fall) in the labor share (markup). Concerning labor margins, we find that higher government spending raises total hours worked, which is driven by changes in the extensive margin (total employment), whereas the intensive margin (hours per worker) barely reacts. Our estimates imply a cost per job created of about €30,000.

We also detect significant sectoral heterogeneity, with the industry and services sectors contributing a disproportionate amount to the aggregate increase in private economic activity. The paper provides further evidence that there are small and mostly insignificant regional fiscal spillovers which stands in contrast to a common view of positive and sizeable fiscal spillovers shared in policy discussions. Finally, we detect notable state-dependencies in regional fiscal multipliers. They are larger in economic recessions and in the core countries of the Eurozone but do not significantly depend on the sign of the fiscal intervention (stimulus versus consolidation).

Our new evidence should contribute to discussions among academics and policymakers about the gains and limitations of fiscal policy in the Eurozone. In particular, our results suggest that fiscal policy is an effective tool to stimulate regional employment, investment, and productivity. Furthermore, despite the deep regional integration within the Eurozone, increased public spending in regions with ample fiscal capacity might have only small spillover effects. Finally, heterogeneous effects across industries, states of the economy, and member states should be taken into account when designing adequate stabilization measures.

Appendix 1.A Data Description

Table 1.A.1. NUTS Structure

NUTS 0	NUTS 1	#	NUTS 2	#	NUTS 3	#
Austria	Groups of states	3	States (Länder)	9	Groups of districts	35
Belgium	Regions	3	Provinces and Brussels (Verviers split in 2)	11	Arrondissements	44
Finland	Mainland, Åland	2	Large areas (Suuralueet / Storområden)	5	Regions (Maakunnat / Landskap)	19
France	ZEAT Overseas Regions	9	Regions	27	Departments	101
Germany	States (Bundesland)	16	Government regions (Regierungbezirk)	39	Districts Kreis	429
Greece	Groups of regions	4	Regions	13	Prefectures	51
Ireland	-	1	Regional Assemblies	3	Regional Authorities	8
Italy	Groups of regions	5	Regions (Trentino-Alto Adige split in 2)	21	Provinces	110
Luxembourg	-	1	-	1	-	1
Netherlands	Groups of provinces	4	Provinces	12	COROP regions	40
Portugal	Mainland and 2 autonomous regions	3	5 Coordination regions 2 autonomous regions	7	Groups of Municipalities	25
Spain	Groups of communities	7	17 Autonomous communities 2 autonomous cities	19	Provinces, Islands Ceuta, Melilla	59
Total		58		167		922

Table 1.A.2. Variables Description

Variable Name	Computation	Definition [Source]
GDP*pc*	GDP / Population	Regional gross domestic product per capita [ARDECO]
Gov. Spending *pc*	(1+Int. Cons)*Non-Market GVA / Population	Regional proxy for government spending per capita [ARDECO]
Employment		Total employment [ARDECO]
Employment Rate	Employment / Population	Total employment per capita [ARDECO]
Hours		Total hours worked [ARDECO]
Hourly Wage	Compensation / Hours	Regional average compensation per hour (all sectors) [ARDECO]
Investment *pc*	Private GFCF/ Population	Total private (all sectors excluding non-market) investment per capita (fixed gross capital formation) [ARDECO]
Labor Share	Private Compensation / private GVA	Private (all sectors excluding non-market) compensation as a share of private GDP [ARDECO]
Productivity	GVA / Hours	Labor productivity, value added per hour (all sectors) [ARDECO]
TFP	Check 1.A.5 for details	Total factor productivity (private sectors) [ARDECO and Gardiner, Fingleton, and Martin (2020)]
Motor Vehicles	# motor vehicles / Population	Stock of all motor vehicles (except trailers and motorcycles) per capita [Eurostat]
Inflation	Growth rate of regional CPI	Austria [statistik], Finland [stat], Germany [destatis], Italy [istat], Portugal [ine], Spain [ine]

1.A.1 ARDECO - Regional European Data

ARDECO is the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy, maintained and updated by the Joint Research Centre. It is a highly disaggregated dataset across both sectoral and subregional dimensions. The databasebuilds on the previous Cambridge Econometrics regional dataset and contains a set of long time-series indicators for EU regions at various statistical scales (NUTS 0, 1, 2, and 3 level) using the NUTS 2016 regional classification. The dataset includes data on demography, labor markets, capital formation and domestic product by six sectors. The six sectors are (1) agriculture, forestry and fishing, (2) industry excluding construction, (3) construction, (4) wholesale, retail, transport, accommodation, and food services, information and communication, (5) financial and business services, and (6) non-market services.

ARDECO data is an annual unbalanced panel covering the period of 1980–2017 for the European Union (EU) and some European Free Trade Association (EFTA) and candidate countries. Its main data source is Eurostat (the Statistical Office of the European Commission), complemented, where necessary, by other appropriate national and international sources. ARDECO is constructed in such a way that the country aggregates its various time series equal to the corresponding time series in the AMECO dataset referring to the National Accounts. Starting from 2002, Eurozone countries publish national series in EUR. National currency data for all years prior to the switch of the country to EUR have been converted using the irrevocably fixed EUR conversion rate. Cross-country comparisons and aggregations should continue to be based only on historical series established in ECU up to 1998 and their statistical continuation in EUR from 1999 onward. Exchange rates and purchasing power parities have been converted in the same manner. We thus use the series with real variables expressed in 2015 constant price in ECU/EUR.

1.A.2 Regional government spending measure

We now explain in detail why our regional measure (GVA plus intermediate consumption of the non-market sector) is indeed a valid proxy for government spending.

First, as previously mentioned, ARDECO's regional data is consistent with the national accounts data by construction. By definition, there exists a close link between government spending and the GVA of the non-market sector, however, they differ in two dimensions: actors and composition. Regarding the first, even though the non-market sector includes other institutional units, the general government is the main actor responsible for changes in non-market GVA.

In particular, the non-market sector consists of six sub-sectors: "Public administration and defense", "Education", "Human health and social work", "Arts, entertainment and recreation", "Other service activities," and "Activities of household and extra-territorial organizations and bodies." The first sub-sector, "Public administration and defense," refers to activities by the general government, but not all government bodies are automatically classified under this sub-sector. For example, a secondary school administered by the central or local government is classified as "Education," and a public hospital is allocated to "Human health and social work." Thus, the two sub-sectors "Education" and "Human health and social work" are also closely linked to the general government in the national accounts, while the last three sub-sectors are linked only loosely.

Relying on Finnish data, we indeed find that 100% of the GVA in the sub-sector "Public administration and defense" is booked as government expenditure in the national accounts. For the second and third sub-sectors, this number is 88% and 75%, respectively.[36] Moreover, for the countries in our sample, these first three sub-sectors, which are most closely linked to activities by the general government, make up the lion's share of the non-market GVA, accounting for 84%.[37] Consequently, almost the entire variation in GVA of the non-market sector refers to activities by the general government.

Concerning the second dimension, we now describe the compositional differences between non-market GVA and government spending. In the national accounts, government spending is defined as follows:

Final consumption expenditure of the general government
=Gross value added of the general government
+Intermediate inputs of the general government
+Social transfers in kind purchased market production
-Market output and output for own final use
-Payments for non-market output

GVA of the general government is the major component of government spending and fully accounted in the GVA of the non-market sector. Country level data show that GVA of the general government accounts for almost 70% of government spend-

36. In our sample, with the exception of Finland, cross-classification tables between NACE and institutional sectors are not publicly available. Statistics Finland's series can be consulted here.

37. According to data collected from Eurostat and for the sample comprising the first twelve Eurozone countries between 1999 and 2017.

ing.[38] Thus, our proxy measures the single-most dominant source of government expenditures. However, the main difference between government spending and the GVA of the general government is due to intermediate inputs and social transfers in kind. When again looking at country level data, we find that GVA and intermediate consumption account for about 97% of government spending. To include intermediate consumption in our government spending measure, we use input-output tables from the PBL EUREGIO database that provide estimates for intermediate consumption of the non-market sector at the NUTS 2 level from 2000–2010. We find that, on average, intermediate consumption accounts for around 30% of total expenditure of the non-market sector at the regional level, which is very similar to the corresponding number when looking at expenditures of the general government at the national level (27%). Moreover, the variation in this ratio for a given region is rather stable over time.[39] Thus, we adjust regional GVA of the non-market sector by a region-specific time-invariant scaling factor to include intermediate consumption in our government spending measure to obtain our proxy for regional government spending.

Second, to quantitatively assess the quality of our proxy, we study its time series properties comparing them to the actual measure of government spending at the national level.[40] In particular, we use intermediate consumption adjusted GVA of the non-market sector from the ARDECO and EUREGIO datasets at the NUTS 0 (country) level and the series on final consumption expenditures of the general government from the OECD and AMECO. The pooled correlation coefficients between the GVA and the government spending series (both in levels and logs) are about 0.99 and highly significant. Such strong positive correlations also hold at the individual country level as can be seen in Table 1.A.3. With the exceptions of Italy and Portugal, the correlation coefficients are around 0.99. Moreover, Table 1.A.4 shows the estimation results from regressing government spending on our proxy in log level with and without country and year fixed effects. All regressions indicate a significant and strong relationship between the two variables with coefficients very close to 1.

So far, the analysis was conducted at the national (NUTS 0) level. We go one step further and compare our regional (NUTS 2) proxy for government spending to the government final consumption expenditure series from the PBL EUREGIO database, which is discussed in more detail in Appendix 1.A.3. The EUREGIO database provides estimates of regional government spending but only for a subset of our sample

38. According to data collected from Eurostat and for the sample comprising the first twelve Eurozone countries between 1999 and 2017.

39. When calculating time-varying intermediate consumption ratios for each region, the average standard deviation is 0.018.

40. Remember that, at the national level, GVA of the non-market sector, intermediate consumption, and government spending are available, whereas at the regional level only GVA of the non-market sector and intermediate consumption are available from national accounts data.

(2000 to 2010). Notwithstanding, when doing this comparison, we find that both series are highly significantly correlated. The correlation coefficient between the two series in logs is close to 1. Table 1.A.5 presents the same regressions as before but now at the regional level. There is a strong and significant relationship between the EUREGIO estimated government spending series and our government spending proxy given that the coefficients are estimated to be close to 1.

In sum, we conclude that regional GVA of the non-market sector is a valid proxy for regional government spending. It is closely linked to government spending in the national accounts, and both series share remarkably similar time series properties.

Table 1.A.3. Correlation Between Government Spending and our proxy by Country

	Correlation w/ OECD Series		Correlation w/ AMECO Series	
Country	Levels	Logs	Levels	Logs
Austria	0.9899	0.9886	0.9876	0.9859
Belgium	0.9762	0.9786	0.9917	0.9917
Finland	0.9698	0.9728	0.9906	0.9910
France	0.9965	0.9967	0.9931	0.9931
Germany	0.9905	0.9907	0.9848	0.9837
Greece	0.9755	0.9751	0.9851	0.9846
Ireland	0.9581	0.9660	0.9967	0.9972
Italy	0.8335	0.8412	0.8928	0.8976
Luxembourg	0.9950	0.9968	0.9946	0.9961
Netherlands	0.9826	0.9845	0.9912	0.9918
Portugal	0.9753	0.9757	0.9143	0.9100
Spain	0.9905	0.9924	0.9869	0.9904
All	0.9976	0.9977	0.9975	0.9988

Notes: This shows, by country, the correlation in levels and logs between our proxy for government spending (from ARDECO) with actual government spending (from OECD and AMECO). Whenever possible, we use data from 1999 to 2017, with the exception of Greece, for which we use the period 2001–2017.

1.A.3 PBL EUREGIO database

To include intermediate consumption in our government spending proxy and for the fiscal spillover analysis in Section 1.4.5, we use the PBL EUREGIO database. This is the first time-series (annual, 2000–2010) of global IO tables with regional detail for the entire large trading bloc of the European Union. This database allows for a regional analysis at the NUTS 2 level consistent with our baseline method. The tables merge data from WIOD (the 2013 release) with regional economic accounts and inter-regional trade estimates developed by PBL Netherlands Environmental Assessment Agency and complemented with survey-based regional input-output data for a limited number of countries. All data used are survey data, and only non-behavioral assumptions have been made to estimate the EUREGIO dataset. These two general rules of data construction allow empirical analyses focused on impacts of changes in behavior without endogenously having this behavior embedded already by construction. More detailed information can be found in Thissen et al. (2018).

Table 1.A.6 shows an example of the type of information provided by the IO tables from Thissen et al. (2018). For each pair of sector-region we have information about how much a specific sector in a specific region imported from each individual sector from each individual region, all measured in million dollars. Given this infor-

Table 1.A.4. Proxy for Government Spending at the National Level

	log *proxy*		
	(1)	(2)	(3)
Panel A: OECD			
log *GovSpend*	0.920***	0.860***	0.840***
	(0.038)	(0.063)	(0.072)
# Obs	223	223	223
Panel B: AMECO			
log *GovSpend*	1.049***	1.113***	1.111***
	(0.031)	(0.053)	(0.082)
# Obs	212	212	212
Country FE	No	Yes	Yes
Time FE	No	No	Yes

Notes: Columns (1) to (3) show the results from regressing the log of the government spending series from OECD and AMECO on the log of our proxy for government spending at the national level (NUTS 0). We use data from 1999 to 2017 and display robust standard errors clustered at the country level in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.A.5. Proxy for Government Spending at the Regional Level

	log *proxy*		
	(1)	(2)	(3)
log *GovSpend*	1.020***	1.032***	0.666***
	(0.045)	(0.113)	(0.199)
Regional FE	No	Yes	Yes
Time FE	No	No	Yes
# Obs	1604	1604	1604

Notes: Columns (1) to (3) show the results from regressing the log of the regional government spending series from EUREGIO on the log of our proxy for government spending from ARDECO at the regional level (NUTS 2). Data from 2000 to 2010. Robust standard errors clustered at the region level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

mation, we aggregate all sectors within a given region so that we have an estimate of the most likely trade flows between regions in the Eurozone. This means that we have an estimate of how much million dollars worth of goods and services a specific region imported from all other individual regions. Finally, we convert this measure into euros using a yearly average of the euro-dollar exchange rate.

Table 1.A.6. Example of IO table from Thissen et al. (2018)

			Burgenland (AT11)													
			ss1	ss2	ss3	ss4	ss5	ss6	ss8	ss9	ss10	ss11	ss12	ss13	ss14	ss15
Burgenland (AT11)	ss1	Agriculture	44.4	0.1	59.0	1.3	0.6	0.2	13.2	1.2	0.8	5.2	0.3	0.1	0.5	0.7
	ss2	Mining quarrying and energy supply	2.8	47.8	4.2	0.8	7.1	2.2	14.3	9.6	5.3	3.9	3.6	1.4	8.7	17.6
	ss3	Food beverages and tobacco	5.9	0.2	18.9	0.9	1.1	0.7	1.7	0.8	2.3	12.0	0.8	0.4	0.6	5.6
	ss4	Textiles and leather	0.1	0.0	0.1	1.0	0.2	0.2	0.3	0.1	0.1	0.0	0.0	0.0	0.0	0.1
	ss5	Coke refined petroleum nuclear fuel and chemicals etc	2.4	0.8	2.5	1.0	6.8	5.4	9.0	5.2	4.8	0.6	2.8	0.6	1.8	4.2
	ss6	Electrical and optical equipment and transport equipment	0.6	1.4	1.0	0.3	0.9	8.2	4.4	4.9	3.2	0.4	1.8	0.9	2.7	2.8
	ss8	Other manufacturing	4.8	2.4	6.8	1.1	6.5	19.8	94.0	50.4	14.3	1.4	4.2	2.9	9.0	11.7
	ss9	Construction	3.0	1.8	1.5	0.4	1.4	1.9	5.3	35.9	3.4	3.5	5.1	3.7	40.4	20.2
	ss10	Distribution	16.6	4.8	26.5	10.4	20.0	32.1	53.6	30.0	31.9	8.0	12.8	3.1	9.4	23.6
	ss11	Hotels and restaurant	0.1	0.0	0.2	0.1	0.1	0.1	0.3	0.2	0.4	0.5	5.9	0.5	0.3	0.3
	ss12	Transport storage and communication	1.6	2.9	5.6	1.4	4.7	4.0	16.6	5.3	17.0	1.5	38.0	5.4	6.2	11.4
	ss13	Financial intermediation	5.1	4.6	5.3	1.5	4.5	6.5	15.6	13.1	24.1	5.5	9.9	39.1	24.2	30.4
	ss14	Real estate renting and business activities	2.5	4.1	10.7	2.1	7.6	11.4	24.7	18.7	47.7	9.6	17.8	20.4	65.5	38.6
	ss15	Non-Market service	3.7	0.9	3.2	0.8	1.7	1.4	9.5	1.6	6.6	3.0	2.4	3.4	25.7	47.8

Notes: This Figure shows an example of an input-output table for just one region from Thissen et al. (2018) (Burgenland, Austria). Each column states the amount of inputs a sector from Burgenland receives from another sector from another (or the same in this case) region. For example, the agricultural sector in Burgenland (first row) gives as inputs 44.4 million dollars worth of goods/services to the agricultural sector in Burgenland and gives 59.0 million dollars to the Food and Beverages and Tobacco sector in Burgenland (first row, third column).

1.A.4 Military Data at the Country Level

Military expenditure data are taken from the Stockholm International Peace Research Institute (SIPRI) Military Expenditure Database 2019. SIPRI collects military spending data from several sources, including government agencies and international organizations. The military spending data include all spending on current military forces and activities such as personnel, procurement, operations, military research and development, and construction. The largest component is usually salaries to and benefits of military personnel. The data are at an annual frequency.

1.A.5 Total Factor Productivity

Contrary to the remaining dependent variables, for which we only use data from ARDECO, TFP measures make use of capital stock estimates from Gardiner, Fingleton, and Martin (2020).[41] Its construction hinges on the methodology used by Derbyshire, Gardiner, and Waights (2013), which makes use of the Perpetual Inventory Method using regional investment series from ARDECO and data from EU KLEMS for the national depreciation rate and national initial capital stock.[42]

TFP is then calculated as a residual with a labor share of two-thirds as is common in the literature. Precisely, we estimate

$$TFP_{i,t} \;=\; exp\Big(ln(GVA_{i,t}) - 1/3 \times ln(K_{i,t}) - 2/3 \times ln(L_{i,t})\Big) \tag{1.A.1}$$

where GVA is total Gross Value-Added, K is capital stock adjusted to constant 2015 EUR using national CPI data from the World Bank, and L is total hours worked. All variables are measured at the regional level i and at year t. We use all measures in private sector terms and obtained them by subtracting the non-market sector values from their total. Hence, there is no need to remove the government spending component as in Brueckner, Valentinyi, and Pappa (2019). We take the exponential of this expression to compute TFP growth rate in the exact same way as we compute it for the remaining variables, instead of taking log differences.

41. It was necessary to adjust the regional division to be in accordance with the most recent NUTS 2016 version for France, Ireland, Poland and the United Kingdom.

42. More details on its construction can be found here.

Appendix 1.B Additional Results

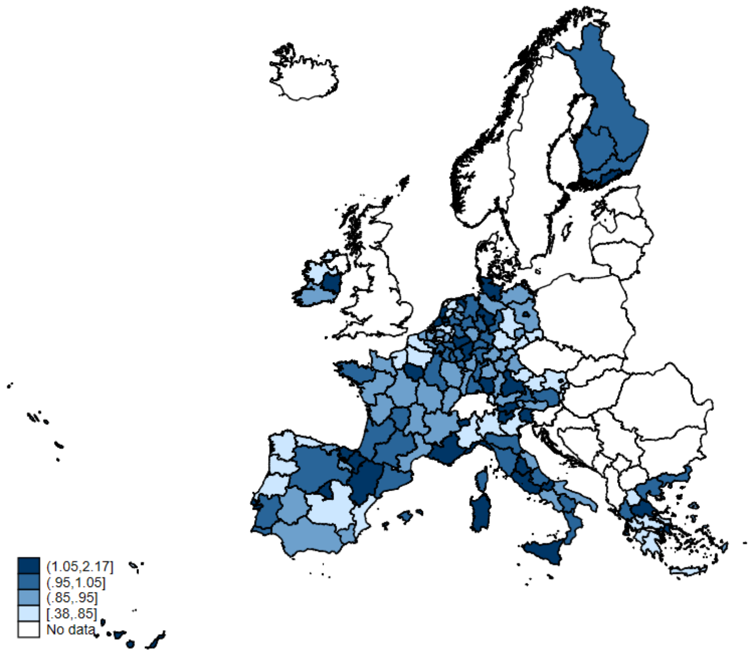


Figure 1.B.1. Sample Regions and the Share s_i. The Figure depicts the map of European NUTS 2 regions with the share s_i used in Bartik instrument construction.

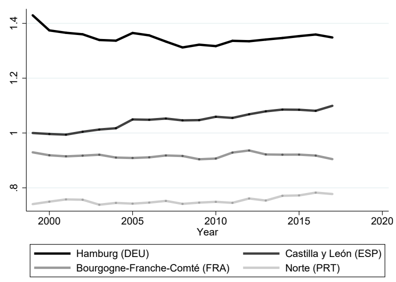
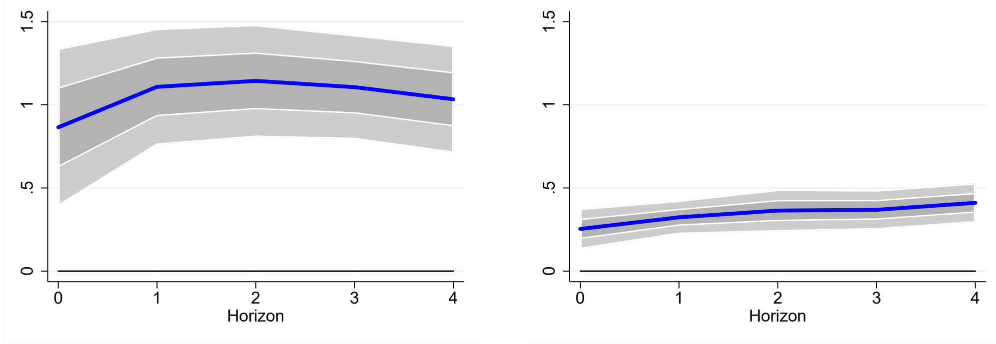


Figure 1.B.2. Ratio between Regional and National per capita Government Sending. This Figure plots the ratio between regional and national per capita government sending over time for selected regions in the sample.



(a) Private Employment Multiplier

(b) Public Employment Multiplier

Figure 1.B.3. Private and Public Employment Multipliers. Panels 1.B.3a and 1.B.3b show the cumulative employment multipliers for private and non-market sectors relative to total employment, respectively. Shaded areas are 68% (dark) and 95% (light) confidence intervals.

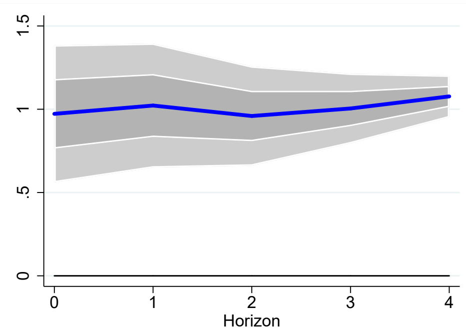


Figure 1.B.4. Investment Multiplier. This figure shows the cumulative relative private investment multiplier (using change in private investment relative to output). Shaded areas are 68% (dark) and 95% (light) confidence intervals.

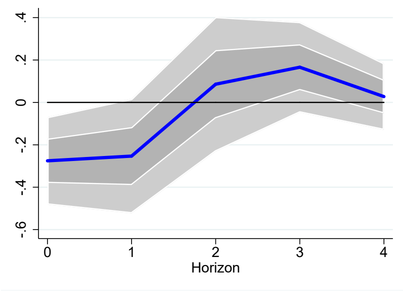


Figure 1.B.5. Impulse Response of Inflation. The figure plots the response of inflation to a one percent increase in per capita government spending relative to per capita GDP. The impulse response is expressed in percentage point changes (its differences). Shaded areas are 68% (dark) and 95% (light) confidence intervals.

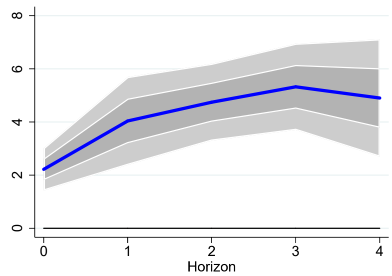
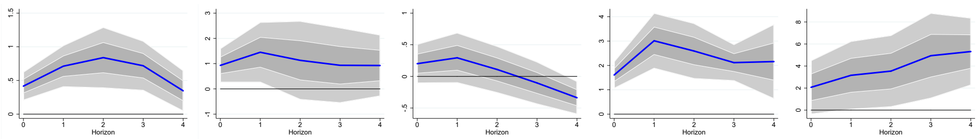
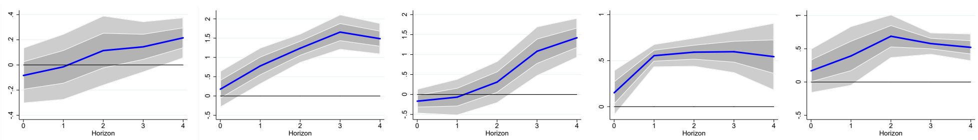


Figure 1.B.6. Impulse Response of Disposable Income. The figure plots the response of per capita disposable income to a one percent increase in per capita government spending relative to per capita GDP. The impulse response is expressed in percent changes (growth rates). Shaded areas are 68% (dark) and 95% (light) confidence intervals.



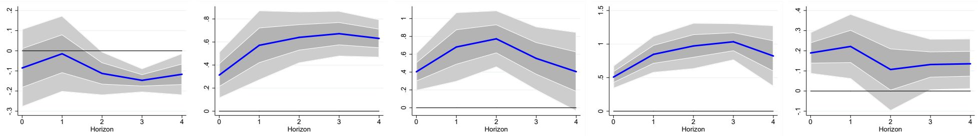
(a) Agriculture **(b)** Industry **(c)** Construction **(d)** Services **(e)** Finance

Figure 1.B.7. Impulse Responses of Investment per Sector. These figures plot the decomposition of the impulse response of private investment across private sectors. All responses are expressed in percent changes (growth rates) relative to private investment. Shaded areas are 68% (dark) and 95% (light) confidence intervals.



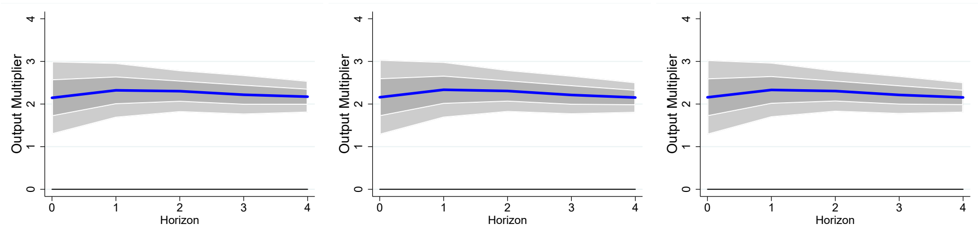
(a) Agriculture **(b)** Industry **(c)** Construction **(d)** Services **(e)** Finance

Figure 1.B.8. Impulse Responses of Hourly Wage per Sector. These figures plot the decomposition of the impulse response of compensation across private sectors. All responses are expressed in percent changes (growth rates) relative to hourly wages in the private sector. Shaded areas are 68% (dark) and 95% (light) confidence intervals.

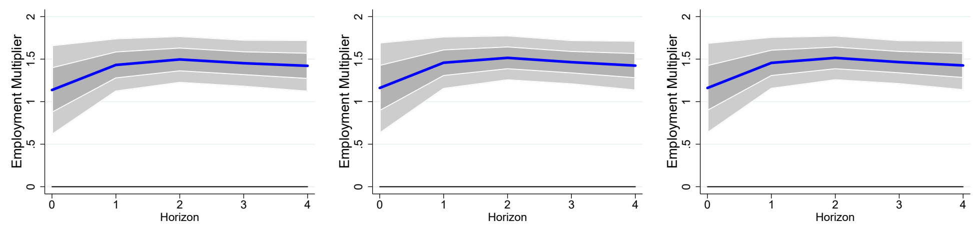


(a) Agriculture **(b)** Industry **(c)** Construction **(d)** Services **(e)** Finance

Figure 1.B.9. Impulse Responses of Total Hours per Sector. These figures plot the decomposition of the impulse response of hours worked across private sectors. All responses are expressed in percent changes (growth rates) relative to total hours in the private sector. Shaded areas are 68% (dark) and 95% (light) confidence intervals.

