The Government Spending Multiplier at the Zero Lower Bound: International Evidence from Historical Data*

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Abstract

Based on a large historical panel dataset, this paper provides robust evidence that the government spending multiplier is significantly higher when interest rates are at, or near, the zero lower bound. We estimate multipliers that are as high as 1.5 during ZLB episodes, but small and statistically indistinguishable from zero during normal times. Our results are robust to different definitions of zero lower bound episodes, alternative ways of identifying government spending shocks, controlling for the exchange rate regime and other potentially important state variables. In particular, we show that the difference in multipliers is not driven by multipliers being higher during periods of economic slack.

Keywords: Government spending multiplier, zero lower bound, local projections.

JEL classifications: E32, E62, E65.

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1 Introduction

In this paper, we provide robust empirical evidence that the government spending multiplier is considerably larger when short-term nominal interest rates are at, or near, the zero lower bound (ZLB). Using a large historical international dataset, we estimate multipliers that are as high as 1.5 during ZLB episodes, but small and statistically indistinguishable from zero during normal times.

The Great Recession brought renewed attention to the question of the effectiveness of government spending in stimulating aggregate economic activity. The revival of fiscal stimulus is fueled by the fact that monetary policy in many countries has been at its maximal stimulus in terms of its conventional tool, the short-term nominal interest rate. The textbook New Keynesian model makes a case for government spending expansions when monetary policy is constrained by the ZLB. It predicts that the impact of a spending expansion is considerably larger at the ZLB than in normal times, resulting in a government spending multiplier above unity – meaning that per dollar of stimulus spending, aggregate output increases by more than one dollar (see, e.g., Christiano, Eichenbaum, and Rebelo 2011 and Eggertsson 2011). Other theoretical contributions question this prediction by showing that modifications to the textbook model may lead to multipliers being lower at the ZLB (see, e.g., Mertens and Ravn 2014, Braun, Korber, and Waki 2013, and Kopiec 2019). In sum, as the theoretical literature provides us with ambiguous results, an empirical evaluation of the effectiveness of spending expansions at the ZLB is needed.

However, providing empirical evidence on the magnitude of government spending multipliers when short-term interest rates are at, or near, the ZLB is a difficult task because ZLB periods are unusual and extremely scarce situations. Two strategies to address the limited number of observations are proposed. First, Nguyen, Sergeyev, and Miyamoto

(2017) rely on the Japanese experience of the prolonged ZLB episode since the mid-1990s. Second, Ramey and Zubairy (2018) use historical data on the U.S. economy to enlarge the number of observations during which the ZLB was binding. These studies provide mixed evidence on the effectiveness of fiscal policy during ZLB episodes. While Nguyen, Sergeyev, and Miyamoto (2017) show that the government spending multiplier is amplified during ZLB episodes, Ramey and Zubairy (2018) do not find that the multiplier is generally larger when interest rates are near the ZLB. In this paper, we follow the route suggested by Ramey and Zubairy (2018), but make use of a large historical *international* dataset and provide robust evidence that the government spending multiplier is larger during ZLB periods than during normal times.[1]

We compile a balanced panel dataset of 13 advanced economies from 1917 through 2016 by using data from Jordà, Schularick, and Taylor (2017)'s Macrohistory Database, the Correlates of War Project, and the SIPRI Military Expenditure Database. We detect 116 episodes, approximately 9% of our sample, as episodes in which short-term interest rates were at, or near, the ZLB. We estimate state-dependent government spending multipliers using the local projection - instrumental variable approach, which builds on Jordà (2005). The responses are allowed to vary depending on whether or not interest rates are at, or near, the ZLB. Identification of government spending shocks stems from variations in military spending that cannot be explained by lags of output, government spending, and other variables. We implement this identification strategy by instrumenting government spending by military spending. In doing so, we control for the direct effects of military conflicts to account for the confounding factors associated with war episodes. We show

[1] In related work, Bonam, de Haan, and Soederhuizen (2017) estimate government spending multipliers at the ZLB for a panel of advanced economies, as we do, but rely on a much shorter time period (1960-2015), thus implying that ZLB periods are mainly detected during and in the aftermath of the Great Recession. Relative to our approach of using a rich historical dataset, relying on this case makes it more difficult to separate evidence of the ZLB episode from that of the Great Recession. Case studies from the Great Depression are provided by Crafts and Mills (2013) and Ramey (2011b).

that military spending is a reasonably strong instrument, passing relevance tests in ZLB and normal periods.

We find that multipliers during ZLB periods are considerably larger than multipliers during normal times. When the economy is stuck at the ZLB, multipliers take values around 1.5, whereas multipliers are estimated to be small and statistically indistinguishable from zero during normal times. We show that our results are a robust feature of the data by conducting a series of robustness checks including using alternative definitions of ZLB states and identifying government spending changes by restricting the contemporaneous response of government spending to economic activity. Moreover, we verify that our main result does not depend on special circumstances during war episodes. We also show that our main result holds when we control for alternative state variables that may influence fiscal multipliers such as the amount of economic slack, the presence of financial crises, the prevailing exchange rate regime, the level of government debt, or the degree of openness of the economy.

We present some evidence that supports the theoretical predictions of the textbook New Keynesian model of an economy subject to the zero lower bound. In particular, at the ZLB, an increase in government spending leads to an increase in inflation, a decrease in the real interest rate and a significant crowding-in of private consumption. Yet, there is also evidence that is at odds with the predictions of the textbook New Keynesian model. In particular, we find that the nominal (and real) short-term policy rate does not increase in normal times. Importantly, though, we show that other than short-term policy interest rates behave differently depending on the stance of monetary policy. Longer-term interest rates, which might proxy for agents' expectations about future monetary conditions, increase significantly after the fiscal shock during normal times, whereas they tend to decline during ZLB periods. This suggests that agents expect a future tightening of monetary conditions in normal times, whereas they anticipate a prolonged period of low monetary policy rates when the policy rate is already low. Furthermore, we provide evidence in favor of two alternative channels that may rationalize our finding of a larger fiscal multiplier in periods of constrained monetary policy. First, a rise in government spending leads to a strong and persistent increase in the rate of employment when monetary policy is constrained, which, according to the model by Rendahl (2016), may lead to a rise in future output that feeds back to a rise in present consumption. By contrast, the employment effect of the fiscal intervention is only marginal during normal times. Second, the fiscal stimulus itself shows a higher degree of persistence when interest rates are at, or near, the ZLB compared to periods of unconstrained monetary policy. It was shown that output multipliers increase with the persistence of government spending (see, e.g., Baxter and King 1993 and Dupaigne and Fève 2016).

Despite the described advantages of relying on historical panel data, our approach comes with some caveats. Our empirical analysis is conducted by pooling observations for a number of countries over a long historical sample period. While this procedure considerably increases the number of degrees of freedom, it imposes homogeneity across countries and stability in the relationship among variables over time. To reduce the amount of heterogeneity, we control for specific country characteristics and common macro shocks by including country and time-fixed effects into the regressions. Overall, our estimates capture average effects across countries and times. Given the scarcity of ZLB episodes, this disadvantage is offset by the rich historical dataset that enables inference based on more than 100 episodes of interest rates at, or near, the ZLB.

The rest of the paper is organized as follows. Section 2 describes the data, the definition of ZLB states, and the empirical strategy. Section 3 presents our main findings concerning the size of the government spending multiplier during ZLB episodes and during normal times. It also presents state-dependent impulse responses of key macroeconomic variables to government spending shocks as well as the results of several robustness checks. In Section 4, we discuss the role of other state variables that may influence the size of fiscal multipliers. Finally, Section 5 concludes.

2 Methodology and Data

In what follows, we describe the data, our definition of ZLB periods, our strategy to identify government spending shocks, and our empirical specification for estimating state-dependent government spending multipliers.

2.1 Data

We compile a balanced panel dataset covering 13 countries for the period 1917-2016, resulting in around 1300 observations. The beginning and the end of the sample are restricted by the data availability for some countries. The countries included are Belgium, Denmark, Finland, France, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and the United States.

We use different sources to collect all necessary data series. Our primary data source is the Jordà-Schularick-Taylor Macrohistory Database (Jordà, Schularick, and Taylor 2017). This database covers several advanced economies with annual series going back until the 19th century and ending in 2016.[2] The variables we use in our baseline specification are: real GDP per capita, real government spending per capita (constructed as govern-

[2]More details on the data collection and original sources of the Macrohistory Database can be found at `http://www.macrohistory.net/JST/JSTdocumentationR2.pdf`.

ment expenditures, deflated with the consumer price index and divided by population), a short-term nominal interest rate, consumer price inflation, and the exchange rate (measured in local currencies relative to the US-dollar); see the Appendix for details on data construction.

Military expenditure data are taken from the National Material Capabilities database of the Correlates of War Project and the SIPRI Military Expenditure Database. The National Material Capabilities database contains series for military expenditures in US dollars of several economies from 1917 to 2012. The SIPRI database contains annual time series on military expenditures in US dollars of multiple countries starting in 1949. We merge both series to construct time series on military expenditures in US dollars of the sample countries for the period 1917–2016. We use the exchange rate series provided by the Macrohistory database to convert the military expenditure series into local currencies. We construct a war dummy by merging the Correlates of War Project war database with the UCDP/PRIO Armed Conflict Dataset. The dummy takes the value one when a country is involved in a military conflict.

There is an obvious advantage of relying on historical panel data for our study. With the exception of the period following the Great Recession, ZLB periods are scarce in the post-WWII data that typically underlies macroeconometric analyses. This limits the validity of estimates based on these samples. Long samples of historical data overcome this challenge since the data series cover many more periods of constrained monetary policy, which provides a reasonable number of observations for conducting inference.

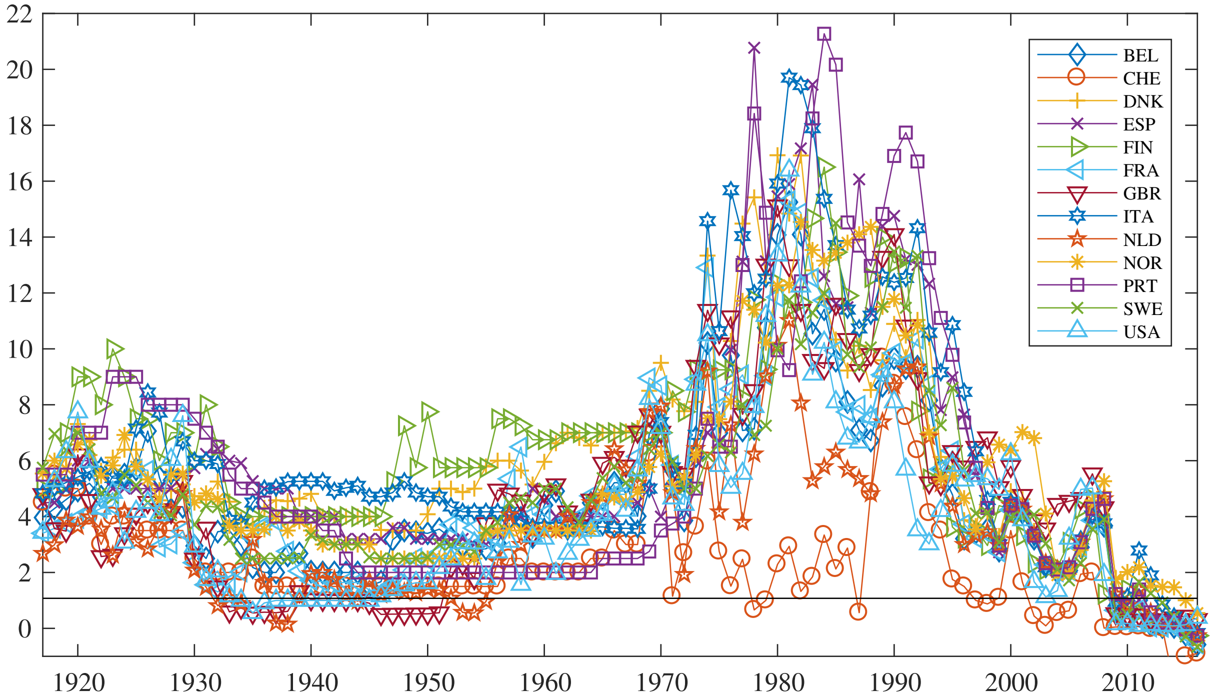
2.2 Definition of ZLB States

The Macrohistory Database does not provide central bank policy interest rates but offers information on other related interest rates with short maturities (e.g., Treasury bill rates or money market rates). We use this country-specific short-term interest rate series to proxy the stance of monetary policy. As our baseline, we define ZLB periods as those episodes when the short-term interest rate is smaller than, or equal to, one percent, following Bonam, de Haan, and Soederhuizen (2017). Our definition may include periods during which the ZLB is not strictly binding, but rather captures a low-interest-rate environment implying that the ability of the monetary authority to change its central instrument is limited.

