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* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

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Abstract

We estimate the impact of tax shocks on output across different stages of the business cycle. We do this for a panel of nine advanced economies using a harmonized dataset of narratively identified exogenous tax changes and a smooth transition local projection model. The output response to an exogenous tax shock is significant, but only during economic expansions. In recessions, the tax multiplier is insignificant, both in the short- and long run. We also find that, during booms, output only responds to tax hikes and is unresponsive to tax cuts. The results on the state-dependent and asymmetric effects of tax shocks are robust to a number of alternative model specifications and definitions of the business cycle.

JEL Classification: E32, E62

Keywords: tax multiplier, state-dependent effects of fiscal policy, local projection method

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1 Introduction

The Global Financial Crisis and the recent COVID-19 crisis have highlighted the importance of fiscal policy in absorbing adverse shocks. The role of fiscal stabilization policy has recently attracted more attention due to the prolonged spell of historically low interest rates and the correspondingly limited scope for expansionary monetary policy. Not surprisingly, a plethora of studies on the size of the fiscal multiplier has emerged in recent years to better our understanding on the effects of fiscal stimulus. A key finding is that the fiscal multiplier is likely to be *state dependent*, i.e. varies with the state of the business cycle (Auerbach and Gorodnichenko, 2012a), the level of the interest rate (Bonam et al., 2020; Miyamoto et al., 2018), the degree of public indebtedness (Huidrom et al., 2019), the exchange rate regime (Ilzetzki et al., 2013), etc. However, most of these results pertain to the effects of government expenditure shocks. Much less is known about the state-dependent effects of tax shocks, most likely due to the highly pro-cyclical nature of budget revenues that makes the identification of exogenous tax changes notoriously difficult. Moreover, the few exceptions that do attempt to estimate tax multipliers typically focus on a single country (usually the US), making it difficult to generalize the results.

We aim to address these issues by using quarterly data on narratively identified exogenous tax changes for a panel of nine advanced economies. The narrative approach of identifying exogenous fiscal shocks has been popularized by Romer and Romer (2010), who estimate the tax multiplier for the US. Pescatori et al. (2011), Amaglobeli et al. (2018) and Cohen-Setton et al. (2019) apply the narrative approach to study the effects of fiscal expansions and consolidations for a large group of advanced and emerging market economies. However, the annual frequency of the data used in these latter studies makes it difficult to detect the potential state dependence of the fiscal multiplier. We therefore collect quarterly data on narrative tax shocks for different countries and from different sources: the US (Romer and Romer, 2010), Austria (Kilic, 2012), the UK (Cloyne, 2013), Germany (Hayo and Uhl, 2013), Portugal (Pereira and Wemans, 2015), the Netherlands

(Geenen, 2017), Japan (Kato et al., 2018), Spain (Gil et al., 2019), and Canada (Hussain and Liu, 2019). One of our contributions is to harmonize and compile these narrative tax series into a consistent panel data set. The data covers the period 1948Q1 to 2017Q1.

Armed with this data set, we employ a panel smooth transition local projection model to estimate the unconditional tax multiplier and the tax multiplier in times of economic recession and expansion. Following Ramey and Zubairy (2018), among others, we use the seven-quarter lagging moving average of real quarterly GDP growth as our measure of economic activity. The transitions between the recessionary and expansionary regimes are governed by a logistic function, as for example in Tenreyro and Thwaites (2016). Importantly, to ensure that the transition probabilities are equivalent across countries, we calibrate the parameters of the logistic function for each country separately.

Our main finding is that the tax multiplier relies heavily on the state of the business cycle. Specifically, we find that the cumulative response of output to a 1% tax increase is negative and statistically significant, *but only during economic expansions*. During recessions, the output response to a tax shock is insignificant, both in the short- and long run, and statistically different from the output response during expansions. Furthermore, the impact of taxes during booms is highly persistent, as we find the cumulative tax multiplier to remain significantly negative for more than three years. Whereas the unconditional impact tax multiplier is estimated at -0.45, we find a much larger impact tax multiplier of -0.87 during expansions. For the peak cumulative tax multiplier, the corresponding numbers are -1.45 (unconditional) and -1.88 (during expansions), respectively. The observed state dependence of the tax multiplier might explain the large dispersion of multiplier estimates found in the literature.

When splitting the sample based on GDP growth quartiles, we find that the conditional peak tax multiplier is significantly negative only within the highest quartile. In other words, tax shocks only have an impact on macroeconomic conditions when the economy experiences very strong growth. Moreover, we find that output only responds to tax shocks when taxes are raised, not when they are lowered, and again only during the expansionary state. These results have important implications for the design of fiscal stabilization policies and for the timing of fiscal consolidations. In fact, governments' attempt to stimulate the economy during recessions through tax reductions may not be successful, whereas revenue-based fiscal consolidations are most costly in terms of output losses during expansions.

We check the validity of our results by performing a number of robustness checks. Particularly, we consider alternative measures for economic activity and approaches to calibrate the logistic function, thereby altering both the nature and prevalence of the recessionary and expansionary regimes used in our analysis. We also change our set of control variables and iteratively remove one country from our panel to avoid potential biases arising from any particular country. In all cases, our main result holds up: the effects of an exogenous tax increase on output is significantly negative during expansions, yet insignificant during recessions.

Our results contrast the conventional view on the effects of government spending shocks, which is that government spending multipliers are larger in times of economic recession than during booms. This idea builds on the Keynesian view that, during recessions, the marginal propensity to consume is likely to be higher as more households and firms find themselves liquidity constrained, which would entail a smaller crowding-out effect arising from increased government spending. The empirical evidence with regards this conventional view is, however, somewhat divided. For instance, while Auerbach and Gorodnichenko (2012b) and Blanchard and Leigh (2013) report spending multipliers that are higher (and greater than unity) in recessions than in expansions, Ramey and Zubairy (2018) find spending multipliers below unity "irrespective of the slack of the economy". Caggiano et al. (2015) find a significant distinction in the size of spending multipliers across business cycle states, but only between very deep recessions and strong expansions. Recently, Berge et al. (2020) argue that such inconsistencies found in the literature may arise from the way researchers define the recessionary and expansionary states. Particularly, they show that the government spending multiplier is higher when the unemployment rate is increasing relative to when it is decreasing, yet do not find the multiplier to depend on whether the unemployment rate is below or above its trend.

The expanding literature on state-dependent tax multipliers also provides conflicting results. For instance, a meta-analysis covering 98 empirical studies from Gechert and Rannenberg (2018) finds that tax multipliers do not exhibit a substantial degree of state dependence. Alesina et al. (2018) find, for a panel of 16 OECD countries and using annual data on narratively identified fiscal consolidations, that tax based consolidations lead to larger output contractions during expansions than during recessions, yet the difference is small. On the other hand, the empirical evidence for the US paints a more consistent picture. Arin et al. (2015), for example, estimate tax multipliers within a Markov-Switching framework using US narrative tax series and find tax multipliers below unity, yet which are 0.5% larger during expansions than during recessions. Similar results are found by Demirel (2016) and Eskandari (2019) who instead use local projection methods. These (and our) results are in line with Sims and Wolff (2018) who, using a medium-scale DSGE model, show that cuts in labor income, capital income and consumption taxes have stronger stimulative effects on output when output is already relatively high. Similarly, Arin et al. (2019) argue that labor income tax hikes are contractionary, but only during expansions, because of a stronger and offsetting labor supply response to the tax shock during recessions. We contribute to this literature by reporting new empirical evidence on the state dependence of tax multipliers for a panel of advanced economies and using quarterly narrative tax series.