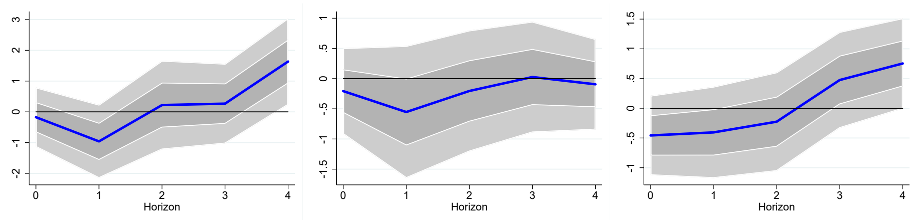


(a) All Regions **(b)** Top Trade Partners **(c)** Same country



(d) All Regions **(e)** Top Trade Partners **(f)** Same country

Figure 1.B.10. Output and Employment Multipliers: Spillover Analysis. Plots in the top row refer to output multipliers, while those in the bottom row refer to employment multipliers. Panels 1.B.10a and 1.B.10d show the multipliers taking into account the spillovers from all regions, Panels 1.B.10b and 1.B.10e consider only the spillovers from the main trade partners (top 10% of the weights), and Panels 1.B.10d and 1.B.10f account for the spillovers from all regions within the country. Shaded areas are 68% (dark) and 95% (light) Driscoll and Kraay (1998) confidence intervals.



(a) Investment **(b)** Motor Vehicles **(c)** Hourly Wage

Figure 1.B.11. Spillover Impulse Responses. Figures show the spillover impulse responses of private investment, registered motor vehicles, and hourly wage. Shaded areas are 68% (dark) and 95% (light) Driscoll and Kraay (1998) confidence intervals.

Appendix 1.C Robustness of the main results

In this section, we demonstrate that our main Eurozone multiplier estimates are robust to several modifications of the baseline model. The estimates change only little when applying alternative ways to construct the Bartik instrument and using different ways to extract unexpected variation in national government spending. Moreover, our findings are robust to changes in the sample and to additionally controlling for national tax policies and sovereign spreads. Finally, we also demonstrate that our results are not prone to dynamic and cross-sectional heterogeneity.

1.C.1 Instrument Construction

We start by exploring alternative ways to construct the Bartik instrument. In the baseline, we use the five years preceding the Eurozone accession to compute the regional share of government spending, s_i. However, as suggested by Nekarda and Ramey (2011), there might have been important structural changes over time that affect the regional distribution of government spending. Taking this possibility into account, we follow Nekarda and Ramey (2011) and compute the regional shares based on all years of the sample. Table 2.4.2 presents the results for the output and employment multipliers, and the first rows also report the baseline estimates. The second panel of Table 2.4.2 (Alternative s_i (I)) shows that our results barely change when using this alternative instrument construction. As a second check, we use absolute levels in regional and national government spending to construct the share s_i. In this case, the regional shares indicate scaling factors and add up to one at the national level. The second panel of Table 2.4.2 (Alternative s_i (II)) presents the results of this exercise, indicating that the multiplier estimates do not change much.

So far, we have used our proxy for government spending at both the NUTS 2 level and the national level. Although official government spending data are not available at the regional level, they are published at the national level and thus, it can be used to compute the Bartik instrument. To be precise, we measure G_I in Equation (1.3.2) as national government spending. The results from Panel B in Table 2.4.2 (National Accounts) show that the multipliers increase slightly, but the overall dynamics remain unchanged.

1.C.2 Unexpected Variation in National Spending

The baseline instrument relies on observed national government spending changes to instrument for regional changes. To account for the possibility of anticipated changes in aggregate government spending, we explore three alternative ways.

First, we rely on a timing assumption to extract unexpected changes in government spending. In particular, we follow the approach by Blanchard and Perotti

Table 1.C.1. Output and Employment Multipliers: Robustness I

	Output Multiplier					Employment Multiplier				
	Impact	1 Year	2 Years	3 Years	4 Years	Impact	1 Year	2 Years	3 Years	4 Years
Panel A: Baseline specification										
Multiplier	2.14***	2.33***	2.33***	2.26***	2.21***	1.12***	1.43***	1.51***	1.47***	1.44***
	(0.40)	(0.32)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel B: Alternative instrument construction										
Alternative s_i (I)	1.89***	2.05***	2.05***	1.99***	1.96***	1.10***	1.41***	1.48***	1.43***	1.39***
	(0.39)	(0.31)	(0.25)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.14)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Alternative s_i (II)	1.74***	1.90***	1.84***	1.82***	1.82***	0.99***	1.22***	1.22***	1.17***	1.02***
	(0.29)	(0.37)	(0.40)	(0.37)	(0.24)	(0.21)	(0.23)	(0.25)	(0.27)	(0.24)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
National Accounts	2.64***	2.71***	2.72***	2.63***	2.49***	1.60***	1.88***	1.96***	1.93***	1.79***
	(0.57)	(0.30)	(0.18)	(0.19)	(0.15)	(0.30)	(0.23)	(0.21)	(0.19)	(0.17)
# Obs	2627	2461	2295	2129	1963	2627	2461	2295	2129	1963
Panel C: Exogenous variation in national spending										
Fiscal Rule	2.00***	2.27***	2.34***	2.30***	2.33***	0.94***	1.32***	1.46***	1.42***	1.49***
	(0.31)	(0.36)	(0.29)	(0.28)	(0.19)	(0.21)	(0.19)	(0.20)	(0.19)	(0.21)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Military Spending	3.27***	3.22***	3.22***	2.99***	2.96***	1.63***	1.71***	1.82***	1.68***	1.76***
	(0.67)	(0.27)	(0.17)	(0.15)	(0.15)	(0.57)	(0.23)	(0.28)	(0.26)	(0.29)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Forecast Errors	3.91***	3.47***	3.03***	2.95***	2.82***	2.14***	1.97***	1.88***	1.95***	1.87***
	(1.02)	(0.34)	(0.29)	(0.19)	(0.23)	(0.77)	(0.34)	(0.29)	(0.25)	(0.27)
# Obs	2410	2258	2119	1967	1813	2410	2258	2119	1967	1813

Notes: Panel A shows the estimates for the baseline fiscal and employment multipliers. Panel B presents the estimates for alternative instrument constructions. First, following Nekarda and Ramey (2011), the share of regional spending used in the instrument is constructed as an average across the whole sample rather than predetermined as in the baseline. In the second alternative specification of s_i, we use the levels of government spending at regional and aggregate levels rather than the per capita values. Then, instead of using the aggregate government spending proxy to compute the Bartik instrument, we use the government spending from National Accounts. Panel C explores alternative identification strategies. Here, we use the residual of an estimated fiscal spending rule, national military spending, and forecast errors on government spending to obtain exogenous and unanticipated national government spending changes to construct the Bartik instrument.

(2002) that policymakers need time to decide on, approve, and implement discretionary changes in fiscal policy. We proceed by first, estimating a government expenditure rule, where we regress the growth rate of per capita national government spending on lagged growth rates of per capita government spending, GDP, and tax revenues, time and country fixed effects. We then interpret the residual of this regression, $\hat{u}_{I,t}$, as the unexpected component of national government spending and use it to construct the Bartik instrument as follows:[43]

$$Bartik_{i,t} \;=\; s_i \times \hat{u}_{I,t},$$

Second, we use military spending as an instrument for unanticipated aggregate spending changes. Hall (2009), Barro and Redlick (2011), and Miyamoto, Nguyen, and Sheremirov (2019), among others, also use aggregate military spending data to identify government spending shocks. Changes in military spending are often large and regularly respond to foreign policy developments, suggesting that these changes are exogenous in the sense that they are less likely to be driven by domestic cyclical forces. In particular, military spending is not correlated with the state of the economy like the state of the business cycle or financial conditions of the private sector.[44] Following Miyamoto, Nguyen, and Sheremirov (2019), we use national variation in per capita military spending to compute the Bartik instrument as follows:[45]

$$Bartik_{i,t} \;=\; s_i \times \Delta M_{I,t},$$

where $\Delta M_{I,t}$ is the change in per capita national military spending.

Third, we use professional forecast errors on national government spending from the study by Born, Müller, and Pfeifer (2020). The underlying idea is that unpredicted changes in government spending by professional forecasters provide a direct measure of fiscal news that is unrelated to the state of the economy (Ramey, 2011). Similarly to the military spending procedure, we use the forecast errors directly in the Bartik instrument construction.[46] Importantly, the respective first stages are sufficiently strong. The F-Statistic varies across horizons and estimates lying between

43. While Blanchard and Perotti (2002) apply their identification strategy on quarterly data, we have to rely on annual time series. Note, though, that Born and Müller (2012) provide robust evidence that the recursive identification is appropriate for annual post-WWII U.S. time-series data. In addition, Beetsma and Giuliodori (2011) point out that budget decisions are typically made once a year, and argue that, consequently, annual data provide a more natural way to reconcile discretionary fiscal policy changes.

44. Nakamura and Steinsson (2014), Dupor and Guerrero (2017), and Auerbach, Gorodnichenko, and Murphy (2020b) use variation in regional military government spending to estimate the effect of a government spending change. However, because regional military spending data are not available for European regions, we combine the idea of unanticipated public spending changes due to military expenditures at the national level with spending changes at the regional level to construct the Bartik instrument.

45. See Appendix 1.A.4 for more details on the military data used and its source.

46. Because our analysis is conducted on annual data, we aggregate the quarterly forecast error series by Born, Müller, and Pfeifer (2020) to the annual level.

76 and 281 for the fiscal rule, 16 and 44 for the military spending, and 6 and 18 for the forecast error exercise. Thus, all instruments are sufficiently strong predictors of variations in regional government spending.

The results of the regional multiplier estimates when applying these alternative strategies to extract unexpected government spending changes at the national level are presented in Panel C of Table 2.4.2. When relying on the residual of the fiscal rule estimation, the multipliers are very similar to our baseline estimates. The four-year output multiplier becomes 2.33 and the respective employment multiplier is estimated to be 1.49. For the other two measures, the estimates are somewhat larger than the baseline results. The four-year output multiplier is 2.96 in the case of the military spending instrument and 2.82 for the forecast errors instrument; the employment multiplier is 1.76 and 1.87, respectively. However, these estimates still support our main finding: an increase in regional government spending significantly boosts regional output and employment. Importantly, our baseline results are robust to using unexpected changes in national spending for constructing the Bartik instrument instead of observed changes in national government expenditures.

1.C.3 Alternative Samples and Controlling for Financing Sources

As additional robustness checks, we test whether our results are robust to changes in the sample. First, we use NUTS 3 level data to estimate output and employment multipliers. This considerably increases the number of regions and therefore the total number of observations. At the NUTS 3 level, the sample consists of 922 regions, compared to 167 in the baseline, and the total number of observations is more than five times larger compared to the NUTS 2 level analysis. Moreover, as previously mentioned, moving to the more disaggregated NUTS 3 level should minimize the problem that individual regions have a direct influence on national government decisions since their economic and political power is further reduced when compared to the NUTS 2 level. As Panel B of Table 1.C.2 shows, the results are similar to our baseline estimates. The four-year output multiplier is now estimated to be 2.5 and the four-year employment multiplier takes a value of 1.58.

Second, we add the late Euro adopters to the sample — namely Slovenia, Malta, Slovakia, Estonia, Latvia, and Lithuania. Panel B of Table 1.C.2 shows that our results hardly change. Notwithstanding, the total number of observations increases only slightly when including the late Euro adopters.

Finally, an important difference between the Eurozone and the U.S. is that the Eurozone does not share a common fiscal authority. While the common monetary policy is conducted by the ECB, fiscal policy is conducted at the national level. In our baseline specification, regional fixed effects absorb heterogeneity across regions and countries and should therefore also capture different national fiscal reactions to the regional government spending change. However, it might be argued that additional covariates are needed to control for country-specific fiscal policies. Thus, we

Table 1.C.2. Output and Employment Multipliers: Robustness II

	Output Multiplier					Employment Multiplier				
	Impact	1 Year	2 Years	3 Years	4 Years	Impact	1 Year	2 Years	3 Years	4 Years
Panel A: Baseline specification										
Multiplier	2.14***	2.33***	2.33***	2.26***	2.21***	1.12***	1.43***	1.51***	1.47***	1.44***
	(0.40)	(0.32)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel B: Alternative samples										
NUTS 3 Data	2.64***	2.71***	2.64***	2.57***	2.50***	1.35***	1.61***	1.64***	1.63***	1.58***
	(0.34)	(0.27)	(0.19)	(0.17)	(0.12)	(0.29)	(0.15)	(0.11)	(0.10)	(0.10)
# Obs	14192	13303	12414	11525	10630	14192	13303	12414	11525	10630
Late Adopter	2.10***	2.28***	2.30***	2.25***	2.20***	1.09***	1.40***	1.48***	1.44***	1.43***
	(0.39)	(0.33)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2666	2494	2323	2152	1979	2666	2494	2323	2152	1979
Panel C: Controlling for financing sources										
Country homogeneity	1.95***	2.22***	2.16***	2.03***	2.04***	0.85***	1.15***	1.11***	0.92***	0.87***
	(0.30)	(0.37)	(0.32)	(0.32)	(0.22)	(0.21)	(0.20)	(0.20)	(0.19)	(0.18)
# Obs	2617	2453	2289	2125	1959	2617	2453	2289	2125	1959
Country heterogeneity	1.65***	2.06***	2.06***	1.92***	2.15***	0.75***	1.03***	0.86***	0.49**	0.52**
	(0.21)	(0.25)	(0.23)	(0.28)	(0.20)	(0.15)	(0.17)	(0.23)	(0.24)	(0.24)
# Obs	2617	2453	2289	2125	1959	2617	2453	2289	2125	1959

Notes: Panel A shows the estimates for the baseline fiscal and employment multipliers. Panel B shows the estimated multipliers using NUTS 3 level data and data for the late Euro adopters. Panel C specifications include additional controls to the baseline. The first estimates in Panel C include the contemporaneous and one-year lag of the change in the national total tax receipts per capita and sovereign spreads. The second estimates include these controls interacted with country fixed effects.

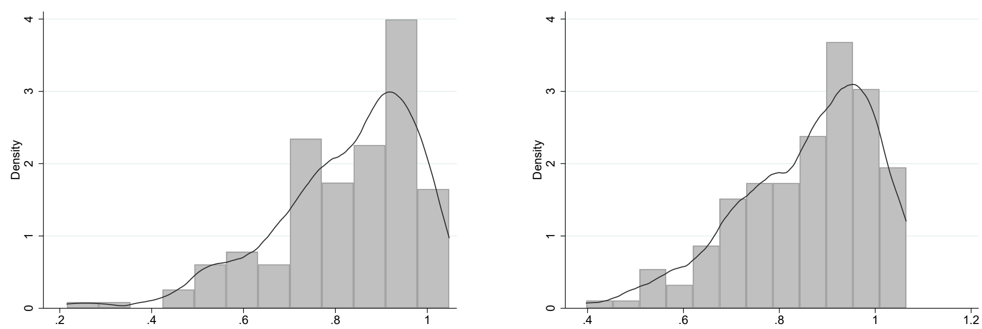
expand our baseline specification and additionally control for per capita national tax receipts and sovereign risk premia. While taxes control for the financing side of the public spending change, risk premia capture financing costs of the government. The risk premia have been shown to play a particular role in the transmission of national government spending in the Eurozone (Corsetti, Kuester, Meier, and Müller, 2013).[47] In particular, we add the contemporaneous and one-year lag of both variables to the vector of control variables. We estimate separate specifications. First, we assume homogeneity and estimate average coefficients across countries. Second, we allow for full country heterogeneity and interact both covariates with country fixed effects such that we estimate specific fiscal policy reactions for all countries of the

47. We compute sovereign spreads as the difference between the national and Germany's 10-year government bond rate. For Germany, we instead use its 10-year government bond rate as control.

sample. Panel C of Table 1.C.2 shows that the multiplier estimates slightly change when additionally controlling for the financing sources of the national governments. The impact output multiplier decreases mildly compared to the baseline estimates. However, four years after the shock, both specifications deliver very similar output multipliers relative to the baseline. The differences are somewhat larger for the employment multiplier, which becomes smaller when controlling for national fiscal policies. Nevertheless, the regional fiscal stimulus still leads to a significant increase in the employment ratio although the four-year employment multiplier drops below 1.[48]

1.C.4 Dynamic and Cross-Sectional Heterogeneity

As shown by Canova (2020) for the case of the U.S., not accounting for dynamic heterogeneity may pose a potential threat to cross-sectional multiplier estimates. As suggested by Canova (2020), we analyze the time-series properties of output and employment by estimating the AR(1) process of these series for each region in the sample. Figure 1.C.1 plots the cross-sectional distribution of the output and employment AR(1) coefficients. Because the persistence coefficients are distributed fairly homogeneously, dynamic heterogeneity does not seem as important here as in the case of the U.S. presented by Canova (2020). Yet, we re-estimate the multipliers excluding the regions with very extreme persistence coefficients, namely the top and bottom 10%. The results are presented in Panel A from Table 1.C.3 in the appendix and reassure that the baseline multipliers are robust.

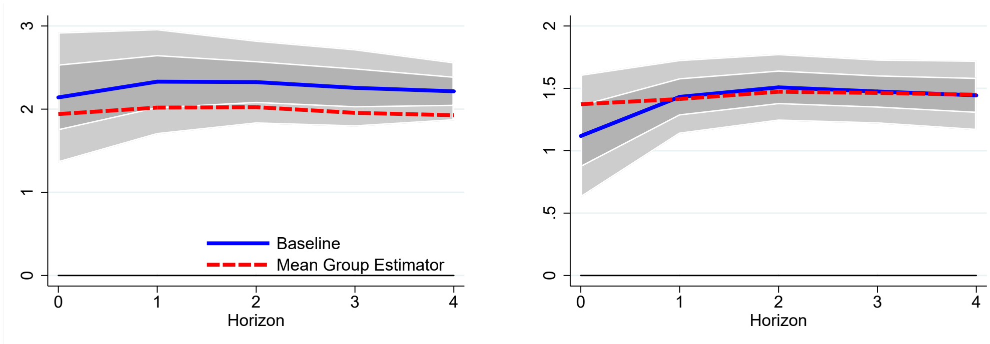


(a) Output **(b)** Employment

Figure 1.C.1. Distribution of Output and Employment Persistence Parameter. This Figure plots the distribution of output and employment persistence parameter from an AR(1) process.

48. It is also important to note that, when estimating country-specific fiscal policies, the number of estimated coefficients increases significantly and the F-Statistic of the first stage decreases substantially for longer horizons, with the lowest value being 27.

Moreover, in the presence of strong cross-sectional heterogeneity, pooling observations across regions and estimating common slope coefficients might not be appropriate. To address this potential problem, we follow Bernardini, Schryder, and Peersman (2020) and estimate output and employment multipliers with a mean group approach that allows for cross-region heterogeneity in the slope coefficients. Since this mean group estimator (MGE) requires a relatively long period of time, we rely on Bayesian methods to calculate fiscal multipliers. In particular, as suggested by Canova (2020) and Miranda-Agrippino and Ricco (2021), we estimate Bayesian local projections employing a normal prior for the output and employment multiplier estimates. Motivated by the existing U.S. evidence on regional fiscal multipliers (Nakamura and Steinsson, 2014; Chodorow-Reich, 2019), the prior mean for the output multiplier is set to 1.9, the one for employment to 1.4, and both variances are set to 2.



(a) Output Multiplier **(b)** Employment Multiplier

Figure 1.C.2. Output and Employment Multipliers: Baseline and Mean Group Estimator. Panels 1.C.2a and 1.C.2b show the baseline (blue solid) and the mean group estimator (red dashed) fiscal and employment multipliers using Bayesian local projections. Normal prior with variance 2 and means 1.9 and 1.4 for output and employment, respectively. Shaded areas are 68% (dark) and 95% (light) confidence intervals of the baseline (pooled) estimation .

Figure 1.C.2 shows the estimated responses for the output and employment multipliers when applying the MGE Bayesian local projections (dashed lines) together with the baseline (pooled) estimates (solid lines and shaded areas for the coefficients and confidence bands, respectively). Notably, the MGE estimates are very similar to the baseline multipliers and lie within the respective confidence bands for all periods of the forecast horizon. The employment multiplier of the MGE estimation is almost identical to the pooled estimation, while the estimated output multiplier is somewhat smaller compared to our baseline results reaching a value slightly below two four years after the fiscal stimulus. Interestingly, also the shape of the responses is similar across both estimation approaches which again supports our pooling assumption. The relatively large cross-sectional dimension and low fre-

quency of our dataset seem to limit the erratic component in the calculated impulse responses which is a more severe problem when estimating local projections on time series with a higher frequency like quarterly and monthly data (Miranda-Agrippino and Ricco, 2021). Overall, we interpret these results as evidence that cross-sectional heterogeneity is not a severe threat for our regional multiplier estimates in the Eurozone and therefore, proceed with the pooled specification in what follows.

1.C.5 Further Checks

We show results for additional robustness checks. First, the baseline multiplier estimates are robust when following closely Nakamura and Steinsson (2014) and using national military spending interacted with region fixed effects as instrument (Panel B of Table 1.C.3). Furthermore, the results do not change much when not including lagged control variables in the regressions or excluding regions that spend disproportionately more per capita than the national average (Panels C and D of Table 1.C.3). We also re-estimated the baseline model when excluding intermediate consumption from our proxy regional government spending series. Then, regional government spending is measured by the GVA of the non-market sector. As expected, the multipliers increase because the shock size (1% of GDP per capita) becomes larger relative to the baseline proxy used (Panel E of Table 1.C.3).

Secondly, to assess how important any individual country is for the results, we re-estimate the baseline regressions by sequentially dropping one country at a time. The obtained results are comparable to the baseline in every case (Table 1.C.4).

Finally, we use a Bayesian approach and estimate multipliers by means of Bayesian local projections. As shown by Miranda-Agrippino and Ricco (2021), Bayesian local projections might reduce erratic movements in impulse response computed with standard local projections. As for the mean group estimator exercise, we employ a normal prior with mean 1.9 for output and 1.4 for employment and set both variances to 2 based on recent U.S. regional multiplier estimates (Nakamura and Steinsson, 2014; Chodorow-Reich, 2019). Figure 1.C.3 in the appendix shows the estimated multipliers when using Bayesian local projections. The results are similar to our baseline estimates which implies that Bayesian local projections do not deliver a significant improvement for our analysis. As already mentioned above, the small differences in the estimated shapes of the responses might be due to the large cross section at annual frequency which already limits the erratic component in the impulse responses.

Table 1.C.3. Output and Employment Multipliers: Robustness III

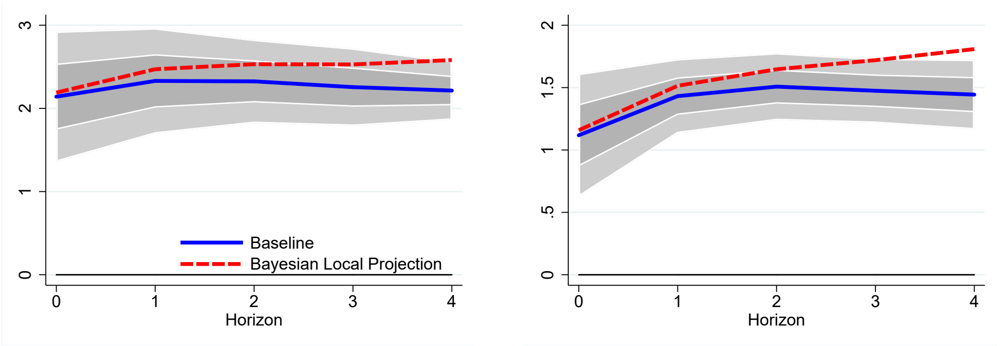
	Output Multiplier					Employment Multiplier				
	Impact	1 Year	2 Years	3 Years	4 Years	Impact	1 Year	2 Years	3 Years	4 Years
Panel A: Baseline specification										
Multiplier	2.14***	2.33***	2.33***	2.26***	2.21***	1.12***	1.43***	1.51***	1.47***	1.44***
	(0.40)	(0.32)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel A: Excluding AR(1) outliers										
Multiplier	2.22***	2.43***	2.44***	2.40***	2.33***	1.06***	1.38***	1.47***	1.41***	1.40***
	(0.42)	(0.33)	(0.24)	(0.20)	(0.17)	(0.24)	(0.15)	(0.13)	(0.12)	(0.14)
# Obs	2112	1979	1846	1713	1579	2109	1977	1845	1713	1579
Panel B: Nakamura and Steinsson (2014) approach with military spending										
Multiplier	0.78*	1.25***	1.49***	1.59***	1.51***	0.35	0.82***	0.98***	1.03***	0.81***
	(0.47)	(0.48)	(0.33)	(0.24)	(0.18)	(0.31)	(0.27)	(0.22)	(0.17)	(0.11)
# Obs	2627	2461	2295	2129	1963	2627	2461	2295	2129	1963
Panel C: No controls										
Multiplier	2.01***	2.14***	2.11***	2.06***	2.02***	1.30***	1.49***	1.53***	1.51***	1.48***
	(0.39)	(0.31)	(0.25)	(0.21)	(0.16)	(0.25)	(0.20)	(0.16)	(0.13)	(0.09)
# Obs	2953	2789	2625	2461	2295	2953	2789	2625	2461	2295
Panel D: Excluding regions in top 10% of s_i										
Multiplier	2.22***	2.39***	2.37***	2.30***	2.27***	1.09***	1.40***	1.48***	1.47***	1.44***
	(0.40)	(0.33)	(0.25)	(0.23)	(0.16)	(0.24)	(0.16)	(0.14)	(0.14)	(0.16)
# Obs	2349	2202	2055	1908	1759	2349	2202	2055	1908	1759
Panel E: Excluding intermediate consumption										
Multiplier	2.83***	3.07***	3.06***	2.97***	2.91***	1.46***	1.87***	1.97***	1.93***	1.89***
	(0.52)	(0.41)	(0.32)	(0.30)	(0.23)	(0.33)	(0.19)	(0.18)	(0.17)	(0.19)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963

Notes: Panel A excludes regions which present very large or small (top and bottom 10%) persistence coefficient from an AR(1) regression. Panel B shows estimates for output and employment multipliers following Nakamura and Steinsson (2014) approach and using as the instrument the interaction between aggregate military spending and regional fixed effects. The results in Panel C show that the estimates are robust to excluding the controls from the baseline regression (lags of government spending and variable of interest). Panel D excludes the regions with the largest shares s_i (top 10%). Panel E shows the results when excluding intermediate consumption from our government spending proxy.