To put this into perspective, our definition implies that the U.S. economy was at the ZLB from 1934 to 1945 and from 2009 to 2016; whereas Ramey and Zubairy (2018) identifies as ZLB episodes the quarters 1932q2-1951q1 and 2008q4-2015q4. While the two definitions detect the same periods for the recent past, our one percent threshold is more conservative than Ramey and Zubairy (2018)'s definition of ZLB periods during the late 1940s and early 1950s. Moreover, our definition implies an episode of constrained monetary policy in Great Britain from 1933 to 1938 which corresponds with the description by Crafts and Mills (2013) who describe the British economy during the mid-1930s' as an economy in which interest rates were at the zero lower bound for a prolonged period.

Figure 1 provides a comprehensive picture of the identified ZLB episodes. It shows the evolution of short-term interest rates for all countries in our panel from 1917-2016. The horizontal line shows the threshold value for the interest rate that is used to define ZLB states. As seen in the figure, the identified ZLB episodes are mainly clustered around two economic crises and their aftermath, the Great Depression and the Great Recession. At the end of the sample, the figure thus clearly illustrates the sharp monetary policy responses to the 2008 crisis in most of the countries. Overall, our definition implies that 116 periods, or approximately 9% of our sample, are defined as ZLB periods, while the

Figure 1: Short-term interest rates and ZLB episodes.



remaining periods are considered to be non-ZLB periods. The average duration of a ZLB episode is 3.74 years, with a standard deviation of 2.91. Thus, a ZLB state typically lasts for several years contrary to regular recessions which are generally more short-lived. In a later section, we will investigate in more detail the differences between economic slumps and ZLB periods and disentangle the effect of the ZLB on fiscal multipliers from the effect of states of the business cycle. Moreover, we will check the robustness of our results to alternative definitions of ZLB periods, including considering a smooth transition between both regimes.

2.3 Identification

Our baseline identification strategy rests on the assumption that changes in military spending are exogenous to economic conditions. Hall (2009), Barro and Redlick (2011), Ramey (2011b), and Miyamoto, Nguyen, and Sheremirov (2019), amongst others, also use military spending data to identify exogenous government spending shocks. This strategy presumes that changes in military spending respond to foreign policy developments, but are not driven by the state of the economy like the state of the business cycle, the monetary policy stance or financial conditions. A potential concern is that military spending changes are correlated with war episodes and that wars are likely to have a direct effect on economic activity. In fact, our sample period includes several armed conflicts – most prominently, World War II. These episodes often coincide with confounding variation associated with aerial bombing, labor shortages, price controls, and other economic shocks. To capture this, we include a war dummy in our regressions that takes the value one when a country is involved in a military conflict.

As Table 1 shows, the data is well suited to study the effects of government spending. Importantly, there is a positive and statistically significant correlation between total government spending growth and military spending growth (column 4), which implies that other components of government spending do not completely offset military spending changes.[3] On average across the full sample, the correlation is 0.36.[4] The average correlation of total government spending growth and military spending growth is larger during ZLB periods than during normal times. Importantly, though, the correlation is statistically significant in both regimes, which supports the choice of military spending as a relevant instrument in both states of the world. Note that we present a formal test of the instrument relevance in the following section. Furthermore, outside and at the ZLB, military spending is highly volatile (column 1). It is, on average, two times more volatile than government spending (column 2) and around four times more volatile than

[3]The p-value on the correlation coefficient, not reported here, is close to zero across the whole sample, during ZLB episodes and during normal times.

[4]This is comparable to the result presented by Miyamoto, Nguyen, and Sheremirov (2019) who use military expenditures to instrument exogenous government spending changes in a large panel of advanced and developing countries.

Table 1: Descriptive statistics.

	Standard deviation				
	Δg^m (1)	Δg (2)	Δy (3)	$\rho(\Delta g^m, \Delta g)$ (4)	Obs. (5)
Full sample	16.92	8.45	3.47	0.36	$1,300$
Outside ZLB	17.36	8.49	3.56	0.34	$1,184$
ZLB	14.12	7.56	2.37	0.69	116

Notes: Columns (1)-(3) summarize the standard deviations of the growth rates of military spending, government spending, and output, averaged across countries. Column (4) reports the correlation of military-spending and government-spending growth rates. Column (5) shows the number of observations.

output (column 3), which helps estimate the effects of government spending shocks more precisely.[5]

2.4 Econometric Specification

We estimate state-dependent government spending multipliers using the local projection - instrumental variable approach that builds on Jordà (2005) and is applied in the fiscal policy literature by, among others, Ramey and Zubairy (2018) and Nguyen, Sergeyev, and Miyamoto (2017). In particular, we are interested in the dynamics of the cumulative multiplier, which measures the cumulative change in GDP relative to the cumulative change in government spending from the time of the government spending innovation to a reported horizon h, where h captures the time dimension, years in our case. Following Ramey and Zubairy (2018), we estimate a series of regressions for the cumulative multiplier at each horizon $h = 0, \dots, 4$:

$$\begin{aligned}\sum_{j=0}^{h} \frac{Y_{i,t+j} - Y_{i,t-1}}{Y_{i,t-1}} = I_{i,t-1} &\left[M_h^A \sum_{j=0}^{h} \frac{G_{i,t+j} - G_{i,t-1}}{Y_{i,t-1}} + \phi_h^A(L) X_{i,t-1} + \gamma_h^A w_{i,t} \right] \\ &+ (1 - I_{i,t-1}) \left[M_h^B \sum_{j=0}^{h} \frac{G_{i,t+j} - G_{i,t-1}}{Y_{i,t-1}} + \phi_h^B(L) X_{i,t-1} + \gamma_h^B w_{i,t} \right] \\ &+ \nu_{i,h} + \delta_{t,h} + \psi_1 t + \psi_2 t^2 + \varepsilon_{i,t+h} \ , \qquad (1)\end{aligned}$$

[5]The growth rates of military spending, government expenditures and output are shown in the Appendix.

where $\frac{Y_{i,t+j}-Y_{i,t-1}}{Y_{i,t-1}}$ is the change in real per capita GDP in country i between time $t-1$ and time $t+j$ and $\frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}$ is the change in real per capita government spending in country i between time $t-1$ and time $t+j$, both divided by real per capita GDP in $t-1$.

We instrument the cumulative change in government expenditures, $\sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}$, with $\frac{G^m_{i,t}-G^m_{i,t-1}}{Y_{i,t-1}}$, where $G^m_{i,t}$ is real per capita military spending. To control for the direct effects of military conflicts on economic activity, such as aerial bombing, labor shortages, price controls, or other macroeconomic shocks, we include a war dummy, $w_{i,t}$, which takes a value of 1 when country i is involved in a military conflict at time t and 0 otherwise. The parameters $\nu_{i,h}$ and $\delta_{t,h}$ represent country and time fixed effects and are included into the regressions to control for country-specific characteristics and common macro shocks. $X_{i,t}$ is a vector of control variables, and t and t^2 are linear and quadratic time trends. In our baseline specification, the vector of control variables includes three lags of real per capita GDP, real per capita government spending, real per capita military spending, the exchange rate (all in logs), consumer price inflation, and the short-term interest rate. The latter variables control for the conduct of monetary policy that is found to shape the macroeconomic effects of fiscal policy in general (see, e.g., Canova and Pappa 2011 and Davig and Leeper 2011).

A potential obstacle for estimating the effects of fiscal shocks is the so-called fiscal foresight problem. It arises when private agents not only react to actual spending increases, but to breaking news about impending future spending plans. In this case, the econometrician cannot recover the true unexpected spending shock because the agents' and the econometrician's information sets are misaligned (Leeper, Walker, and Yang 2013). The literature proposes different solutions for the fiscal foresight problem. One is to include a fiscal news variable in the empirical model that captures anticipated changes in government spending (see, e.g., Ramey 2011b and Fisher and Peters 2010). Another approach to account for fiscal foresight is to add a series of professional forecasts of government spending to the set of control variables (see, e.g., Auerbach and Gorodnichenko 2012). Both approaches are not feasible in our case because the required information is not available for our historical sample.

The literature, though, suggests other approaches for addressing the anticipation problem. One is to use annual data, as we do (see, e.g., Beetsma and Giuliodori 2011 and Ramey 2011b). The argument goes that it is less likely that policy shocks are anticipated one year in advance than one quarter before. A further approach to deal with the anticipation problem is to include forward-looking variables as controls, because those variables may capture information about future fiscal policy actions (see, e.g., Yang 2007, Forni and Gambetti 2010, and Beetsma and Giuliodori 2011). We follow this route. In particular, the vector of control variables $X_{i,t}$ includes the log of the exchange rate following the suggestion of Forni and Gambetti (2010) to include a financial market variable that, due to its forward-looking nature, helps to account for fiscal foresight. Yang (2007) shows that short-term interest rates and prices, which are included in our baseline vector of control variables, also include information about future fiscal policy shocks.

The dummy variable $I_{i,t}$ captures the state $\{A, B\}$ of the economy prior to the government spending shock, where $I_{i,t} = 1$ if the monetary authority is constrained by the ZLB. We include a one-period lag of $I_{i,t}$ in the regressions to minimize contemporaneous correlations between fiscal shocks and the state of the economy. Given our specification, M_h^A provides an estimate of the cumulative government spending multiplier during ZLB episodes, whereas M_h^B provides the cumulative multiplier during normal times. Note that the responses incorporate the average transition of the economy from one state to another.

Table 2: First-stage estimates.

	Government spending	
	ZLB	Outside ZLB
Military spending	0.093***	0.234***
	(0.021)	(0.051)
First stage F statistic	19.479	21.370
Observations	116	1,184

Notes: Estimation results of first-stage regression. Driscoll-Kraay standard errors are reported in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

In other words, if the government spending shock affects the stance of monetary policy, this effect is then absorbed into the estimated coefficients M_h^A and M_h^B.

We construct standard errors using the Driscoll and Kraay (1998) correction, which takes into account heteroskedasticity as well as serial and cross-sectional correlation. Moreover, the standard errors in Equation (1) are adjusted in order to take into account instrument uncertainty.

Before we turn to our main results, we present the results of the first-stage regression in order to test the relevance of our instrument. Table 2 shows the results of the regression of changes in total government spending on changes in military spending (both normalized by lagged output), interacted with the ZLB dummy. The regression includes all control variables used in the second stage: country and time fixed effects, a linear-quadratic time trend, the war dummy, as well as three lags of real per capita GDP, real per capita government spending, real per capita military spending, the exchange rate, consumer price inflation, and the short-term interest rate. As shown in Table 2, in both states, the first-stage F-statistic is around 20, suggesting that weak instruments are unlikely to be a concern for our analysis. The effect of military spending on total government spending tends to be larger during ZLB periods than during normal times. More importantly, though, the coefficient is highly significant in both states, which suggests that the relevance condition on the instrument is fulfilled.

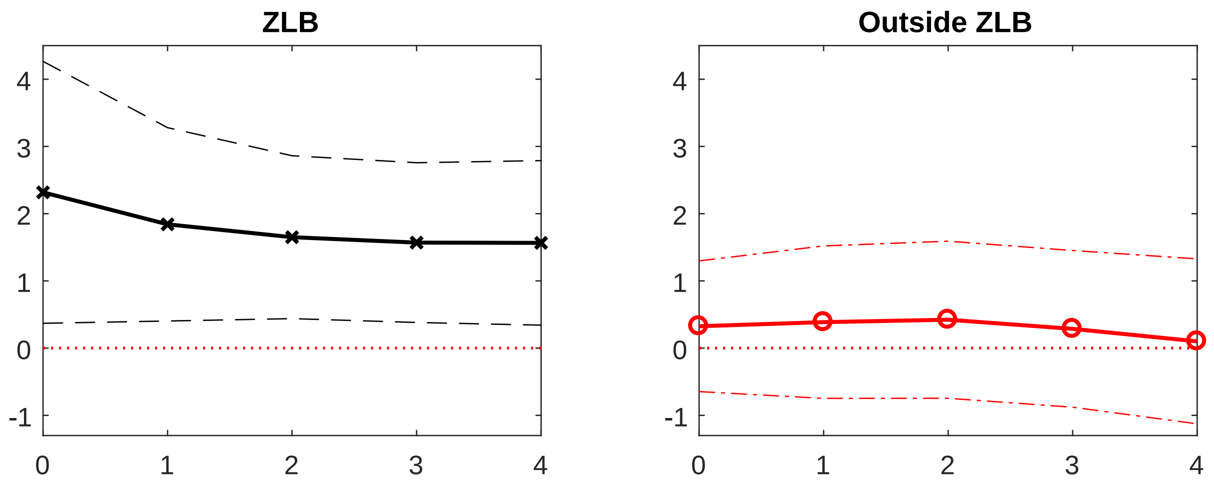
3 Results

In this section, we first present the output multiplier estimates of our baseline specification. Afterwards, we estimate and discuss state-dependent impulse responses of exogenous innovations in government spending. Finally, we show that our main result of a larger multiplier during ZLB episodes compared to normal periods is a robust feature of the data by presenting results of a series of robustness checks.