The sequence of the paper is structured as follows. Section 2 details the construction of the narrative panel data set, the econometric framework and the data used. Section 3 presents our main results and those from the robustness analyses. Section 4 concludes.

2 Empirical strategy

In order to estimate the effects of tax shocks at different stages of the business cycle, we use a panel narrative data set of exogenous legislated tax changes and a panel smooth transition local projection model. In this section, we first describe how the data set is constructed. Next, we provide details on the model. We close with a discussion on the rest of the data used for estimation.

2.1 A narrative panel data set of exogenous tax changes

The narrative approach to identify exogenous tax changes was initially proposed by Romer and Romer (2010, hereafter RR). The idea behind this approach is to use narrative records to classify legislated tax changes as either endogenous or exogenous. The former reflect the government's contemporaneous discretionary response to business cycle fluctuations, while the latter refer to legislative changes independent from economic considerations. The ultimate aim is to transform these tax innovations into projected annual revenue changes as a share of annual GDP. The series capturing the exogenous tax changes can then be used to measure the effects of tax shocks on macroeconomic aggregates using plain OLS.

Application of the narrative approach has recently become more common in the study of the effects of fiscal policy. Pescatori et al. (2011), for instance, use the narrative approach to study the effects of fiscal consolidations in a panel of advanced economies. Similarly, Cohen-Setton et al. (2019) study fiscal expansions using the narrative approach. Amaglobeli et al. (2018) examine the effects of narratively identified tax changes in both advanced and emerging economies, yet do not distinguish between endogenous and exogenous tax changes. The main drawback of the narrative tax series used in these studies, however, is that they are annual. The use of annual data not only worsens potential anticipation bias, but also makes it more difficult to reveal the potential state dependence of the effects of tax shocks.

Over the years, narrative tax series at a quarterly frequency have been constructed by different researchers and for different countries. RR constructed a quarterly narrative series of exogenous tax changes for the US, Kilic (2012) for Austria, Cloyne (2013) for the UK, Hayo and Uhl (2013) for Germany, Pereira and Wemans (2015) for Portugal, Geenen (2017) for the Netherlands, Kato et al. (2018) for Japan, Gil et al. (2019) for Spain, and Hussain and Liu (2019) for Canada. In this paper, we compile and harmonize these nine series into a panel narrative data set of exogenous legislated tax innovations. Because these series were constructed using the approach of RR, we shall describe some important details about the methodology and also highlight a few noteworthy discrepancies across the aforementioned studies.

First of all, in order to transform the narratively identified tax changes into a quarterly time series, the following assignment rule is used. The reference point for the assignment is the date at which the tax bill enters into force. If this date falls within the first (second) half of the corresponding quarter, the bill is assigned to the same (next) quarter.[1,2] This procedure implies an important assumption with regards the timing of the tax shock: by assigning the shock to the quarter in which tax liabilities actually change, it is assumed that agents do not respond to the shock in advance. Potential anticipation effects, that may arise following the announcement (rather than implementation) of the tax change, are therefore ignored. However, when considering alternative assignment dates, RR do not find evidence of such anticipation behavior for the US. Moreover, findings from

[1]Furthermore, when legislated tax changes are assigned to the same quarter, their projected revenue impact is summed up. If a legislated tax change occurs step-wise, the shocks are assigned to the corresponding quarters. Temporary changes are offset in the subsequent quarter. Legislated changes that result in switching revenues over different quarters are disregarded.

[2]Pereira and Wemans (2015) employ a slightly different assignment rule for Portugal for corporate taxes. If the legislated tax change occurs in the second half of the quarter, half of the projected revenue change is assigned to the current quarter and the other half to the subsequent quarter. However, when the tax change is of a permanent nature (which usually is the case), an exception is made and the assignment rule of RR is applied. For Spain, Gil et al. (2019) also use a slightly different rule for corporate taxes. Spanish firms need to pay their tax liabilities in three installments, starting in the second quarter. If the tax legislation comes into effect within the first quarter, the shock is assigned to the second quarter. After April, most of the weight of the tax liability falls in the fourth quarter.

Shapiro and Slemrod (1993) and Johnson et al. (2006) support the claim that agents mostly respond to current rather than future expected changes in their disposable income.

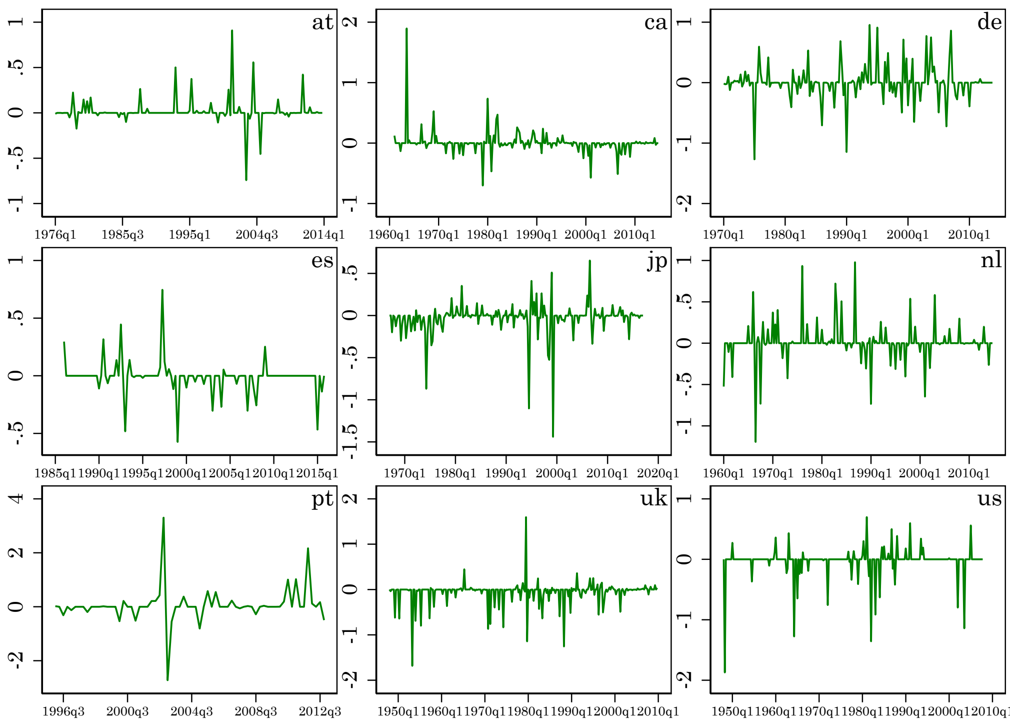
Second, the projected revenue changes due to the legislated tax change are normalized by annualized GDP of the corresponding quarter. Whenever annualized GDP is not available, annual GDP is used instead. Note that, if the tax shock did not occur within the last quarter of the year, the use of annual GDP could bias the size of the tax shock either upwards or downwards, depending on how the tax shock affected GDP during the year.