Table 1.C.4. Output and Employment Multipliers: Robustness IV

	Output Multiplier					Employment Multiplier				
	Impact	1-Year	2 Years	3 Years	4 Years	Impact	1-Year	2 Years	3 Years	4 Years
Panel A: Baseline specification										
Multiplier	2.14***	2.33***	2.33***	2.26***	2.21***	1.12***	1.43***	1.51***	1.47***	1.44***
	(0.40)	(0.32)	(0.26)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2621	2457	2293	2129	1963	2621	2457	2293	2129	1963
Panel B: Excluding individual countries iteratively										
Multiplier Austria	2.15***	2.34***	2.33***	2.24***	2.20***	1.12***	1.43***	1.50***	1.46***	1.43***
	(0.41)	(0.33)	(0.26)	(0.24)	(0.18)	(0.26)	(0.16)	(0.14)	(0.14)	(0.14)
# Obs	2477	2322	2167	2012	1855	2477	2322	2167	2012	1855
Multiplier Belgium	2.17***	2.36***	2.35***	2.27***	2.23***	1.14***	1.47***	1.54***	1.50***	1.47***
	(0.38)	(0.31)	(0.25)	(0.24)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.15)
# Obs	2445	2292	2139	1986	1831	2445	2292	2139	1986	1831
Multiplier Germany	1.76***	2.01***	2.06***	1.98***	1.93***	1.03***	1.32***	1.38***	1.31***	1.28***
	(0.30)	(0.26)	(0.23)	(0.22)	(0.17)	(0.17)	(0.12)	(0.14)	(0.13)	(0.16)
# Obs	2013	1887	1761	1635	1507	2013	1887	1761	1635	1507
Multiplier Greece	1.84***	1.96***	1.94***	1.77***	1.74***	1.08***	1.44***	1.54***	1.38***	1.20***
	(0.37)	(0.28)	(0.28)	(0.33)	(0.26)	(0.24)	(0.21)	(0.27)	(0.32)	(0.36)
# Obs	2439	2288	2137	1986	1833	2439	2288	2137	1986	1833
Multiplier Spain	2.28***	2.41***	2.35***	2.34***	2.29***	1.18***	1.47***	1.52***	1.60***	1.61***
	(0.40)	(0.35)	(0.28)	(0.24)	(0.18)	(0.25)	(0.15)	(0.11)	(0.08)	(0.08)
# Obs	2317	2172	2027	1882	1735	2317	2172	2027	1882	1735
Multiplier Finland	2.13***	2.33***	2.34***	2.28***	2.24***	1.12***	1.43***	1.50***	1.47***	1.43***
	(0.41)	(0.33)	(0.26)	(0.24)	(0.18)	(0.26)	(0.16)	(0.14)	(0.14)	(0.15)
# Obs	2541	2382	2223	2064	1903	2541	2382	2223	2064	1903
Multiplier France	2.19***	2.38***	2.34***	2.29***	2.26***	1.11***	1.43***	1.51***	1.48***	1.47***
	(0.43)	(0.38)	(0.31)	(0.28)	(0.22)	(0.27)	(0.18)	(0.15)	(0.13)	(0.14)
# Obs	2189	2052	1915	1778	1639	2189	2052	1915	1778	1639
Multiplier Ireland	2.27***	2.46***	2.47***	2.41***	2.35***	1.15***	1.47***	1.56***	1.53***	1.50***
	(0.41)	(0.32)	(0.23)	(0.22)	(0.19)	(0.26)	(0.15)	(0.13)	(0.13)	(0.14)
# Obs	2582	2419	2256	2093	1930	2582	2419	2256	2093	1930
Multiplier Italy	2.14***	2.32***	2.33***	2.28***	2.25***	1.08***	1.39***	1.47***	1.44***	1.43***
	(0.41)	(0.33)	(0.25)	(0.22)	(0.16)	(0.26)	(0.16)	(0.14)	(0.13)	(0.14)
# Obs	2285	2142	1999	1856	1711	2285	2142	1999	1856	1711
Multiplier Luxembourg	2.15***	2.34***	2.33***	2.26***	2.22***	1.13***	1.44***	1.52***	1.48***	1.46***
	(0.40)	(0.32)	(0.25)	(0.23)	(0.18)	(0.25)	(0.15)	(0.14)	(0.13)	(0.14)
# Obs	2605	2442	2279	2116	1951	2605	2442	2279	2116	1951
Multiplier Netherlands	2.25***	2.46***	2.43***	2.32***	2.28***	1.13***	1.48***	1.57***	1.51***	1.48***
	(0.41)	(0.31)	(0.25)	(0.22)	(0.15)	(0.27)	(0.15)	(0.13)	(0.12)	(0.14)
# Obs	2429	2277	2125	1973	1819	2429	2277	2125	1973	1819
Multiplier Portugal	2.18***	2.37***	2.36***	2.30***	2.25***	1.15***	1.43***	1.50***	1.47***	1.44***
	(0.41)	(0.33)	(0.25)	(0.23)	(0.17)	(0.26)	(0.15)	(0.13)	(0.12)	(0.13)
# Obs	2509	2352	2195	2038	1879	2509	2352	2195	2038	1879

Notes: This table shows the output and employment multiplier estimates using the baseline specification but excluding individual countries iteratively from the base sample.



(a) Output Multiplier

(b) Employment Multiplier

Figure 1.C.3. Output and Employment Multipliers: Baseline and Bayesian Local Projection. Panels 1.C.2a and 1.C.2b show the baseline (blue solid) and the Bayesian local projections (red dashed) fiscal and employment multipliers. Normal prior with variance 2 and means 1.9 and 1.4 for output and employment, respectively. Shaded areas are 68% (dark) and 95% (light) confidence intervals of the baseline estimation.

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Chapter 2

The Political Costs of Austerity

Joint with Ana Sofia Pessoa and Mathias Klein

2.1 Introduction

Anti-establishment and EU-skeptic parties have gained significant support since the Great Recession and the subsequent European Sovereign Debt Crisis. Higher vote shares for these parties have increased partisan conflict and led to more fragmented parliaments. The resultant polarized political environment is economically significant, as political tension is generally associated with higher policy uncertainty and lower economic growth (Azzimonti, 2011; Azzimonti, 2018; Funke, Schularick, and Trebesch, 2020; Carozzi, Cipullo, and Repetto, 2022). Interestingly, the rise in support for extreme parties occurred in a period of significant fiscal policy interventions. In particular, several European countries have implemented large-scale fiscal consolidation measures to reduce high levels of public debt, thereby averting the risk of sovereign default. The massive reductions in public spending faced significant opposition and resulted in an anti-austerity movement. In this paper, we empirically investigate the causal link between fiscal consolidations and rising polarization and provide new evidence on the political costs of fiscal austerity.

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To this end, we assemble a novel regional dataset on election outcomes that provides detailed voting results on regional, national, and European elections. We combine data from Schakel (2013) with information from various national and regional sources. Our final dataset covers 124 European regions from 8 countries and spans from 1980 to 2015. We collect data on more than 200 elections; roughly 20 elections per region and, on average, one election every two years. Thus, our dataset provides considerable granular variation in election outcomes for estimating the causal effect of fiscal consolidations on voting behavior. We rely on party classifications by Funke, Schularick, and Trebesch (2016) and Algan, Guriev, Papaioannou, and Passari (2017) to define parties at the far-right and far-left of the political spectrum. Our data supports the main narrative of a significant correlation between fiscal consolidations and extreme voting. First, we find a strong increase in extreme parties' vote share across European regions in the years after the Great Recession and the Sovereign Debt Crisis. Second, our data indicates a negative correlation between changes in regional government spending and patterns of extreme voting in recent years.

To test for the causal relationship between austerity and voting outcomes, we identify exogenous changes in regional public spending using a Bartik-type instrument (Bartik, 1991) that combines *regional* sensitivities to changes in *national* government expenditures with the narrative national consolidation measure proposed by Alesina, Favero, and Giavazzi (2020). The narrative series contains only those changes in the national primary balance-to-GDP ratio that are motivated by a desire to reduce budget deficits. The identified fiscal actions represent responses to past decisions and economic conditions rather than to current and prospective conditions. Therefore, there should be no systematic correlation between the identified national fiscal actions and other developments that affect economic activity in the short term. This narrative approach has been used in several studies to gauge the economic effects of fiscal consolidations at the national level (Guajardo, Leigh, and Pescatori, 2014; Jordà and Taylor, 2016; Alesina, Favero, and Giavazzi, 2019). In contrast to these approaches, we use the narrative series as the shift component in a Bartik instrument to identify exogenous reductions in government spending at the regional level. We further employ an instrumental variable local projections approach to estimate the causal effect of reductions in regional public spending on election outcomes. Importantly, our Bartik measure provides a strong instrument for regional government spending reductions, with a first-stage F-statistic well above the critical threshold, suggesting that weak instruments are unlikely to be a concern for our analysis.

Our results show that fiscal consolidations are associated with significant political costs: a 1% reduction in regional public spending leads to an increase in extreme parties' vote share of around 3 percentage points. The higher vote share captured by extreme parties can be explained by a fall in voter turnout together with an increase in the total votes for these parties. Thus, in response to fiscal consolidations, fewer people vote and those who do exhibit a higher tendency to vote for extreme parties. In addition, austerity increases fragmentation, which, based on previous evidence on the negative economic impact of partisan conflict (Azzimonti, 2011; Funke, Schularick, and Trebesch, 2020), suggests that austerity affects economic outcomes through a more polarized political environment. We use a forecast error variance decomposition (FEVD) exercise to quantify the magnitude of regional cuts in public spending in driving more extreme voting. Our results suggest that around 10% of the variation in extreme parties' vote share is indeed due to fiscal consolidations, which further highlights the importance of austerity in understanding shifts in voters' preferences toward the more extreme ends of the political spectrum.

We conduct a battery of robustness checks to verify our findings. The results still hold for different samples and also remain unaffected when changing the construction of the national austerity measure or the share variable of the Bartik instrument. Notably, the rise in extreme parties' vote share to fiscal consolidations persists when dropping the Great Recession period and the subsequent years of the European Debt Crisis, which makes us confident that the political costs of austerity are not merely driven by the extreme events in the recent past but describe a general pattern in the data.

When differentiating between election types and far-left and far-right parties, we find only mild differences in political outcomes. While austerity leads to the largest shift toward extreme parties for European elections, the movement away from more traditional parties is also present for national and regional elections. Moreover, although both extremes gain vote shares as a result of fiscal consolidations, far-right parties experience a slightly stronger rise in voters' support. We further test for potentially important state dependencies and find that the increase in extreme parties' vote share is significantly larger when the fiscal consolidation is implemented during a recession as opposed to a period of expansion. In addition, the effects are somewhat stronger in rural and poor regions, but not statistically significantly different from the ones observed in urban and rich regions, respectively.

To rationalize our main findings on the political consequences of austerity, we also estimate the economic effects of fiscal consolidations at the regional level. Austerity leads to a significant fall in regional output, employment, investment, durable consumption, and wages. Furthermore, the reduction in public spending lowers the labor income share thereby inducing a redistribution of income away from working households. These contractionary effects of austerity support previous evidence on the economic impact of fiscal consolidations conducted at the national level (Guajardo, Leigh, and Pescatori, 2014; Jordà and Taylor, 2016). Moreover, these findings highlight the close relationship between detrimental economic developments and voters' support for extreme parties.

Finally, we try to understand whether austerity-driven recessions yield different political outcomes than general economic downturns do. We differentiate between recessions that coincide with fiscal consolidations ("austerity recessions") and those not related to austerity ("non-austerity recessions") and estimate the response of extreme parties' vote share in both episodes of economic slack. Our estimates imply that austerity recessions lead to a significantly larger increase in the vote share for extreme parties than other recessions. In addition, in a recession that coincides with a fiscal consolidation, a reduction in regional government spending implies a larger increase in extreme voting compared to lowering public spending in non-austerity recessions. We relate this result to a potential trust channel of fiscal consolidations by showing that people's trust in the government deteriorates much more strongly during austerity recessions compared to non-austerity recessions. This might point toward a "doom loop" between distrust in the political system and more extreme voting following fiscal consolidations. In sum, austerity-driven recessions are special in the sense that they considerably amplify the political costs of economic downturns by creating more distrust in the political environment.

Related literature. Our paper is related to several strands of literature. We mainly contribute to a growing body of work on the economic drivers of populism. Guriev (2018), Guiso, Herrera, Morelli, and Sonno (2019) and Guiso, Herrera, Morelli, and Sonno (2020), Berman (2021), Baccini and Sattler (2021) and Guriev and Papaioannou (2022) provide a good overview on the causes of populism in Europe and other advanced economies by analyzing both demand- and supply-side explanations of populism and focusing on economic grievance–based explanations. Regarding right-wing populism, the usual economic explanations focus on how globalization and trade integration have generated discontent and division among citizens by making life more insecure for the working and middle classes (Colantone and Stanig, 2018; Rodrik, 2020; Pastor and Veronesi, 2021). On the other hand, left-wing populism seems to be more related to specific economic considerations coming from neoliberalism and economic policies. In particular, the left-wing rise after the Great Recession in Europe was fueled by massive anti-austerity movements in Greece (Stavrakakis and Katsambekis, 2014), Portugal (Accornero and Ramos Pinto, 2015), and other European countries (Calossi, 2016; Della Porta, Fernández, Kouki, and Mosca, 2017).

Focusing on austerity, there are several papers worth mentioning. Ponticelli and Voth (2020) use a panel dataset for 25 European countries covering the period 1919 to 2008 to show a clear link between the magnitude of expenditure cutbacks and increases in social unrest. Galofré-Vilà, Meissner, McKee, and Stuckler (2021) study the link between fiscal austerity and Nazi electoral success. Focusing on the "age of austerity" in the UK, Bray, Braakmann, and Wildman (2022) show that for each £100 loss per working age adult, racially or religiously motivated crimes rose by approximately 5-6% between 2013 and 2015. In addition, Hübscher, Sattler, and Wagner (2021b) presents survey evidence that in Germany, Spain, Portugal, and Italy a government's re-election chances greatly decrease if it proposes austerity measures with voters objecting strongly to spending cuts, while Alesina, Furceri, Ciminelli, Saponaro, et al. (2021) argue that an austerity package worth 1% of GDP reduces the vote share of the leader's party by about 7%. These findings materialize the idea that austerity-fueled social unrest contributed to a feeling of disconnect from the established political parties and institutions and encouraged voters to support more extreme policy positions or engage in protest voting (Myatt, 2017; Panunzi, Pavoni, and Tabellini, 2020; Hübscher, Sattler, and Wagner, 2021a). The majority of these protest votes are cast in anti-establishment (or populist) parties that usually fall into two categories: far right and far left, both of which have historically benefited from poor economic conditions (Algan et al., 2017; Birch and Dennison, 2019). We add to the latter literature by focusing on finer regional level data and taking a longer time horizon perspective, which enables us to investigate whether voting for extreme parties systematically increased after austerity measures and whether economic insecurity is a possible economic channel through which austerity affects voting behavior.

We also contribute to the literature evaluating the economic effects of fiscal policy, and, in particular, the effects of narratively identified austerity episodes (Devries, Guajardo, Leigh, and Pescatori, 2011; Guajardo, Leigh, and Pescatori, 2014; Alesina, Favero, and Giavazzi, 2015; Jordà and Taylor, 2016; Alesina, Azzalini, Favero, Giavazzi, and Miano, 2018; Alesina, Favero, and Giavazzi, 2020). Our main contribution is the evaluation of the economic costs of austerity at the regional level by combining regional government spending data with narratively identified spending-based austerity measures at the national level.

The closest related work to our study is the paper by Fetzer (2019), which shows that austerity-induced welfare reforms in the UK led to a rise in support for the UK Independent Party and for Leave in the referendum on European Union membership. However, our analysis differs in several important dimensions. First, while Fetzer (2019) focuses only on the UK, we provide novel cross-country evidence on the severe political costs of austerity. The significant time and cross-sectional variation that we rely on allows further quantification of the economic significance of fiscal consolidations in explaining extreme voting. Second, our detailed election and party classifications permit us to undertake an in-depth analysis on potentially significant differences across European, national, and regional elections and between extreme parties on the left and right. Third, we also provide a thorough investigation on the economic costs of austerity and thus highlight the close relationship between economic developments and voters' support for extreme parties. Finally, we conduct a careful comparison between austerity-driven and non-austerity-driven recessions and show that the political costs of economic downturns are considerably amplified during austerity recessions.

The remainder of the paper is organized as follows. Section 3.2 describes the economic and political data used in the analysis. Section 2.3 presents the empirical methodology and discusses the identification strategy. Section 3.4 shows our empirical results. Finally, Section 3.5 concludes.

2.2 Data

In our analysis, we draw on a broad set of annual data covering the period from 1980 to 2015 for 124 regions in eight European countries: Austria, Finland, France, Germany, Italy, Portugal, Spain, and Sweden. In the following, we describe the main variables used in our analysis. Table 2.A.1 in the Appendix provides more information on the regional structure and 2.A.2 provides additional information regarding data definitions and sources.

2.2.1 Economic data

To measure regional economic developments, we rely on data from the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy (ARDECO), which is a highly disaggregated dataset across sectoral and regional dimensions. The database contains several long time-series indicators for European regions at different statistical scales and expands the Cambridge Econometrics Dataset used by much of the literature on European regional dynamics.

The database provides regional measures for output (gross domestic product (GDP) and gross value added (GVA)), investment, earnings, hours worked, and employment for different economic sectors like industry, construction, financial, non-financial, and non-market services. The dataset is an annual panel covering the period 1980–2017 for the European Union (EU) and some European Free Trade Association (EFTA) and candidate countries. By construction, ARDECO's regional data is consistent with the commonly used national accounts data.[1] In particular, the regional ARDECO time series are constructed in such a way that the country aggregates equal the corresponding time series in the National Accounts reported in the AMECO dataset.[2]

The data are divided into NUTS (Nomenclature of Territorial Units for Statistics) regions. NUTS is a geocode standard for referencing the subdivisions of countries for statistical purposes. The hierarchy of three NUTS levels (NUTS 1, 2, 3) is established by Eurostat in agreement with each member state, and for most countries the respective NUTS level corresponds to a specific administrative division within the country. ARDECO provides all data series at these regional disaggregation levels except for NUTS 3, for which it reports only population, employment, GDP, and GVA.

Official data on final consumption expenditure of the general government (henceforth, government spending) is not available at the European regional level.

1. See Lequiller and Blades (2014) for more details on the construction of the National Accounts data.

2. See Gabriel, Klein, and Pessoa (2021) and Appendix 2.A.1 for more information.

Hereinafter, in the spirit of Brueckner, Valentinyi, and Pappa (2019) and closely following Gabriel, Klein, and Pessoa (2021), we use the sum of GVA and intermediate consumption of the non-market sector as a proxy for government spending. GVA of the non-market sector is computed as the sum of compensation to employees (including social contributions), consumption of fixed capital (which measures the decline in the value of fixed assets owned as a result of normal wear, tear, and obsolescence), and taxes less subsidies on production. Because GVA of the non-market sector does not include intermediate consumption, which is, however, one of the main components of government spending, we use input-output (IO) tables from the PBL EUREGIO database to calculate regional intermediate consumption shares of the non-market sector, which we then add to the GVA of the non-market sector.

Our regional measure (GVA plus intermediate consumption of the non-market sector) is a valid proxy for government spending for several reasons. First, as previously mentioned, ARDECO's regional data is consistent with the national accounts data by construction. By definition, there exists a close link between government spending and the GVA of the non-market sector. Consequently, almost the entire variation in the GVA of the non-market sector refers to activities by the general government. Second, government spending and our proxy measure show very similar statistical properties. Both measures are very tightly linked at the national and regional levels. We will thus refer to our regional proxy for government spending as government spending throughout the paper. For a more detailed justification of our proxy choice, see also Gabriel, Klein, and Pessoa (2021).

2.2.2 Narrative austerity episodes

Our data for narrative fiscal consolidations comes from Alesina, Favero, and Giavazzi (2020) and spans from 1978 to 2014.[3] Building on Devries et al. (2011) and Alesina, Favero, and Giavazzi (2015), Alesina, Favero, and Giavazzi (2020) address the potential endogeneity of shifts in fiscal variables using the "narrative" approach in the spirit of Romer and Romer (2010) and carefully dividing variables into spending- and tax-based consolidations.

The measure is constructed by examining contemporaneous OECD policy documents that outline the economic situation, fiscal consolidation strategy, and major consolidation measures for each of the OECD member countries. The country notes in each report are used to identify "exogenous" consolidations as they lay out the government's rationale for pursuing fiscal adjustment. To be precise, it is possible to identify consolidation periods that were motivated by a desire for deficit reduction, meant to correct its long-run trend, or driven by other motives unrelated to the state of the business cycle, thus excluding adjustments connected to short-run, countercyclical concerns. Consolidations are measured in terms of their impact on

3. Data can be found here.

total revenue and expenditure (relative to a baseline without policy intervention) and scaled by the output level prior to the intervention announcement.

The main advantage of identifying fiscal consolidations via the narrative measure, compared to changes in the current account primary balance (CAPB) as suggested by Alesina and Ardagna (2010), is that they are exogenous to current economic developments while changes in the CAPB are correlated with the business cycle. Guajardo, Leigh, and Pescatori (2014) show that there is a significant positive correlation between GDP forecast revisions and changes in the CAPB, whereas the null hypothesis of no correlation between forecast revisions and the narrative measure cannot be rejected.

Alesina, Favero, and Giavazzi (2020) classify as spending-based consolidations all measures related to government spending and investment, including expenditure on goods and services, salaries, managing costs of state-provided services (such as education and healthcare), and government gross fixed capital formation expenditures. Regarding tax-based consolidations, they account for all direct and indirect tax changes.

Throughout the paper, the narratively identified austerity episodes at time t in country I ($\tilde{g}_{I,t}$) measure only spending-based consolidations, excluding episodes driven by significant changes in the tax system. The regional government spending proxy used in the analysis does not include tax revenues and mainly encompasses the public wage bill and, to a lesser extent, the consumption of fixed capital and intermediate consumption. Therefore, excluding consolidation episodes driven by significant changes in the tax system allows for a stronger and clearer relationship between the narrative national austerity episodes and the regional government spending measures.[4]

Following the definition in Devries et al. (2011), we construct $\tilde{g}_{I,t}$ as the sum of unanticipated shifts in government spending at time t ($g^u_{I,t}$) and changes in spending that are implemented at time t but had been announced in previous periods ($g^a_{I,t-1,t}$):

$$\tilde{g}_{I,t} \;=\; g^u_{I,t} + g^a_{I,t-1,t}. \tag{2.2.1}$$

For our sample, we observe 95 consolidation episodes, which is roughly one-third of all country-year observations. The mean (median) consolidation amounts to 0.86% (0.73%) of GDP. The largest intervention by 3.75% of GDP occurred in Portugal in 2012 during the Sovereign Debt Crisis. As described in more detail below, we combine the narrative consolidation episodes at the country level with regional sensitivities to changes in national spending to get an instrument for an exogenous fall in regional government spending that varies across time and regions. We also

4. For the identification strategy described in section 2.3, focusing on spending-based fiscal adjustments implies maximizing the link between the exogenous shift variable and the outcome variable of the first-stage regression.

show that our results are hardly affected when only considering the unexpected component of the fiscal consolidation measure ($g^u_{I,t}$).[5]

2.2.3 Election data

One main contribution of our paper consists in assembling a new comprehensive dataset on regional election outcomes. This new dataset, encompassing the years from 1980 to 2015, includes detailed information on elections to the European and national parliaments and also non-nationwide (regional or local) elections. The data is harmonized such that for each election the dataset provides the valid votes and eligible voters as well as the number of votes for each party at the NUTS 2 level.

The information on the votes cast in each election at the regional level comes from different sources. Part of our data comes from the "Regional Elections" project (Schakel, 2013). There, we collect data for European, national, and regional election results disaggregated at the NUTS 2 level for five out of the eight countries in our sample (Austria, France, Italy, Spain, and Sweden). We extend this data by collecting information from national sources to include election outcomes for the most recent years. For the remaining countries (Finland, Portugal, and Germany), the election data was collected from national sources. All sources are listed in Appendix 2.B.

Altogether, we collected information on more than 200 elections, which results in roughly 20 elections per region and, on average, one election every two years. The final dataset comprises a total of 2,890 election observations, from which 1,157 belong to national elections, 937 to regional elections, and 796 to European elections. For the baseline analysis, we use the full extent of the dataset and study the evolution of political outcomes over time and across election types. In the event of two or more elections in one year (e.g., in 2009, when all national, regional, and European elections look place in Portugal), we aggregate all elections by assigning the same weight to each individual vote. Following this approach, 2,380 election observations are used in the empirical analysis.[6]

Based on the raw election data, we then group the votes along several dimensions. The most important one relies on adding together votes for the *far-left* or the *far-right*. To categorize parties as far left or far right, we rely on the existing economic and political science literature and follow, among others, Massetti and Schakel (2015), Funke, Schularick, and Trebesch (2016), and Algan et al. (2017). In the spectrum of far-left parties, we include all parties that take up traditional Marxist-Leninist and/or communist positions, such as following an anti-capitalist ideology. On the far-right, we include parties of the "New Right" that present a moderate tone

5. When using, like Alesina, Favero, and Giavazzi (2020), long-term fiscal spending plans as the austerity measure, i.e., additionally including spending shifts announced at time t to be implemented in the future, our results only change slightly.

6. In section 2.4.1.2 below, we test for different outcomes across election types.

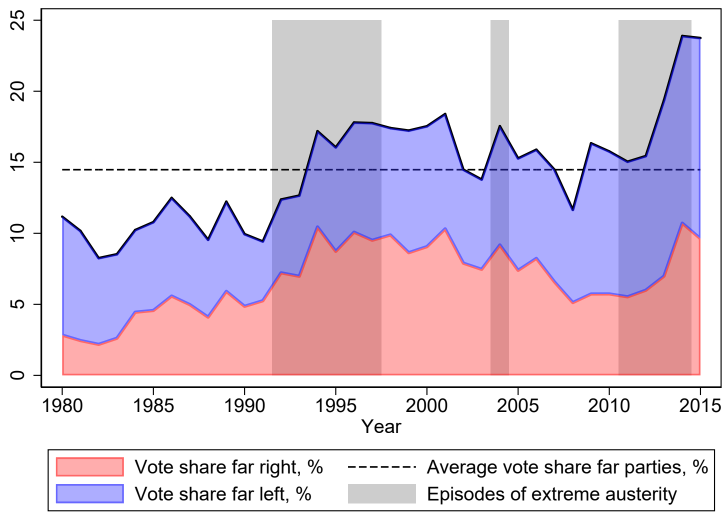


Figure 2.2.1. Vote share for extreme parties and austerity at the country level Vote shares are computed relative to total valid votes. Average vote share of extreme parties includes both far-left and far-right parties. Extreme austerity episodes are identified as above the 70^{th} percentile after summing the shocks across countries.

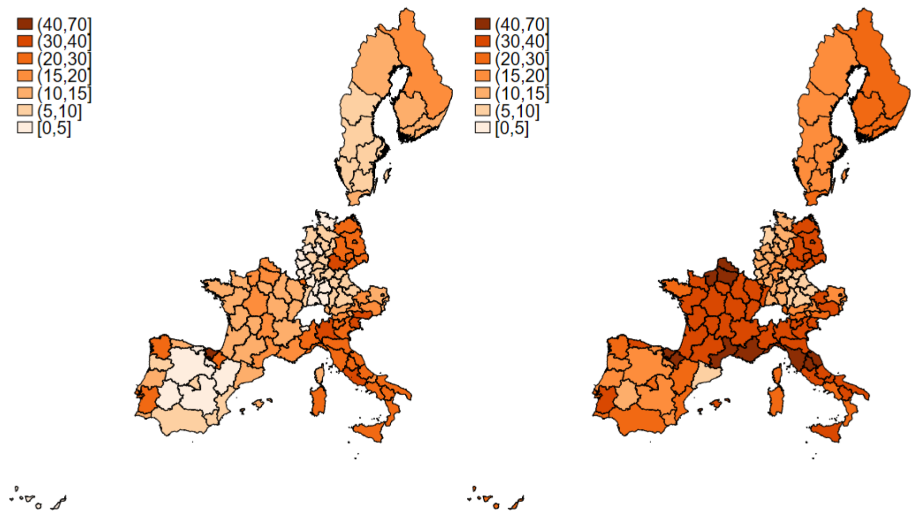
when referring to their ethnocentric and nationalistic views but nevertheless lie in the gray area between far-right extremism and right-wing populism (Funke, Schularick, and Trebesch, 2016). Importantly, we should emphasize that far-right parties are not shy about using anti-austerity narratives to capture votes (Della Porta et al., 2017). Following Massetti and Schakel (2015) and Algan et al. (2017), we also focus on populist parties that usually lie on the EU-skeptic spectrum or have strong regionalism views with suggested policies tilting to one of the extremes, with the latter being fundamental to keep some consistency between (supra-)national and regional elections. Tables 2.B.1, 2.B.2, and 2.B.3 in the Appendix provide further details and present the list of parties that are classified as either far left or far right.

2.2.4 A first look at the data

Figure 2.2.1 gives a first impression of the data and the relationship between vote shares for extreme parties and implemented fiscal consolidation programs. It shows the evolution of vote shares for far-left and far-right parties across all countries and election types in the sample together with episodes of extreme austerity indicated by the gray areas.[7] The figure highlights some important messages. First, the vote share for extreme parties is relatively volatile with an average of 15% across all years

7. Extreme austerity episodes are defined as those periods in which the narrative fiscal consolidation measure is above the 70^{th} percentile.

and countries. Second, both extreme parties' vote shares show strong co-movement with local spikes in the mid-1990s and, most recently, in the aftermaths of the Great Recession and Sovereign Debt Crisis. Third, the share of extreme votes generally increases during large-scale austerity episodes.



(a) 2007 **(b)** 2015

Figure 2.2.2. Regional vote shares on extreme parties in 2007 and 2015. Figures 2.2.2a and 2.2.2b depict, in percent, the sum of the far-left and far-right vote shares for European regions at the NUTS 2 level in 2007 and 2015, respectively. If elections do not take place in these specific years, the map shows the outcome from the previous ballot.