3.1 Baseline Multiplier Estimates

Figure 2 displays the cumulative government spending multiplier for each horizon from impact to four years after the government spending shock. The left panel shows the multiplier during ZLB periods, whereas the right panel shows the multiplier during normal periods. Dashed lines indicate 90% confidence intervals. The figure shows that fiscal policy is considerably more effective when implemented during ZLB episodes than during normal times. While the multiplier is significant with point estimates of above unity during ZLB periods, it is small and indistinguishable from zero outside of it. At the ZLB, we find an impact multiplier of above two that decreases slightly to a value of 1.5 at the medium horizon. Outside of ZLB periods, the cumulative multiplier is estimated to stay almost constant at a value of around 0.3 during the first years before falling to zero four years after the fiscal shock materialized. In terms of magnitude, our estimate for the medium-term multiplier during ZLB episodes is remarkably similar to the estimate of Nguyen, Sergeyev, and Miyamoto (2017), which is based on Japanese data from 1980 to 2014. Note that Japan is not part of our sample, which means that our results cannot

Figure 2: Output multipliers.



Notes: The left panel shows the cumulative output multiplier in ZLB states, the right panel shows the cumulative output multiplier in non-ZLB states. Dashed lines show 90% confidence bands.

be driven by the prolonged low-interest-rate episode of the Japanese economy.[6] Our estimated multiplier during normal times is in the ballpark of linear (state-independent) estimates (see, e.g., Ramey 2011a and Perotti 2008). The difference in multipliers across states is not only quantitatively important but, on impact and at the four-year horizon, it is also statistically significant at the 90% level. For the remaining years, the difference is still significant at the 68% level, which is also frequently used in the empirical macro literature.[7]

Our main finding of higher fiscal multipliers at the ZLB is robust to different re-specifications of our baseline model, including dropping time trends, allowing for country-specific time trends, leaving out the exchange rate, the interest rate, and inflation as control variables, and changing the lag length. Our main result is also robust to controlling for the financing side of the government budget when identifying government spending shocks by adding tax revenues to the set of control variables.[8] We also show robustness

[6]Nguyen, Sergeyev, and Miyamoto (2017) estimate an impact and two-year multiplier of 1.5 and 1.7, respectively.

[7]The estimate of the difference in multipliers is based on a standard F-test with a null hypothesis of equal multipliers in both states.

[8]See Mehrotra (2018) for a theoretical analysis of deficit-financed fiscal multipliers at the ZLB.

to normalizing changes in output and government spending by an estimate of potential, or trend, GDP following Gordon and Krenn (2010) and Ramey and Zubairy (2018).[9] Our estimates are also not driven by any key country in the sample. Details on these robustness checks can be found in the Appendix. Before discussing the results of further sensitivity analyses of our baseline result, we first present impulse responses of key macroeconomic variables to government spending shocks.

3.2 Impulse Response Analysis

To shed light on possible mechanisms that could help to understand the differences in multipliers between both states, we estimate state-dependent impulse responses of a set of key macroeconomic variables to government spending shocks. To do so, we estimate the following regression for horizon $h = 0, \dots, 4$:

$$\begin{aligned} Z_{i,t+h} - Z_{i,t-1} = \nu_{i,h} + \delta_{t,h} + \psi_1 t + \psi_2 t^2 + I_{i,t-1} \left[\beta_h^A \frac{G_{i,t} - G_{i,t-1}}{Y_{i,t-1}} + \phi_h^A(L) X_{i,t-1} + \gamma_h^A w_{i,t} \right] \\ + (1 - I_{i,t-1}) \left[\beta_h^B \frac{G_{i,t} - G_{i,t-1}}{Y_{i,t-1}} + \phi_h^B(L) X_{i,t-1} + \gamma_h^B w_{i,t} \right] + \varepsilon_{i,t+h} \ , \qquad (2) \end{aligned}$$

where the dependent variable $Z_{i,t}$ is, respectively, government spending, GDP, consumption, investment, employment, inflation, the nominal interest rate, the real interest rate, the return on safe assets, the average tax rate, and the fiscal deficit; see the Appendix for details on data construction. In case the variable of interest is measured in levels, the dependent variable is normalized by its respective lagged value, i.e. the left-hand side is divided by $Z_{i,t-1}$. The government expenditure variable, $\frac{G_{i,t} - G_{i,t-1}}{Y_{i,t-1}}$, is again instrumented with the change in military spending $\frac{G^m_{i,t} - G^m_{i,t-1}}{Y_{i,t-1}}$. The regressions include all control variables described above as well as three lags of the respective dependent variable.

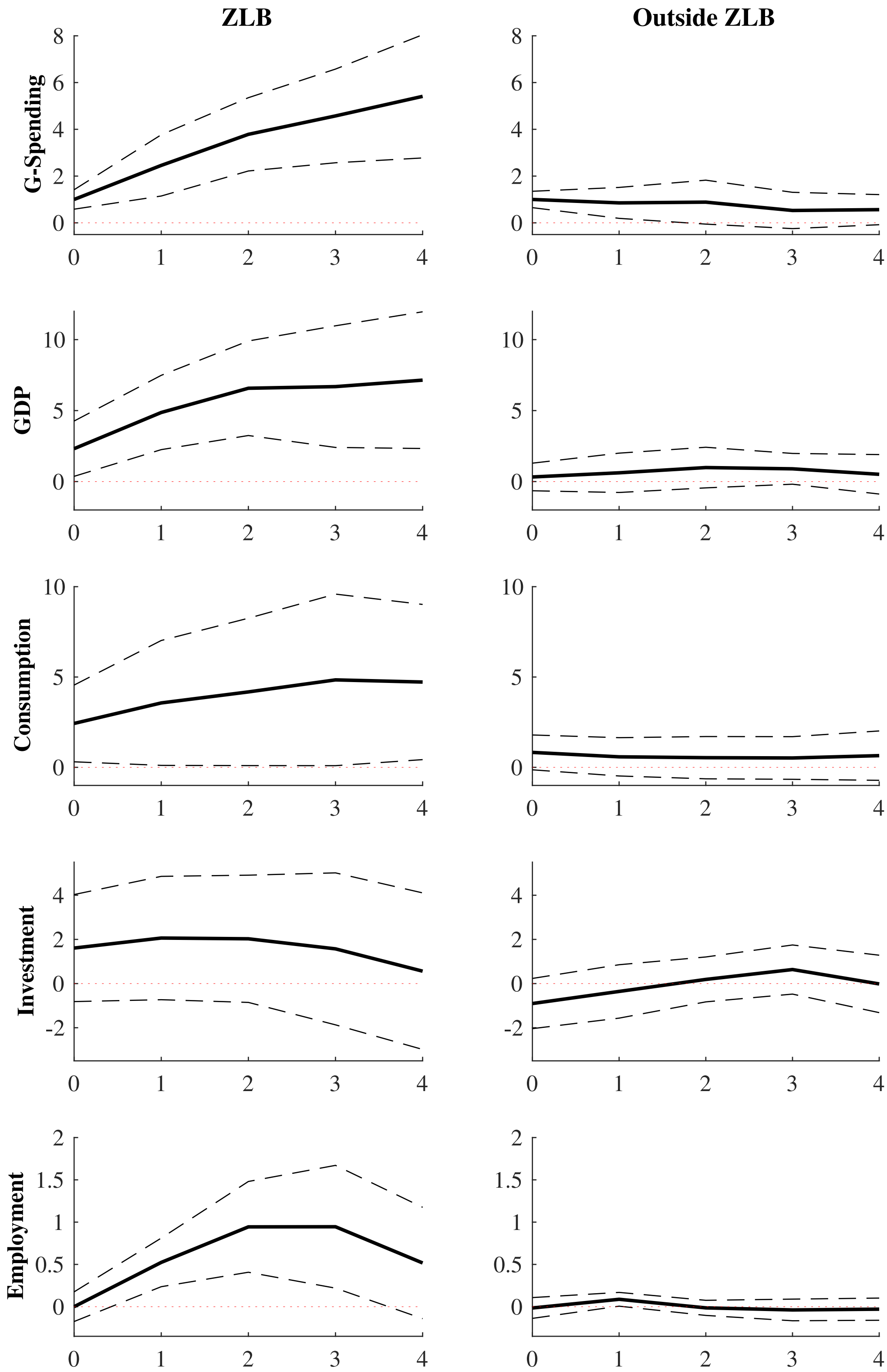
[9] We measure potential GDP by the long-run component of country-specific HP-trends with a large smoothing parameter ($\lambda = 10,000$).

Figure 3 shows impulse responses to a government spending shock occurring during normal times (right column) and during ZLB episodes (left column). While the impact effect on government spending is normalized to one percent in both states, the endogenous dynamic adjustment of government spending differs between both states. At the ZLB, government spending is predicted to increase further. Four years after the fiscal shock, government spending is around 5 percent above its pre-shock level. During normal times, by contrast, government spending gradually declines towards its initial level after the initial rise. The second row of Figure 3 shows the state-dependent output responses. Output rises significantly in response to an increase in government spending during ZLB episodes, while we do not find evidence for a significant positive effect on economic activity during normal times. This result mirrors our multiplier estimates, discussed above.

Turning to the components of aggregate demand, we find evidence of a significant crowding-in of private consumption after a fiscal expansion at the ZLB. Consumption expenditures increase strongly and peak three years after the fiscal shock. Private consumption tends to increase in normal times, too; though the increase is not statistically significant and it is substantially smaller compared to the response at the ZLB. The point estimates also suggest that a fiscal expansion leads to an increase in private investment during ZLB episodes and a decline in investment outside of ZLB episodes. Note, though, that the estimates are statistically significant only at the 68% confidence level. The last row of Figure 3 shows impulse responses of the employment rate.[10] As can be seen, there is a significant and hump-shaped increase in aggregate employment during ZLB episodes. The employment rate rises by one percentage point after two years. By contrast, there is no discernible increase in the aggregate employment rate during normal times.

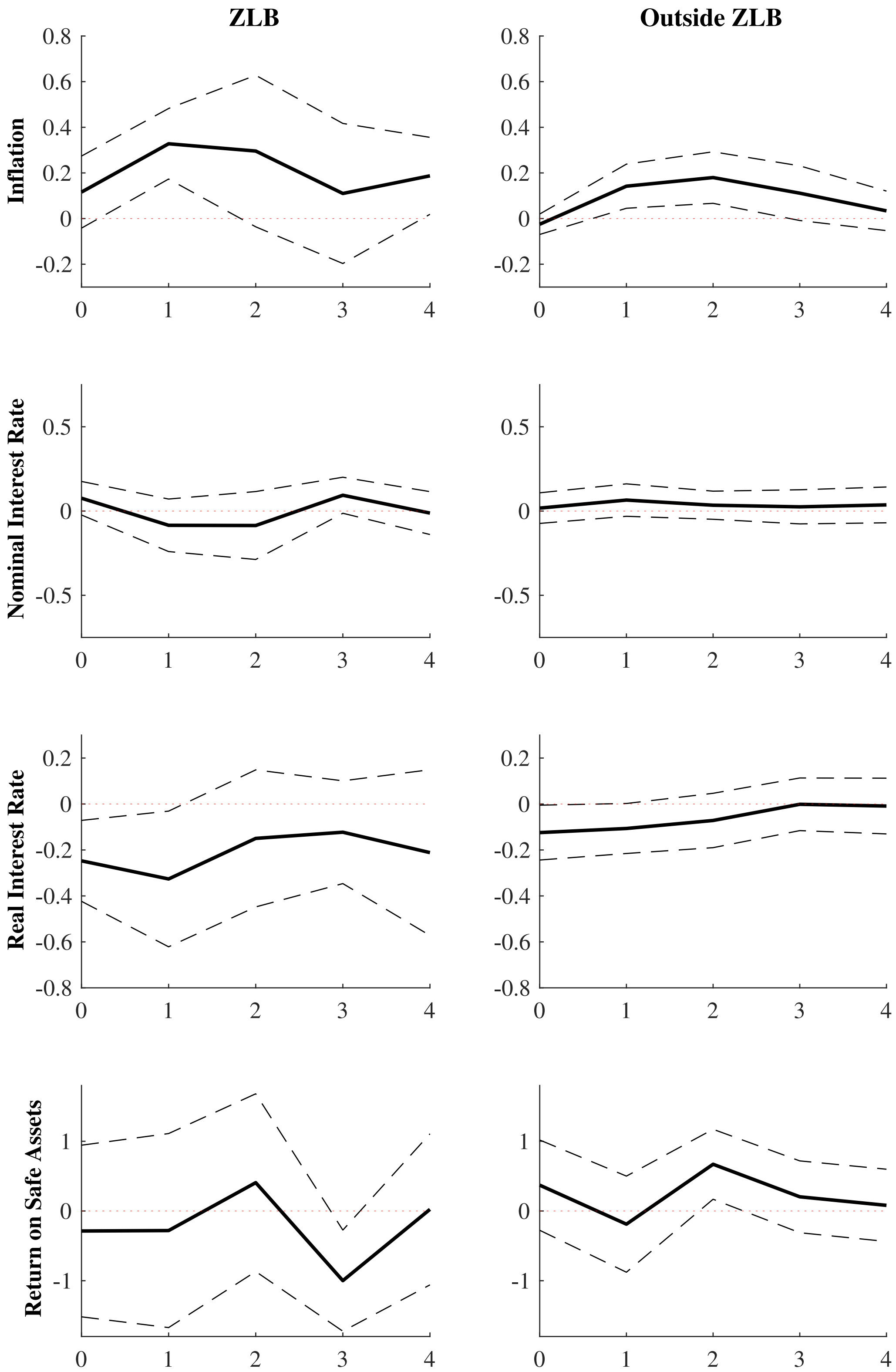
[10]Data on the employment rate are taken from the Long-Term Productivity database, see Bergeaud, Cette, and Lecat (2016).