Third, RR focus only on those tax changes that received considerable attention during the legislation process, so as to avoid the inclusion of tax changes that occur automatically, e.g. due to specific characteristics in the tax code, or are otherwise not driven by clear policy objectives. Hayo and Uhl (2013) and Gil et al. (2019) take a different, yet similar approach by considering only those tax changes that have a projected revenue impact of at least 0.01% and 0.05% of GDP, respectively. It turns out that almost all exogenous tax changes in our narrative panel data set meet this criterion.

Finally, a few notes on some country-specific tax series. We extended the German tax series constructed by Hayo and Uhl (2013) using the series from Gechert et al. (2016), which also includes exogenous changes to social security contributions (as is the case for most of the tax series from the other countries in our panel). The Austrian narrative tax series from Kilic (2012) is partly denominated in schillings. We converted this series into euros using the official conversion rate from schilling to euro on the date of the euro's introduction. The Japanese tax series from Kato et al. (2018) also includes local legislative tax changes that have a national impact, which differs from the other sources that only focus on federal tax changes. Table 6 in the Appendix provides an overview of the narrative tax series and their respective sources.

Figure 1 plots the narrative tax series for the nine advanced economies in our panel. Differences in the amplitude of the tax shocks across countries are small, with the notable exception of a sizable tax intervention in Portugal in 2002. Furthermore, note that

Figure 1: Narratively identified exogenous tax series



Notes: Figures are expressed as the % change in the yearly projected tax revenue as a share of GDP.

substantially more tax shocks have been identified in Japan and the UK, which could be explained by differences in their respective legislative systems or in the granularity of the available narrative records (recall that the Japanese narrative tax series also includes local legislative tax changes). All series are tested stationary using an Augmented Dickey-Fuller test.

2.2 The econometric framework

With the narrative panel data set at our disposal, we can proceed by estimating the effects of tax changes on output directly using the local projection method as suggested by Jordà (2005). Specifically, letting $y_{i,t}$ denote the log of real GDP in country i and $\tau_{i,t}$ the narratively identified exogenous tax shock, we estimate the cumulative response of output in period $t+h$ by running the following OLS regressions:

$$\sum_{j=0}^{h} \Delta y_{i,t+j} = \beta_h \tau_{i,t} + \gamma_h X'_{i,t,h} + \alpha_{i,h} + \mu_{t,h} + \epsilon_{i,t,h}, \tag{1}$$

for $h = 0, 1, 2, ..., H-1$, with H the impulse response horizon. The coefficient β_h, which is assumed to be common across countries, measures the cumulative effect on output in period $t+h$ of a tax shock occurring at t. Recall that $\tau_{i,t}$ is the projected revenue change due to an exogenous change in taxes. Hence, β_h can naturally be interpreted as the cumulative tax multiplier in period $t+h$.

In Equation (1), $\alpha_{i,h}$ and $\mu_{t,h}$ capture the country- and time-fixed effects, respectively, while $\epsilon_{i,t,h}$ is the error term with $E[\epsilon_{i,t,h}] = 0$. $X_{i,t,h}$ is a vector of controls and includes four lags of $\Delta y_{i,t}$, which seems appropriate given the quarterly frequency of the data, and h leads of $\tau_{i,t}$, which are meant to account for the potential effects of intermediate tax shocks along the impulse response horizon (see Stock and Watson, 2018; Alloza et al., 2019).

One issue of local projection models is the residual serial correlation among the H

regressions. Additionally, since we estimate a panel fixed-effects model, serially correlated residuals across countries might exist. Hence, standard errors need to be robust to cross-country correlation.[3] To address these issues, we follow Auerbach and Gorodnichenko (2013) and use Driscoll and Kraay (1998) standard errors in all regressions. The maximum lag autocorrelation is set to H (as in Jordà, 2005).

In order to account for the potential state dependence of the effects of tax shocks, we modify (1) and interact the tax shock with a regime indicator function, denoted by $F(z_{i,t}) \in [0,1]$, that determines whether the economy is in one of two states of the business cycle: **r**ecession, labeled r, and **e**xpansion, labeled e:

$$\sum_{j=0}^{h} \Delta y_{i,t+j} = F(z_{i,t}) \left(\beta_h^r \tau_{i,t} + \gamma_h^r X'_{i,t,h} + \alpha_{i,h}^r\right) \\ + \left[1 - F(z_{i,t})\right] \left(\beta_h^e \tau_{i,t} + \gamma_h X'_{i,t,h} + \alpha_{i,h}^e\right) + \mu_{t,h} + \epsilon_{i,t,h}, \quad (2)$$

The regime indicator $F(z_{i,t})$ goes to 1 (0) if it is more likely that the economy is in a severe recession (strong expansion). Hence, whereas β_h in (1) represents the *unconditional* cumulative tax multiplier, β_h^r and β_h^e are the *state-dependent* cumulative tax multipliers.

Following Granger et al. (1993), we use a logistic function for $F(z_{i,t})$:

$$F(z_{i,t}) = \frac{\exp\left(-\theta_i \frac{z_{i,t}-c_i}{\sigma_{z_i}}\right)}{1+\exp\left(-\theta_i \frac{z_{i,t}-c_i}{\sigma_{z_i}}\right)}, \quad (3)$$

with $z_{i,t}$ a measure of economic activity, σ_{z_i} its standard deviation and c_i the threshold value that determines whether the economy is in a recessionary or expansionary phase of the business cycle. Since $F(z_{i,t})$ can take any value between 0 and 1, the model allows for a smooth transition between the two states, which is more appealing than using a binary dummy approach for the following reasons. First, using a dummy to split the data has the disadvantage of reducing the sample size and, thus, the degrees of freedom. Second,

[3] The post estimation test for cross-sectional independence, proposed by Pesaran (2004), shows evidence of cross-sectional correlation (results available upon request).

when $z_{i,t}$ lies close to the threshold c_i, the associated observations are not categorically treated as belonging to either one of the two states. Instead, these observations are assigned a probability weight for each possible state, thereby also acknowledging that the response variable is likely to behave similarly across states when $z_{i,t}$ is close to c_i. Third, using a smooth transition indicator allows the state of the economy to vary over the impulse response horizon. Hence, the impulse response functions generated by (2) are akin to generalized impulse response functions.

Following the majority of the literature (e.g. Tenreyro and Thwaites, 2016; Alesina et al., 2018; Ramey and Zubairy, 2018), we define $z_{i,t}$ as the seven-quarter lagged moving average of real quarterly GDP growth. We choose a lagged, rather than a centered, MA representation to avoid having leads of the dependent variable on the right-hand side of (1) and (2). Moreover, a lagged MA representation also avoids contemporaneous correlation between the shock and the business cycle indicator. Since the choice for $z_{i,t}$ is of paramount importance for the results, in the sense that it determines the prevalence of the economic regimes, we experiment with alternative business cycle indicators in Section 3.2.