Figure 2.2.2 is not only informative about the detailed regional variation that our new dataset on extreme voting captures, but also suggests a strong rise in political extremism after periods of austerity. The figure presents the regional vote shares for extreme parties for all 124 regions of the sample for the years 2007 and 2015, just before the start of the Great Recession and after the height of the Sovereign Debt Crisis. The figure shows that more extreme voting in the recent past is a shared phenomenon across countries and regions. Particularly strong increases in the vote shares of extreme parties can be observed for regions in France, Spain, and Italy. However, there are also significant differences across regions within the same country. For example, while regions in the western and southern part of Germany show lower vote shares for extreme parties, voters in the eastern part favor extreme parties more strongly. In our econometric analysis, we will make use of the large variation in voting behavior over time and across regions.

To further highlight the close connection between fiscal consolidations and extreme voting, Figure 2.2.3 presents, from 2011 onwards, the change in regional government spending and votes for extreme parties in national elections for all regions of the sample. The figure shows a clear negative correlation between government spending and extreme voting. The correlation coefficient is −0.4 and is significant at the 1 percent level. Put differently, a reduction in public spending is associated with an increase in extreme parties' vote share. While Figures 2.2.1 and 2.2.3 are informative about the unconditional correlation between voting for extreme parties and fiscal consolidations, they do not provide a causal interpretation. In the rest of the paper, we conduct a thorough econometric analysis to investigate whether austerity causes more extreme voting.

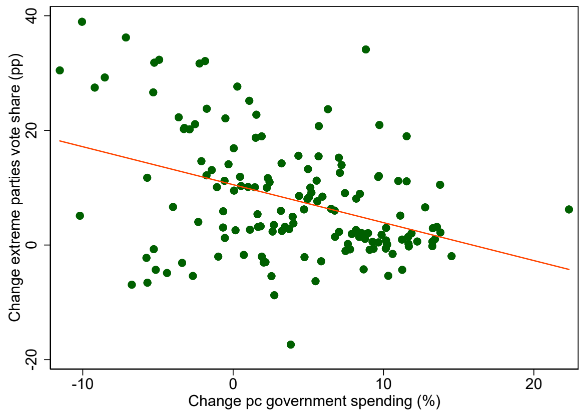


Figure 2.2.3. Extreme votes and public spending at the regional level. The y-axis plots the percentage point change in the voting share of the far-right and far-left parties between national elections. The x-axis represents the percent change in *per capita* government spending between the years of consecutive national elections. The sample includes NUTS 2 regions since 2011 and vote data for national parliament elections.

2.3 Methodology

In estimating the dynamic effects of austerity on regional political and economic outcomes, we closely follow the econometric specification by Funke, Schularick, and Trebesch (2016). To that end, we use local projections following the method pioneered by Jordà (2005) and estimate, for each horizon h = 0, ..., 4, the following equation:

$$z_{i,t+h} = \alpha_{i,h} + \beta_h \frac{G_{i,t} - G_{i,t-1}}{G_{i,t-1}} + \gamma_h(L)X_{i,t-1} + u_{i,t+h}, \tag{2.3.1}$$

where $z_{i,t+h}$ is the change in the variable of interest. More specifically, when we focus on political outcomes, $z_{i,t+h} = Z_{i,t+h} - Z_{i,t-1}$ is the percentage point change of the vote share for the far-left and far-right parties in region i between time $t-1$ and time $t+h$. The extreme parties' vote share is constructed as the number of all votes for far-left and far-right parties divided by the number of all counted votes for a given election. $\frac{G_{i,t}-G_{i,t-1}}{G_{i,t-1}}$ is the growth rate in real per capita government spending in region i between time $t1$ and $t-1$. $(L)X_{i,t-1}$ is a vector of lagged control variables and $\alpha_{i,h}$ are region fixed-effects to control for region-specific (unobserved) characteristics. Throughout, the vector of additional control variables includes two lags of the endogenous variable and two lags of regional real per capita government spending and real per capita output growth to account for lagged dynamics in regional economic activity and public expenditures, respectively. When focusing on economic outcomes in Section 2.4.2, $z_{i,t+h}$ is the growth rate of the variable of interest, $\frac{Z_{i,t+h}-Z_{i,t-1}}{Z_{i,t-1}}$, for all variables except the labor share, for which we consider $z_{i,t+h}$ to be the difference in levels, $Z_{i,t+h}-Z_{i,t-1}$.

The main focus of our analysis consists of estimating the parameter $-\beta_h$, which directly yields at horizon h, the response of the variable of interest to a fall in regional government spending by one percent. Throughout, we cluster the standard errors at the regional level. Similar to Funke, Schularick, and Trebesch (2016), we do not include time fixed-effects in the baseline regression because they would absorb part of the variation in elections that are held by all regions in the same year (for example European elections). However, we will show below that our main findings remain when allowing for time fixed-effects.

For the identification of exogenous fiscal consolidations, we instrument the change in regional government spending with a Bartik-type instrument (Bartik, 1991) where we rely on the narratively identified spending-based austerity shocks from Alesina, Favero, and Giavazzi (2020) as described in Section 2.2.2. In particular, the Bartik instrument is computed as follows:

$$\frac{\overline{G_i}}{\overline{G_I}} \times \tilde{g}_{I,t},$$

where $\tilde{g}_{I,t}$ is the narrative national consolidation measure and $\overline{G_i}$ and $\overline{G_I}$ are averages of *per capita* government spending in region i and country I, respectively. To compute these averages, we follow Nekarda and Ramey (2011) and use data from the full sample to control for structural changes across regions over the sample period. Intuitively, if $\frac{\overline{G_i}}{\overline{G_I}}$ is above 1, region i spends more *per capita* than the national average. This implies that a disproportionate amount is spent in this region compared to other regions in the country. By interacting these regional sensitivities with narrative accounts of national fiscal consolidation programs, we assume that regions that rely more heavily on public spending cut back government expenditures more strongly when the national government implements austerity measures. Thus, the idea of the instrument is to scale national fiscal consolidation plans such that spending varies more in regions with a larger share of per capita national government spending. To be precise, we estimate the following first-stage regression:

$$\frac{G_{i,t} - G_{i,t-1}}{G_{i,t-1}} = \alpha_i + \zeta \frac{\overline{G_i}}{\overline{G_I}} \times \tilde{g}_{I,t} + \gamma(L) X_{i,t-1} + \epsilon_{i,t}. \tag{2.3.2}$$

Figure 2.C.1 in the Appendix shows a heat map depicting the share $s_i = \frac{\overline{G_i}}{\overline{G_I}}$ for the NUTS 2 regions used in the sample. There is considerable cross-sectional variation in this measure, ranging from 0.72 to 1.57. We calculate the lowest shares for Norte (Portugal, 0.72), Niederbayern (Germany, 0.74), and Niederösterreich (Austria, 0.75), and the highest shares for Lazio (Italy, 1.57), Wien (Austria, 1.52), and Área Metropolitana de Lisboa (Portugal, 1.43). There is only small variation in the shares over time. When calculating time-varying shares for each region, we find that the average standard deviation is around 0.05. This limited time variation justifies our choice of constant regional shares even though the results are robust when using a time-varying measure of the spending share.

Our identifying assumption is that central governments do not adopt austerity measures because regions that receive a disproportionate amount of government spending are experiencing certain economic and political outcomes relative to other regions. For example, the government does not cut expenses because a certain region is doing better economically or because political polarization is not rising. This is likely for two reasons. First, the data used is disaggregated at the NUTS 2 level. Intuitively, the main assumption might be violated when focusing on high aggregation levels with only few regions within a country because politically and economically important regions could directly influence central government decisions. Second, we are using narrative-identified austerity shocks that are by construction not driven by economic conditions and primarily motivated by national budgetary motives.

Another potential concern with our estimation strategy would arise if regions receiving large amounts of national spending were more cyclically sensitive than other regions and therefore might face stronger voter turnover for extreme parties. We use the standard deviation of output growth to compare the cyclical sensitivity of regions that receive large and small amounts of national spending. The standard deviations are very similar in regions with above-median national spending shares and in regions with below-median national spending shares (0.034 versus 0.031), indicating that a divergence in overall cyclical sensitivity does not bias our results. Following the same approach for the election data, we find that the standard deviation of the change in vote share of extreme parties is similar for regions with spending above and below the national median (0.050 and 0.042). As pointed out by Goldsmith-Pinkham, Sorkin, and Swift (2020), our empirical strategy using the Bartik instrument is valid even if the spending shares are correlated with the *level* of the extreme parties' vote share. Instead, our strategy asks whether differential exposure to national fiscal consolidations leads to differential *changes* in the outcome.

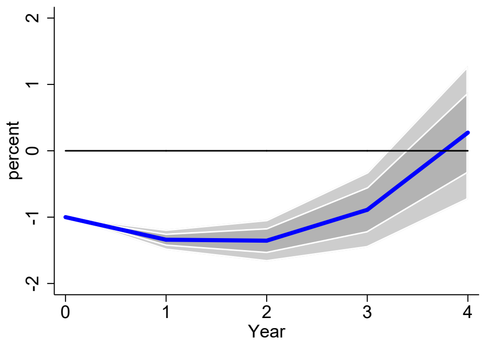


Figure 2.3.1. Government spending response to austerity. The figure plots the percent change of per capita government spending in response to an austerity induced change in government spending by one percent. Bands are 68% (dark) and 90% (light) confidence intervals.

Importantly, our instrument fulfills the relevance condition. The first-stage Olea and Pflueger (2013) F-statistic is above 70 and thus well above the threshold of 23 for a 5% critical value, implying that weak instruments are not a severe concern for our analysis. In addition, Figure 2.3.1 shows the estimated response of regional government spending to the consolidation shock. The dark and light shadings are, respectively, 68% and 90% confidence bands based on robust standard errors clustered at the regional level. The response is normalized so that spending falls by 1% in year 0. We find a significant and persistent fall in regional government spending following the austerity shock. Two years after the shock materialized, the reduction in government spending reaches its trough with around 1.25%. Thereafter, government spending converges back to its pre-shock level and the response becomes insignificant four years after the shock, which shows the transitory impact of our identified fiscal interventions. In what follows, we will use the estimated reduction in regional government spending and test whether there is a causal effect of lower public spending on voting for extreme parties.

Whereas our main analysis focuses on characterizing whether austerity shocks affect voting behavior, below we also assess the quantitative importance of this relationship. In doing so, we conduct a forecast error variance decomposition (FEVD) exercise. The local projection framework allows computing the contribution of the austerity shocks to the forecast error variance of our variables of interest. First, we consider the share of the variance in the vote shares that can be accounted for by austerity shocks from 1980 until 2014. The fraction of the variance in the vote shares at different horizons accounted for by austerity shocks can be recovered directly from the estimates of Equation (2.3.1). This measure therefore provides a metric of the extent to which austerity shocks are quantitatively important in driving voting dynamics.

We closely follow **Born2020bempty citation**, who extend the approach by Coibion, Gorodnichenko, Kueng, and Silvia (2017) and Gorodnichenko and Lee (2020) to a panel setting. In particular, we compute the variance share of the regional consolidation shock at horizon h as the R^2 of the following regression:

$$\hat{u}_{i,t+h} \;=\; \lambda_0 \hat{\epsilon}_{i,t+h} + \ldots + \lambda_h \hat{\epsilon}_{i,t} + \nu_{i,t+h}. \tag{2.3.3}$$

where $\hat{u}_{i,t+h}$ is the forecast error of the local projection (2.3.1) at horizon h and $\hat{\epsilon}_{i,t+h}$ are the (horizon-specific) predicted values of the first-stage regressions (2.3.2).

2.4 Results

In this section, we present and discuss our main empirical findings. We start by showing that an exogenous fall in regional government spending leads to a significant and persistent increase in the vote share for antiestablishment extreme parties, lower voter turnout, and more fragmentation. Moreover, we conduct a FEVD exercise to evaluate the quantitative importance of the identified consolidation episodes in explaining variation in voting for extreme parties. Then, we show that our main result is robust to several modifications of the baseline model and further decompose our baseline response across several dimensions: the increase in extreme-party voting is rather similar across election types (regional, national, European elections) and is not being driven by one side of the political spectrum with both the far-left and far-right vote shares rising in response to austerity. We also investigate the economic consequences of fiscal consolidations and show that the austerity-induced decrease in regional government spending has strong recessionary effects. Taken together, these findings are consistent with the idea that voters react to the negative economic impact of spending-based austerity episodes by shifting their vote toward more antiestablishment and extreme parties. Finally, we differentiate between economic recessions driven by fiscal consolidations and economic downturns that are unrelated to austerity and show that the political costs of economic downturns are considerably amplified when they coincide with fiscal consolidations.

2.4.1 Political Costs

Figure 2.4.1 presents our main result regarding the response of the vote share for extreme parties following a fiscal consolidation. The reduction in regional government spending leads to a significant increase in the extreme parties' vote share. A fall in public spending by 1% raises the extreme parties' vote share by more than 1.5 percentage points in the year of the fiscal policy implementation. Additionally, the vote share increase is very persistent. Two years after the shock, extreme parties have gained more than 3 percentage points. Even four years after the consolidation was implemented, the vote share is still more than 2.5 percentage points above its pre-shock level. Thus, austerity induces large and long-lasting political costs with voters moving away from more traditional parties to extreme ones.

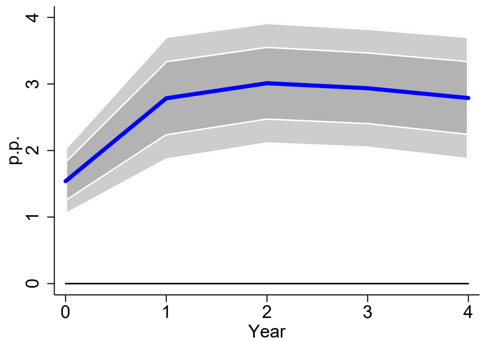


Figure 2.4.1. Response of extreme parties' vote share to austerity. The figure plots the impulse response in percentage points of the vote share for the extreme parties to an austerity-induced change in government spending by one percent. Bands are 68% (dark) and 90% (light) confidence intervals.

The documented increase in extreme voting following fiscal consolidations might be due to two different effects. First, holding turnout constant, if more people vote for extreme parties, their vote share increases. Second, austerity might discourage people from participating in the ballot and thus lower turnout. If this effect disproportionately applies for non-extreme voters, the vote share of extreme parties raises even without an increase in total votes for extreme parties. To test whether our results are driven by one of these effects or a combination of both, we re-estimate Equation (2.3.1) using either the change in turnout or total votes for extreme parties, respectively, as the dependent variable. Turnout is computed as the number of all counted votes relative to all eligible votes and total votes for extreme parties is constructed as the ratio between the number of votes for extreme parties and the number of all eligible votes.

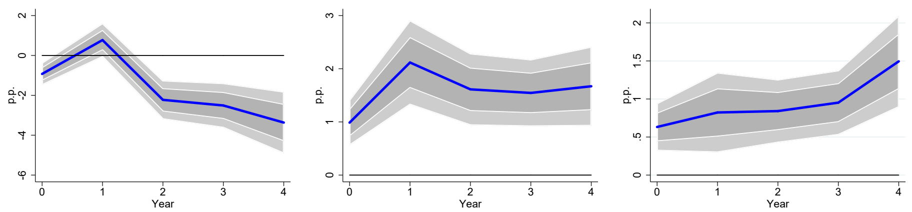
Figure 2.4.1 displays the estimation results, where the left panel shows the response of voter turnout and the middle panel presents the impact of austerity on total votes for extreme parties. Voter turnout significantly falls following a reduction in regional government spending. Four years after the fiscal intervention, turnout is reduced by almost 3.5 percentage points. In addition, the total number of votes for extreme parties significantly increases, reaching a peak of more than 2 percentage points in the year after the fiscal intervention. Therefore, the increase in extreme parties' vote share following austerity can be explained by fiscal consolidations leading to a combination of fewer people voting with higher tendency to vote for extreme parties.

We also study the impact of fiscal consolidations on fragmentation, which we construct following (Laakso and Taagepera, 1979). In particular, we rely on a measure of concentration taken from the industrial economics literature—the Herfindahl-Hirschmann concentration index—or, more precisely, its complement. This is known as the Effective Number of Parties, *ENP*, and is defined as:

$$\text{ENP}_{i,t} = \sum_{j=1}^{n} p_{j,t}^2,$$

where n is the number of parties in the election and p_j is party *j*'s share in the total votes (between 0% and 100%). The lower the *ENP*, the higher the level of fragmentation. This measure takes two important dimensions of fragmentation into account: the number of parties involved in the decision-making process (political fragmentation) and the size inequalities between the participants (size fragmentation) (Geys, 2004). When there is more than one election per year, we use the average across elections. We estimate the same local projection but replace the extreme parties' vote share by the fragmentation variable given by $(1 - \text{ENP}_{i,t})$.

The right panel of Figure 2.4.1 presents the estimation results. Austerity implies a significant increase in fragmentation, which amounts to around 1.5 percentage points at the end of the forecast horizon. Based on previous evidence on negative economic consequences of higher political fragmentation (Azzimonti, 2018; Funke, Schularick, and Trebesch, 2020), this finding might suggest that, besides direct economic effects, fiscal consolidations also shape economic outcomes indirectly by leading to a more polarized political environment.



(a) Voter turnout **(b)** Total votes for extremes **(c)** Fragmentation

Figure 2.4.2. Responses of voter turnout, total votes for extreme parties, and fragmentation. The figure plots the impulse response in percentage point changes of the voter turnout, the total number of votes for extreme parties, and the political concentration to an austerity-induced change in government spending by one percent. Voter turnout is the ratio between valid votes and total eligible voters. "Total votes for extreme parties" is the sum of votes for far-left and far-right parties. Political fragmentation is measured by one minus the Herfindahl-Hirschmann concentration index, measured using the effective number of parties. Bands are 68% (dark) and 90% (light) confidence intervals.

In Table 2.4.1, we report the contribution of austerity shocks to the forecast error variance of the vote shares for a forecast horizon up to four years, where the estimates are based on Equation (2.3.3). It is evident that austerity shocks account for an economically significant part of extreme voting, and in particular in the medium run. At the four-year horizon, austerity explains 9.7% of the variation in extreme parties' vote share. We further differentiate between parties on the far left and far right. Interestingly, fiscal consolidations account for a larger part of voting for far-left parties than for far-right ones (9.1% versus 2.7% at the four-year horizon).

Table 2.4.1. Forecast error variance decomposition

Horizon	Far	Far left	Far right
1	0.6%	3.8%	1.1%
2	4.1%	5.4%	0.5%
3	7.5%	8.6%	1.7%
4	9.7%	9.1%	2.7%

Notes: Forecast error variance decomposition of far, far left, and far right vote shares based on local projections (2.3.3).

In summary, our main findings show that austerity has significant political costs. Fiscal consolidations lead to a strong and persistent increase in vote shares for extreme parties, lower voter turnout, and increased fragmentation. These findings are not only significant from an econometric point of view, but also from an economic perspective, with austerity accounting for a large share of voters favoring more extreme parties.

2.4.1.1 Robustness

In this section, we demonstrate that our main result of an increase in extreme parties' vote share following a fiscal consolidation is robust to several modifications of the baseline model. We start by modifying our aggregate narrative consolidation measure such that we only consider the unexpected component of the austerity series, i.e., $g^u_{i,t}$ from Equation 2.2.1. This rules out the hypothesis that our main finding could be driven by the anticipated component of the fiscal consolidation measure used, $g^a_{I,t-1}$. Table 2.4.2 presents the results, where the first upper panel also reports the baseline estimates. The estimated effects of a fiscal consolidation on the extreme parties' vote share are similar when only considering the unexpected component of the austerity measure. For example, four years after the consolidation was implemented, both estimations show an increase in the vote share of around 3 percentage points. Thus, our main finding is not due to strong anticipated effects of the fiscal policy change.

Jordà and Taylor (2016) suggest another way to control for significant anticipation effects in the narrative consolidation measure. They regress the austerity measure on a set of lagged macro control variables and take the residual of that regression as the new narrative consolidation series. This new measure is orthogonal to past economic developments and should thus capture only unexpected changes in fiscal policy. We follow their strategy, first regressing our narrative measure on several lagged macro covariates and then using the residual as the shift component in the construction of the Bartik instrument. Motivated by the set of regressors chosen by Jordà and Taylor (2016) and Klein (2017), the vector of control variables in the first regression includes country and time fixed-effects and lagged values of real GDP growth, real private consumption growth, the government debt-to-GDP ratio and real short-term interest rates.[8] The estimates presented in Table 2.4.2 (entry "Unpredicted austerity") show a similar finding compared to our baseline specification: austerity significantly increases extreme parties' vote share, although point estimates are larger when relying on the unpredicted austerity measure. In sum, this last result again suggests that anticipated changes in fiscal policy do not significantly drive our main findings.

Next, we verify that our result is not an artifact of the Great Recession and Sovereign Debt Crisis years by dropping the years 2008 and later and focusing on the pre-Great Recession sample. Table 2.4.2 shows that our finding is not significantly affected by this sample change. Put differently, the causal link between a reduction in regional public spending and an increase in extreme voting is by no means a result of the Great Recession and Sovereign Debt Crisis years but describes a general tendency in the data since the 1980s.

In our baseline estimation, we clustered the standard errors at the regional level. To also take into account serial correlation and heteroskedasticity among the residuals over time, we rerun the baseline model using Driscoll and Kraay (1998) standard errors. As shown in Table 2.4.2, standard errors become slightly larger when relying on the Driscoll and Kraay (1998) adjustment, but statistical significance remains.

Although Figure 2.2.1 does not indicate a clear time trend in the vote share for extreme parties, we want to ensure that our results do not capture a general movement toward more extreme parties over time. Therefore, we extend our baseline model by including time fixed-effects that should also control for common shocks across regions. Table 2.4.2 shows that the estimates are very similar to our baseline results.

As an additional check, we recalculate our Bartik instrument by using the lagged value of $s_{i,t}$ instead of the average value s_i as used in the baseline specification. Thus, we allow for a time-varying regional elasticity to national public spending changes

8. Data are taken from ARDECO, the Jordà-Schularick-Taylor Macrohistory Database (Jordà, Schularick, and Taylor, 2017), and OECD.

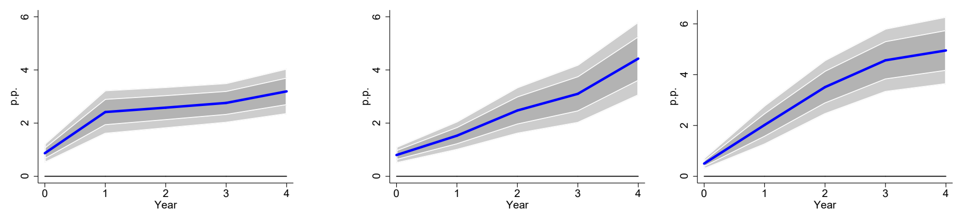
and use its lag to rule out any contemporaneous correlation between the national consolidation measure and the regional spending share. Again, as presented in Table 2.4.2, the results are very similar to the baseline estimates, indicating that our finding is robust to different ways of calculating the share measure used in the construction of the Bartik instrument.

Table 2.4.2 also presents the results when using the original Devries et al. (2011) consolidation measure, which includes both spending- and tax-based narratively identified fiscal consolidations, instead of the adjusted Alesina, Favero, and Giavazzi (2020) series. While the effect is somewhat smaller on impact, at the end of the forecast horizon both measures imply an increase in extreme parties' vote share of more than 2 percentage points.

Finally, the last two rows of Table 2.4.2 show the results when changing the sample. First, we exclude capital regions given that capitals have on average a higher government spending share. Second, we drop all regions with the top 10% highest government spending shares. It is evident that both sample changes do not significantly affect our findings. We also show in the appendix that our results are not driven by any particular country in the sample. When separately dropping one country at a time from the sample, results change only slightly (see Table 2.C.1).

Taken together, the results presented in this subsection provide confidence that the significant rise in extreme parties' vote share following a fiscal consolidation is a robust feature of the data not driven by the way we construct the national austerity measure—the share variable of the Bartik instrument—and holds for different changes in the sample.

2.4.1.2 Election types and far-left/far-right vote shares

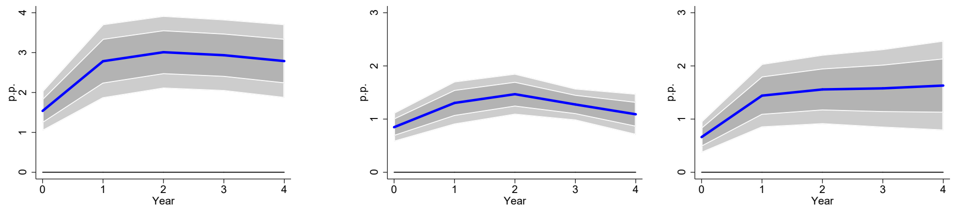


(a) National **(b)** Regional **(c)** European

Figure 2.4.3. Response of extreme parties' vote share to austerity by election type The figures plot by election type the impulse response in percentage points of the vote share for the extreme parties to an austerity-induced change in government spending by one percent. Bands are 68% (dark) and 90% (light) confidence intervals.

In the baseline estimation, we included voting results from all election types (European, national, regional). Next, we investigate whether there is significant heterogeneity across elections. In doing so, we separately restrict the sample to national, regional, or European elections. Figure 2.4.3 presents the results of this exercise; the left panel shows the response for national elections, the middle panel for regional elections, and the right panel for European elections. The figure shows that the increase in extreme parties' vote share following a fiscal consolidation is present for all election types. The rise is most pronounced for European elections, which can be interpreted as evidence that austerity is mainly seen as implemented by European institutions; thus, they are therefore also blamed the most. However, extreme parties also significantly gain in regional elections, with an increase of larger magnitude compared to national elections.

As a further check, we study whether the increase in extreme vote shares is driven by either far-left or far-right parties. In particular, we re-estimate our baseline model but now separately focus only on the far-left or far-right parties' vote share. The obtained results are shown in Figure 2.4.4: the left panel repeats the estimates of the baseline model (the sum of far-left and far-right vote shares), the middle panel presents the vote share response for far-left parties, and the right panel for far-right parties. Austerity leads to a significant and persistent vote share increase for both extremes. The peak responses amount to around 1.5 percentage points. However, estimation uncertainty is larger for the far-right parties' vote share, whereas the far-left parties' vote share response is estimated more precisely.[9]



(a) Baseline **(b)** Far Left **(c)** Far Right

Figure 2.4.4. Response of total extreme, far-left, and far-right parties' vote share to austerity. The figures plot the impulse response in percentage points of the vote share for the total extreme, far-left, and far-right parties to an austerity-induced change in government spending by one percent. Bands are 68% (dark) and 90% (light) confidence intervals.

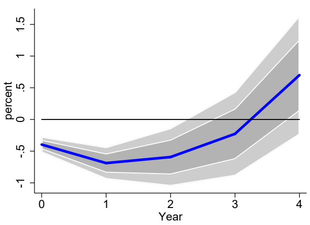
2.4.2 Economic Costs

Our main results indicate strong political costs of fiscal austerity. We have documented that a reduction in public spending leads to a significant increase in the

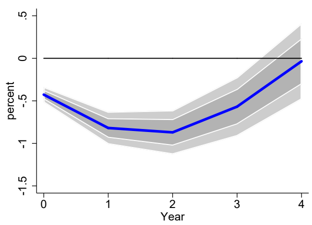
9. The smaller (larger) estimation uncertainty regarding the left (right) parties' vote share response might be related to the larger (smaller) variation accounted for by austerity as presented before in Table 2.4.1.

vote share for extreme parties. In the following, we try to answer what drives this voter movement away from more traditional parties toward extreme ones. A related stream of literature has shown that voter support for extreme parties is closely linked to economic developments. For example, Funke, Schularick, and Trebesch (2016) find that following a financial recession, the vote share of far-right parties rises significantly and persistently. In addition, Guriev (2018) show that higher unemployment rates during the Great Recession have considerably contributed to the recent rise of antiestablishment sentiment. To check whether the austerity-induced increased support for extreme parties is also related to a worsening of regional economies, we proceed by estimating the economic costs of fiscal consolidations. This issue is of interest on its own because studies at the aggregate (national) level provide mixed evidence. Some papers estimate that fiscal consolidations cause an economic recession (Guajardo, Leigh, and Pescatori, 2014), whereas others find only mild or even expansionary effects from austerity (Alesina, Ardagna, Perotti, and Schiantarelli, 2002).