Figure 3: Impulse responses: government spending, gdp, consumption, investment, and employment.



Notes: Impulse responses to a one-percent government spending shock. The left panels show responses in ZLB states, the right panels show responses in non-ZLB states. Dashed lines show 90% confidence bands.

Figure 4: Impulse responses: inflation and interest rates.



Notes: Impulse responses to a one-percent government spending shock. The left panels show responses in ZLB states, the right panels show responses in non-ZLB states. Dashed lines show 90% confidence bands.

We now investigate potential channels that can rationalize why output multipliers are larger in ZLB periods than in normal times. To start with, we analyze the real interest rate channel emphasized by the standard New Keynesian model, see, e.g., Christiano, Eichenbaum, and Rebelo (2011) and Eggertsson (2011). The New Keynesian model predicts a crowding-in of private economic activity in response to a government spending expansion at the ZLB. The rationale is that the additional demand induced by the expansion of government spending generates inflation, which, when the ZLB on the nominal interest rate binds, leads to a fall in the real interest rate that, in turn, drives up private spending.

As can be seen in Figure 4, the responses at the ZLB are in line with the predictions of the baseline New Keynesian model for an economy stuck at the ZLB due to fundamental shocks. An expansionary fiscal policy shock puts upward pressure on inflation and leads to a significant decline in the real interest rate (constructed as the difference between the short-term nominal interest rate and inflation one period ahead). This is also associated with a significant increase in private consumption expenditures when monetary policy is constrained, as discussed before.

Yet, the estimated responses in normal times are partly at odds with the predictions of the textbook New Keynesian model. In contrast to the model's predictions, we do not observe an increase in the real interest rate (quite the contrary, the real interest even tends to fall). The reason is that, despite upward pressure on inflation, the short-term nominal interest rate remains almost constant also outside of periods where the ZLB binds.

While our finding of moderate fiscal multipliers outside of ZLB episodes despite falling or constant nominal and real monetary policy rates is in line with many empirical studies (see, e.g., Mountford and Uhlig 2009, Ramey 2016, and Bredemeier, Juessen, and Schabert

2017), the question remains how we can explain why multipliers are small despite a seemingly accommodative monetary policy reaction outside the ZLB. To understand this, it is important to emphasize that a key driver for large multipliers is the expectation that the nominal interest rate will not respond to increases in government spending. While we cannot directly observe the expectations of agents in our sample, we can investigate the responses of other than short-term policy interest rates to a government spending shock to capture private beliefs about monetary conditions. In particular, we investigate the response of the nominal return on safe assets, which is an equally weighted average of the total return on short-term government securities and long-term government bonds, constructed by Jordà, Knoll, Kuvshinov, Schularick, and Taylor (2019). Thus, this series captures movements in long-term interest rates which might proxy for agents' expectations about future monetary conditions. Notably, the impulse responses, shown in the last row of Figure 4, reveal significant differences across states. While the return on safe assets declines during ZLB episodes, it increases significantly during normal times. This divergent pattern suggests that agents' expectations about future monetary policy differ depending on whether or not interest rates are at, or near, the zero lower bound. In particular, the documented increase in the return on safe assets outside the ZLB indicates that agents expect a future monetary tightening, despite a rather constant policy interest rate response as shown in Figure 4. By contrast, the fall in the long-term interest rate during ZLB periods suggests that agents expect a prolonged period of loose monetary conditions.

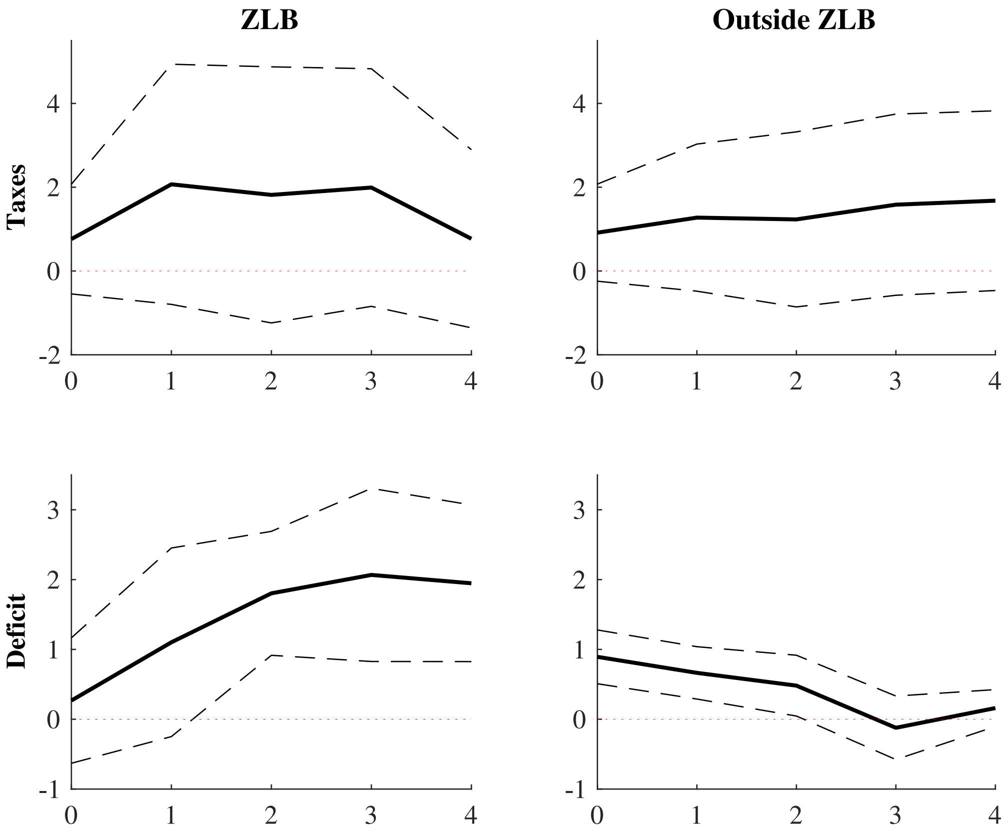
It is also worth emphasizing that theoretical papers have reduced the importance of the real interest rate channel by proposing alternative mechanisms that can explain differences in output multipliers between ZLB periods and normal times. Rendahl (2016), for example, establishes an (un)employment channel. He shows, in a New Keynesian model with labor market frictions, that unemployment dynamics significantly shape government spending multipliers. If government spending causes a persistent decline in unemployment, households start consuming more, which, in turn, triggers a further decline in the rate of unemployment. Rendahl (2016) demonstrates that there is such a virtuous employment-spending cycle at the ZLB that amplifies the fiscal output multiplier. In fact, as already shown in Figure 3, we find evidence for a strong increase in the rate of employment (data on unemployment is not available) and a crowding-in of private consumption during ZLB episodes, perfectly in line with the employment channel.

A number of additional mechanisms can explain why the output multiplier is larger during ZLB periods than during normal times. To start with, it is worth noting that output multipliers are shown to increase with the persistence of the exogenous spending process, see, e.g., Baxter and King (1993) and Dupaigne and Fève (2016). The argument is that a persistent spending hike may increase private investment generating larger output multipliers. This is because a persistent spending increase is associated with a strong negative wealth effect of future taxation. One the one hand, this leads to a strong decline in private consumption (which, in isolation, lowers the output multiplier). On the other hand, the wealth effect induces households to supply more labor. A stimulus to employment raises the marginal product of capital, and therefore investment, and the more so, the stronger is the wealth effect. As Figure 3 indicates, there is some support for this mechanism. We observe a much stronger employment rise at the ZLB where the spending increase is also found to be much more persistent. At the same time, investment tends to rise during ZLB episodes, whereas it tends to fall in response to the less-persistent spending increase that characterizes non-ZLB periods. The difference in the persistence of the endogenous response of government spending to the fiscal shock might be driven by the different responses of the costs of serving future government liabilities: interest rates on government bonds increase in normal times and decline during ZLB periods.[11] This, in isolation, puts downward pressure on the endogenous response of government spending to the fiscal shock in normal times, which may explain why the response of government spending exhibits less persistence outside of ZLB episodes.

A further channel that can rationalize why multipliers are larger during ZLB episodes is a more accommodating tax policy during those episodes. In particular, if tax rates rise less strongly in response to a government spending shock at the ZLB than outside of it, then a government spending expansion may be more effective in stimulating the economy when monetary policy is constrained. As Figure 5 indicates, this story is not supported by the data. Here, we plot the state-dependent impulse responses of the tax rate, defined as the ratio between tax revenues and GDP, as well as of the budget deficit, defined as the difference between government expenditures and government revenues, expressed in percent of GDP. As can be seen, spending expansions tend to increase the average tax rates in both states. Despite similar responses of tax rates, the budgetary impact of a spending expansion differs across states. During ZLB episodes, the deficit does not change on impact and rises significantly only with a delay. The rationale is that the spending hike is partly self-financing, reflecting the fact that tax revenues increase enough to finance higher spending even at unchanged tax rates. Outside of ZLB periods, the government spending expansion is associated with an immediate increase in the fiscal deficit; the shape of the deficit response resembles closely the shape of the government spending response.

[11] Recall that the return on safe assets, which is a weighted average of government bond and bill returns, increases (decreases) during non-ZLB (ZLB) periods, see Figure 4.

Figure 5: Impulse responses: taxes and deficit.



Notes: Impulse responses to a one-percent government spending shock. The left panels show responses in ZLB states, the right panels show responses in non-ZLB states. Dashed lines show 90% confidence bands.

To summarize, we argue that state-dependent changes in interest rates, and especially longer-term interest rates, different employment dynamics, and the difference in the persistence of the endogenous response of government spending to the fiscal shock could be responsible for understanding differences in multipliers across periods of constrained and unconstrained monetary policy. In contrast, we did not find evidence that different tax policies explain our main findings.

3.3 Sensitivity Analysis of Baseline Result

In the following, we present the results of a series of sensitivity analyses. First, we show that our main result of a higher output multiplier at the ZLB holds for alternative definitions of ZLB states. Second, we show that our main result is robust when controlling for geopolitical risk to proxy for expected changes in military spending. Third, we consider an alternative identifying assumption for government spending shocks that is a general form of the recursiveness assumption put forward by Blanchard and Perotti (2002). Finally, we show that our result holds across different exchange rate regimes.

Alternative Definition of ZLB States. Erceg and Lindé (2014) show that the duration of a ZLB episode affects the size of the government spending multiplier. In particular, they find that the longer monetary policy is constrained, the larger is the spending multiplier. To test whether this hypothesis is supported by our data, we redefine our indicator variable and identify ZLB states as those episodes in which the short-term interest rate is smaller than, or equal to, one percent for two or more consecutive years. Table 3 presents the results of this exercise and compares them to our baseline case. The table displays the impact multiplier, the 2-year cumulative multiplier, and the 4-year cumulative multiplier during ZLB episodes (left column) and during normal times (right column). When relying

Table 3: Output multpliers for alternative definition of ZLB episodes.