Another important issue is the choice for c_i and θ_i, which govern the shape of the logistic function in (3). The parameter c_i is the threshold that marks the transition from the recession to the booming state, whereas θ_i determines how abruptly the economy transitions between the two states, with $F(z_{i,t})$ taking binary values as $\theta_i \to \infty$. There are various ways to go about calibrating these parameters. One way is to fix θ_i to be the same across countries and, following Tenreyro and Thwaites (2016), set c_i such that the implied probability of being in the recession state equals each country's 'recession ratio', x_i. The recession ratio is defined as the number of negative growth quarters divided by the length of the sample. Alternatively, following Alesina et al. (2018), one could set c_i equal to the mean of $z_{i,t}$ and calibrate θ_i for each individual country to match x_i with the implied probability of being in the recession state. As in Auerbach and Gorodnichenko (2012b), θ_i

Table 1: Recession ratios, x_i, and country-specific calibration of θ_i

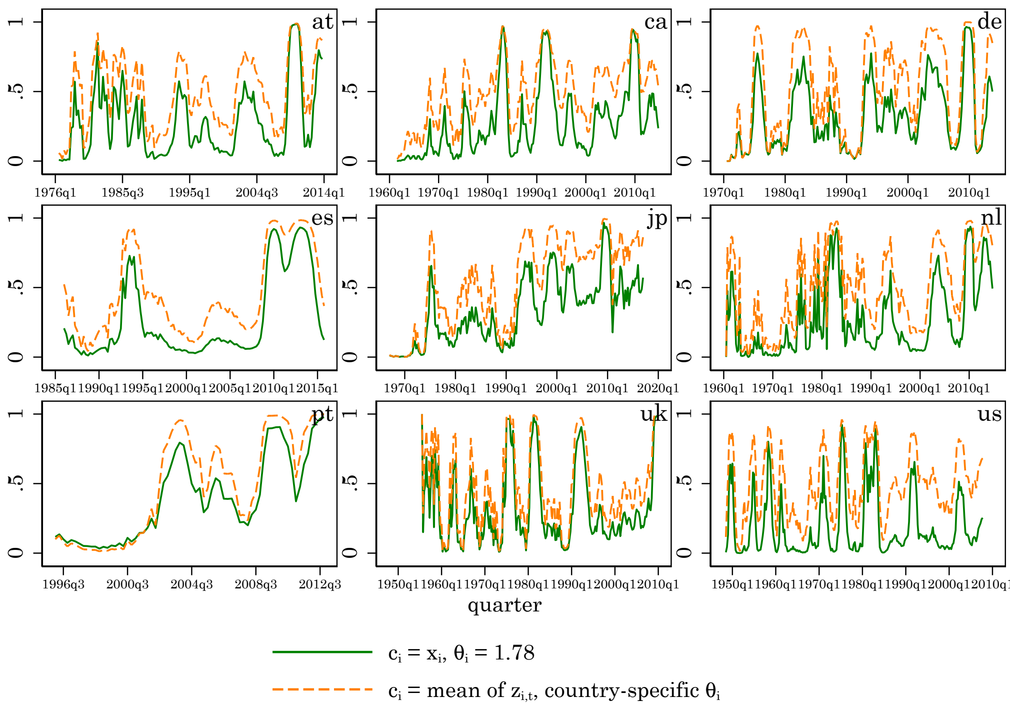
Country	x_i	θ_i
AT	0.17	1.44
CA	0.13	1.3
DE	0.27	2.80
ES	0.23	1.84
JP	0.22	2.29
NL	0.20	1.73
PT	0.25	2.80
UK	0.21	2.05
US	0.13	1.23

Notes: The recession ratio for country i is calculated as the number of negative growth quarters divided by the length of the corresponding sample. θ_i determines how abruptly the economy switches between the recessionary and expansionary states, see Equation (3).

is then calibrated such that each country spends on average 20% in the recessions state, i.e. such that $\mathbb{P}\left[F\left(z_{i,t}, \theta_i\right) > 0.8\right] = x_i$. Since both approaches rely on country-specific characteristics, the implied transition probabilities are equivalent across countries, which might not be the case if *both* θ_i and c_i were set to be the same for each country.

The values for θ_i resulting from the second calibration approach are reported in Table 1, along with each country's recession ratio. The average value of θ_i across the countries in our sample (weighted by the number of observations) is $\overline{\theta} = 1.78$, which lies in between the values used by Auerbach and Gorodnichenko (2013) and Tenreyro and Thwaites (2016), i.e. 1.5 and 3 respectively. Figure 2 plots the regime indicator $F\left(z_{i,t}\right)$ as derived under both calibration approaches, where for the first approach we set $\theta_i = \overline{\theta}$ for all i. Note that the regime indicator behaves less erratically when using country-specific values for θ_i (dashed lines). Nevertheless, both approaches suggest that the expansionary business cycle phase is most prevalent in all countries we consider (as one would expect). Furthermore, the recessionary state is generally somewhat short-lived. We use the first calibration approach for our baseline model and apply the second approach in one of our robustness checks.

Figure 2: Probability of being in a recession



Notes: The probability of being in a recession, $F(z_{i,t})$, is given by Equation (3). $z_{i,t}$ is a measure of the business cycle. c_i is the threshold value that demarcates the recessionary and expansionary states of the business cycle. θ_i determines how abruptly the economy transitions between the two states. The country-specific θ_i's, along with the recession ratio's x_i, are shown in Table 1.

2.3 Data

Our main dependent variable of interest is quarterly GDP. As a robustness check, we add a few additional control variables, i.e. government expenditures (to control for endogenous fiscal policy responses) and the three-month interbank rate (to control for the monetary policy stance). The GDP and government expenditure series are seasonally adjusted and transformed into real series using the GDP deflator. For the interest rate, we use the 3-month inter-bank rate for Germany, Spain and Portugal, and the 24th immediate inter-bank rate for Japan and Austria. For the US, we use the 3-month Treasury bill rate until 1950 and the Federal Funds rate from then onward. For the Netherlands, we use the ordinary end-of-day rate until 1999 and the ECB's marginal lending rate afterwards. Finally, for the UK, we use the overnight bank rate.

All data are obtained from the OECD Main Economic Indicator Database, except for data on the GDP deflator for Japan, Spain and Austria. For these countries, we follow the OECD methodology of combining two time series in order to obtain a longer time series for the GDP deflator. For Japan, we use data from the Japanese Cabinet's Office, while for Spain and Austria we use data from Datastream.

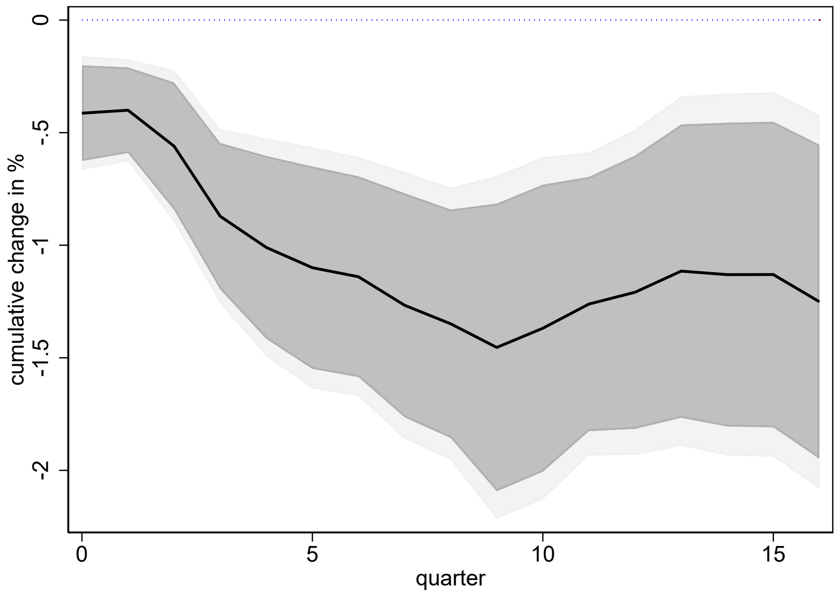
3 Results

This section presents our main results on both the unconditional and state-dependent cumulative tax multiplier. The first section discusses our benchmark results. In section 3.2, we perform a series of robustness checks.