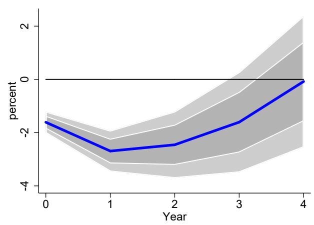
Figure 2.4.5 presents the responses of several economic variables to the regional austerity shock based on equation (2.3.1). All of them are expressed in percent changes (growth rates), with the exception of the labor share variable, which is presented in percentage points.



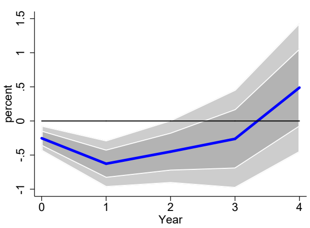
(a) Output



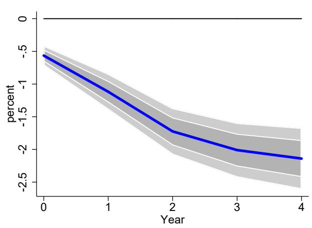
(b) Employment



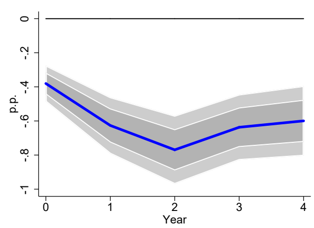
(c) Investment



(d) Motor Vehicles



(e) Hourly Wage



(f) Labor share

Figure 2.4.5. Economic responses to austerity. These figures plot the response of a one percent increase in government spending. All responses are expressed in percent changes (growth rates), with the exception of the labor share variable, which is presented as a percentage point change (its difference). Shaded areas are 68% (dark) and 90% (light) confidence intervals.

Panel 2.4.5a of Figure 2.4.5 shows the regional output response to the fiscal consolidation. We find that lower public expenditures lead to a significant fall in regional output. On impact, output is reduced by 0.4%, then declines further up to 0.7%, before slowly converging back to its equilibrium level at the end of the forecast horizon. When relating the output response to the extreme parties' vote share response shown in Figure 2.4.1, our results imply that an exogenous reduction in government spending that lowers regional GDP by 1% triggers an increase in extreme parties' vote share by around 5 percentage points.[10]

To put these results in perspective, we can compare our GDP and vote-share estimates to the ones reported in Funke, Schularick, and Trebesch (2016) and Jordà, Schularick, and Taylor (2013). Funke, Schularick, and Trebesch (2016) estimate that extreme vote shares increase by around 30% in the five years after a financial recession, and Jordà, Schularick, and Taylor (2013) show that a financial recession lowers GDP by 4%. Because our results for the vote shares are in percentage points, a direct comparison to our baseline estimates is not directly possible. So, when re-estimating the model with the vote-share variable expressed in percent changes as in Funke, Schularick, and Trebesch (2016), we find that austerity leads to an increase in the extreme parties' vote share of almost 27% four years after the shock. Thus, our results indicate that a fiscal consolidation-induced recession leads to a stronger movement toward extreme parties than a financial recession. In particular, while both economic downturns lead to an increase of extreme parties' vote share of around 30%, the reduction in GDP following austerity is much lower than the one triggered by a financial recession (0.7% versus 4%). Therefore, the political costs of economic downturns are considerably amplified when fiscal policy causes the increase in economic slack.[11] Below, we will discuss in more detail the different impact of normal (non-austerity-driven) and austerity-induced recessions on extreme voting.

As Panel 2.4.5b of Figure 2.4.5 indicates, fiscal consolidations do not only have negative real consequences, but also imply severe labor market consequences. The employment rate falls by almost 1% two years after the austerity measure was implemented. In the Appendix, we also report the corresponding output and employment government spending multipliers (see Section 2.D), where the estimation procedure closely follows Bernardini, Schryder, and Peersman (2020) and Gabriel, Klein, and Pessoa (2021). The output multiplier is estimated slightly below two, whereas the employment multiplier takes a value of slightly above two. These values are in the range of other estimates on regional government spending multipliers (Nakamura

10. Two years after the fiscal consolidation, output is lowered by 0.6% percent, whereas the vote share for extreme parties is up by 3 percentage points ($\frac{3}{0.6} = 5$).

11. It is necessary to keep in mind that the different aggregation levels in our study and Funke, Schularick, and Trebesch (2016) and Jordà, Schularick, and Taylor (2013) (regional versus national) make a direct comparison somewhat more difficult.

and Steinsson, 2014; Bernardini, Schryder, and Peersman, 2020; Gabriel, Klein, and Pessoa, 2021).

Panels 2.4.5c and 2.4.5d present the responses of private investment and the number of motor vehicles that we use as a proxy for durable consumption following Mian, Rao, and Sufi (2013) and Demyanyk, Loutskina, and Murphy (2019). Both private demand components significantly fall following the reduction in public expenditures. While the decrease in private investment is stronger than the one in output, the fall in durable consumption closely mimics the regional GDP response. Households' consumption expenditure should be closely linked to their disposable income stream in the sense that a lower income might well lead to lower (durable) consumption spending. Panel 2.4.5e indeed supports this hypothesis. Here, we report the real wage response expressed as average real compensation per hour worked. Wages fall significantly and persistently in response to the fiscal consolidation. On impact, wages decline by more than 0.5% and continue to fall until the end of the forecast horizon. Finally, Panel 2.4.5f presents the response of the labor share, which is significantly reduced by the austerity measure. Thus, the reduction in public spending induces a redistribution of income away from working households.

Taken together, these last results indicate severe economic costs of fiscal consolidations and therefore support previous evidence on the contractionary impact of austerity at the national level (Guajardo, Leigh, and Pescatori, 2014; Jordà and Taylor, 2016). Moreover, they highlight the close relationship between detrimental economic developments and voters' support for extreme parties.

2.4.3 State-Dependencies

So far, we have assumed that the political costs of fiscal consolidations are common across European regions as our baseline model is estimated as a pooled regression. However, it might well be argued that specific economic environments amplify or dampen the impact of austerity on extreme voting. In the following, we investigate how the state of the business cycle and regional characteristics like urbanization and economic development affect our estimates.

To test for potential state dependencies, we extend our baseline specification (2.3.1) and estimate for each horizon $h = 0, ..., 4$, the following regression:

$$\begin{aligned} z_{i,t+h} =& I_{i,t}\left[\beta_h^A \frac{G_{i,t} - G_{i,t-1}}{G_{i,t-1}} + \gamma_h^A(L)X_{i,t-1}\right] + (1 - I_{i,t})\left[\beta_h^B \frac{G_{i,t} - G_{i,t-1}}{G_{i,t-1}} + \gamma_h^B(L)X_{i,t-1}\right] \\ &+ \alpha_{i,h} + u_{i,t+h}. \end{aligned} \tag{2.4.1}$$

$I_{i,t}$ is an indicator variable for the defined state in period t. We now instrument spending changes with the Bartik instrument interacted with the state indicator. β_h^A

and β_h^B directly yield, for each horizon h and states A and B, the response of the extreme parties' vote share.

We start by looking at how the state of the business cycle affects the political costs of austerity. Recessions (expansions) are defined as periods in which the regional growth rate of per capita GDP is negative (positive). Panel A of Table 2.4.3 shows the results. We find that the increase in extreme parties' vote share following a fiscal consolidation is generally larger during recessions. Four years after the consolidation was implemented, extreme parties gain 4.08 (2.01) percentage points when austerity is done in a period of high (low) economic slack. As shown by the Anderson and Rubin (1949) and HAC test results, at longer horizons the difference in both states becomes statistically significant. This result is closely related to a literature documenting that economic recessions considerably amplify the negative economic consequences of austerity (Jordà and Taylor, 2016) and again shows the close relationship between the state of the economy and voting behavior.

Next, we allow for different effects between rural and urban regions. Rural and urban areas are defined according to regional density computed as the ratio between the population and total area of the region. Regions are classified as urban if density is higher than the country's median and classified as rural otherwise.[12] We find that the effects are generally larger in rural regions than urban regions, although the differences are relatively small and not statically significant.

Finally, we also compare the effects in poor and rich regions, where regions are classified as poor (rich) when their per capita GDP is below (above) the country's median. At all horizons, the increase in extreme parties' vote share is somewhat larger in poor regions than rich regions. However, we find only small differences that are estimated to be indistinguishable different from zero.

2.4.4 Austerity-recessions and non-austerity recessions

In Section 2.4.2, we have shown that there is a close link between the political and economic consequences of fiscal consolidations. Austerity leads to an increase in extreme parties' voting and lowers economic activity. A related literature has also shown that vote shares of extreme parties rise following severe economic downturns (Funke, Schularick, and Trebesch, 2016; Guriev, 2018). This might raise the question of whether our main findings are simply a reflection of economic recessions leading to higher vote shares for extreme parties. In other words, do austerity-driven recessions lead to different political outcomes than other economic downturns? In the following, we will show that the political costs of economic downturns are significantly amplified when recessions are indeed driven by fiscal consolidations.

We extend our baseline equation (2.3.1) and estimate for each horizon $h = 0, \ldots, 4$ the following regression:

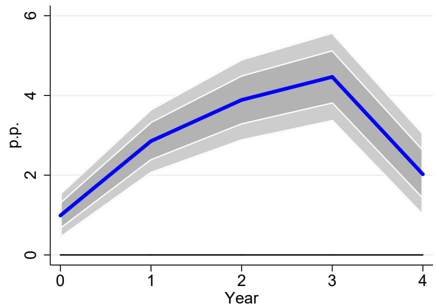
12. Data on the regional area at NUTS 2 was retrieved from Eurostat.

$$
\begin{aligned}
z_{i,t+h} = & I^{ra}_{i,t}\left[\zeta^{ra}_h + \beta^{ra}_h \frac{G_{i,t}-G_{i,t-1}}{G_{i,t-1}} + \gamma^{ra}_h(L)X_{i,t-1}\right] \\
& + I^{r}_{i,t}\left[\zeta^{r}_h + \beta^{r}_h \frac{G_{i,t}-G_{i,t-1}}{G_{i,t-1}} + \gamma^{r}_h(L)X_{i,t-1}\right] \\
& + (1-I^{ra}_{i,t}-I^{r}_{i,t})\left[\zeta^{e}_h + \beta^{e}_h \frac{G_{i,t}-G_{i,t-1}}{G_{i,t-1}} + \gamma^{e}(L)X_{i,t-1}\right] + \alpha_{i,h} + u_{i,t+h}.
\end{aligned}
\tag{2.4.2}
$$

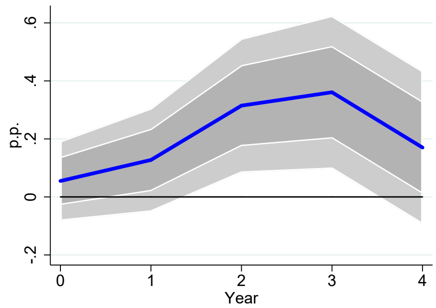
$I^{ra}_{i,t}$ is a dummy variable that takes a value of one in year t, when region i contemporaneously experiences negative per capita GDP growth and implements fiscal consolidation measures (i.e., when the Bartik instrument is larger than zero). On the other hand, $I^{r}_{i,t}$ is a dummy variable that takes a value of one when the regional per capita GDP growth rate is negative and we identify no fiscal consolidation (when our Bartik instrument is equal to zero). Thus, $I^{ra}_{i,t}$ captures recessions that coincide with austerity ("austerity-recessions") and $I^{r}_{i,t}$ measures economic downturns that are not directly related to fiscal consolidations but can be described as a combination of different negative shocks that lead to lower economic activity ("non-austerity recessions"). We also include a dummy for all remaining episodes when there is positive economic growth (economic expansions), $1-I^{ra}_{i,t}-I^{r}_{i,t}$, to use the entire variation of the sample. The coefficients ζ^{ra}_h and ζ^{r}_h capture the average impact of austerity recessions and non-austerity recessions, respectively, on the vote shares of extreme parties. In addition, β^{ra}_h and β^{r}_h indicate the marginal effect of lowering regional government spending by 1% in austerity recessions and non-austerity recessions, respectively.[13] If ζ^{ra}_h is larger (smaller) than ζ^{r}_h, this would imply that economic downturns driven by fiscal consolidations lead to a larger (smaller) increase in extreme voting than other downturns. The same logic also applies to the marginal effect coefficients β^{ra}_h and β^{a}_h.

The first row of Figure 2.4.6 presents the estimation results, where the upper left panel shows the difference between ζ^{ra}_h and ζ^{r}_h and the upper right panel shows the difference between β^{ra}_h and β^{r}_h. The difference in the average recession effect (ζ^{ra}_h-ζ^{r}_h) is positive and highly statistically significant. Thus, austerity recessions lead to a larger increase in the vote shares for extreme parties than non-austerity recessions. Furthermore, the difference in the marginal coefficients is also estimated to be positive and becomes statistically significant at longer horizons. This implies that, in recessions coinciding with fiscal consolidations, a reduction in regional government spending implies a larger increase in extreme voting compared to lowering public spending in non-austerity recessions. These results suggest that austerity recessions

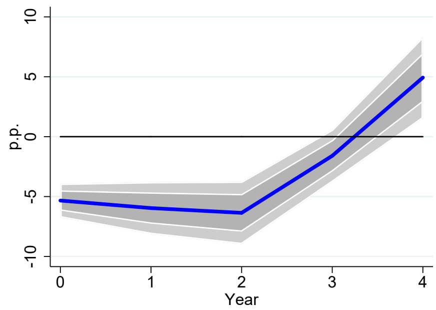
13. As before, we normalize the responses such that regional government spending falls by 1% in the impact period.



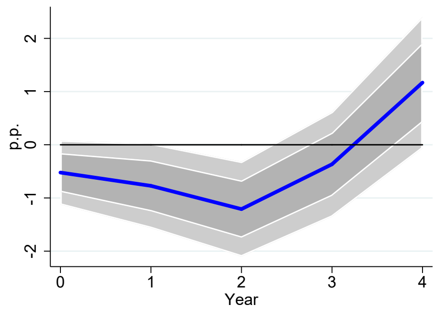
(a) Far vote share: average effect (ζ_h^{ra}-ζ_h^{r})



(b) Far vote share: marginal effect (β_h^{ra}-β_h^{r})



(c) Trust: average effect (ζ_h^{ra}-ζ_h^{r})



(d) Trust: marginal effect (β_h^{ra}-β_h^{r})

Figure 2.4.6. Difference in responses between austerity-recessions and non-austerity recessions. Panels 2.4.6a and 2.4.6b on the first row show the difference of the average and marginal effects between austerity recessions and normal recessions on the vote share of extreme parties estimated through Equation 2.4.2. Panels 2.4.6c and 2.4.6d on the second row depict the equivalent for the outcome variable trust on national parliaments. Bands are 68% (dark) and 90% (light) confidence intervals.

are special in the sense that they considerably amplify the political costs of economic downturns. Thus, our main results do not simply capture a general tendency of more voting for extreme parties during economic downturns but instead point toward a specific transmission mechanism underlying fiscal consolidations.

How could such a transmission mechanism operate? One potential channel is related to trust in the political system and the government. If voters' trust in the government falls more during austerity recessions than non-austerity recessions, the heightened skepticism about the political environment might lead to a stronger movement away from traditional parties to more extreme ones. To test this hypothesis, we use data assembled by Algan et al. (2017) and investigate the impact of austerity recessions and non-austerity recessions on voters' trust in the country's parliament. The trust index varies between zero and one and is based on micro data from the European Social Survey (ESS). People are asked to state the level of trust in the country's parliament from zero to ten, where zero means no trust at all and ten means complete trust. The survey is conducted biennially, from 2000 until 2014, and provides data at the NUTS 2 level for most of the countries in our sample, with the exception of Finland and France. The results are presented in the second row of Figure 2.4.6. Both estimated differences are negative and significant, which implies that trust in the countries' parliament falls much more during austerity recessions than non-austerity recessions. Voters seem to become more skeptical about the political environment when the higher economic slack they experience is related to active policy interventions like fiscal consolidations. Given that voters might blame the government for part of the economic downturn, they tend to punish established parties and instead support more extreme ones.

2.5 Conclusion

While the economic consequences of fiscal consolidations are studied extensively, the political costs of austerity are less well understood. In this paper, we provide new evidence on how reductions in government spending affect election outcomes. Using a novel regional dataset on election outcomes for several European countries, we find that fiscal consolidations lead to a significant increase in vote shares of extreme parties, raise fragmentation, and lower voter turnout. A reduction in regional public spending by 1% causes a rise in extreme parties' vote share of around 3 percentage points. We highlight the close relationship between economic developments and voters' support for extreme parties by showing that austerity induces severe economic costs by lowering GDP, employment, and the labor share. Importantly, we show that austerity recessions significantly amplify the political costs of economic downturns compared to non-austerity recessions.

Table 2.4.2. Response of extreme parties' vote share to austerity: Robustness

	Impact	1 Year	2 Years	3 Years	4 Years
Baseline	1.54^{***}	2.79^{***}	3.01^{***}	2.94^{***}	2.79^{***}
	(0.30)	(0.56)	(0.55)	(0.55)	(0.56)
(1) Unexpected component $g^u_{i,t}$	2.00^{***}	3.39^{***}	3.12^{***}	3.17^{***}	3.02^{***}
	(0.38)	(0.77)	(0.59)	(0.57)	(0.56)
(2) Unpredicted austerity	2.12^{***}	3.29^{***}	2.53^{***}	2.94^{***}	4.48^{***}
	(0.59)	(1.02)	(0.81)	(0.75)	(0.90)
(3) Dropping Great Recession	1.42^{***}	2.28^{***}	2.32^{***}	2.38^{***}	2.04^{***}
	(0.20)	(0.34)	(0.44)	(0.47)	(0.45)
(4) Baseline with DK std. errors	1.54^{**}	2.79^{***}	3.01^{***}	2.94^{***}	2.79^{**}
	(0.70)	(1.05)	(0.86)	(0.92)	(1.17)
(5) Including time fixed effects	1.38^{**}	2.30^{**}	2.12^{***}	2.17^{**}	2.73^{**}
	(0.61)	(0.97)	(0.82)	(1.02)	(1.24)
(6) Lagged $s_{i,t}$	1.43^{***}	2.63^{***}	2.88^{***}	2.82^{***}	2.70^{***}
	(0.27)	(0.52)	(0.52)	(0.51)	(0.53)
(7) IMF austerity shock	0.49^{***}	2.07^{***}	2.18^{***}	2.59^{***}	2.06^{***}
	(0.12)	(0.35)	(0.33)	(0.35)	(0.32)
(8) Excluding capitals regions	1.64^{***}	2.85^{***}	3.03^{***}	3.00^{***}	2.72^{***}
	(0.29)	(0.54)	(0.51)	(0.50)	(0.51)
(9) Excluding regions in top 10% of s_i	1.64^{***}	2.86^{***}	3.02^{***}	2.92^{***}	2.68^{***}
	(0.31)	(0.59)	(0.56)	(0.52)	(0.53)

Notes: For regression (1), the instrument is computed using only the unexpected consolidation shock $g^u_{i,t}$ from Equation 2.2.1. Estimation (2) takes into account possible anticipation effects by using as the instrument the residuals from regressing the austerity shock on a set of macroeconomic variables, including two lags of output and consumption growth, debt-to-GDP ratio, and real short- and long-term interest rates. Regression (3) drops observations since 2008, regression (4) presents Driscoll and Kraay standard errors, and regression (5) adds time fixed-effects. In regression (6), lagged $s_{i,t}$ is used in the instrument construction instead of s_i. Regression (7) uses IMF narrative-identified austerity shocks instead of the baseline shocks. In regressions (8) and (9), the sample excludes regions with the capital cities and the regions with the largest shares s_i (top 10%). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4.3. Response of total far vote share: state dependencies

	Total far vote share				
	Impact	1 Year	2 Years	3 Years	4 Years
Baseline	1.54***	2.79***	3.01***	2.94***	2.79***
	(0.30)	(0.56)	(0.55)	(0.55)	(0.56)
# Obs	3880	3880	3768	3692	3568
Panel A: recessions vs expansions					
Recessions	1.67***	3.03***	3.81***	3.95***	4.08***
	(0.52)	(0.81)	(0.84)	(0.85)	(1.34)
Expansions	1.57***	2.81***	2.62***	2.35***	2.01***
	(0.31)	(0.60)	(0.61)	(0.63)	(0.58)
HAC test	0.84	0.75	0.13	0.05	0.14
AR test	0.83	0.75	0.12	0.05	0.12
Panel B: urban vs rural					
Rural	1.58***	2.74***	3.06***	3.03***	2.90***
	(0.37)	(0.71)	(0.69)	(0.69)	(0.74)
Urban	1.27***	2.43***	2.50***	2.51***	2.19***
	(0.40)	(0.74)	(0.72)	(0.74)	(0.75)
HAC test	0.57	0.75	0.57	0.60	0.50
AR test	0.82	0.85	0.63	0.73	0.83
Panel C: poor vs rich					
Poor	1.55***	2.83***	3.06***	3.11***	2.83***
	(0.37)	(0.72)	(0.72)	(0.70)	(0.71)
Rich	1.52***	2.72***	2.95***	2.74***	2.68***
	(0.46)	(0.81)	(0.81)	(0.80)	(0.87)
HAC test	0.96	0.92	0.92	0.72	0.89
AR test	0.96	0.92	0.90	0.73	0.89

Notes: In panel A, recession (expansion) is the state when the growth rate of per capita output is negative (positive). In panel B, observations are classified as urban if the (lagged) population density is above the country's median for that year. Otherwise, the observations are in the rural state. In a similar fashion, for a given year, regions are labeled as poor (rich) when their per capita output is below (above) the country's median. The AR test presents the p-value of the difference between states using the Anderson and Rubin (1949) test, while the HAC test indicates the HAC-robust p-values of the difference between states. Clustered standard errors are presented between brackets. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix 2.A Data Appendix

Table 2.A.1. NUTS structure in final sample

NUTS 0	NUTS 1	#	NUTS 2	#
Austria	Groups of states	3	States (Länder)	9
Finland	Mainland	1	Large areas (Suuralueet / Storområden)	4
France	ZEAT	13	Regions	22
Germany	States (Länder)	16	Government regions (Regierungbezirke)	38
Italy	Groups of regions	5	Regions (Trentino-Alto Adige split in 2)	21
Portugal	Mainland	1	Coordination regions	5
Spain	Groups of communities	7	Autonomous communities	17
Sweden	Lands (Landsdelar)	3	National Areas (Riksområden)	8
Total		44		124

2.A.1 ARDECO - Regional European Data

ARDECO is the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy and is maintained and updated by the Joint Research Centre. It is a highly disaggregated dataset across both sectoral and sub-regional dimensions. The database contains a set of long time-series indicators for EU regions at various statistical scales (NUTS 0, 1, 2, and 3 level) using the NUTS 2016 regional classification. The dataset includes data on demography, labor markets, capital formation and domestic product by six sectors. The six sectors are (1) agriculture, forestry and fishing, (2) industry excluding construction, (3) construction, (4) wholesale, retail, transport, accommodation, and food services, information and communication, (5) financial and business services, and (6) non-market services.

ARDECO data is an annual unbalanced panel covering the period of 1980–2018 for the European Union (EU) and some European Free Trade Association (EFTA) and candidate countries. Its main data source is Eurostat (the Statistical Office of the European Commission), supplemented, where necessary, by other appropriate

Table 2.A.2. Variables Description

Variable Name	Computation	Definition [Source]
Far-left/far-right votes	Sum of all votes cast to far-left and far-right parties	Massetti and Schakel (2015), Funke, Schularick, and Trebesch (2016), and Algan et al. (2017) and their sources
GDP*pc*	GDP / population	Regional gross domestic product per capita [ARDECO]
Gov. Spending *pc*	non-market GVA / population	Regional gross value added of the non-market sector per capita [ARDECO]
Employment		Total employment [ARDECO]
Investment *pc*	private gross fixed capital formation / population	Total private (all sectors excluding non-market) Investment per capita (fixed gross capital formation) [ARDECO]
Hourly Wage	compensation of employees / total hours worked	Regional average compensation per hour (all sectors) [ARDECO]
Labor Share	private compensation / private GVA	Private (all sectors excluding non-market) compensation as a share of private GVA [ARDECO]
Motor Vehicles	# motor vehicles / population	Stock of all motor vehicles (except trailers and motorcycles) per capita [Eurostat]
Trust	Index between 0 and 1 based on micro data from the European Social Surveys (ESS).	Trust in country's parliament (Algan et al., 2017)

national and international sources. ARDECO is constructed in such a way that the country aggregates its various time series equal to the corresponding time series in the AMECO dataset referring to the National Accounts. Starting in 2002, Eurozone countries have published national series in EUR. National currency data for all years prior to the switch of the country to EUR have been converted using the irrevocably fixed EUR conversion rate. Cross-country comparisons and aggregations should continue to be based only on historical series established in ECU up to 1998 and their statistical continuation in EUR from 1999 onward. Exchange rates and purchasing power parities have been converted in the same manner. We thus use the series with real variables expressed in 2015 constant price in ECU/EUR.

Appendix 2.B Coding of Elections and their variables

Figure 2.B.1 provides a chronology of elections from 1975–2015 by country. Altogether, we identify more than 200 elections, and the final sample of coded elections includes more than 2,000 election-region observations. We include all general elections to the European parliament (eu), to the national parliament (nat), and also regional elections (reg). The latter might happen in different years for different re-

gions in Spain, Italy, and Germany. For national parliament elections, in the case of a bicameral legislative, we only consider results from the lower legislative chamber. This means that we focus on the following national elections: Austria: National Council (lower house); Germany: Bundestag (unicameral); Spain: Congress of Deputies (lower house); Finland: Eduskunta (unicameral); France: National Assembly (lower house); Italy: Chamber of Deputies (lower house); Portugal: Assembly of the Republic (unicameral); Sweden: Riksdag (unicameral). Data sources for Austria, France, Italy, Spain, and Sweden are Schakel (2013) and Schakel (2021) and his project on Regional Elections. For the other countries we relied on national sources: Finland (Statistics Finland), Germany (Federal Returning Officers), and Portugal (Pordata).

Country	Election	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994
Finland	eu																				
Finland	nat									2				2				2			
Finland	reg		2				2				2				2				2		
Portugal	eu													2		2					2
Portugal	nat		2			2	2			2		2		2				2			
Portugal	reg		2			2			2			2				2				2	
France	eu					1					1					1					1
France	nat				1			1					1		1					1	
France	reg												1						1		
Austria	eu																				
Austria	nat					1				1			1				1				1
Austria	reg			1					1					1				1			
Germany	eu					2					2					2					2
Germany	nat						2			2				2			2				2
Germany	reg	2	2		2	2	2		2	2	2		2	2	2		2	2	2		2
Italy	eu					1					1					1					1
Italy	nat		1			1				1				1					1		1
Italy	reg	1	1		1	1	1	1		1	1	1	1		1	1	1	1		1	1
Spain	eu													1		1					1
Spain	nat					1			1				1			1				1	
Spain	reg					1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Sweden	eu																				
Sweden	nat					1			1			1			1			1			1
Sweden	reg					1			1			1			1			1			1

Country	Election	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Finland	eu		2			2					2					2					2	
Finland	nat	2				2				2				2				2				2
Finland	reg		2				2				2				2				2			
Portugal	eu					2					2					2					2	
Portugal	nat	2				2			2			2				2		2				2
Portugal	reg			2				2				2				2				2		
France	eu					1					1					1					1	
France	nat			1					1					1					1			
France	reg				1	1					1						1					1
Austria	eu		1			1					1					1					1	
Austria	nat	1				1			1				1		1					1		
Austria	reg		1				1					1					1					1
Germany	eu					2					2					2					2	
Germany	nat				2				2			2				2				2		
Germany	reg	2	2		2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
Italy	eu					1					1					1					1	
Italy	nat		1					1					1		1					1		
Italy	reg	1	1		1	1	1	1		1	1	1	1		1	1	1	1	1	1	1	1
Spain	eu					1					1					1					1	
Spain	nat		1				1				1				1			1				1
Spain	reg	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1			1
Sweden	eu	1				1					1					1					1	
Sweden	nat				1				1				1				1				1	
Sweden	reg				1				1				1				1				1	

Figure 2.B.1. Elections' data table. The table provides a chronology of elections from 1975–2015 by country. We include all general elections to the European parliament (eu) and to the national parliament (nat), as well as regional elections (reg). For national parliament elections, in the case of a bicameral legislative, we only consider results from the lower legislative chamber. This means that we focus on the following national elections: Austria: National Council (lower house); Germany: Bundestag (unicameral); Spain: Congress of Deputies (lower house); Finland: Eduskunta (unicameral); France: National Assembly (lower house); Italy: Chamber of Deputies (lower house); Portugal: Assembly of the Republic (unicameral); Sweden: Riksdag (unicameral). Data sources for Austria, France, Italy, Spain, and Sweden are Schakel (2013, 2021) and his project on Regional Elections [1]. For the other countries (Finland, Germany, and Portugal), we relied on national sources [2].