	ZLB	Outside ZLB
Baseline		
Impact	2.317*	0.326
	(1.184)	(0.591)
2 Year	1.650**	0.422
	(0.736)	(0.710)
4 Year	1.566**	0.101
	(0.744)	(0.746)
Prolonged ZLB episode		
Impact	2.369**	0.281
	(1.159)	(0.608)
2 Year	1.592***	0.328
	(0.522)	(0.709)
4 Year	1.853***	0.329
	(0.614)	(0.741)
Smooth transition		
Impact	1.569	0.239
	(1.168)	(0.541)
2 Year	1.751*	0.904
	(0.913)	(0.566)
4 Year	1.857**	1.129
	(0.735)	(0.699)
Constant interest rates		
Impact	1.316	0.332
	(0.932)	(0.569)
2 Year	1.667*	0.282
	(0.922)	(0.566)
4 Year	1.621*	0.069
	(0.860)	(0.589)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

on this alternative state definition, we again find that the multiplier is large and statistically significant during ZLB episodes, while it is small and statistically indistinguishable from zero during normal times. However, we find no clear evidence that the government spending multiplier changes significantly with the duration of the ZLB episode.

Thus far, our indicator variable $I_{i,t}$ was computed as a dummy variable, with observations 0 and 1. To account for a more gradual change in the monetary policy space, we consider an indicator function that fluctuates smoothly between both extreme cases. To do so, we follow closely Romer and Romer (2018) and use a cumulative normal distribution with a mean of 1% and a standard deviation of 0.5% to capture the smooth transition. The indicator function measures the probability that the monetary authority is constrained. Our specific numbers imply that the measure of monetary space is essentially 0 at a policy rate of 0, 0.16 at a policy rate of 0.5%, 0.5 at 1% and essentially 1 at 2% or more. Table 3 presents the results when using this alternative state definition. It is evident that our main findings are not affected when applying this smooth transition indicator function. The government spending multiplier is considerably larger in ZLB states compared to situations in which the monetary authority is not constrained. Note that these results are also robust to different values of the mean and standard deviation used to calculate the cumulative normal distribution.[12]

In our baseline, ZLB episodes are defined as periods in which the short-term nominal interest rate is below, or equal to, one percent. The typical New Keynesian model emphasizes that the key driver of large multipliers is the expectation that the nominal interest will not respond to increases in government spending. Note that we cannot directly test for fiscal multipliers when interest rates are expected to be constant due to the lack of historical data on interest-rate expectations. What we can do though is to test for multipliers when realized short-term interest rates are constant for some extended period of time. To do so, we redefine our indicator variable and identify 'ZLB' states as those episodes in which the change in the short-term interest rate has been approximately zero for two or more consecutive years, irrespective of the level of the interest rate. As seen in Table 3, multipliers tend to be higher in these episodes, too, but multipliers are also estimated with more statistical uncertainty.

The two definitions of ZLB episodes are obviously not mutually exclusive as many periods with an interest rate below one percent are also periods in which the interest rate stayed constant. To disentangle the effects of the level of the interest rate from the effects of a zero change in interest rates, we run a regression where we consider both state

[12] Detailed results of these exercises are available from the authors upon request.

definitions simultaneously. Doing so enables us to estimate the fiscal multiplier in low-interest-rate episodes, after having controlled for zero changes in interest rates. Likewise, we can test for fiscal multipliers during periods in which the short-term interest rate stayed constant (relative to the previous years), independent of the level of the interest rate. Interestingly, we find that multipliers are large and statistically significant when interest rates are low. By contrast, we do not find evidence of larger multipliers during episodes of constant interest rates, when we control for a low-interest-rate environment.[13] Thus, it seems that the level of the short-term nominal interest rate shapes fiscal multipliers more than the constancy of the interest rate. An explanation for this result could be that, in a low-interest-rate environment, it is more likely that agents expect the nominal interest rate to stay constant after an increase in government spending, compared to periods characterized by constant but higher interest rates.

Controlling for Geopolitical Risk. In our baseline, we use a financial market variable (the exchange rate) to capture information about future fiscal policies. Table A1 in the Appendix shows that our results also go through when we use stock prices as an alternative financial market variable. We now show that our results are also robust to controlling for anticipated changes in military spending in a more direct way. We do so by adding a measure of geopolitical risk to the set of control variables. A high geopolitical risk rating may induce people to expect that military spending increases in the future. We use the Geopolitical Risk Index constructed by Caldara and Iacoviello (2018) and interact it with country-specific fixed effects. Thus, we allow for a country-specific reaction to changes in global political risk. The Geopolitical Risk Index is constructed by counting the occurrence of words related to geopolitical tensions in leading international newspapers.

[13]Detailed results of this exercise can be found in the Appendix.

Table 4: **Output multipliers when controlling for geopolitical risk.**

	ZLB	Outside ZLB
Impact	2.234*	0.386
	(1.242)	(0.613)
2 Year	1.643**	0.515
	(0.747)	(0.758)
4 Year	1.576**	0.173
	(0.755)	(0.803)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

As Table 4 shows, our results are robust to controlling, in this way, for expected changes in military spending.

Alternative Identification. In what follows, we show that our results are robust to an alternative identification scheme. Specifically, we now instrument the cumulative change in real per capita government expenditures, $\sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}$, by a purified measure of government expenditures, $\widetilde{g}_{i,t}$, constructed as

$$\widetilde{g}_{i,t} = g_{i,t} - \mu y_{i,t}, \tag{3}$$

where $g_{i,t}$ is government expenditure and $y_{i,t}$ is output, both expressed in log real per capita terms. The second term on the right-hand side characterizes the systematic contemporaneous response of government expenditure to changes in aggregate economic activity. A positive value of μ corresponds to a pro-cyclical behavior of government expenditure, while a negative value of μ indicates countercyclical government expenditure. The limiting case $\mu = 0$ implies that government expenditure does not react contemporaneously to output (similar to Blanchard and Perotti 2002's recursive identification approach). This assumption of a zero elasticity requires that government expenditure does not contain components that automatically fluctuate with the business cycle. Moreover, it requires that policymakers need time to decide on, approve, and implement discretionary changes in fiscal policy, a requirement that is more restrictive when imposed at an annual frequency. Since both requirements are not *ex ante* assured for our annual historical data, we consider a wide range of values of the elasticity of government expenditure with respect to current output μ, following Nguyen, Sergeyev, and Miyamoto (2017), Beetsma and Giuliodori (2011), and Beetsma, Giuliodori, and Klaassen (2008). We identify discretionary expenditure changes by restricting the contemporaneous response of government expenditure to economic activity, i.e., by calibrating the parameter μ.

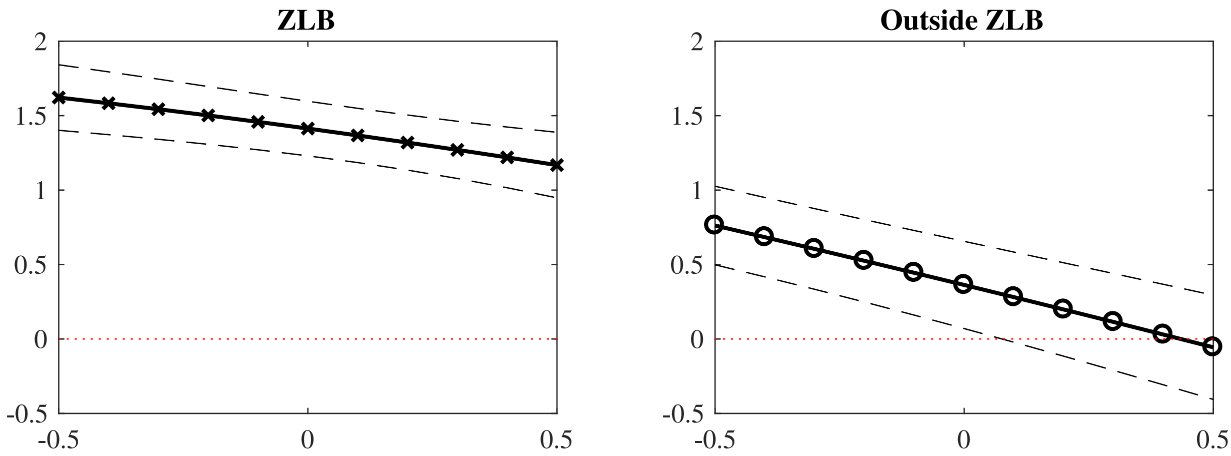
Figure 6 displays the impact multiplier, the 2-year cumulative multiplier, and the 4-year cumulative multiplier in ZLB and non-ZLB states for values of the expenditure elasticity to current output ranging from -0.5 (highly countercyclical government expenditure, a 1% increase in output lowers government expenditures by 0.5%) to 0.5 (highly procyclical government expenditure, a 1% increase in output increases government expenditures by 0.5%). To put these values into perspective, Caldara and Kamps (2017) use exogenous non-fiscal policy instruments to estimate the contemporaneous elasticity of government spending, the sum of government consumption and government investment, to output in post-WWII U.S. data. They find a value of -0.15, i.e. a slightly countercyclical behavior of government spending. When applying their methodology for U.S. aggregate government expenditures, we find a value of $\mu = -0.31$. When estimating a simple expenditure rule in the spirit of Fatás and Mihov (2012), we find a value of 0.17 for μ in our sample.[14] Thus, our chosen interval of elasticities covers a rather wide range of possible values. As before, the left panels show multipliers in ZLB states, the right panels show multipliers in normal times.

As seen in Figure 6, the multiplier estimates depend on the calibration of the spending elasticity to current output. Similar to Caldara and Kamps (2017), we find that

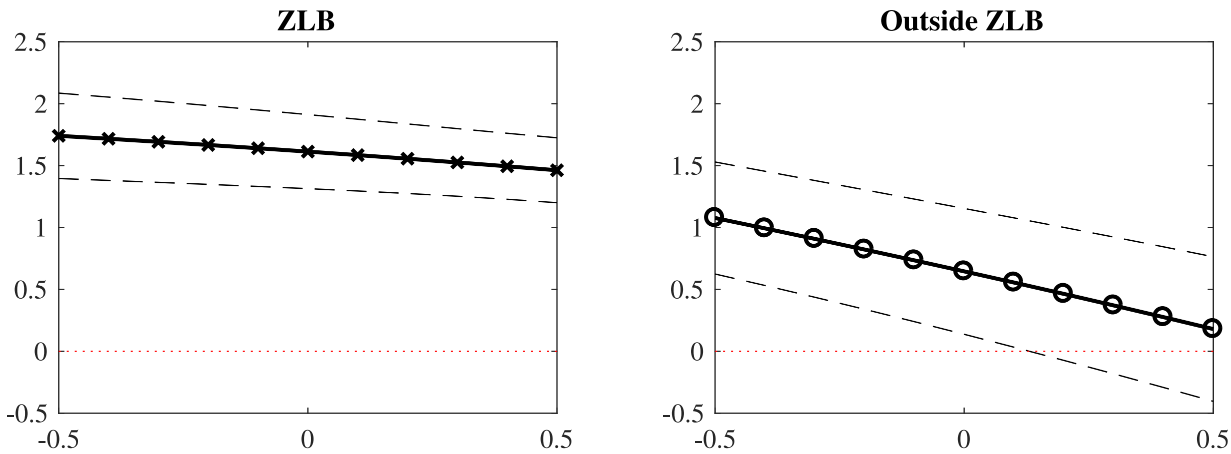
[14] In the estimation, we regress the log of real per capita government expenditure on the log of contemporaneous real per capita GDP, two lags of log real per capita government expenditure, a linear and a quadratic time trend, as well as country and time fixed effects. While Fatás and Mihov (2012) estimate the fiscal rule based on government consumption, we have to rely on government expenditures in our analysis.

Figure 6: Output multipliers as a function of spending elasticity to output.