3.1 The state-(in)dependent effects of tax shocks

Figure 3 shows the *unconditional* cumulative response of real GDP to a 1% tax increase in our panel of nine advanced economies, along with the 90% and 95% confidence intervals. We find that the tax shock leads to a significant contraction in output, already within the

Figure 3: Unconditional response of GDP to a 1% tax increase



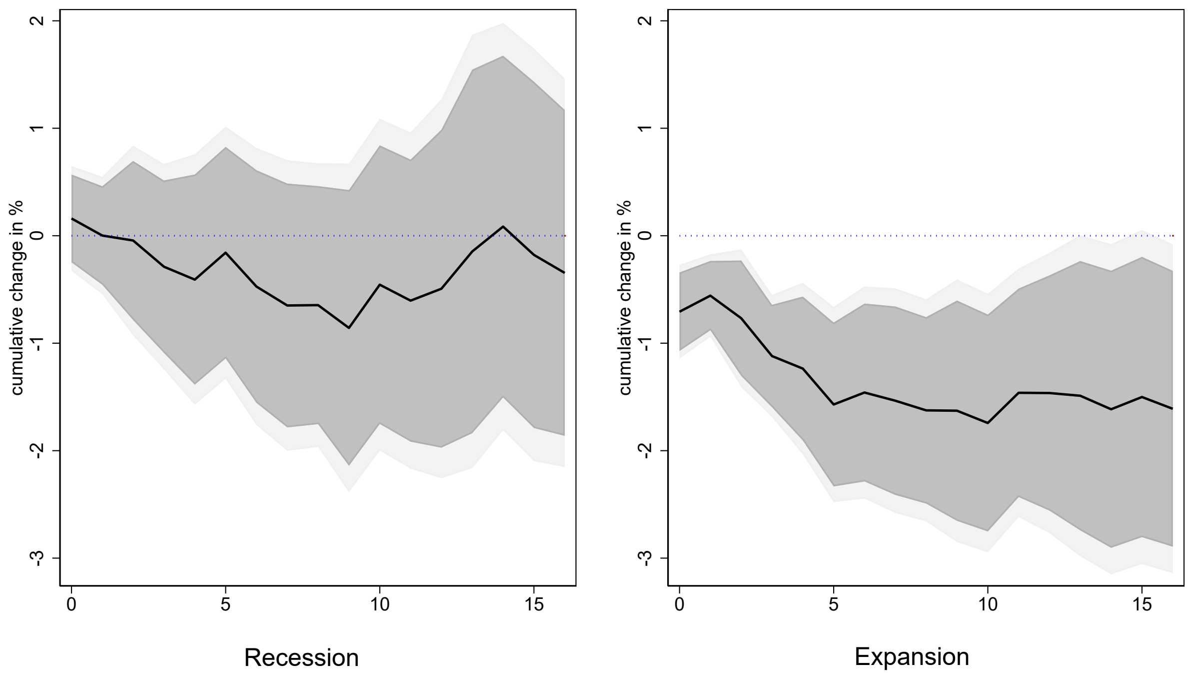
Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

first quarter. Particularly, we estimate a significant unconditional impact tax multiplier of -0.41. The cumulative tax multiplier then rises for about two years following the shock, where it peaks at -1.45. Subsequently, the effect of the tax shock starts to wane and GDP slowly recovers.[4] Importantly, we find that the output response remains significantly negative over the entire impulse response horizon of $H = 17$ quarters. Our estimate of the peak cumulative tax multiplier lies within the bandwidth of previously reported narrative estimates, with higher values shown for the US and the UK.

Figure 4 shows the *state-dependent* output response to a 1% tax increase, with the left (right) panel showing the response when the economy experiences a recession (expansion). The output response to the tax shock is strikingly different across the two regimes. During recessions, the cumulative output response has a similar hump-shaped pattern as in the unconditional case, yet is insignificant along the entire impulse response horizon. The path test for joint significance confirms this finding, as the null of no joint signifi-

[4] This inverted hump-shaped contraction of output is also found in Chahrour et al. (2012), who study the output response of tax innovations in the Blanchard-Perotti and DSGE models.

Figure 4: State-dependent response of GDP to a 1% tax increase



Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

cance over the entire horizon could not be rejected.[5] Recall that our smooth transition local projection model allows for the estimated output response to take into account regime switches along the horizon. Given that recessions tend to be relatively short-lived as compared to expansions (see Figure 2), we could expect the impact of a tax shock on output to be weaker at the longer end of the impulse horizon, as the probability weight of being in a recession is likely to fall as h increases. However, our results show that, even in the short run, output is statistically unresponsive to exogenous tax shocks when the economy faces a recession.

On the other hand, the right panel of Figure 4 shows that, during economic expansions, output contracts *significantly* following an exogenous tax increase. Compared to the unconditional case, we find a larger impact tax multiplier of -0.71 and a higher peak

[5]We conduct a χ^2 path test for the joint significance over the entire impulse response horizon, using seemingly unrelated regressions with clustered standard errors. Note that it is not possible to conduct this test with Driscoll and Kraay standard errors. We use pooled OLS with country dummies for this procedure instead of a fixed-effects estimator, because the test cannot be implemented otherwise. Nevertheless, the point estimates remain exactly the same.

Table 2: Cumulative response of GDP to a 1% tax increase

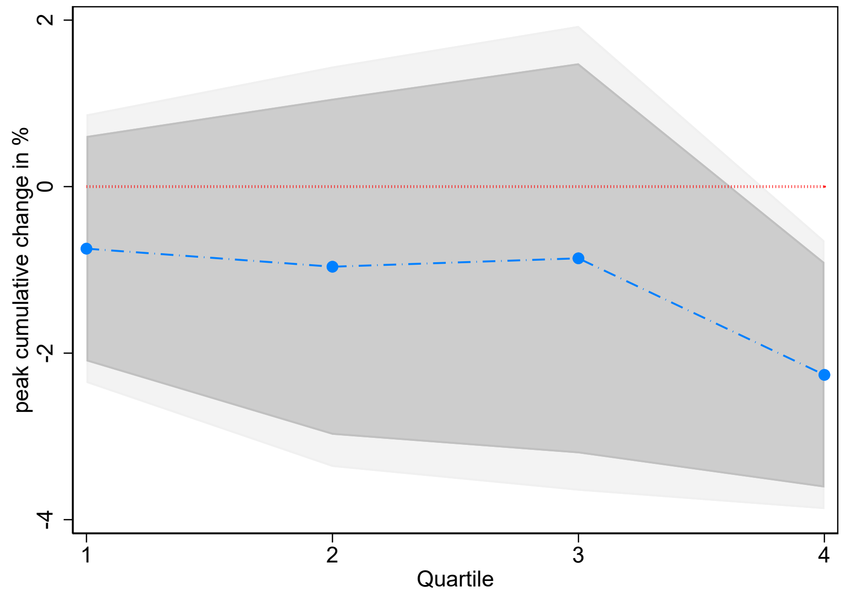
Quarter	Unconditional	Recession	Expansion
0	-0.41**	0.16	-0.71**
	(0.13)	(0.125)	(0.22)
4	-1.01***	-0.41	-1.24**
	(0.25)	(0.59)	(0.41)
8	-1.35***	-0.65	-1.63**
	(0.31)	(0.67)	(0.53)
12	-1.21**	-0.49	-1.47*
	(0.37)	(0.9)	(0.67)
16	-1.25**	-0.35	-1.61*
	(0.42)	(0.92)	(0.78)

Note: Standard errors in parentheses. *, **, *** indicate significance at 5%, 1% and 0.1%, respectively.

multiplier of -1.74 in the tenth quarter. Beyond this peak, output remains low but slowly recovers. The path test clearly rejects the null of no joint significance along the entire horizon. It thus seems that the results for the unconditional tax multiplier are driven mostly by the effects of tax shocks during expansions, rather than during recessions. Table 2 provides an overview of the results shown in Figures 3 and 4.