2.B.1 Coding of far-right and far-left parties

Table 2.B.1 shows our full list of far-left and far-right parties in the period from 1980 to 2015. We mainly follow the classification in Funke, Schularick, and Trebesch (2016) and Algan et al. (2017) and draw on their own sources such as Ignazi (1992), Ignazi (2003), March (2012), Minkenberg (2011), Mudde (2002), Mudde (2005), and Mudde (2016), Döring and Manow (2016), Bernhard and Kriesi (2019) as well as country reports by Stiftung (2010) and a large number of country-specific sources. We further supplement their classification by evaluating political parties that only contest in regional elections by Massetti and Schakel (2015). Moreover, we relied on

specific case studies to determine whether specific regionalist parties were perceived as far-winged or not, as the case of Galician Nationalist Bloc (Cachafeiro, 2009).

Table 2.B.1. List of far right (R) and far left (L) parties since 1980 by country

Country	Party	Party name (Code)
Austria	R	Alliance for the Future of Austria (BZO); Freedom Party of Austria (FPO, FPS, FPK); National Democratic Party (NDP); A Heart for Natives (Herz)
	L	Communists and Left Socialists (KB); Communist Party of Austria (KPO); Socialist Left Party (SLP); Radical Socialist Worker's Party (RSA); Marxist–Leninist Party (MLÖ); Left (LINKE)
Finland	R	Finns Party (PS); Finish Rural Party (PS); Finnish People's Blue-whites (SKS)
	L	Communist Worker's Party (KTP); Communist Party of Finland (SKP); Finnish People's Democratic League (VAS); Left Alliance (VAS)
France	R	Movement for France (MPF); National Front (FN); National Republican Movement (MNR); France Arise (DLF); Republic Arise (DLR); Alsace d'Abord (ADA); Right Radicals (RD); League of the South (LDS); Republican People's Union (UPR); Nationalist League (LIN); Anti-replacement List (AP); Party of New Forces (PFN); French Party (PDF); Extreme Right (EXD); Right Union (UDN)
	L	French Communist Party (PCF); Left Front (PG); Revolutionary Communist League (LCR); Worker's Struggle (LO); Worker's Party (MPPT); Independent Worker's Party (POI); New Anticapitalist Party (NPA); Communists (COM); Extreme Left (EXG); Union Democratic of Bretagne (UDB); Abertzaleen Batasuna (AB); Corsica Libera (CL)
Germany	R	Alternative for Germany (AfD); Freedom - Civil Rights Movement Solidarity (BFBDO); Law and Order Offensive (Schill); National Democratic Party of Germany (NPD); STATT Party; Pro Germany Citizens' Movement (ProD); The Republicans (REP); Patriots for Germany (Patrioten); German People's Union (DVU); The Right (DR); German Social Union (DSU); Bayernpartei (BP)
	L	The Left (LINKE); Party of Democratic Socialism (PDS); Communist Party of Germany (KPD); Marxist-Leninist Party of Germany (MLPD); League of West German Communists (BWK); German Communist Party; Socialist Equality Party (SGP); Spartacist Workers' Party (SpAD)
Italy	R	Brothers of Italy (FDICN); Casa Pound (CAPI); Italian Social Movement (MSIDN); National Alliance (ANA); New Force (FNU); No Euro (NEUR); Northern League (LN); Lombarda League (LLO); Veneta League (LVE);Piemont Autonomia Regionale (PIEAR); Social Alternative(ASM); The Freedomites (DF); The Right(LDES); Tricolour Flame (FT); Fronte Nazionale; Alternativa Sociale; Movimento Idea Sociale; Io Amo l'Italia; Io Sud; Wahlverband des Heimatbundes; Südtiroler Heimatbund; Freiheitliche Partei Südtirols; Union für Südtirol; Süd-Tiroler Freiheit; Valli Unite; L'Alto Adige nel Cuore; SOS Italia; Lega Padana Lombardia; Autonomie per l'Europa; Lega Padana; Destre Unite; Lega d'Azione Meridionale; Noi con Salvini; Lega Sardegna; Lega Sarda; Nello Musumeci Presidente; Sovranita
	L	Civil Revolution (RC); Communist Refoundation Party (PRC); Critical Left (SINC); Communist Worker's Party (PCDL); Party of Italian Communists (PDCI); Party of Proletarian Unity for Communism (PDUP); Five Star Movement (M5S); Anticapitalist Left (SA); Un'Altra Regione; La Sinistra della Libertà; L'Altra Europa con Tsipras; Nuova Sinistra; Democrazia Proletaria; Lega Socialista Rivoluzionaria; Lega Comunista Rivoluzionaria; Sardegna Natzione; Alleanza Lombarda Autonomia; L'Altra Europa con Tsipras; La Sinistra-L'Arcobaleno; Independentia Repubrica de Sardigna; Sinistra Ecologia Libertà; Partito di Alternativa Comunista

Notes: This classification is combines the classification from Massetti and Schakel (2015), Funke, Schularick, and Trebesch (2016), and Algan et al. (2017) and their sources.

Table 2.B.2. List of far right (R) and far left (L) parties since 1980 for Italy and Spain

Country	Party	Party name (Code)
Portugal	R	National Renovator Party (PNR); People's Monarchist Party (PPM); New Democracy Party (PND); Christian Democratic Party (PDC,PPV,CDC)
	L	Democratic Unitarian Coalition (CDU); Left Bloc (BE); Left Revolutionary Front (FER); People's Democratic Union (UDP); People's Socialist Front (FSP); Portuguese Communist Party (PCP); Portuguese Workers' Communist Party (PCTP); Revolutionary Socialist Party (PSR); United People Alliance (APU); Re-Organized Movement of the Party of the Proletariat (MRPP); Workers Party of Socialist Unity (PT, POUS, MUT); Socialist Alternative Movement (MAS); Portuguese Labour Party (PTP); Movement of the Party of the Proletariat (MRPP); Internationalist Communist League (LCI); Movement of Socialist Left (MES); Marxist–Leninist Communist Organization (OCMLP); Revolutionary Labor Party (PRT); Left-wing Union for the Socialist Democracy (UEDS)
Spain	R	Basque Nationalists (EAJ-PNV); Falange Española (FE); Vox; Fuerza Nueva; Democracia Ourensana; Democracia Nacional; Partido Familia y Vida; Identidad Española; Partido Nacionalista Cantabro; Partido Democrata Español; Plataforma per Catalunya; Movimiento Social Republicano; Grupo Independiente Liberal; Alternative Española; España 2000; Plataforma España 2000; Coalicio Valenciana; Unió Valencia; Partido Nacional de los Trabajadores; Frente Nacional-MSR; Juntas Españolas; Movimiento Catolico Español; Estado Nacional Europeo; Partido Union Nacional; Solidaridad Española

Notes: This classification combines the classification from Massetti and Schakel (2015), Funke, Schularick, and Trebesch (2016), and Algan et al. (2017) and their sources.

Table 2.B.3. List of far right (R) and far left (L) parties since 1980 for Spain

Country	Party	Party name (Code)
Spain	L	Communist Party of Spain (PCE); Communist Party of Spain (Marxist–Leninist) (PCEML); Unified Socialist Party of Catalonia (PSUC-PCE); United Left (IU); Podemos (PODEMOS); Galician Nationalistic Bloc (BNG); Workers' Party of Marxist Unification (POUM); Esquerda Galega; Partido Socialista Galego; Izquierda de los Pueblos; En Marea; Frente Popular Galego; Liga Comunista Revolucionaria; P. Comunis de Galicia Mar-Rev; Partido Socialista de los Trabajadores; Movimiento Comunista; Assembleia Do Povo Unido; Coalición por un nuevo Partido Socialista; Nós-Unidade Popular; Partido Socialista; Partido Comunista Obrero Español; Unificacion Comunista De España; Accion Republicana; Mesa Para La Unidad De Los Comunistas; Partido Comunista de los pueblos de España; Euskal Komunistak; Partido de los Trabajadores de Espana-Unidad Comunista; Nación Andaluza; Izquierda Andaluza; Recortes Cero; Adelante Andalucia; Partido Comunista Aragonés; Unidad Popular Republicana; Coalición Lucha Popular; Coalición Unión Pueblo Canario; Frente Popular De Canarias; Awañac; Más Madrid; Congreso Nacional de Canarias; Izquierda Nacionalista Canaria; Iniciativa Canaria; Coalición Canaria por la Independencia; Agrupación Electoral Izquierda Cantabria Unida; Partido Obrero Socialista Internacionalista; Izquierda Castellana; Alternativa Socialista; Coalició d'Esquerra d'Alliberament Nacional-Unitat Popular; Nacionalistes d'Esquerra; Partit Comunista Obrer de Catalunya; Coalición Unidad Comunista; Unitat Popular Socialisme; Partit Socialista Unificat de Catalunya; Candidatura d'Unitat Popular Alternativa d'Esquerres; Partido de los Obreros Revolucionarios de Espana; Partit dels Comunistes de Catalunya; Iniciativa Per Catalunya Verds; Lucha Internacionalista; Catalunya Sí que es Pot; Partido Socialista del Pueblo de Ceuta; Liga Comunista; Plataforma de Izquierdas; Agrupación Electores AUZOLAN; Euskadiko Ezkerra; Herri Batasuna; Partido Carlista; Amaiur; Union Navarra De Izquierda; Batzarre; Euskal Herritarrok; Aralar; Nafarroa Bai; Euskal Herria Bildu; Geroa Bai; Esquerra Nacionalista Valenciana; Bloque Popular Extremadura; Partit Socialista de Menorca; Partit Socialista de Mallorca; Entesa de l'Esquerra de Menorca; PSM-Nacionalistes de les Illes; Més per Menorca; Ensame Nacionalista Astur; Eusko Alkartasuna; Partido Comunista de las Tierras Vascas; Anticapitalistas; Partido Obrero Revolucionario; Organizacion Revolucionaria De Los Trabajadores; Partido de los Trabajadores de Euskadi; Movimiento Comunista; Partit Revolucionari dels Treballadors; Partido del Trabajo de España; Unidá Nacionalista Asturiana; Candidatura De Unidad Comunista; Los Pueblos Deciden; Mesa Para La Unidad De Los Comunistas; Izquierda Anticapitalista Revolucionaria
Sweden	R	New Democracy (NYD); National Democrats (ND); Sweden Democrats (SD,SVD); National Socialist Front (NSF); Progress Party (FRA,FRP); Party of the Swedes (SVP) Scania Party (SKAP,SP); Nordic Resistance Movement (NMR); European Worker's Party (EAP)
	L	Communist Party of Sweden (SKP); Communist League Marxists-Leninists (KFML); Communist League Marxist–Leninists (KPMLR); Workers' Party – The Communists (APK) Communists (KOM); National Communist Party (NKP); Socialist Justice Party (RS) The Left Party (V); Socialist Party (SOP, SOC)

Notes: This classification combines the classification from Massetti and Schakel (2015), Funke, Schularick, and Trebesch (2016), and Algan et al. (2017) and their sources.

Appendix 2.C Results Appendix

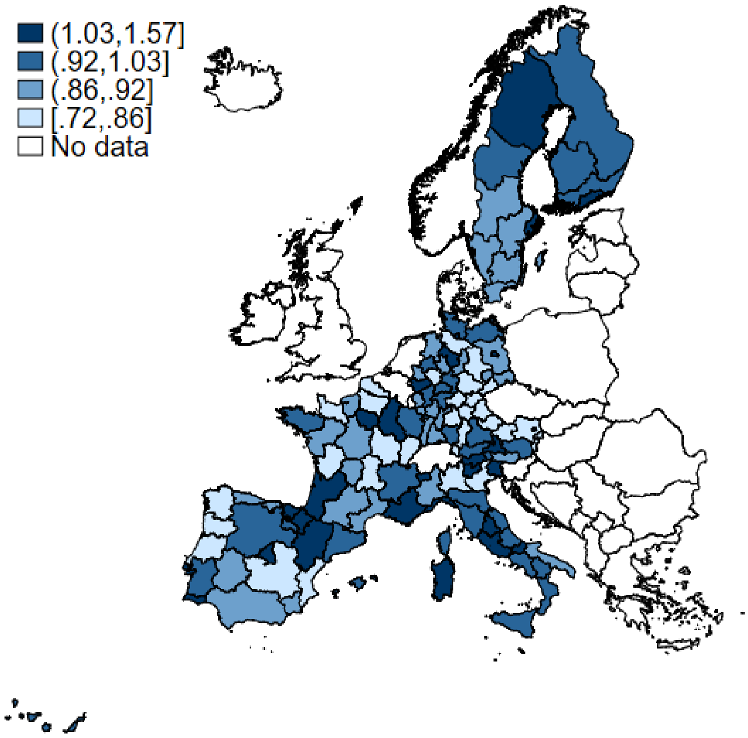


Figure 2.C.1. Sample regions and the share s_i. The figure depicts the map of European NUTS 2 regions with the share s_i used in Bartik instrument construction.

Table 2.C.1. Response of far vote share: Robustness dropping one country at the time

	Impact	1 Year	2 Years	3 Years	4 Years
Baseline	1.54***	2.79***	3.01***	2.94***	2.79***
	(0.30)	(0.56)	(0.55)	(0.55)	(0.56)
Austria	1.66***	3.02***	3.26***	3.21***	3.12***
	(0.32)	(0.59)	(0.57)	(0.57)	(0.56)
Finland	1.57***	2.84***	3.08***	3.07***	3.03***
	(0.30)	(0.56)	(0.55)	(0.54)	(0.54)
France	1.27***	2.36***	2.53***	2.34***	2.50***
	(0.28)	(0.52)	(0.48)	(0.47)	(0.55)
Germany	1.63***	2.81***	3.24***	3.23***	3.05***
	(0.33)	(0.61)	(0.62)	(0.62)	(0.63)
Italy	0.45***	0.63***	0.81***	1.14***	0.92**
	(0.16)	(0.24)	(0.25)	(0.30)	(0.47)
Portugal	1.96***	3.53***	3.67***	3.66***	3.13***
	(0.35)	(0.66)	(0.64)	(0.63)	(0.63)
Spain	2.39***	4.49***	4.59***	4.22***	4.07***
	(0.54)	(0.98)	(0.94)	(1.00)	(0.97)
Sweden	1.47***	2.70***	2.86***	2.80***	2.58***
	(0.30)	(0.56)	(0.54)	(0.53)	(0.55)
Italy and Spain	0.76*	1.25**	0.96	0.92	0.36
	(0.42)	(0.59)	(0.61)	(0.67)	(1.44)

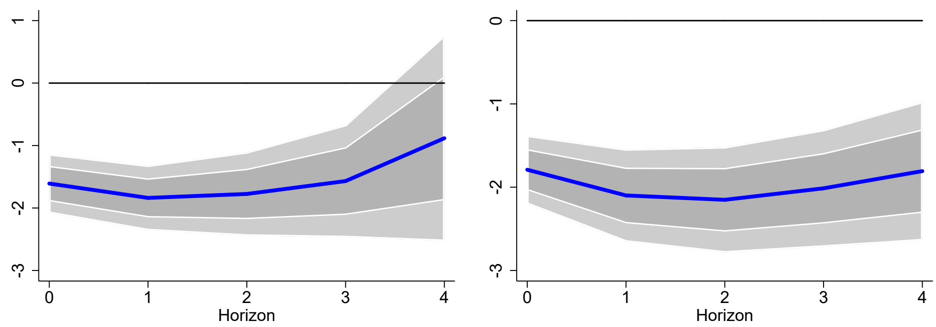
Notes: This table shows the response of extreme vote share to an austerity-induced fiscal spending shock using the baseline specification but excluding individual countries iteratively from the base sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix 2.D Output and employment multipliers

In estimating output and employment government spending multipliers, we follow Gabriel, Klein, and Pessoa (2021) but use the identification strategy from the baseline analysis described in Section 2.3. We use local projections (Jordà, 2005) and estimate for each horizon $h = 0, ..., 4$, the following equation:

$$\sum_{m=0}^{h} z_{i,t+m} = \beta_h \sum_{m=0}^{h} \frac{G_{i,t+m} - G_{i,t-1}}{Y_{i,t-1}} + \gamma_h(L)X_{i,t-k} + \alpha_{i,h} + \varepsilon_{i,t+m}, \qquad (2.D.1)$$

where $z_{i,t+m}$ is either the change in real per capita GDP, $\frac{Y_{i,t+m}-Y_{i,t-1}}{Y_{i,t-1}}$, or the change in the employment rate, $\frac{E_{i,t+m}-E_{i,t-1}}{E_{i,t-1}}$, in region i between time $t-1$ and time $t+m$. $(L)X_{i,t-k}$ is a vector of control variables with $k = 2$, including lags of the dependent variable and of GDP and government spending growth, and $\alpha_{i,h}$ are region fixed-effects. Figure 2.D.1 depicts the cumulative GDP and employment multipliers, where the solid lines show the point estimate β_h over a horizon of four years and the dark and light shadings are 68% and 90% confidence bands, respectively. Standard errors are clustered at the regional level.



(a) Output multiplier

(b) Employment multiplier

Figure 2.D.1. Output and employment multipliers. Panels 2.D.1a and 2.D.1b show the cumulative relative fiscal and employment multipliers estimated according to Equation (2.D.1). Shaded areas are 68% (dark) and 90% (light) confidence intervals.

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Chapter 3

Monetary Policy and the Wage Inflation-Unemployment Tradeoff

The relationship between the slack in the economy or unemployment and inflation was a strong one 50 years ago ... and has gone away. (...) At the end of the day, there has to be a connection because low unemployment will drive wages up (Powell, 2019).

3.1 Introduction

The wage inflation-unemployment tradeoff claims that changes in monetary policy push wage inflation and unemployment in opposite directions (Mankiw, 2001). Such relation is traditionally thought of in the form of a Phillips curve and is at the

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core of monetary policy (Barnichon and Mesters, 2020; Eser, Karadi, Lane, Moretti, and Osbat, 2020). Over the last years, many have questioned the importance of the Phillips curve, arguing that it had flattened out of favor. A flatter Phillips curve suggests that economic activity has a smaller effect on inflation. Under this scenario, central bankers' ability to steer inflation with policy-induced changes becomes weaker. Nevertheless, is this weaker wage inflation-unemployment tradeoff unique to the last two decades? Does the strength of the tradeoff vary over time and differ across states of the economy?

In this study, I revisit the historical relationship between wage inflation and unemployment, which is the focus of Phillips' (1958) original work, to answer these two questions. My analysis proceeds in four steps. First, I assemble annual historical data on nominal wages and unemployment rates since 1870 for 18 advanced economies. Second, I uncover considerable variation in the wage Phillips curve slope over time and find that its recent flattening is not a unique feature of the last 150 years. Third, based on carefully identified monetary policy shocks, I show that monetary interventions have large and significant effects on wage inflation and unemployment rates. Finally, I show that changes in the price inflation environment possibly shape the wage inflation-unemployment tradeoff. The data suggest that the tradeoff is weaker in times of low price inflation, which is consistent with the New Keynesian model's predictions (Benati, 2007).

I start by reporting time-varying estimates of a micro-founded panel wage Phillips curve, in the spirit of Galí (2011). I provide evidence that the wage Phillips curve has always been "alive and well". Interestingly, similar to the last two decades, it was flatter during the Gold Standard. This novel finding suggests that the recent weakening of the wage inflation-unemployment tradeoff is not a unique feature of the last 20 years. Thus, it is essential to use historical data to better understand what shapes the relationship between these two macroeconomic variables. Furthermore, I find that there is a correlation between periods characterized by a low price inflation and a flatter slope.

These results carry on in a setting without the straitjacket of any assumed functional relation between wage inflation and unemployment. To be precise, I estimate a Phillips multiplier in the spirit of Barnichon and Mesters (2020), which is related to the impulse response-based statistic presented in Galí and Gambetti (2020). The main idea is to trace the evolution over time of the dynamic wage inflation-unemployment multiplier by comparing their impulse response functions to a monetary policy shock. While on impact the multiplier is undetermined, at longer horizons the statistic becomes negative and statistically significant. Such a large negative tradeoff implies that a transitory policy-induced change in unemployment has a persistent effect on wage inflation and therefore, that central banks have sufficient ability to steer inflation with conventional monetary policy tools.

Finally, I test the hypotheses that the tradeoff is different for two different subsamples and that low price inflation weakens the wage inflation-unemployment tradeoff using a state-dependent local projection instrumental variable approach. Both results support the hypothesis that, at longer horizons, the tradeoff is smaller during periods of low price inflation. Thus, reinforcing the idea that policymakers' ability to explore this tradeoff is impaired in a low inflation environment.

By revisiting the historical relationship between wage inflation and unemployment, this paper aims at contributing to three strands of literature. First, this study adds to the classical literature of the Phillips curve (Phillips, 1958). Using long-run data for a panel of 18 countries, I expand the findings of a Phillips curve which is “alive and well" documented not only in the US (Coibion and Gorodnichenko, 2015; Blanchard, Erceg, and Lindé, 2016; Del Negro, Lenza, Primiceri, and Tambalotti, 2020; Höynck, 2020; Ascari and Haber, 2021; Hazell, Herreño, Nakamura, and Steinsson, 2021), but also in Europe (Levy, 2019; Onorante, Saber, and Moretti, 2019; Bonam, Haan, and Van Limbergen, 2021) and even worldwide (Coibion, Gorodnichenko, and Ulate, 2019).[1]

In the current empirical literature, there is a large amount of sampling uncertainty with different researchers using different data vintages to compute Phillips curves (Mavroeidis, Plagborg-Møller, and Stock, 2014). This work introduces two newly assembled historical data series on unemployment rates and wages for a set of 18 countries and a clean identification strategy in the hope of taking one step further to an empirical consensus. The use of such a long-run panel is of utmost importance because it allows uncovering the time-varying nature of the tradeoff and whether the inflation environment is indeed a historical driver of the wage inflation-unemployment tradeoff. Moreover, it also allows exploring more variation in wage inflation, thereby reducing the results' sensitivity to the data vintage that arises when using, for example, only one country and recent data. To the best of my knowledge, this is the first paper to bring such a historical perspective to the debate on the wage inflation-unemployment tradeoff. Such an approach keeps up with the recent trend of using long-run and cross-country perspectives to inform central debates in monetary and financial policy as in Reinhart and Rogoff (2009) and Schularick and Taylor (2012).

This work also contributes to the literature about the effects of monetary policy using long run panel data (Jordà, Schularick, and Taylor, 2019; Alpanda, Granziera, and Zubairy, 2021). By using the trilemma instrumental variable (IV) to identify the effect of monetary policy, I build not only on the seminal work of Di Giovanni, McCrary, and Von Wachter (2009) but also on recent studies by Jordà, Schularick, and Taylor (2019) and Schularick, Ter Steege, and Ward (2021). Moreover, this paper applies the Phillips multiplier statistic which was first presented in the study

1. A good summary of the literature since the inception of the Phillips curve can be found in Gordon (2011), while more recent discussions can be found in Mavroeidis, Plagborg-Møller, and Stock (2014) and Coibion, Gorodnichenko, and Kamdar (2018).

of Barnichon and Mesters (2020) and applied by Eser et al. (2020), who estimated it respectively for the US and the UK, and the Eurozone. This paper's novelty lies in applying the state-of-art methodology to a historical setting with long run data series that allows testing the response of wage inflation and unemployment rates to a monetary policy surprise, and whether these responses are state-dependent.

Finally, this paper's empirical findings resonate with recent theoretical developments that link the wage inflation-unemployment tradeoff to the level of price inflation. According to the New Keynesian model, an increase (decrease) in trend inflation should cause an increase (decrease) in the frequency of price adjustment, leading to a decrease (increase) in the steepness of the wage Phillips curve (Benati, 2007). This rationale that low price inflation weakens the wage inflation-unemployment tradeoff is consistent with two other strands of the literature, namely the state-dependent pricing (Alvarez, Beraja, Gonzalez-Rozada, and Neumeyer, 2019; Costain, Nakov, and Petit, 2021) and the nominal price rigidities literatures (Tobin, 1972; Benigno and Ricci, 2011; Daly and Hobijn, 2014).

Since Ball, Mankiw, Romer, Akerlof, Rose, et al. (1988), the empirical literature has not paid enough attention to this low price inflation mechanism. Some notable exceptions are Benati (2007), who documented a positive correlation between the time-varying average gain of real activity and inflation, Vavra (2014), who rejected a New Keynesian Phillips curve with constant inflation output tradeoff in favor of a slope that increases with microeconomic volatility, Gertler and Hofmann (2018), who found a weak money-inflation link in regimes characterized by low inflation, and Ascari and Haber (2021) who provide evidence supporting non-linear effects in the response of the price level depending on the trend inflation regime, though using only aggregate US data. I complement these findings by showing a negative and strong historical correlation between a time-varying Phillips curve and price inflation, and also by estimating a weaker wage inflation-unemployment tradeoff in times of low price inflation due to a weaker response of wage inflation to monetary policy.

The remainder of this paper is structured as follows. Section 3.2 introduces the data and presents the descriptive statistics. Section 3.3 describes the empirical strategy. The results are presented in Section 3.4, and Section 3.5 concludes.

3.2 Data and Descriptive Statistics

3.2.1 Data

I construct a new historical dataset composed of wage inflation and unemployment rates series that go as far as the nineteenth century in order to uncover the historical tradeoff between wage inflation and unemployment. The newly assembled yearly data include a wage index measure and the unemployment rate for 18 advanced economies — Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States. The sample spans from 1870 to 2020 and draws on more than 60 different sources.[2] Before the Bretton Woods epoch, available data is mostly at an annual frequency for both variables, so using panel data to study the wage inflation-unemployment tradeoff is of paramount importance. With the exception of wage inflation and unemployment, the macroeconomic data series used in this paper, such as price inflation, come from the Macrohistory Database (Jordà, Schularick, and Taylor, 2017).

When possible, the *unemployment rate* is defined as the percentage of unemployed in the total labor force. According to Rasmussen and Pontusson (2018), most countries had no unemployment insurance system until after the World Wars. Hence, citizens without a job had little incentive to enroll in a labor bureau since there was no compulsory unemployment insurance.

The earlier data, which comes mainly from Mitchell (2013), Tabin and Togni (2013), Maddison (1982), and Galenson and Zellner (1957), build upon the previous caveat and present unemployment rates within smaller subsets of the active population such as trade unions or within people insured against unemployment. The underlying assumption is that the unemployment growth rates within smaller subsets of the active population are the same (or at least, highly correlated) as the national unemployment growth rate.

The most recent data follows the preferred definition and is based on either the Current Population Survey or the EU Labour force survey from the International Labour Organization (ILOSTAT). As a complement, data from the National Statistics agencies ensure the robustness of the series.

When possible, the *wage* series are an index of the average earnings of all employees. However, the earlier data may build upon series of specific sectors according to their availability. I construct this nominal index using old publications of statistical offices, financial history books, and articles. The most recent data is based on the International Monetary Fund (IMF) wage index series and the Organization for Economic Cooperation and Development (OECD).

3.2.2 Descriptive Statistics

Table 3.2.1 lists selected summary statistics of the dataset for the entire sample and five separate periods. Both wage and price inflation series are computed as growth rates of nominal indices. The average wage inflation rate for the entire sample is 5.05%, almost two percentage points above the average price inflation. On average, the unemployment rate throughout the sample is 5.65%.

2. Table A.1 in the online Appendix summarizes the data coverage by country. All data sources and further description of their construction are provided in the online Data Appendix.