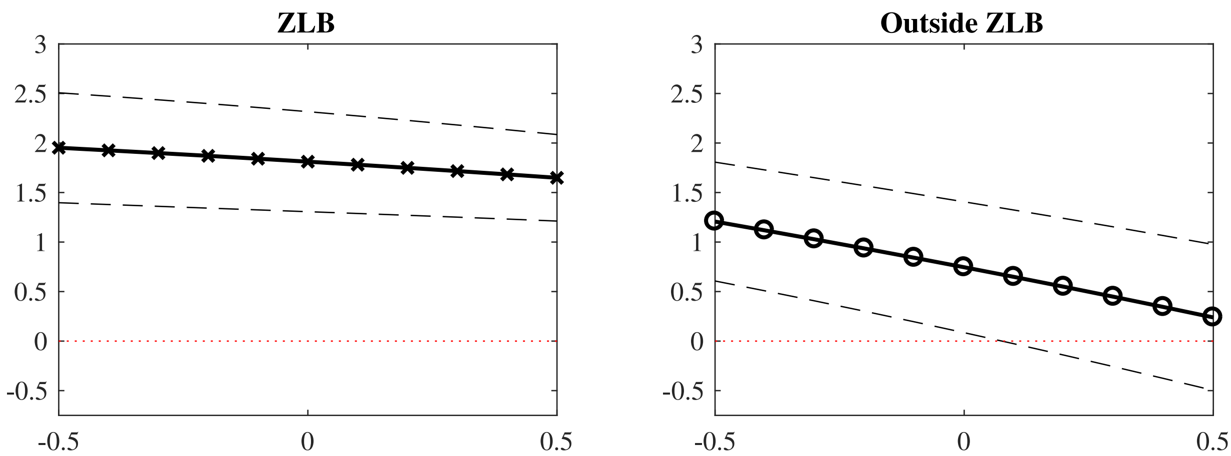
(a) $h = 0$



(b) $h = 2$



(c) $h = 4$



Notes: Cumulative output multipliers for different horizons h across alternative values of the elasticity of government spending with respect to current output μ. The left panels show multipliers in ZLB states, the right panels show multipliers in non-ZLB states. Dashed lines show 90% confidence bands.

the multiplier is larger when government spending displays a countercyclical behavior, whereas procyclical government spending reduces the estimated multiplier. For example, the impact multiplier in normal times is around 0.75 when $\mu = -0.5$, whereas it is approximately zero when $\mu = 0.5$. Most importantly, though, this estimation bias does not significantly affect the relative effectiveness of fiscal policy across monetary stances. For the limiting case of a zero elasticity of government expenditures to output ($\mu = 0$), we find that irrespective of the forecast horizon considered, the multiplier at the ZLB is around twice as large compared to the multiplier in times of unconstrained monetary policy. Overall, fiscal policy is estimated to be considerably more effective during ZLB episodes than during normal times, irrespective of how we calibrate μ. Thus, our main finding is robust to applying this alternative identification scheme.

Controlling for Exchange Rate Regimes. In this section, we show that our result of larger multipliers at the ZLB proves to be robust when controlling for fixed and flexible exchange rate regimes. Ilzetzki, Mendoza, and Vegh (2013), amongst others, find that the government spending multiplier is larger under fixed exchange rate regimes, in line with the Mundell-Fleming textbook model. Against this background, our main finding of larger multipliers when interest rates are at, or near, the ZLB could be driven by the fact that ZLB periods mainly coincide with episodes of fixed exchange rate regimes. However, this rationale is not supported by the data. First, out of the 116 periods defined as ZLB episodes, 63 periods are also classified as fixed exchange rate regimes, while the remaining 53 periods coincide with flexible exchange rate regimes. Second, when we condition on a specific exchange rate regime, we find that the government spending multiplier is significantly larger during ZLB episodes, irrespective of the exchange rate regime considered.

To investigate the role of the exchange rate regime for our results, we differentiate between fixed (C) and flexible (D) regimes and estimate the following specification separately for both exchange rate regimes:

$$
\begin{aligned}
\sum_{j=0}^{h} \frac{Y_{i,t+j} - Y_{i,t-1}}{Y_{i,t-1}} = {} & I^{S}_{A,i,t-1} \left[M^{SA}_{h} \sum_{j=0}^{h} \frac{G_{i,t+j} - G_{i,t-1}}{Y_{i,t-1}} + \phi^{SA}_{h}(L) X_{i,t-1} + \gamma^{SA}_{h} w_{i,t} \right] \\
& + I^{S}_{B,i,t-1} \left[M^{SB}_{h} \sum_{j=0}^{h} \frac{G_{i,t+j} - G_{i,t-1}}{Y_{i,t-1}} + \phi^{SB}_{h}(L) X_{i,t-1} + \gamma^{SB}_{h} w_{i,t} \right] \\
& + I^{S}_{O,i,t-1} \left[M^{SO}_{h} \sum_{j=0}^{h} \frac{G_{i,t+j} - G_{i,t-1}}{Y_{i,t-1}} + \phi^{SO}_{h}(L) X_{i,t-1} + \gamma^{SO}_{h} w_{i,t} \right] \\
& + \nu^{S}_{i,h} + \delta^{S}_{t,h} + \psi^{S}_{1} t + \psi^{S}_{2} t^{2} + \varepsilon^{S}_{i,t+h}, \quad \text{for } S \in \{C, D\}.
\end{aligned} \tag{4}
$$

$I^{S}_{A,i,t}$ and $I^{S}_{B,i,t}$ now indicate ZLB and non-ZLB states within the exchange rate regime $S \in \{C, D\}$. In the estimation for the fixed exchange rate regime, $I^{C}_{A,i,t}$ indicates ZLB episodes that coincide with periods of fixed exchange rate regimes. $I^{C}_{B,i,t}$ indicates non-ZLB episodes that coincide with periods of fixed exchange rate regimes. $I^{C}_{O,i,t}$ is then a dummy variable for being in the opposing exchange rate regime (which is the flexible regime), irrespective of the monetary policy stance. M^{CA}_{h} and M^{CB}_{h} then provide the state-dependent multipliers during ZLB and outside-ZLB episodes within the fixed exchange rate regime, respectively. Analogously, in the estimation for flexible exchange rate regimes, $I^{D}_{A,i,t}$ ($I^{D}_{B,i,t}$) indicates ZLB (non-ZLB) episodes that coincide with periods of flexible exchange rates and $I^{D}_{O,i,t}$ is the dummy variable for being in the opposing exchange rate regime (which is now the fixed exchange rate regime). M^{DA}_{h} and M^{DB}_{h} then provide the state-dependent multipliers during ZLB and outside-ZLB episodes within the flexible exchange rate regime, respectively.

We classify exchange rate regimes based on Reinhart and Rogoff (2011) and Ilzetzki, Reinhart, and Rogoff (2017). For the years prior to 1940, we use the years of the Gold Standard provided by Reinhart and Rogoff (2011) as fixed exchange rate regimes. For

Table 5: Output multpliers when controlling for exchange rate regime.

	ZLB	Outside ZLB
Fixed exchange rate regime		
Impact	3.514***	0.417
	(0.807)	(0.577)
2 Year	2.849***	0.605
	(0.389)	(0.402)
4 Year	3.197***	0.805***
	(0.380)	(0.307)
Flexible exchange rate regime		
Impact	2.176*	0.431
	(1.159)	(0.625)
2 Year	1.505***	0.546
	(0.582)	(0.679)
4 Year	1.379**	0.565
	(0.673)	(0.721)
Excluding Euro Area		
Impact	2.045*	0.287
	(1.156)	(0.528)
2 Year	1.604**	0.298
	(0.809)	(0.610)
4 Year	1.513**	0.041
	(0.722)	(0.654)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

the post-1940 sample, we follow the definition of Ilzetzki, Mendoza, and Vegh (2013) to differentiate between fixed and flexible exchange rate regimes: as fixed exchange rate regimes, we classify regimes with no legal tender, hard pegs, crawling pegs, and de facto or pre-announced bands or crawling bands with margins no larger than $\pm$ 2%. All other episodes are classified as flexible exchange rates.

As Table 5 indicates, our main findings are robust to controlling for the exchange rate regime. The government spending multiplier is estimated to be considerably larger during ZLB episodes, irrespective of the specific exchange rate regime. Comparing the point estimates across exchange rate regimes shows that government spending multipliers tend to be larger under fixed exchange rate regimes, in line with the typical Mundell-Fleming model.

Another possible concern with our baseline findings is that we do not control explicitly for the existence of common currency unions (currency unions are considered in the previous exercise as one form of a fixed exchange rate regime). Farhi and Werning (2016) consider theoretical implications for multipliers in a liquidity trap and a currency union. They show that during liquidity traps, multipliers are large, but for a currency union, they show smaller self-financed multipliers. To consider this possibility, we re-estimate our baseline model but exclude all observations pertaining to the European Monetary Union from our sample. This implies that we focus solely on periods of constrained and unconstrained monetary policy outside the European currency union. As the lower part of Table 5 shows, our main findings are not affected by this re-estimation. When only considering periods outside the European currency union, we still find that the government spending multiplier is considerably larger during ZLB periods. Interestingly, the point estimates are remarkably similar to our baseline estimation, which may imply that the existence of a currency union does not significantly reduce the spending multiplier during a liquidity trap.

4 The Role of Other State Variables

Thus far, we allowed the effects of government spending expansions to vary depending on two alternative states, defined as ZLB and non-ZLB states. In this section, we check whether our results hold when we control for other states of the economy that may influence the size of government spending multipliers such as the state of the business cycle or the degree of financial market stress. To do so, we follow Bernardini and Peersman (2018) and estimate the following augmented state-dependent local projection - instrumental variable model:

$$
\begin{aligned}
\sum_{j=0}^{h} \frac{Y_{i,t+j}-Y_{i,t-1}}{Y_{i,t-1}} = m_h^O \sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}} &+ \phi_h^O(L) X_{i,t-1} + \gamma_h^O w_{i,t} \\
&+ I_{i,t-1}^A \left[m_h^A \sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}} + \phi_h^A(L) X_{i,t-1} + \gamma_h^A w_{i,t} \right] \\
&+ I_{i,t-1}^B \left[m_h^B \sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}} + \phi_h^B(L) X_{i,t-1} + \gamma_h^B w_{i,t} \right] \\
&+ \nu_{i,h} + \delta_{t,h} + \psi_1 t + \psi_2 t^2 + \varepsilon_{i,t+h} \ , \qquad (5)
\end{aligned}
$$

where I_{it}^A indicates ZLB states, whereas I_{it}^B is an indicator for an additional state variable (e.g. an economic slump). The coefficient m_h^O is the multiplier in the "neutral state" (here, no ZLB and no slump). The coefficients m_h^A and m_h^B measure the additional effect of moving from the "neutral state" to the ZLB state and the additional state, respectively. State-dependent multipliers are the sum of the coefficients $m_h^O + m_h^A$ and $m_h^O + m_h^B$. Otherwise, the specification is as in our baseline model including that the cumulative change in government spending, $\sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}$, is again instrumented with military spending changes. Because the multiplier in the neutral state, m_h^O, is the same across the ZLB and the additional state, in what follows we just report the additional effects m_h^A and m_h^B.

To start with, we assess whether our results are not spuriously driven by an overlap of war periods with our identified ZLB episodes. To give an example, the U.S. participated in World War II while it simultaneously experienced a prolonged ZLB episode that lasted from 1934-1945. Overall, 10% of our ZLB episodes coincide with war periods. To consider ZLB regimes and war regimes simultaneously, we use our war dummy as the additional state indicator I_{it}^B. Table 6 displays the estimated coefficients m_h^A and m_h^B for horizons $h = 0, 2, 4$, measuring the additional effect on the impact, 2-year, and 4-year cumulative multiplier during ZLB episodes and during war episodes, respectively. As can be seen, our main result of higher multipliers during periods of constrained monetary policy is robust to explicitly allowing for different multipliers during war periods. For all forecast horizons, we observe a significant increase in the cumulative output multiplier during ZLB episodes. When moving from the non-war, non-ZLB regime into the ZLB regime, the impact and 2-year cumulative multiplier increase by around 2.5. Thus, the finding of higher multipliers during periods of constrained monetary policy is not driven by the overlap of ZLB episodes with military conflicts.

Next, we control for the state of the business cycle. Thus far, we have provided extensive evidence that the output multiplier is significantly larger in periods when the economy is constrained by the ZLB. Other studies suggest that the effects of fiscal policy are amplified during periods of economic slack; see, e.g., Auerbach and Gorodnichenko (2012, 2013), Caggiano, Castelnuovo, Colombo, and Nodari (2015). Note, though, that this finding is disputed by Owyang, Ramey, and Zubairy (2013) and Ramey and Zubairy (2018), based on evidence from U.S. historical data. Both states are obviously not mutually exclusive as ZLB episodes arise when central banks cut rates during periods of severe economic downturns (the Great Recession is a recent example). In fact, average GDP growth during ZLB episodes is around half of that during normal times (2.30 outside of ZLB episodes and 1.25 during ZLB episodes). Moreover, we find that about 75% of all ZLB episodes coincide with periods of economic slumps, based on the definition of booms and slumps described below. Given this, it is possible that our emphasis on nonlinear effects of fiscal policy across monetary regimes is simply a relabeling of nonlinear effects across the business cycle. In this subsection, we show that this is, however, not the case.

Table 6: Additional effect on output multiplier for alternative states.