Another approach to illustrate the state dependence of tax shocks is to split the sample based on the quartiles of each country's GDP growth rate and then to run the linear model (1) for each of these sub-samples. Although a disadvantage of this approach is that one loses a large number of observations, it is a straightforward way to check whether the observed non-linearity in the tax multiplier does not stem from the smooth transition functions used in (2), but rather arises from differences in the propagation of fiscal shocks across the business cycle. Figure 5 shows the peak tax multiplier along the GDP growth rate distribution. These results reveal that, not only are tax multipliers significantly negative during expansions and insignificant during recessions, but also that the peak tax multiplier turns significant only when economic growth is at the far right of its distribution (see also Caggiano et al., 2015). When GDP growth falls within the last quartile, the peak output response to an exogenous tax increase is -2.26%. Across the first three quartiles, the peak output response is slightly below -1%, yet insignificant.

Figure 5: Peak tax multiplier conditional on GDP growth rate quartiles



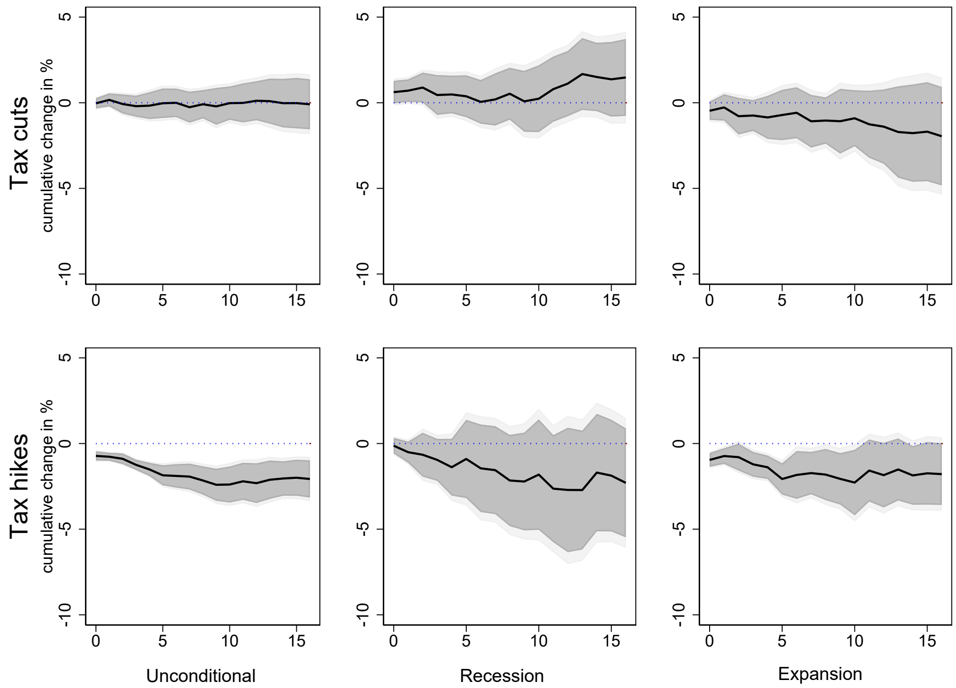
Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

So far, we ignored the *sign* of the tax shocks when investigating their effect on output and thus implicitly assumed the tax multiplier to be symmetric. However, it is plausible that GDP reacts differently to tax hikes as compared to tax cuts, which in turn may depend on the state of the business cycle. To determine whether the tax multiplier depends on the sign of the tax shock, we start by defining positive tax shocks as $\tau^{+}_{i,t} = \tau_{i,t}$ if $\tau_{i,t} \geq 0$ and analogously for negative tax shocks, $\tau^{-}_{i,t}$ (see Kilian and Vigfusson, 2011, for a similar approach). We then re-estimate the linear and state-dependent local projection models using each series separately.

The first row of Figure 6 shows the response of GDP to a positive tax shock, while the second row shows the response to a negative tax shock, both for the unconditional case (first column) and state-dependent case (second and third columns). We find that tax hikes have a significant contractionary impact on output during expansions, yet the effect on output is insignificant during recessions. The impact of tax cuts is, surprisingly, insignificant, regardless of the state of the business cycle.[6]

[6]These results echo those of Ghassibe and Zanetti (2019), who find that the tax cut multiplier is not

Figure 6: Unconditional and state-dependent response of GDP to a 1% tax shock: tax cuts versus tax hikes

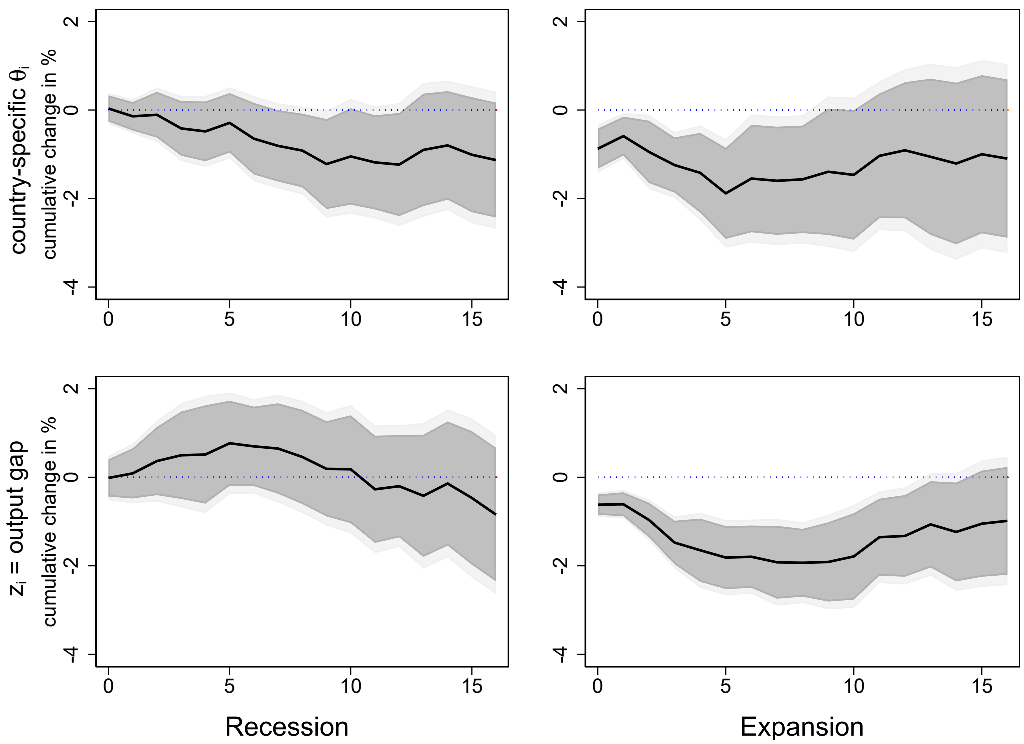


Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

The results shown in Figures 4 to 6 suggest that the conventional view on the effects of fiscal policy, i.e. that fiscal shocks have stronger effects during recessions than during expansions, does not seem to hold for tax changes. Instead, we find that tax shocks only affect GDP during economic expansions, and only if these shocks are positive. This result has important implications for the design of expansionary discretionary fiscal policy, aimed at addressing output shortfalls, and for the timing of fiscal consolidation policies, aimed at curtailing government indebtedness. In fact, governments' attempt to stimulate economic conditions during recessions through tax reductions may not be successful, whereas revenue-based fiscal consolidations are most costly in terms of output losses during expansions.