Table 3.2.1. Descriptive statistics

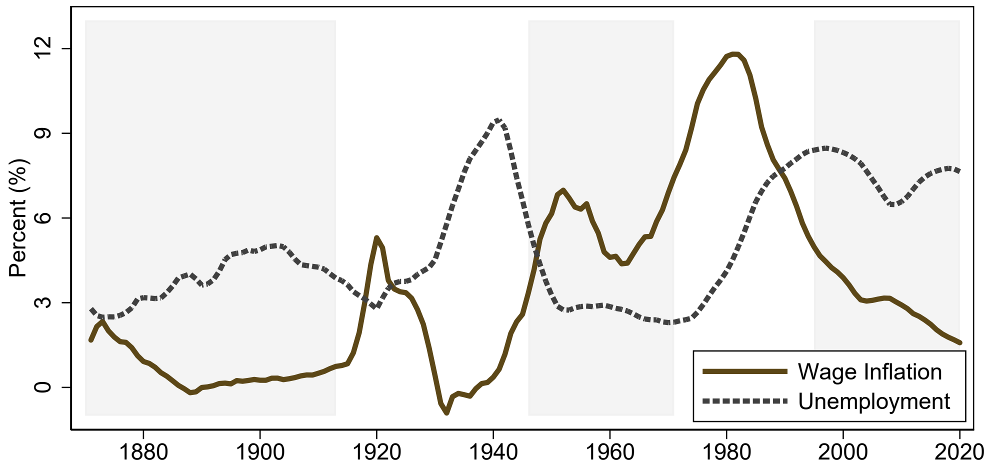
	N	Mean	Std. Dev.	Min	Max
1870-1913					
Unemployment rate	223	4.08	2.75	0.20	18.40
Wage inflation	223	1.68	2.63	-6.71	10.26
Price inflation	223	0.39	3.21	-10.94	11.56
1920-1938					
Unemployment rate	268	7.17	4.99	0.60	24.90
Wage inflation	268	1.26	8.63	-27.72	43.97
Price inflation	268	-0.29	7.23	-18.45	30.43
1946-1971					
Unemployment rate	428	2.60	1.83	0.04	9.92
Wage inflation	428	7.77	5.10	-10.78	35.29
Price inflation	428	4.08	3.76	-6.87	20.38
1972-1999					
Unemployment rate	504	7.07	4.30	0.04	24.21
Wage inflation	504	8.30	6.27	-1.42	32.28
Price inflation	504	6.56	5.51	-0.71	37.88
2000-2020					
Unemployment rate	377	7.05	3.52	2.00	26.09
Wage inflation	377	2.32	1.84	-6.14	7.50
Price inflation	377	1.63	1.28	-4.48	5.57
Total					
Unemployment rate	1800	5.65	4.12	0.04	26.09
Wage inflation	1800	5.05	6.29	-27.72	43.97
Price inflation	1800	3.16	5.28	-18.45	37.88

Notes: All statistics are expressed in percent. The war periods (1914-1919 and 1939-1945) and the German hyperinflation episode (1920-1925) are not included. This table only uses *unweighted* country-year observations for which there is data for the unemployment rate, and price and wage inflation. Table 3.A.2 presents descriptive statistics for the unrestricted sample.

In the wake of the Great Recession, it was surprising to observe how stable and low the inflation rates were (Miles, Panizza, Reis, and Ubide, 2017). In fact, to observe such a pattern, one has to go back more than 100 years when most of the studied countries were part of the Gold Standard agreement.

Moreover, although only 8 out of the 18 countries in the sample are explicit inflation targeters (Svensson, 2010), Table 3.2.1 indicates that using price inflation as the nominal anchor instead of the price of gold makes the volatility of price and wage inflation smaller albeit the higher means.[3] Hence, the inflation targeting regime successfully keeps inflation under control with the lowest volatility ever observed.

In addition, Figure 3.2.1 summarizes the data's cross-country trends by plotting a time-varying estimate of the mean wage inflation and the mean unemployment rate for the 18 countries using a 10-year rolling window. We observe stable wage inflation and unemployment series during the Gold Standard epoch, until 1913. That picture dramatically changes once we enter the war period with a large swing in the inflation series. The period from 1946 to 1971 corresponds to the Bretton Woods epoch and shows persistently low unemployment and high wage inflation rates. Then, after 1972, we can observe a peak for the inflation series, partly driven by the two oil price shocks in 1973 and 1979. This peak is followed by a general decrease in inflation and an increase in unemployment stemming from the Great Moderation period.



Notes: This figure plots a time-varying estimate of the mean wage inflation (solid line) and mean unemployment rate (dashed line) using a 10-year rolling window and the full matched sample.

Figure 3.2.1. Mean wage inflation and unemployment rate

Summing up, Figure 3.2.1 points to a strong negative co-movement between the two variables, which is also corroborated at the country level (see Table 3.A.4 in the Appendix). Nevertheless, during the Gold Standard and the last twenty years, wage inflation and unemployment series were more stable, suggesting a weaker co-movement and thus, unveiling a potentially time-varying wage inflation-unemployment tradeoff.

3. The higher means should come without surprise given that targeting the price of gold implicitly yields a zero inflation expectation, contrary to a 2% inflation target.

3.2.3 Historical Wage Phillips Curves

To give more structure to the previous exploratory analysis, I turn my attention to the wage Phillips curve across historical periods. I depart from the wage Phillips curve derived from the micro-founded New Keynesian model presented in Galí (2011) and estimate the following equation:

$$\pi^w_{c,t} \;=\; \mu_c + \varphi u_{c,t} + \gamma \pi^p_{c,t-1} + \epsilon_{c,t} \tag{3.2.1}$$

where $\pi^w_{c,t}$ denotes the annual wage inflation in country c at time t; α is a constant; $u_{c,t}$ denotes the unemployment rate in country c at time t; $\pi^p_{c,t-1}$ is the lagged price inflation, the measure by which wages are indexed; and $\epsilon_{c,t}$ is an error term proxying for time-varying cost-push shocks to wages.[4] The twist of exploring the Phillips curve using a panel approach has been recently explored by Coibion, Gorodnichenko, and Ulate (2019), Levy (2019), De Schryder, Peersman, and Wauters (2020), and Hazell et al. (2021) at both national and regional levels. Following the empirical literature, I include time-invariant country fixed effects μ_c.

Here, I implicitly assume that, when there is no reoptimization, wages are indexed to ($\pi^p_{c,t-1}$), where γ represents the degree of indexation on past price inflation.[5] Given an increase in the price level in $t-1$, workers bargain for a higher wage in t due to an increase in the cost of living in $t-1$.[6]

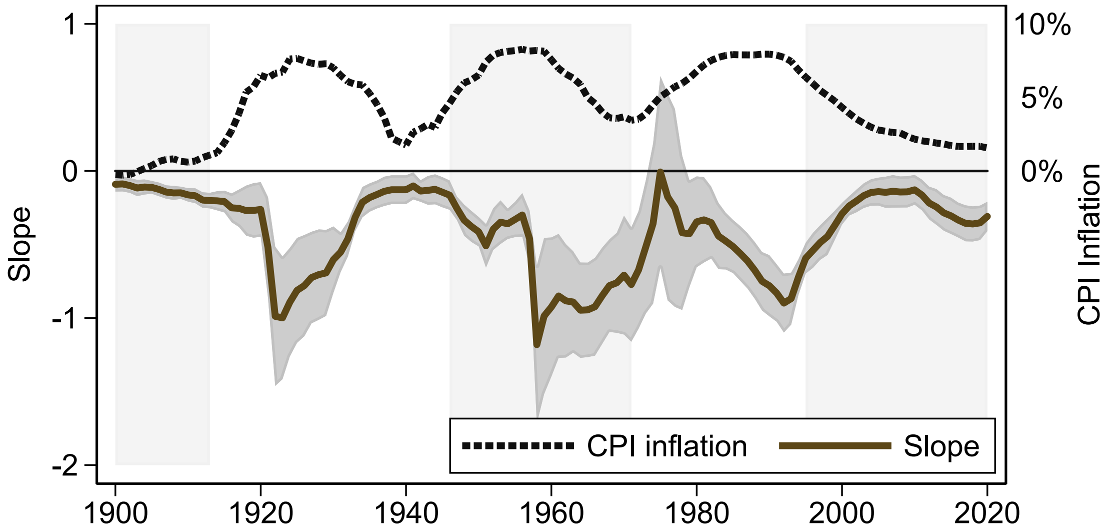
Figure 3.2.2 shows the time-varying estimates of its slope (φ) based on the Panel-OLS regression of Equation (3.2.1) using a 20-year rolling window. The estimates support the *low inflation hypothesis* which proposes that the slope of the wage Phillips curve is significantly flatter following periods of low price inflation.

There are three key features from Figure 3.2.2 which deserve to be highlighted. First, it displays the consecutive steepening and flattening of the wage Phillips curve after the end of the Bretton Woods agreement. This pattern is already well documented, especially for the US (Ball and Mazumder, 2011; Blanchard, Cerutti, and Summers, 2015; Blanchard, Erceg, and Lindé, 2016; Galí and Gambetti, 2020) and

4. The majority of the literature argues for the use of the unemployment gap instead of its level. However, that approach ignores the problem of measurement error arising from the computation of a natural unemployment rate. In my setting, due to the use of historical data, I believe that the latter poses a bigger threat because it is not possible to use detailed data to get the best estimates of the natural unemployment rate.

5. Another possible interpretation is that firms look at the previous period price inflation as a good measure of inflation expectations, which then affects their decision in changing both their products' prices and workers' wages.

6. Table A.3 in the online Appendix corroborates this idea by displaying a correlation between price inflation in $t-1$ and wage inflation in t of more than 0.5 for almost every country.



Notes: This figure plots a time-varying estimate of the slope of the wage Phillips curve (parameter φ, in Equation (3.2.1)), using OLS and annual data from 1870 to 2020 for all 18 countries. It is computed based on a rolling OLS regression using a 20-year window and displays a 90% confidence band. In Appendix, Figure 3.A.1 shows the estimate for the persistence coefficient (γ) while Figure 3.A.2 presents the same regression when adding year fixed effects.

Figure 3.2.2. Panel-OLS 20-year Rolling Window

Europe (Bonam, Haan, and Van Limbergen, 2021). However, the fact that I am using a panel of 18 advanced economies to perform this analysis might indicate that this flattening could be considered a global phenomenon.

Second, the wage Phillips curve was also flatter during the Gold Standard period and the beginning of the Bretton Woods epoch. This novel finding suggests that the recent weakening of the wage inflation-unemployment tradeoff is not a unique feature of the last 20 years.

Third, it seems that during periods of low price inflation, the slope of the wage Phillips curve becomes flatter. One potential explanation for this correlation is the *low inflation hypothesis* which will be tested in Section 3.4. During the majority of the three periods shaded in gray, inflation was strongly anchored either to the price of gold or to a composite price measure (CPI), and thus, countries experienced a persistent low price inflation environment (as we saw in Table 3.2.1). Consequently, firms adjusted prices and wages less often (Gagnon, 2009; Nakamura and Steinsson, 2018; Alvarez et al., 2019) and promoted a disconnect between wage inflation and movements in the labor market.

A major element of modern Phillips curve estimations are *inflation expectations*. Hazell et al. (2021) show that not accounting for the decline in long-run inflation expectations during the Volcker disinflation may introduce an upward bias in estimates of the slope of the United States (price) Phillips curve during that period. Taking this into account might question the use of Galí (2011) framework that may be overly restrictive on the nonexistent role of inflation expectations. Moreover, even though this work focuses only on the relationship between wage inflation and unemployment, it is still important to acknowledge that there is a strong correlation between price and wage inflation (Table 3.A.4) and therefore it might be important to have this issue into account. While the absence of historical data on inflation expectations makes it impossible to add it as a control variable, I collect OECD data on inflation forecast starting in the 1990s for the European countries and starting in the 1960s for the remaining ones to 2020 and run the same analysis for this sub-sample.

Empirically, Ciccarelli and Mojon (2010) have estimated a common factor in countries' price inflation that accounts for nearly 70% of their variance. They include 22 OECD countries in their sample - the 18 countries in my sample plus Austria, Greece, Luxembourg, and New Zealand - from 1960 to 2008. With this in mind, it seems worthwhile to include time fixed effects as a way to control not only for the dynamics of global inflation but also to control for the common component (across countries) of inflation expectations.

Figure 3.A.2 in appendix thus presents the estimates when including year fixed effects or inflation forecast from OECD. It is important to emphasize the difference in the slope estimates from 1990 to 2000 might be due to differences in the sample as most European countries only have expectations data starting in 1991. Not surprisingly, the confidence bands become wider. Notwithstanding, the three key features highlighted before are shown to be robust.

This Section thus provides sufficient and robust *motivation* to explore the time-varying tradeoff between wage inflation and unemployment in more detail while using a more appropriate econometric method.

3.3 Empirical Strategy

The literature has extensively documented the empirical challenges in estimating both the price and wage Phillips curves (Galí, 2011; Mavroeidis, Plagborg-Møller, and Stock, 2014; McLeay and Tenreyro, 2020) and, more generally, the wage inflation-unemployment tradeoff which is at the center of this work (Barnichon and Mesters, 2020; Galí and Gambetti, 2020). The main concern is the simultaneity bias arising from the correlation between the measures of economic slack and inflation with the error term. Departing from an AS-AD model framework, cost-push shocks might affect both the dependent and independent variables. These might be either shocks to input prices such as imported goods, oil and other important commodities, or input quantities such as a freeze in raw materials production or even wars which drain the labor force.

McLeay and Tenreyro (2020) made the case that the empirical disconnect between inflation and economic slack is expected to be emphasized when monetary policy is set optimally. Even absent of supply shocks, a purely inflation targeting central bank would neutralize any aggregate demand fluctuations to achieve constant inflation at its target. Hence, inducing a negative correlation between price inflation and economic slack and making it harder to uncover the true relationship between them. It is worth noting, however, that the wage inflation-unemployment tradeoff is less prone to this later criticism because many central banks do not explicitly target the unemployment rate. This observation is undeniably true for the majority of the sample in this study in which only two central banks (United States and Australia) started targeting unemployment in recent decades.

Acknowledging these issues, I use monetary policy shocks to identify the wage inflation-unemployment tradeoff in the same spirit as Jordà and Nechio (2020). To be precise, I apply the trilemma IV, strategy pioneered by Di Giovanni, McCrary, and Von Wachter (2009) and recently applied by Jordà, Schularick, and Taylor (2019) and Schularick, Ter Steege, and Ward (2021). This allows taking advantage of the fact that economies with fixed exchange rates and under perfect capital mobility are unable to implement independent monetary policies.

When a country pegs its exchange rate, its interest rate from then on has to closely follow that of the base country; otherwise, there will be unsustainable capital outflows. Moreover, since changes in the base country's interest rate are mainly determined by the base country's economic conditions, their variation is exogenous to the economic conditions in the pegged countries. Notwithstanding, in order to isolate unpredictable movements in the base country's interest rates Δr_b, I also subtract the predicted changes in the base country's interest rate $\Delta \hat{r}_b$.[7]

The trilemma IV, $z_{c,t}$, for local policy rate changes, $\Delta r_{c,t}$, can only be computed when a country's exchange rate is fixed with respect to a base country b and is thus defined as follows:

$$z_{c,t} \equiv (\Delta r_{b(c,t),t} - \Delta \hat{r}_{b(c,t),t}) \times k_{c,t} \tag{3.3.1}$$

where c and t are the country and year indices, respectively; $b(c,t)$ denotes country c's base country in year t; $\Delta r_{b(c,t),t} - \Delta \hat{r}_{b(c,t),t}$ can be interpreted as a Taylor residual of the base country $b(c,t)$; and $k_{c,t}$ is the degree of capital openness from Quinn, Schindler, and Toyoda (2011), this index ranges from 0 to 1, with 0 indicating a low degree and 1 a high degree of capital mobility. Both studies by Jordà, Schularick, and Taylor (2019) and Schularick, Ter Steege, and Ward (2021) show that the

7. To predict $\Delta \hat{r}_b$, I follow Jordà, Schularick, and Taylor (2019) and use the first lags of the growth rates of GDP, consumption, investment, stock prices, and credit (all CPI deflated), as well as changes in nominal long-term interest rates, nominal short-term interest rates, the CPI inflation rate, and the current account-to-GDP ratio.

trilemma IV is relevant due to its strong relation with changes in pegs' domestic short-term interest rates. In my sample, the instrument exhibits a statistically significant coefficient of 0.65 over the full sample (SE $= 0.08$) and for both the pre- and post-World War II periods, with the slope coefficients being approximately 0.64 (SE $= 0.15$) and 0.65 (SE$= 0.09$), respectively (see Table 3.A.5 in the Appendix for more details).

Another main challenge that persists even after correcting for endogeneity is specification uncertainty. One can think of estimating a non-parametric version of the Phillips curve without the straitjacket of any ad-hoc functional relation between inflation and economic slack (Galí and Gambetti, 2020). Inspired by the fiscal multiplier literature (Ramey and Zubairy, 2018), Barnichon and Mesters (2020) proposed estimating a Phillips multiplier defined as the expected cumulative change in inflation caused by a demand shock that affects expected cumulative unemployment. This statistic directly captures the central bank's inflation-unemployment tradeoff across different horizons, which is consistent with the definition of Mankiw (2001).

In the following section, I start by tracing the effect of a one percentage point surprise increase in policy rates on average wage inflation and average unemployment rate. To be precise, I estimate impulse response functions (IRFs) by making use of a panel local projections instrumental variable (Panel LP-IV) approach (Jordà, 2005; Stock and Watson, 2018) as follows:

$$\bar{X}_{c,t:t+h} \; = \; \alpha^X_{c,h} + \beta^X_h z_{c,t} + \zeta^X_h W_{c,t} + e^X_{c,t+h} \tag{3.3.2}$$

where $\bar{X}_{c,t:t+h} \equiv \frac{1}{h}\sum_{j=0}^{h} X_{c,t+j}$ is either the average value of wage inflation or the unemployment rate over $[t, t+h]$, $\alpha^X_{c,h}$ denotes country fixed effects, $z_{c,t}$ is the trilemma IV as introduced in Equation (3.3.1), and $W_{c,t}$ is a vector of controls including the world GDP growth and two lags of wage inflation and unemployment.[8] To remove potential extreme values, throughout the analysis I remove the war periods and observations for which yearly wage inflation is above 50%.[9]

Building on these IRFs, I estimate the Phillips multiplier as in Barnichon and Mesters (2020). The Phillips multiplier ($\mathscr{P}_h$) can be estimated using a Panel LP-IV approach from the following cumulative regression:

$$\sum_{j=0}^{h} \pi^w_{c,t+j} \; = \; \alpha_{c,h} + \mathscr{P}_h \sum_{j=0}^{h} \hat{u}_{c,t+j} + \zeta_h \boldsymbol{W}_{c,t} + \epsilon_{c,t+h} \tag{3.3.3}$$

where $\alpha_{c,h}$ denotes country fixed effects; $\boldsymbol{W}_{c,t}$ is the same vector of control variables as in Equation (3.3.2); and $\sum_{j=0}^{h} \hat{u}_{c,t+j}$ is instrumented by the trilemma IV, $z_{c,t}$,

8. Please note for later reference that I include a global real GDP growth variable to parsimoniously remove global business cycle effects as including time-fixed effects would require over a hundred additional parameter estimates.

9. Alternatively, I trimmed the first and last percentiles of wage inflation and results go through.

the exogenous changes in the short-term interest rate in country c. These monetary shocks are orthogonal to supply shocks and to the natural unemployment rate under the common assumption that monetary policy is neutral under flexible prices (Galí, 2015). Through this IV approach, the Phillips multiplier allows estimating the tradeoff without bias from confounding supply shocks and without the need to measure the natural unemployment rate.

Intuitively, the Phillips multiplier, $\mathscr{P}_h$, measures the impact of a policy that induces a 1 percentage point increase in unemployment on cumulative wage inflation. A negative multiplier ($\mathscr{P}_h < 0$) indicates that a transitory increase in unemployment yields a persistent wage inflation decrease. In other words, central banks can trigger a persistent change in wage inflation at a finite unemployment cost through a transitory increase in their policy interest rates, which is exactly the type of tradeoff monetary policymakers want to explore.

The impulse response functions from Equation (3.3.2) are estimated in such a way that we can obtain the Phillips multiplier directly from $\mathscr{P}_h \equiv \frac{\beta_h^{\pi^w}}{\beta_h^u}$. The advantage of doing the one-step estimation of the Phillips multiplier in Equation (3.3.3) is to directly obtain the correct confidence bands. Nevertheless, the two-step estimation is consistent once the samples are matched (Ramey and Zubairy, 2018).

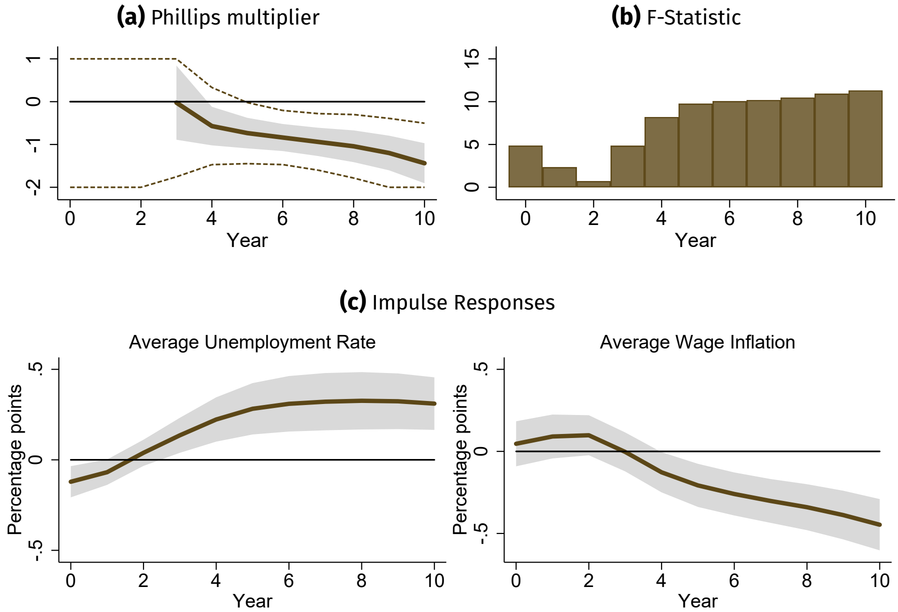
3.4 Results

Can central banks “transform" unemployment into inflation (and vice-versa) through their policy interest rates? And, if so, is this tradeoff time-varying and undermined by a low price inflation environment? This section presents the answers provided by the empirical results. I begin by reporting that the central bank’s ability to control inflation depends on the unemployment cost of reducing inflation and that its ability is high when considering the full sample. In a second step, I uncover that this ability is impaired when the economy is in a low price inflation environment displaying a different multiplier for two different sub-samples.

3.4.1 Phillips multiplier

Figure 3.4.1 displays my estimate for the Phillips multiplier over a 10-year horizon (Figure 3.4.1a), its F-statistic (Figure 3.4.1b), and the underlying impulse responses for the average unemployment rate and average wage inflation (Figure 3.4.1c). The statistic is initially undetermined, decreasing over the horizon and becomes significantly negative after 5 years, diverging further on. A 1 percentage point (p.p.)

policy-induced increase in cumulative unemployment leads to a 1.4 p.p. decrease in cumulative wage inflation 10 years after the shock.[10]



Notes: Phillips multiplier estimations using the trilemma IV as instrument, using a matched sample of approximately 1000 observations, and controlling for two lags of unemployment and wage inflation, country fixed effects, and world GDP growth as explained in Equation (3.3.3). For the multiplier (upper-left), the shaded area corresponds to the 90% confidence interval implied by the normal limiting distribution of the 2SLS estimator, while the dashed lines correspond to the two-sided 90% Anderson-Rubin confidence sets robust to weak instruments. The F-statistics (upper-right) are computed using the method presented in Olea and Pflueger (2013). The impulse responses (bottom panels) for *average* wage inflation and *average* unemployment are obtained from the OLS regressions (3.3.2) and display 90% confidence sets. Impulse responses for non-averaged cumulative unemployment and wage inflation can be found in Figure 3.A.3.

Figure 3.4.1. Phillips multiplier and IRFs

As Barnichon and Mesters (2020) noted, a large tradeoff in the longer-run implies that a transitory policy-induced change in unemployment has a persistent effect on wage inflation. Hence, Figure 3.4.1a suggests that, over the last 170 years, central banks had sufficient ability to steer inflation.

Figure 3.4.1b reports the Olea and Pflueger (2013) F-statistics from the first-stage regression of Equation (3.3.3) and documents that monetary policy shocks are correlated with cumulative unemployment. Since the F-statistic estimates are not above the threshold of Olea and Pflueger (2013) but are still above 5 for most periods, I rely on weak instrument robust methods to compute the confidence bands

10. In the short-term, the multiplier cannot be interpreted because the value of one of the impulse responses is very close to zero. The uncertainty in the estimation is in line with what Barnichon and Mesters (2020) also report.

of the Phillips multiplier. I compute 90% Anderson and Rubin (1949) confidence bands that are robust to weak instruments and display them in dashed lines in Figure 3.4.1a.[11]

Figure 3.4.1c decomposes the Phillips multiplier into the response of both the average wage inflation and average unemployment rate to a monetary policy surprise. While the average unemployment response starts mean-reverting after horizon $t = 5$, the average wage inflation cumulative response decreases persistently. This implies that after the shock, the Phillips multiplier keeps decreasing over time and there is an exploitable tradeoff between unemployment and wage inflation.

3.4.2 The Phillips multiplier is different across sub-samples

Building on the previous Phillips multiplier analysis, I can test whether the wage inflation-unemployment tradeoff is different across different sub-samples. The baseline specification is thus augmented to include an interaction term. Therefore, in the reamining exercises I estimate a state-dependent Phillips multiplier as follows:

$$\begin{aligned}\sum_{j=0}^{h} \pi^{w}_{c,t+j} =& \mathscr{I}_{c,t}\left[\alpha^{(\mathscr{I})}_{c,h} + \mathscr{P}^{(\mathscr{I})}_{h}\sum_{j=0}^{h}\hat{u}_{c,t+j} + \zeta^{(\mathscr{I})}_{h}\boldsymbol{W}_{c,t}\right] \\ &+ (1-\mathscr{I}_{c,t})\left[\alpha^{(1-\mathscr{I})}_{c,h} + \mathscr{P}^{(1-\mathscr{I})}_{h}\sum_{j=0}^{h}\hat{u}_{c,t+j} + \zeta^{(1-\mathscr{I})}_{h}\boldsymbol{W}_{c,t}\right] + \epsilon_{c,t+h}\end{aligned} \tag{3.4.1}$$

where $\mathscr{I}_{c,t}$ is the indicator variable different for each sub-sample analysis. This exercise allows comparing the evolution of the Phillips multiplier in each sub-sample and directly test whether $\mathscr{P}^{(\mathscr{I})}_{h} = \mathscr{P}^{(1-\mathscr{I})}_{h}$.

3.4.2.1 The Phillips multiplier is smaller during the Gold Standard and the last 20 years

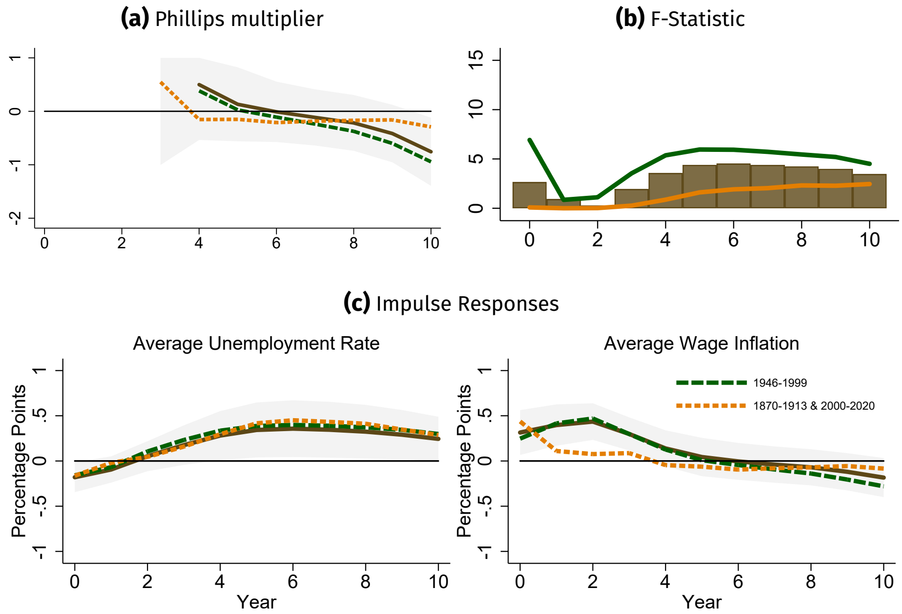
Motivated by Figure 3.2.2, I now test in a more robust empirical setting whether the wage inflation-unemployment tradeoff is different across sub-samples. To be precise, I am going to aggregate the years where inflation was more credibly anchored - the last 20 years (2000-2020) and the Gold Standard epoch (1870-1913) - and compare them against the post-war period (1946-1999) leaving the between-war period (1920-1938) out of this analysis.