	ZLB m_h^A	Additional state m_h^B
Wars		
Impact	2.517***	−0.900
	(0.691)	(0.608)
2 Year	2.542***	0.202
	(0.937)	(0.339)
4 Year	2.389**	0.239
	(1.042)	(0.334)
Slump		
Impact	1.351***	−0.605
	(0.468)	(0.406)
2 Year	1.239***	−0.309
	(0.386)	(0.325)
4 Year	0.892**	0.129
	(0.453)	(0.356)
Deep Slump		
Impact	0.947	−0.247
	(0.687)	(0.248)
2 Year	0.954***	0.226
	(0.353)	(0.296)
4 Year	1.019***	0.649*
	(0.253)	(0.364)
Financial Crises		
Impact	1.557	0.768
	(1.001)	(0.528)
2 Year	1.379**	0.257
	(0.603)	(1.118)
4 Year	1.559**	1.696
	(0.683)	(1.368)
Low Government Debt		
Impact	0.957*	−0.384
	(0.530)	(0.554)
2 Year	1.346**	0.103
	(0.589)	(0.557)
4 Year	1.353**	0.348
	(0.685)	(0.606)
Low Inflation		
Impact	1.128	1.408*
	(1.044)	(0.745)
2 Year	1.670**	1.015
	(0.713)	(0.669)
4 Year	1.574**	0.259
	(0.723)	(0.796)
Closeness of Economy		
Impact	2.897**	1.492**
	(1.261)	(0.747)
2 Year	1.855**	1.341
	(0.785)	(1.009)
4 Year	1.630**	0.507
	(0.758)	(1.030)

Notes: The table reports the additional effects on the neutral cumulative multiplier and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

To analyze the role of the business cycle for our results, we proceed as follows. We define slumps (booms) as periods with a negative (positive) output gap, calculated as the deviation of a country's real GDP per capita from its long-run HP-trend, where we set the smoothing parameter to $10,000$.[15] As can be seen in Table 6, the results confirm that multipliers are considerably higher during ZLB episodes, irrespective of the state of the business cycle. We find evidence of a significant rise in the impact, 2-year, and 4-year cumulative output multiplier during ZLB episodes. By contrast, there is no evidence of a significant rise in the cumulative output multiplier in periods of economic slack. This finding holds for all forecast horizons h considered. This result suggests that although economies often face periods of economic slack and constrained monetary policy at the same time, the government spending multiplier is not generally amplified during business-cycle contractions, in line with the findings of Owyang, Ramey, and Zubairy (2013) and Ramey and Zubairy (2018). In contrast, fiscal multipliers are larger during ZLB periods, even after controlling for the overlap between periods of constrained monetary policy and economic slack.

This result could be driven by the way we define economic slumps. To check this, we now consider deep economic slumps. In this case, we define slumps as periods in which the negative output gap deviation from trend is larger than the average negative deviation from trend. Based on this definition, 15% of all observations are classified as deep slumps and 26% of all ZLB episodes coincide with periods of deep economic slumps. The third block of Table 6 shows that multipliers tend to be higher during deep economic slumps, a finding that cannot be explained by an overlap of deep economic slumps with ZLB periods. We find a significantly positive effect on the 4-year cumulative multiplier when moving

[15]Note that the results are robust to different values of the smoothing parameter.

from the neutral state (no deep slump, no ZLB) into the deep slump regime.[16] Most importantly, spending multipliers are still significantly higher in periods of constrained monetary policy. The ZLB raises the 4-year cumulative multiplier by around one, even if we control for the presence of deep economic slumps.

As our identified ZLB periods are clustered around the periods of the Great Depression and the Great Recession that are both associated with financial market distress, it could be the case that our findings are a general feature of financial crises. In particular, higher multipliers at the ZLB could simply be a reflection of multipliers being generally larger during financial crises. To test for this hypothesis, we use the financial crisis dummy in the Macrohistory Database to control for times of financial distress. As Table 6 shows, we again find that multipliers are considerably larger during ZLB episodes, even if we control for financial market distress. Periods of financial turmoil tend to be associated with larger multipliers, too, though this effect is estimated with substantial uncertainty.

Of course, since our identified ZLB periods mainly occur during the Great Depression and the Great Recession, we cannot rule out completely that our result of fiscal multipliers being significantly larger during ZLB episodes is a reflection of circumstances other than a constrained monetary policy which are associated with extremely severe and persistent economic crises. However, our analysis suggests that the documented nonlinear fiscal policy effects across monetary regimes are not simply a reflection of multipliers being generally higher during any economic downturn or during any period of financial turmoil.

As a further check, we control for the level of government debt. Perotti (1999),amongst others, shows that the effects of government spending expansions differ depending on the

[16] This is roughly in line with the results of Caggiano, Castelnuovo, Colombo, and Nodari (2015) who show that fiscal multipliers are not statistically different across different phases (recessions/expansions) of the U.S. business cycle, except when comparing extreme phases of the business cycle (deep recessions vs. strong expansions).

initial level of government debt. In particular, he shows that fiscal multipliers are large when government debt is low. By contrast, fiscal expansions at high levels of government debt are associated with a substantial crowding-out of private economic activity and hence with a small fiscal multiplier. In order to control for the level of government debt, we use the government debt-to-GDP ratio and define periods of low government debt as periods in which a country's debt-to-GDP ratio is below its long-run trend.[17] This procedure implies that 38% of our ZLB episodes are also classified as periods of low government debt. As can be seen in Table 6, we again confirm our main finding of a significant additional effect on output multipliers during ZLB episodes. In our sample, we do not find evidence that a low level of government debt significantly increases the size of the fiscal multiplier.

Next, we investigate whether our results are driven by an overlap of periods of low trend inflation with ZLB periods. To do so, we calculate, for each country in our sample, trend inflation by using an HP trend with a high smoothing parameter ($\lambda = 10,000$). We then define a low-inflation regime by a period in which a country's trend inflation is below the first quartile of the distribution of trend inflation in that country. Table 6 shows that fiscal multipliers increase when moving from the neutral state to the ZLB state, even after controlling for a low-inflation environment.

Finally, we control for the degree of openness of a country. Ilzetzki, Mendoza, and Vegh (2013) provide evidence that the multiplier is a negative function of openness to trade. Economies that are relatively closed have significantly larger multipliers than relatively open economies. This finding is in line with the textbook Mundell–Fleming model predicting that the fiscal multiplier will be lower in a more open economy because

[17] As before, we calculate country-specific time trends by relying on an HP trend with a high smoothing parameter ($\lambda = 10,000$).

of a reduction in net exports following a government spending expansion. There could potentially be an overlap of periods when the nominal interest rate reached the zero lower bound with periods in which countries were relatively closed. In order to control for the degree of openness, we construct a 'closeness' dummy that takes the value one when the trade (imports plus exports) to GDP ratio in country i at time t is below the first quartile of trade-to-GDP ratios across countries and time. Table 6 shows that fiscal multipliers indeed tend to be higher when a country is relatively closed. Controlling for openness, however, does not affect our main finding of significantly larger multipliers during ZLB episodes.

5 Conclusion

Using historical panel data for 13 advanced countries, we provide robust evidence that the output effects of fiscal policy are considerably larger during ZLB periods than during normal times. This result is not a simple reflection of other states that may affect the size of fiscal multipliers such as the degree of economic slack in an economy or the presence of financial turmoil. From a policy perspective, our findings suggest that the large fiscal stimulus programs implemented in several countries whose nominal interest rate was at, or near, zero were effective in counteracting the Great Recession and stimulating the economy. Likewise, our results may imply that the lower bound constraint on monetary policy amplified the negative effects of large-scale austerity programs implemented by many countries in the aftermath of the Great Recession.

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Appendix

A1 Data Definitions and Sources

The baseline sample covers the period 1917-2016 and the countries Belgium, Denmark, Finland, France, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and the United States.

The following series are taken from the Macrohistory Database, if not mentioned otherwise. The definition and construction of the respective variables are as follows:

- **Real GDP per capita**: series-ID: rgdppc.

- **Real government spending per capita:** construction: Nominal government expenditures deflated by consumer prices and divided by population, series-IDs: expenditure/(cpi*pop).

  The following gaps in the government expenditures series are filled by linear interpolation: Belgium, 1913-1919, 1940; Denmark, 1936; Spain, 1936-1939;

- **Real military spending expenditures:** construction: Merged series from the National Material Capabilities database and the SIPRI Military Expenditures Database. The exchange rate by the Macrohistory database is used to construct an expenditure series in local currencies. To obtain real per capita units, series is deflated by consumer prices and divided by population.

- **War dummy:** construction: Merged series from Correlates of War Project war database and UCDP/PRIO Armed Conflict Dataset. The dummy equals one when a country is involved in a military conflict.

- **Short-term interest rate:** series-ID: stir.

  The following gaps in the interest rate series are filled by linear interpolation: Belgium, 1915-1919; Spain, 1915-1919; France, 1915-1921; Italy, 1915-1921; Norway, 1966.

- **Inflation:** construction: Growth rate of consumer prices, series-ID: $\log(\text{cpi}_t)$ - $\log(\text{cpi}_{t-1})$.

- **Exchange rate:** series-ID: xrusd.

- **Real stock prices:** construction: Nominal stock prices deflated by consumer prices, series-IDs: stock prices/cpi.

- **Taxes:** construction: Nominal tax revenues as a ratio of nominal GDP, series-IDs: revenue/(rgdppc*cpi*pop).

  The following gaps in the government expenditures series are filled by linear interpolation: Belgium, 1913-1919; Spain, 1936-1939; Norway, 1944-1948.

- **Financial crisis dummy:** series-ID: crisisJST.

- **Real consumption per capita**: series-ID: rconpc.

- **Real investment**: construction: Difference between GDP and the sum of private consumption, government expenditures, and net exports, all expressed in real, per-capita terms. Real net exports are constructed by exports minus imports deflated by consumer prices and divided by population.

- **Employment rate**: construction: Series taken from Bergeaud, Cette, and Lecat (2016) (series name: Lhab).

- **Return on safe assets:** series-ID: safe-tr. The following gaps in the series are filled by linear interpolation: Belgium: 1914-1919; Spain: 1936-1940; France: 1915-1921; Italy: 1917-1921.

- **Fiscal deficit:** construction: Nominal government spending minus nominal taxes as a fraction of nominal GDP, series-IDs: (expenditures-revenue)/(rgdppc*cpi*pop).

- **Government debt:** series-ID: debtgdp. The following gaps in the series are filled by linear interpolation: Belgium, 1914-1919, 1940-1945, 1980-1981; Denmark, 1947-1952, 1957-1959, 1997; Spain, 1936-1939; France: 1915-1919, 1939-1948; Netherlands: 1940-1945; Norway: 1940-1946.

- **Geopolitical risk:** Series taken from Caldara and Iacoviello (2018) (series name: GPRH).

- **Trade to-GDP-ratio:** construction: Sum of nominal imports and exports divided by nominal GDP, series-IDs: (imports+exports)/(rgdppc*cpi*pop).

Figure A1: Military spending growth.

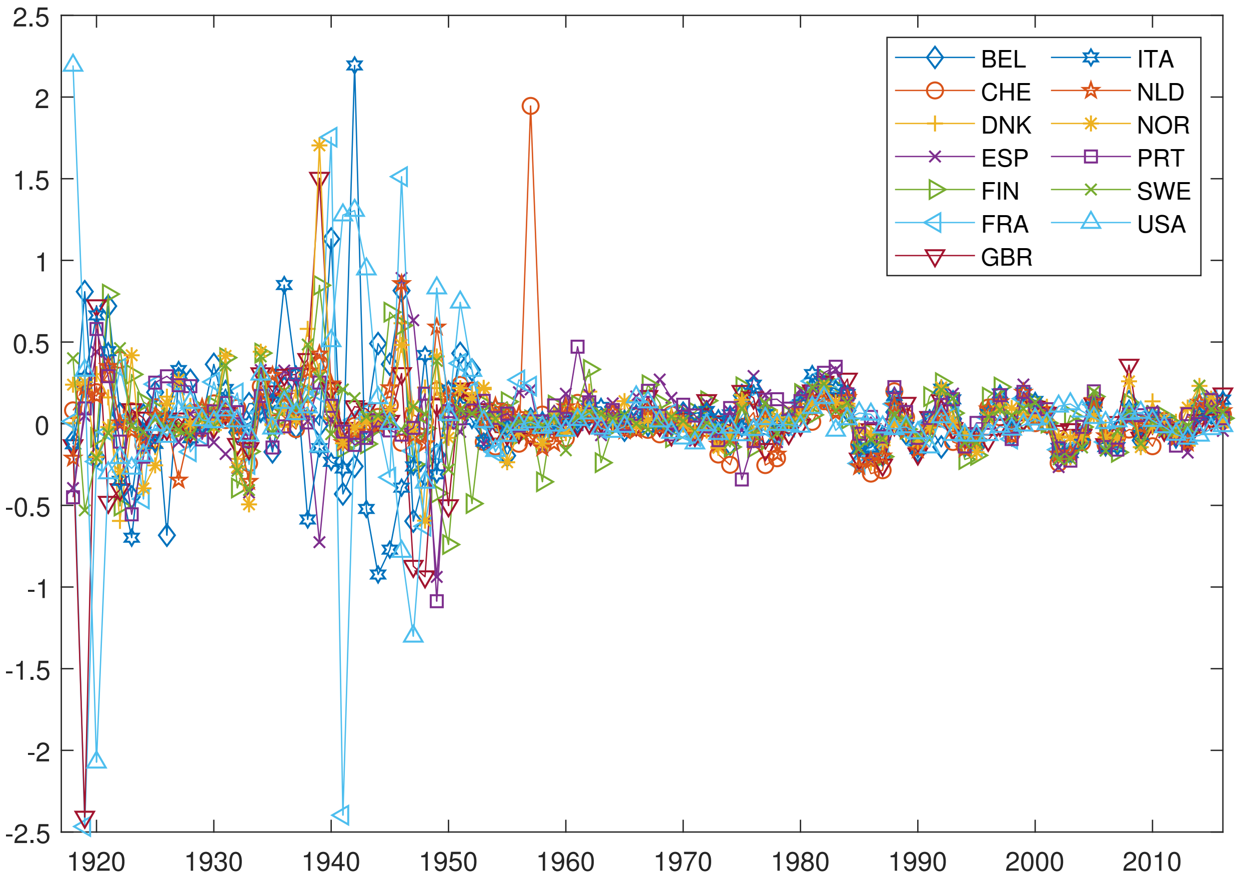


Figure A2: Government spending growth.