significantly different from zero in both expansions and recessions. However, when controlling for the source of business cycle fluctuations, they find that the tax cut multiplier is significant during a supply-driven recession.

Figure 7: State-dependent response of GDP to a 1% tax increase: alternative specifications of $F(z_{it})$



Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

3.2 Robustness analysis

3.2.1 Alternative specifications of the regime indicator

The results from the smooth transition local projection model depend on two key features of the regime indicator $F(z_{i,t})$: 1) the parameters c_i and θ_i, and 2) the measure of economic activity, $z_{i,t}$. Whereas in our baseline model, we fixed θ_i to be equal across the countries in our sample, we now take a different approach using country-specific values of θ_i (see Table 1 and Section 2.2 for a discussion on the calibration of the country-specific θ_i's). Next, following Ramey and Zubairy (2018), we use the output gap as an alternative variable, $z_{i,t}$, to capture the state of the business cycle. We calculate the output gap as the de-meaned and HP-filtered trend of GDP, using a smoothing parameter of 10,000 (see also Eskandari, 2019). Using the output gap rather than GDP growth as the $z_{i,t}$ variable not only helps us verify the robustness of our baseline results, but also addresses some of the concerns recently raised by Berge et al. (2020) that the state-dependent effects of fiscal shocks might also depend on whether the business cycle indicator is expressed in levels or in changes.[7]

Figure 7 shows the impulse responses of output following an exogenous tax shock under these alternative specifications of $F(z_{it})$. As before, we find that the impact and cumulative tax multipliers are insignificant during recessions (left column), yet significantly negative during expansions (right column). Moreover, when using the output gap as our measure of economic activity, we find a much larger peak multiplier of -1.93 in the eighth quarter of the economic boom, compared to our baseline estimate of -1.74. Furthermore, the difference between the output responses across business cycle states and the negative output response during expansions are much more pronounced as compared to our baseline results. Table 3 summarizes the results shown in Figure 7.

[7]We also experimented with alternative smoothing parameters to calibrate the HP-filter, different threshold parameters, c_i, and the unemployment gap as a measure of $z_{i,t}$. These results, which are available upon request, yield qualitatively similar results.

Table 3: Cumulative response of GDP to a 1% tax increase: alternative specifications for $F(z_{it})$

	Country-specific θ_i		z_{it} = **output gap**	
Quarter	Recession	Expansion	Recession	Expansion
0	0.03	-0.87**	-0.02	-0.62***
	(0.18)	(0.27)	(0.26)	(0.14)
4	-0.48	-1.42*	0.51	-1.65***
	(0.41)	(0.55)	(0.67)	(0.43)
8	-0.92	-1.57*	0.46	-1.93***
	(0.51)	(0.74)	(0.65)	(0.46)
12	-1.23	-0.91	-0.20	-1.33*
	(0.71)	(0.93)	(0.70)	(0.56)
16	-1.13	-1.10	-0.85	-0.98
	(0.79)	(1.09)	(0.92)	(0.74)

Note: Standard errors in parentheses. *, **, *** indicate significance at 5%, 1% and 0.1%, respectively.

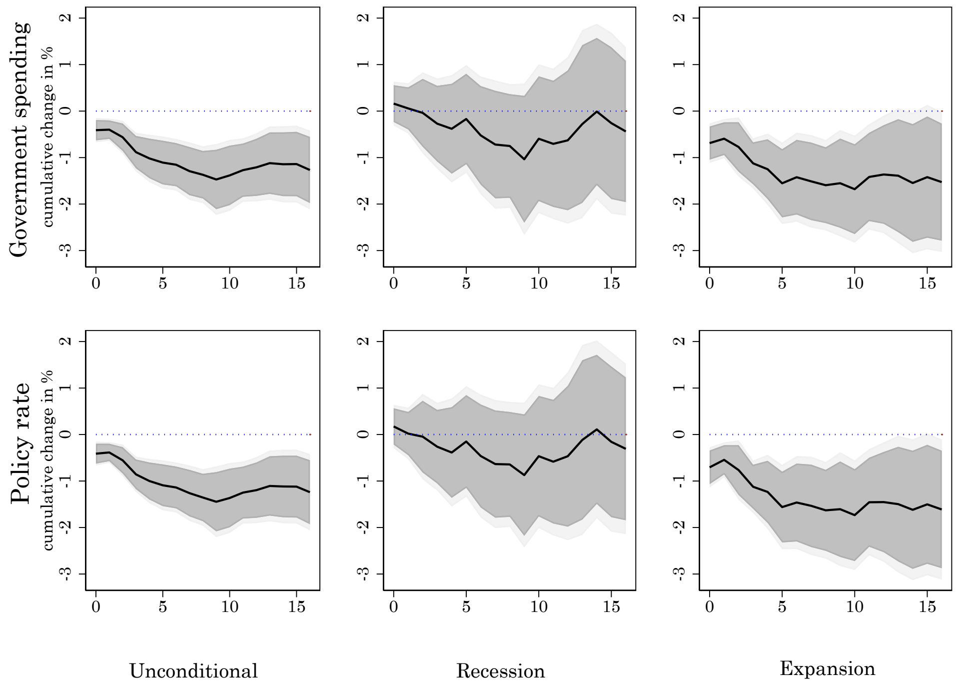
3.2.2 Alternative control variables and sample

Although the narratively identified tax shocks used in this study ought to be uncorrelated with GDP, they might move in tandem with government spending, e.g. if budgetary restrictions force policymakers to balance their budgets. As argued by RR, if this is the case and tax hikes were systematically associated with government spending increases, our estimates of the tax multiplier might suffer from an underestimation bias. Therefore, in our next robustness check, we control for the potential endogenous response of government spending by adding four lags of government consumption to our baseline specification.

Another endogeneity issue may arise from monetary policy: to the extent that central banks foresee and either aim to accommodate or counteract a tax hike, they may systematically adjust their policy interest rate around the period of the tax shock. To control for this possibility, we also estimate our model with four lags of the three-month interbank rate (or similar), as in Borio et al. (2017) and Ramey (2019).

Figure 8 shows the output responses to a 1% tax hike when controlling for government spending (top row) and monetary policy (bottom row). These results are very similar (in terms of point estimates and confidence intervals) to our baseline results and once again show that tax multipliers are significant only when the economy experiences a boom.