Therefore, I estimate equation 3.4.1 where $\mathscr{I}_{c,t}$ is an indicator of the post-war period defined as a dummy variable, which is equal to 1 for the years between 1946

11. While the asymptotic distribution of the AR statistic does not depend on the strength of the instrument, the confidence bands of the Phillips multiplier will be larger when the instrument is weaker.

and 1999 and equal to 0 for the years where inflation was more credibly anchored. This exercise allows comparing the evolution of the Phillips multiplier in these sub-samples and directly test whether $\mathscr{P}_h^{(\mathscr{I})} = \mathscr{P}_h^{(1-\mathscr{I})}$.

Figure 3.4.2 displays the estimates of both the baseline and state-dependent Phillips multipliers over a 10-year horizon (Figure 3.4.2a), their F-statistics (Figure 3.4.2b), and their underlying impulse responses (Figure 3.4.2c) in the historical sub-samples.



Notes: Phillips multiplier estimated using the trilemma IV as instrument according to Equation (3.3.3). For the multiplier (upper-left), the shaded area corresponds to the 90% confidence interval implied by the normal limiting distribution of the 2SLS estimator. The F-statistics (upper-right) are computed as discussed in Olea and Pflueger (2013). The impulse responses (bottom panels) for *average* wage inflation and unemployment are obtained from the OLS regressions (3.3.2) and display 90% confidence sets. Across all figures, one can distinguish the state by its color and shape, short-dashed orange shape for the period with more credible anchored inflation (1870-1913 & 2000-2020) and long-dashed green shape for the post-war period (1946-1999).

Figure 3.4.2. State-Dependent Phillips multiplier and IRFs

Figure 3.4.2a displays a bigger Phillips multiplier for the post-war period. Its difference becomes statistically significant from horizon $t = 7$ onward with the weak instrument robust Anderson-Rubin p-values being 0.031, 0.019, 0.025, and 0.089 for horizons 7, 8, 9, and 10 respectively.[12] This result is in line with the idea put forward by Figure 3.2.2 in which we see that the correlation between unemployment and wage inflation is weaker in the last 20 years and during the Gold Standard.

12. See Table 3.A.6 in the Appendix for a more detailed description of this result.

Figure 3.4.2c indicates that the wage inflation response is the main driver of the weaker tradeoff in the credible inflation anchor periods. Although the average unemployment rate response is virtually identical in both the baseline and the state dependencies for longer horizons, the average wage inflation response is muted for longer horizons.

3.4.2.2 The Phillips multiplier is smaller in a low inflation environment

Research on the wage inflation-unemployment tradeoff, traditionally inferred from a Phillips curve, pointed to the hypothesis that an increase (decrease) in trend inflation should lead to an increase (decrease) in the frequency of price adjustment, thereby decreasing (increasing) the steepness of the wage Phillips curve (Benati, 2007).

In this exercise, I test whether the wage inflation-unemployment tradeoff is shaped by a low price inflation environment. Therefore, I estimate equation 3.4.1 where $\mathscr{I}_{c,t}$ is an indicator of low price inflation defined as a dummy variable, which is equal to one for periods when countries experienced lagged price inflation below the threshold of 2% and above -2% ($\mathscr{I}_{i,t} = 1 \text{ if } -2\% < \pi^{p}_{i,t-1} < 2\%$) and equal to 0 when countries experienced high price inflation ($\mathscr{I}_{i,t} = 0 \text{ if } 2\% \leq \pi^{p}_{i,t-1} < 40\%$). This exercise allows comparing the evolution of the Phillips multiplier in times of low versus high price changes and directly test whether $\mathscr{P}_h^{(\mathscr{I})} = \mathscr{P}_h^{(1-\mathscr{I})}$.

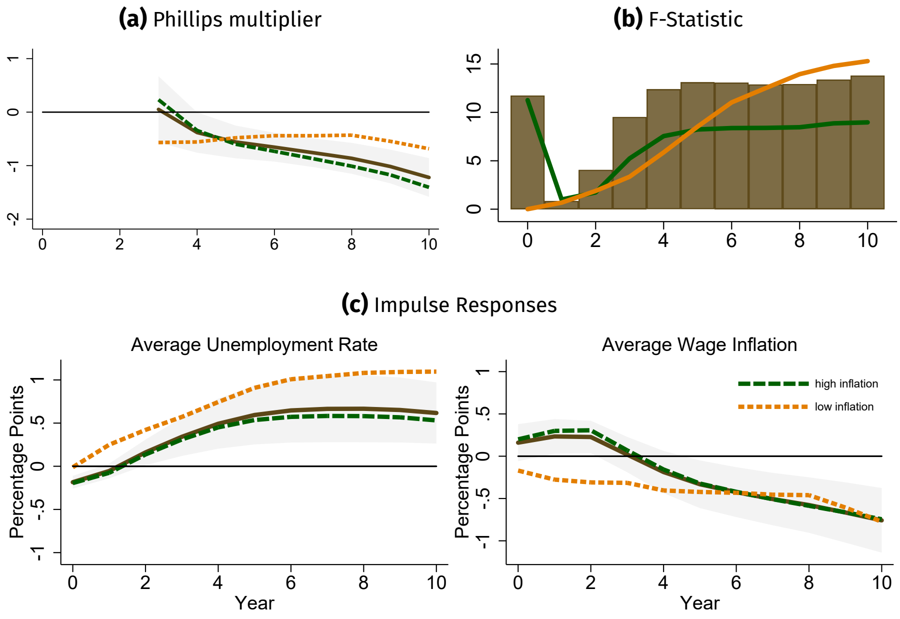
The choice of the 2% threshold can be rationalized by the inflation target strategy of many of the central banks present in the analyzed sample. Over the last 20 years of the sample, most central banks were targeting inflation either implicitly or explicitly. Most of them disclaimed that their goal was to achieve inflation close to or even below 2%. With such division of the sample, I assign 64% of the sample to a high-inflation state and 31% to the low-inflation state while the remaining 5% of the sample is left out of this sub-sample analysis.

Figure 3.4.3 displays the estimates of both the baseline and state-dependent Phillips multipliers over a 10-year horizon (Figure 3.4.3a), their F-statistics (Figure 3.4.3b), and their underlying impulse responses (Figure 3.4.3c) in periods of high and low price inflation.

Figure 3.4.3a displays a smaller Phillips multiplier in times of low inflation and a higher multiplier in times of high inflation. Its difference becomes statistically significant from horizon $t = 8$ onward with the weak instrument robust Anderson-Rubin p-values being 0.069, 0.072, and 0.061 for horizons 8, 9, and 10 respectively.[13] This result is in line with recent work by Forbes, Gagnon, and Collins (2021) who show that the Phillips curve becomes non-linear when inflation is low.

Figure 3.4.3c indicates that the unemployment rate response is the main driver of the weaker tradeoff in low inflation periods. Although the average wage inflation

13. See Table 3.A.7 in the Appendix for a more detailed description of this result.



Notes: Phillips multiplier estimated using the trilemma IV as instrument according to Equation (3.3.3). For the multiplier (upper-left), the shaded area corresponds to the 90% confidence interval implied by the normal limiting distribution of the 2SLS estimator. The F-statistics (upper-right) are computed as discussed in Olea and Pflueger (2013). The impulse responses (bottom panels) for *average* wage inflation and unemployment are obtained from the OLS regressions (3.3.2) and display 90% confidence sets. Across all figures, one can distinguish the state by its color and shape, short-dashed orange shape for low inflation and long-dashed green shape for high inflation.

Figure 3.4.3. State-Dependent Phillips multiplier and IRFs

response is virtually identical in both the baseline and the state dependencies for longer horizons, the average unemployment rate response is much more pronounced during low inflation periods. As a robustness check, I also used an unmatched sample and a longer horizon (see Figure 3.A.4 and Table 3.A.8 in the Appendix). Regardless of the sample trimming process or the horizon chosen, the results do not qualitatively change.

These two exercises together lend empirical substance to the concern that monetary policy effects are time-variant and state-dependent. In particular, during periods of low price inflation, the long-run tradeoff between wage inflation and unemployment is less exploitable. In other words, given the weaker tradeoff, central banks are less able to steer wage inflation when facing a low-price inflation environment.

3.5 Conclusion

The wage inflation-unemployment tradeoff is a key building block for monetary policy. However, its existence has been questioned with some commentators arguing that it has flattened out of favor. This paper introduces newly assembled data on wages and unemployment rates for a set of 18 advanced economies starting in 1870, in order to revisit the historical relationship between wage inflation and unemployment, the focus of Phillips' (1958) original work. The empirical analysis starts by uncovering a historical time-varying Phillips correlation. This paper documents a weaker correlation between wage inflation and unemployment during the Gold Standard and the last 20 years, periods characterized by credibly anchored inflation expectations and a low price inflation environment.

I capitalize on the assembled historical data to study a factor that is possibly driving this time-varying pattern. First, in order to account for the possible endogeneity and model misspecification issues arising from the Phillips curve framework, I make use of monetary policy shocks and the Phillips multiplier framework to identify the historical wage inflation-unemployment tradeoff. The results provide evidence in favor of the hypothesis that the observed time-variation pattern is due to the price inflation environment: the tradeoff is weaker in periods of low price inflation.

These results add a new perspective to the current debate about the existence of the wage inflation-unemployment tradeoff and its state dependency. In particular, this paper's empirical evidence points to an impaired ability in exploring the tradeoff in times of low inflation driven by a muted wage inflation response to a monetary policy surprise. Such a finding uncovers a hidden dichotomy: central banks cannot simultaneously target a low price inflation (2%) and expect conventional monetary policy tools to work to their full extent.

Appendix 3.A Supporting tables and figures

Table 3.A.1. Data Coverage

Country	Wages	Unemployment	Inflation Forecast	CB Foundation
Australia	1870-2020	1901-2020	1961-2020	1911
Belgium	1870-2020	1921-2020	1992-2020	1850
Canada	1870-2020	1916-2020	1993-2020	1934
Denmark	1870-2020	1874-2020	1968-2020	1818
Finland	1870-2020	1920-2020	1991-2020	1811
France	1870-2020	1895-2020	1991-2020	1800
Germany	1870-2020	1887-2020	1996-2020	1876
Ireland	1943-2020	1960-2020	1996-2020	1943
Italy	1871-2020	1919-2020	1991-2020	1893
Japan	1870-2020	1930-2020	1961-2020	1882
Netherlands	1870-2020	1870-2020	1991-2020	1814
Norway	1870-2020	1904-2020	1961-2020	1816
Portugal	1870-2020	1953-2020	1991-2020	1846
Spain	1870-2020	1933-2020	1993-2020	1874
Sweden	1870-2020	1911-2020	1963-2020	1668
Switzerland	1870-2020	1913-2020	1963-2020	1907
United Kingdom	1870-2020	1870-2019	1991-2020	1694
United States	1870-2020	1890-2020	1961-2020	1913

Notes: This Table shows the earliest and the latest data point for each country's series: the wages nominal index and the unemployment rate. There are gaps in the unemployment rate data which mostly correspond to the war periods, for more information on the gaps and the sources please consult the Data Appendix. Data from inflation forecast comes from the OECD. All central bank foundations dates came from the central banks' websites.

Table 3.A.2. Descriptive statistics - full sample

	N	Mean	Std. Dev.	Min	Max
1870-1913					
Unemployment rate	225	4.08	2.73	0.20	18.40
Wage inflation	730	1.81	4.23	-24.64	23.80
Price inflation	731	0.46	4.76	-26.91	33.31
World Wars					
Unemployment rate	130	3.63	2.97	0.40	17.20
Wage inflation	213	14.85	32.90	-19.94	412.23
Price inflation	228	20.29	71.10	-37.68	975.64
1920-1938					
Unemployment rate	270	7.12	5.00	0.60	24.90
Wage inflation	314	3.13	13.16	-27.72	86.48
Price inflation	333	0.67	9.84	-19.42	73.13
1946-1971					
Unemployment rate	436	2.60	1.84	0.04	9.92
Wage inflation	465	10.11	19.44	-55.42	225.19
Price inflation	468	5.38	10.37	-17.60	125.33
1972-1999					
Unemployment rate	504	7.07	4.30	0.04	24.21
Wage inflation	504	8.30	6.27	-1.42	32.28
Price inflation	504	6.56	5.51	-0.71	37.88
2000-2020					
Unemployment rate	377	7.05	3.52	2.00	26.09
Wage inflation	378	2.32	1.84	-6.14	7.50
Price inflation	378	1.63	1.28	-4.48	5.57
Total					
Unemployment rate	1942	5.49	4.08	0.04	26.09
Wage inflation	2604	5.85	14.41	-55.42	412.23
Price inflation	2642	4.40	22.54	-37.68	975.64

Notes: All statistics are expressed in percent. The hyperinflation period in Germany (1920-1925) is not included. All remaining observations available in the dataset are used in this Table.

Table 3.A.3. Descriptive statistics - weighted by population size

	N	Mean	Std. Dev.	Min	Max
1870-1913					
Unemployment rate	223	4.97	3.42	0.20	18.40
Wage inflation	223	1.56	2.14	-6.71	10.26
Price inflation	223	0.51	2.15	-10.94	11.56
1920-1938					
Unemployment rate	268	8.34	6.14	0.60	24.90
Wage inflation	268	1.46	8.40	-27.72	43.97
Price inflation	268	0.04	6.97	-18.45	30.43
1946-1971					
Unemployment rate	428	3.25	2.03	0.04	9.92
Wage inflation	428	7.36	4.92	-10.78	35.29
Price inflation	428	3.91	3.69	-6.87	20.38
1972-1999					
Unemployment rate	504	6.76	3.76	0.04	24.21
Wage inflation	504	7.02	5.87	-1.42	32.28
Price inflation	504	5.57	4.77	-0.71	37.88
2000-2020					
Unemployment rate	377	6.75	3.37	2.00	26.09
Wage inflation	377	2.15	1.88	-6.14	7.50
Price inflation	377	1.62	1.23	-4.48	5.57
Total					
Unemployment rate	1800	6.01	4.01	0.04	26.09
Wage inflation	1800	4.65	5.64	-27.72	43.97
Price inflation	1800	3.07	4.50	-18.45	37.88

Notes: All statistics are expressed in percent. The war periods (1914-1919 and 1939-1945) and the German hyperinflation episode (1920-1925) are not included. This table only uses *weighted* by population country-year observations for which there is data for the unemployment rate, and price and wage inflation. Table 3.A.2 presents descriptive statistics for the unrestricted sample.

Table 3.A.4. Wage Inflation Correlations Table

	π_t^p	π_{t-1}^p	u_t
Australia	0.699	0.693	-0.459
Belgium	0.406	0.595	-0.191
Canada	0.806	0.497	-0.424
Denmark	0.657	0.674	-0.093
Finland	0.372	0.475	-0.356
France	0.835	0.726	-0.514
Germany	0.691	0.625	-0.531
Ireland	0.832	0.654	-0.175
Italy	0.635	0.771	-0.109
Japan	0.287	0.383	-0.744
Netherlands	0.627	0.581	-0.321
Norway	0.814	0.729	-0.626
Portugal	0.586	0.621	-0.235
Spain	0.548	0.467	-0.275
Sweden	0.806	0.775	-0.498
Switzerland	0.627	0.714	-0.483
UK	0.762	0.574	-0.266
USA	0.857	0.573	-0.286

Notes: Correlation between wage inflation and price inflation, lagged price inflation, and unemployment by country in the main sample excluding outliers as defined in the text.

Table 3.A.5. First-Stage of trilemma IV

Dependent	No controls			With controls		
variable: Δr_{it}	All years	Pre-WW2	Post-WW2	All years	Pre-WW2	Post-WW2
trilemma $z_{i,t}$	0.60***	0.41***	0.68***	0.65***	0.64***	0.65***
	(0.08)	(0.09)	(0.09)	(0.08)	(0.15)	(0.09)
t-statistic	[7.65]	[4.42]	[7.99]	[8.47]	[4.29]	[7.58]
N	1316	505	811	1011	215	796

Notes: This table presents the first-stage estimates of the trilemma IV on the country's interest rate. The standard errors are in parentheses and the T-statistics are in square brackets. The full sample covers 1870–2020, excluding the World Wars and the German hyperinflation episode. The pre-WW2 sample covers 1870–1938, excluding 1914–1919, while the post-WW2 sample covers 1948–2020. The estimates in the last three columns (with controls) include country fixed effects and two lags of wage inflation and unemployment rate. In addition, I include world GDP growth to capture global cycles.

Table 3.A.6. Estimates of multipliers across sub-samples

Horizon	Linear Model	1946-1999	1870-1913 & 2000-2020	AR p-value
4	0.499	0.383	-0.154	0.310
	(0.629)	(0.581)	(0.191)	
5	0.131	0.026	-0.150	0.167
	(0.421)	(0.411)	(0.107)	
6	-0.010	-0.109	-0.211	0.091
	(0.343)	(0.336)	(0.076)	
7	-0.115	-0.239	-0.184	0.031
	(0.319)	(0.307)	(0.089)	
8	-0.217	-0.374	-0.169	0.019
	(0.317)	(0.308)	(0.104)	
9	-0.416	-0.601	-0.161	0.025
	(0.327)	(0.322)	(0.125)	
10	-0.757	-0.945	-0.292	0.089
	(0.388)	(0.407)	(0.116)	

Notes: This table presents the multiplier estimates corresponding to the ones in Figure 3.4.2a. The values in parentheses under the multipliers indicate the correspondent standard errors. The last column indicates the weak instrument robust Anderson-Rubin p-values for the difference in multipliers across states.

Table 3.A.7. Estimates of multipliers across states of inflation

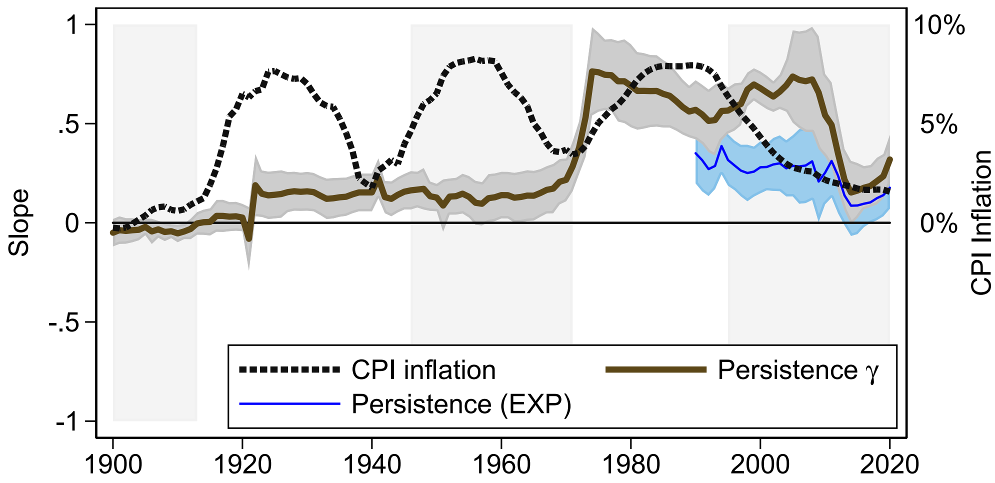
Horizon	Linear Model	High Inflation	Low Inflation	AR p-value
3	0.051	0.233	-0.568	0.111
	(0.376)	(0.518)	(0.453)	
4	-0.381	-0.343	-0.557	0.483
	(0.229)	(0.284)	(0.337)	
5	-0.558	-0.599	-0.479	0.728
	(0.186)	(0.228)	(0.275)	
6	-0.655	-0.732	-0.440	0.314
	(0.164)	(0.214)	(0.242)	
7	-0.758	-0.870	-0.441	0.140
	(0.163)	(0.224)	(0.230)	
8	-0.863	-1.010	-0.430	0.069
	(0.176)	(0.253)	(0.231)	
9	-1.016	-1.172	-0.545	0.072
	(0.192)	(0.287)	(0.222)	
10	-1.221	-1.405	-0.684	0.061
	(0.220)	(0.348)	(0.213)	

Notes: This table presents the multiplier estimates corresponding to the ones in Figure 3.4.3a. The values in parentheses under the multipliers indicate the correspondent standard errors. The last column indicates the weak instrument robust Anderson-Rubin p-values for the difference in multipliers across states.

Table 3.A.8. Estimates of multipliers across states of inflation

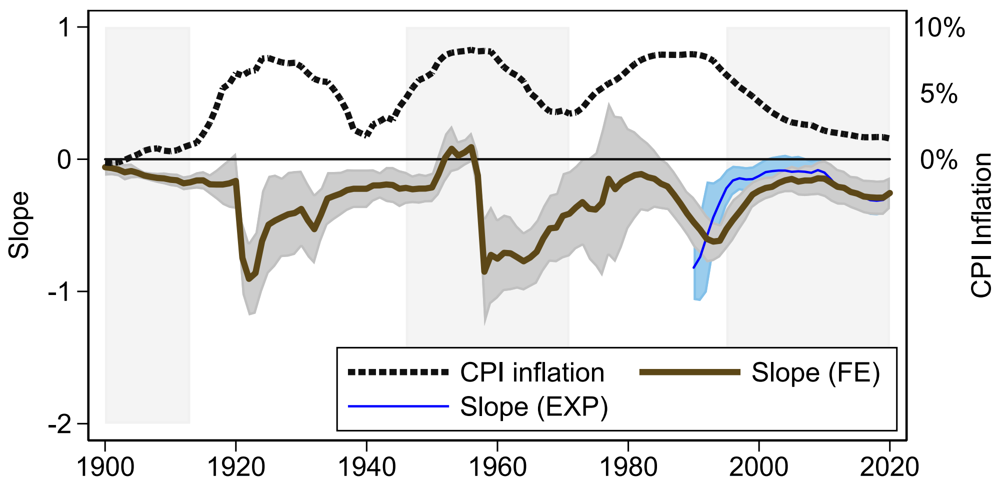
Horizon	Linear Model	High Inflation	Low Inflation	AR p-value
3	-0.041	0.285	-0.677	0.137
	(0.354)	(0.648)	(0.329)	
4	-0.460	-0.391	-0.679	0.438
	(0.200)	(0.309)	(0.241)	
5	-0.650	-0.696	-0.668	0.933
	(0.162)	(0.241)	(0.237)	
6	-0.760	-0.830	-0.719	0.709
	(0.155)	(0.224)	(0.297)	
7	-0.810	-0.956	-0.595	0.180
	(0.148)	(0.232)	(0.210)	
8	-0.886	-1.058	-0.606	0.089
	(0.165)	(0.254)	(0.248)	
9	-1.004	-1.177	-0.571	0.051
	(0.176)	(0.277)	(0.180)	
10	-1.221	-1.405	-0.684	0.061
	(0.220)	(0.348)	(0.213)	
11	-1.403	-1.592	-0.543	0.037
	(0.270)	(0.410)	(0.344)	
12	-1.474	-1.671	-0.598	0.049
	(0.271)	(0.425)	(0.342)	
13	-1.677	-1.898	-0.794	0.092
	(0.307)	(0.514)	(0.350)	
14	-1.776	-1.966	-0.831	0.122
	(0.337)	(0.545)	(0.344)	
15	-1.918	-2.096	-0.724	0.119
	(0.401)	(0.621)	(0.364)	

Notes: This table presents the multiplier estimates corresponding to the ones in Figure 3.A.4a. The values in parentheses under the multipliers indicate the standard errors. The last column indicates the weak instrument robust Anderson-Rubin p-values for the difference in multipliers across states.



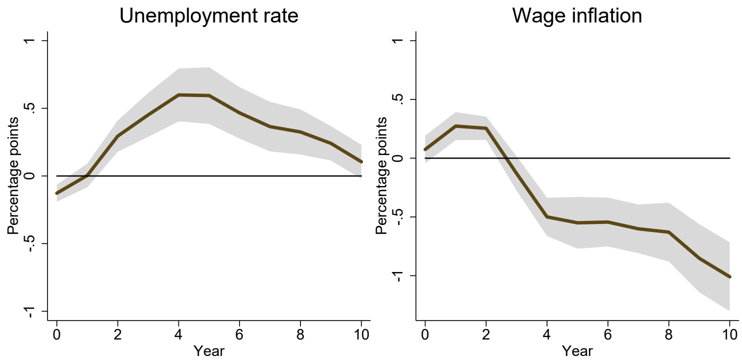
Notes: This figure plots a time-varying estimate of the persistence coefficient of the wage Phillips curve (parameter γ, in Equation (3.2.1)), using OLS and annual data from 1870 to 2020 for all 18 countries. In blue, I estimate the parameter γ also controling for inflation expectations by estimating: $\pi^w_{c,t} = \mu_c + \pi^e_{t+1} + \varphi u_{c,t} + \gamma \pi^p_{c,t-1} + \varepsilon_{c,t}$. It is computed based on a rolling OLS regression using a 20-year window and displays a 90% confidence band.

Figure 3.A.1. Panel-OLS 20-year Rolling Window



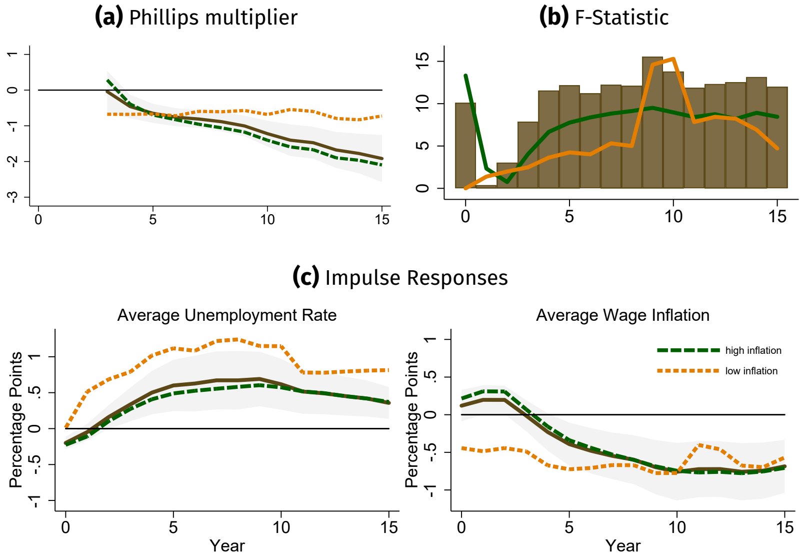
Notes: This figure plots a time-varying estimate of the slope of the wage Phillips curve for two different specifications of Equation (3.2.1). In brown, I estimate the parameter φ by estimating: $\pi^w_{c,t} = \mu_c + \delta_t + \varphi u_{c,t} + \gamma \pi^p_{c,t-1} + \varepsilon_{c,t}$. In blue, I estimate the parameter φ by estimating: $\pi^w_{c,t} = \mu_c + \pi^e_{t+1} + \varphi u_{c,t} + \gamma \pi^p_{c,t-1} + \varepsilon_{c,t}$. In both specifications I am using Panel-OLS and annual data from 1870 to 2020 for all 18 countries. It is computed based on a rolling OLS regression using a 20-year window with year fixed effects (δ_t) and displays a 90% confidence band.

Figure 3.A.2. Panel-OLS 20-year Rolling Window with year fixed effects



Notes: These impulse responses for cumulative unemployment and cumulative wage inflation are obtained from the OLS regressions (3.3.2) by changing the dependent variable from the average $\frac{1}{h}\sum_{j=0}^{h} y_{c,t+j}$ to the difference $y_{c,t+j} - y_{c,t+1}$. They display 90% confidence sets and show the temporary effect of the monetary policy shock to unemployment and the persistent effect to the wage inflation (in line with the persistent effect to price inflation in Jordà, Schularick, and Taylor (2019)).

Figure 3.A.3. Impulse Responses of Cumulative Changes in Unemployment and Wage Inflation



Notes: This figure presents a robustness exercise with a higher horizon (15 years) and an unmatched sample, that is, using all available information and abstracting from eventual sample changes across each horizon as the number of observations decreases from 1000 to approximately 650. Here, I also control for two lags of unemployment and wage inflation, country fixed effects, and world GDP growth. The Olea and Pflueger (2013) effective F-statistic of the IRFs are around 30 and 50, for unemployment and wage inflation respectively, and always above the 10% TSLS threshold. Figures display 68% and 90% confidence bands for the baseline scenario. The state-dependent multipliers are significantly different for horizons between years 9 and 12 as one can confirm in Table 3.A.8. Across all figures, one can distinguish the state by its color and shape, short-dashed orange shape for low inflation and long-dashed green shape for high inflation.

Figure 3.A.4. State-Dependent Phillips multiplier and IRFs

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