Figure 8: Unconditional and state-dependent response of GDP to a 1% tax increase: controlling for government spending and monetary policy



Notes: The dark (light) gray shaded areas reflect the 90% (95%) confidence interval.

Table 4 summarizes these results. The similarities in the results shown in Figures 4 and 8 suggest that governments do not generally face (binding) balanced budget rules, nor that tax shocks are strongly correlated with monetary policy.

As a followup exercise, we also investigate the robustness of our results by omitting countries from our sample. In particular, we iteratively remove one country from the sample and re-estimate the model to ensure that our results are not driven by one country in particular. Table 5 shows the impact and peak tax multipliers across the different business cycle states when one of the countries is excluded from the sample. In all cases, we find that both the impact and peak tax multipliers are significantly negative when economies experience a boom, yet are insignificant during recessions. The sizes of the (state-dependent) tax multipliers are also very similar to our baseline findings. Hence, it does not seem likely that our results are determined by any country-specific dynamics.

Table 4: Cumulative response of GDP to a 1% tax increase:
controlling for government spending and monetary policy

	Controlling for...					
	government spending			**monetary policy**		
Quarter	Unconditional	Recession	Expansion	Unconditional	Recession	Expansion
0	-0.41**	0.16	-0.69**	-0.41**	0.17	-0.71**
	(0.13)	(0.24)	(0.22)	(0.13)	(0.24)	(0.22)
4	-1.02***	-0.38	-1.25**	-1.00***	-0.39	-1.23**
	(0.26)	(0.59)	(0.39)	(0.24)	(0.59)	(0.41)
8	-1.37***	-0.75	-1.59**	-1.35***	-0.64	-1.63**
	(0.31)	(0.68)	(0.50)	(0.31)	(0.69)	(0.53)
12	-1.21***	-0.63	-1.36*	-1.20**	-0.47	-1.45**
	(0.37)	(0.91)	(0.64)	(0.36)	(0.92)	(0.66)
16	-1.27**	-0.44	-1.53*	-1.24**	-0.31	-1.61*
	(0.43)	(0.92)	(0.77)	(0.42)	(0.93)	(0.77)

Note: Standard errors in parentheses. *, **, *** indicate significance at 5%, 1% and 0.1%, respectively.

Table 5: Impact and peak cumulative response of GDP to a 1% tax increase:
removing one country from the estimation sample

	Unconditional		Recession		Expansion	
Omitted country	Impact	Peak	Impact	Peak	Impact	Peak
Austria	-0.42**	-1.53***	0.18	-0.86	-0.73**	-1.79 **
	(0.13)	(0.40)	(0.25)	(0.82)	(0.24)	(0.67)
Canada	-0.36**	-1.43**	0.20	-0.78	-0.67**	-1.64 *
	(0.13)	(0.46)	(0.30)	(0.85)	(0.24)	(0.78)
Germany	-0.42**	-1.48***	-0.22	0.13	-0.75**	-1.90 **
	(0.16)	-0.45	(0.77)	(0.26)	(0.25)	(0.67)
Spain	-0.45**	-1.48***	0.13	-0.83	-0.74***	-1.81 **
	(0.14)	(0.40)	(0.26)	(0.77)	(0.22)	(0.67)
Japan	-0.44**	-1.45***	0.22	-1.05	-0.73**	-1.49**
	(0.13)	(0.32)	(0.26)	(0.84)	(0.23)	(0.61)
Netherlands	-0.32***	-1.13**	0.13	-1.50	-0.58***	-1.19 **
	(0.09)	(0.37)	(0.32)	(0.94)	(0.17)	(0.43)
Portugal	-0.45**	-1.55**	0.25	-0.92	-0.70**	-1.69**
	(0.17)	(0.47)	(0.31)	(1.09)	(0.24)	(0.66)
United Kingdom	-0.39**	-1.61***	0.17	-0.51	-0.72**	-2.43**
	(0.12)	(0.42)	(0.25)	(0.82)	(0.24)	(0.65)
United States	-0.46**	-1.43**	-0.06	-0.91	-0.66**	-1.78*
	(0.16)	(0.48)	(0.23)	(0.89)	(0.24)	(0.69)

Note: Standard errors in parentheses. *, **, *** indicate significance at 5%, 1% and 0.1%, respectively.

4 Conclusion

As countries worldwide struggle with the devastating economic effects of the pandemic, and since monetary policy is expected to remain constrained by the effective lower bound for some time to come, fiscal policy has been called for to carry much of the burden of macroeconomic stabilization. Although the potency of expenditure-based fiscal stimulus has been studied extensively in the literature, much less is known about the effectiveness of tax changes and, in particular, how the tax multiplier varies across the business cycle.

In this paper, we fill this gap and provide new empirical evidence on the state dependence of the tax multiplier. For our analysis, we use a harmonized panel data set of narratively identified exogenous tax changes for nine advanced economies and a smooth transition local projection model to assess the impact of these tax changes on output conditional on the business cycle. Our main finding is that the output response to a rise in taxes is significantly and persistently negative, *yet only during economic expansions*. When economies go through a recessionary phase, the tax multiplier is insignificant, both in the short- and long run. Hence, whereas government spending multipliers have been found to be highest when times are bad, tax multipliers appear highest when times are good. A number of robustness checks verifies this (somewhat surprising) result.

Two other results deserve summarizing. First, when conditioning the tax multiplier on GDP growth quartiles, we find that the peak output response to a tax increase is significantly negative only when the GDP growth rate falls within the highest quartile. This result suggests that only in times characterized by very strong economic growth are tax shocks able to affect macroeconomic conditions. Second, in addition to tax multipliers being strongly state dependent, we also find them to be asymmetric. In particular, during economic booms, only tax hikes elicit a significant (negative) response of output, whereas the latter is unresponsive to tax cuts, regardless of the business cycle state.

Our results provide a key input into the design of fiscal stabilization policies and for the timing of fiscal consolidations. Furthermore, they require one to rethink the transmission channel of tax shocks under alternative economic conditions within popular theoretical models, such as the New Keynesian paradigm. We leave this important endeavor for future research.

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A Additional tables

Table 6: Narratively identified exogenous tax series: sources and statistics

Country	Source	Time period	Obs.	Mean	Min	Max	SD
Austria	Kilic (2012)	1976Q1-2013Q4	152	0.02	-0.74	0.91	0.14
Canada	Hussain and Liu (2019)	1961Q1-2014Q4	216	0.00	-0.70	1.90	0.18
Germany	Hayo and Uhl (2013), Gechert et al. (2016)	1974Q1-2013Q4	176	0.01	-1.27	0.95	0.28
Japan	Kato et al. (2018)	1967Q1-2017Q1	201	-0.01	-0.57	0.75	0.14
Netherlands	Geenen (2017)	1960Q1-2014Q4	220	-0.03	-1.44	0.65	0.19
Portugal	Pereira and Wemans (2015)	1996Q1-2012Q4	68	0.01	-1.19	0.98	0.2
Spain	Gil et al. (2019)	1986Q1-2015Q4	120	0.06	-2.72	3.30	0.65
United Kingdom (∗)	Cloyne (2013)	1948Q1-2009Q4	248	-0.06	-1.68	1.60	0.25
United States	Romer and Romer (2010)	1948Q1-2007Q4	240	-0.03	-1.87	0.70	0.24

(∗) In our estimations, we use the series starting in 1955Q1 due to data availability.

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