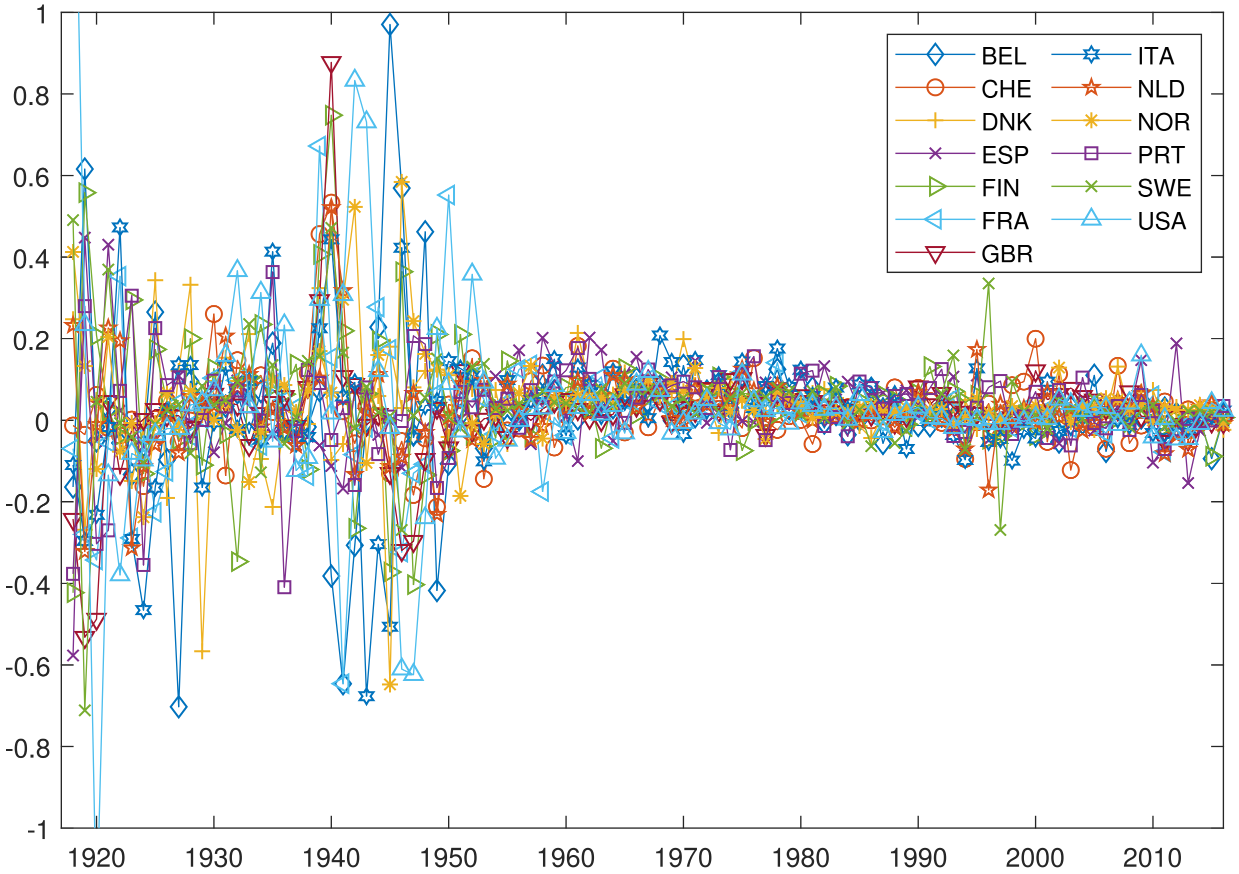
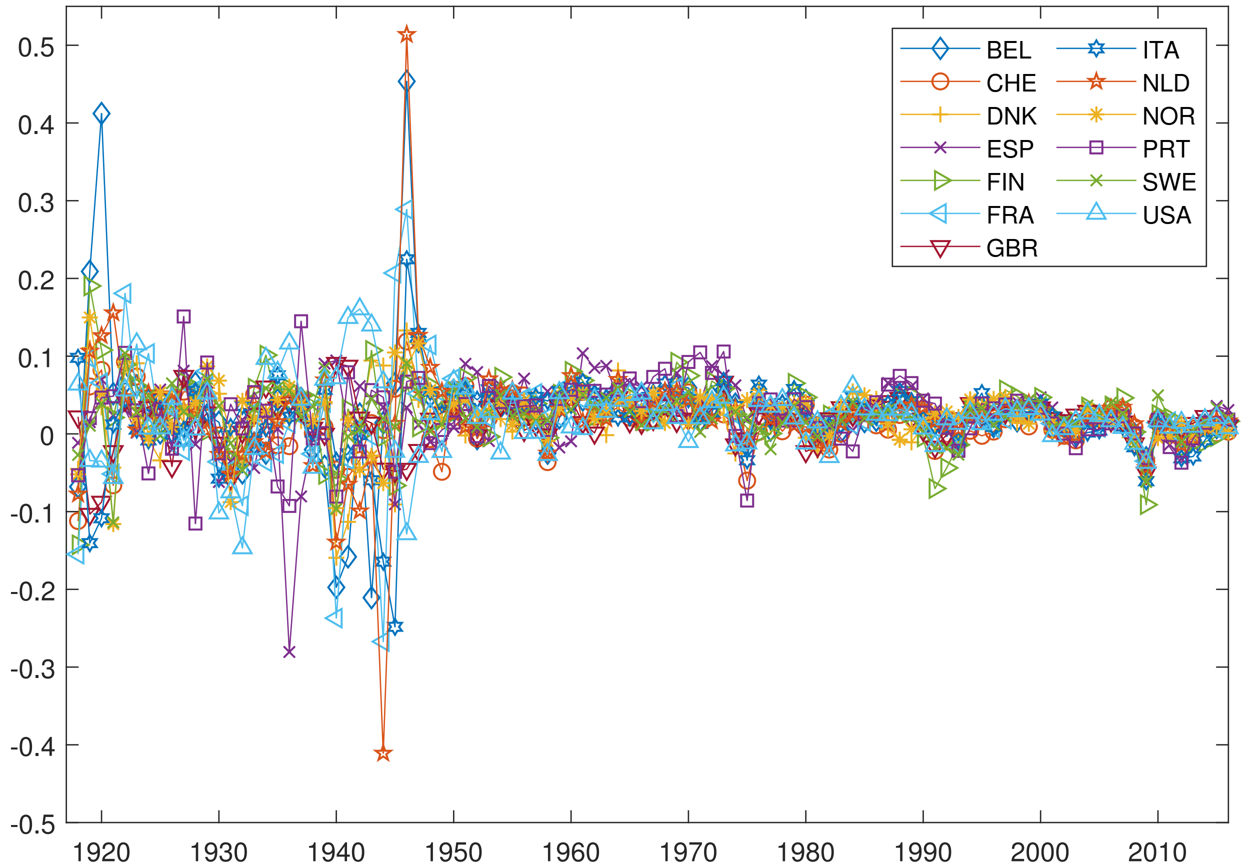


Figure A3: Output growth.



A2 Robustness

Alternative specifications of the baseline model. Table A1 presents the results of various robustness tests mentioned in the main text. It shows results for the cumulative multiplier at horizon $h = 0$, $h = 2$, and $h = 4$ during ZLB and non-ZLB episodes when i) excluding time trends from the estimations; ii) including country-specific time trends; iii) excluding the interest rate, inflation rate, and the exchange rate from the vector of control variables; iv) using one and two lags of the control variables; v) controlling for stock prices; vi) controlling for tax responses by including tax revenues as control variable; and vii) normalizing the change in output and government spending by potential GDP, the latter estimated using the HP-filter. The estimates indicate that our main findings are robust to all of these modifications.

Dropping one country at a time. To assess how important any individual country is for the results, we re-estimate the local projections by sequentially dropping one country at a time. As Table A2 indicates, the results are comparable to the baseline in each case.

Table A1: Output multipliers for alternative specifications of baseline model.

	ZLB	Outside ZLB
Baseline		
Impact	2.317*	0.326
	(1.184)	(0.591)
2 Year	1.650**	0.422
	(0.736)	(0.710)
4 Year	1.566**	0.101
	(0.744)	(0.746)
Excluding time trends		
Impact	1.623*	0.303
	(0.982)	(0.671)
2 Year	1.382**	0.499
	(0.704)	(0.871)
4 Year	1.279*	0.384
	(0.713)	(0.999)
Country-specific time trends		
Impact	2.433**	0.369
	(0.955)	(0.608)
2 Year	1.636***	0.657
	(0.515)	(0.677)
4 Year	1.527***	0.642
	(0.528)	(0.831)
Excluding additional controls		
Impact	2.088*	0.226
	(1.239)	(0.525)
2 Year	1.687*	0.225
	(0.913)	(0.692)
4 Year	1.646*	−0.300
	(0.951)	(0.783)
One lag of control variables		
Impact	2.683**	0.560
	(1.152)	(0.451)
2 Year	1.946*	0.702
	(1.052)	(0.655)
4 Year	1.948*	0.473
	(1.151)	(0.906)
Two lags of control variables		
Impact	2.617**	0.335
	(1.201)	(0.527)
2 Year	1.707*	0.278
	(0.874)	(0.572)
4 Year	1.549*	−0.083
	(0.789)	(0.613)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

Table A1: Output multipliers for alternative specifications of baseline model (continued).

	ZLB	Outside ZLB
Controlling for stock prices		
Impact	2.409*	0.312
	(1.234)	(0.589)
2 Year	1.642**	0.400
	(0.774)	(0.705)
4 Year	1.556*	0.078
	(0.861)	(0.671)
Controlling for tax response		
Impact	2.384**	0.362
	(1.189)	(0.579)
2 Year	1.642**	0.469
	(0.724)	(0.684)
4 Year	1.459**	0.391
	(0.740)	(0.685)
Normalization using potential GDP		
Impact	1.429*	0.036
	(0.744)	(0.749)
2 Year	0.919***	−0.074
	(0.237)	(0.619)
4 Year	0.888***	−0.151
	(0.187)	(0.789)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

Table A2: Dropping one country at a time.

Country excluded	ZLB	Outside ZLB
None (Baseline)		
Impact	2.317*	0.326
	(1.184)	(0.591)
2 Year	1.650**	0.422
	(0.736)	(0.710)
4 Year	1.566**	0.101
	(0.744)	(0.746)
Belgium		
Impact	2.209***	0.431
	(0.813)	(0.589)
2 Year	1.471**	0.351
	(0.666)	(0.797)
4 Year	1.312*	0.035
	(0.744)	(1.050)
Switzerland		
Impact	1.791*	0.418
	(1.001)	(0.609)
2 Year	1.265*	0.355
	(0.650)	(0.740)
4 Year	1.311*	0.050
	(0.776)	(0.676)
Denmark		
Impact	2.455**	0.520
	(1.119)	(0.635)
2 Year	1.684**	0.400
	(0.738)	(0.797)
4 Year	1.579**	0.116
	(0.747)	(0.907)
Spain		
Impact	2.624**	0.656
	(1.212)	(0.671)
2 Year	1.823**	0.505
	(0.738)	(0.783)
4 Year	1.618**	−0.158
	(0.715)	(0.923)
Finland		
Impact	2.805**	0.434
	(1.292)	(0.619)
2 Year	1.711**	0.352
	(0.749)	(0.738)
4 Year	1.567**	0.031
	(0.728)	(0.725)
France		
Impact	2.073*	1.392**
	(1.089)	(0.673)
2 Year	1.617**	0.963
	(0.780)	(0.749)
4 Year	1.537**	0.480
	(0.757)	(0.875)
Great Britain		
Impact	2.937***	0.285
	(0.960)	(0.699)
2 Year	2.057***	0.419
	(0.391)	(1.006)
4 Year	1.656***	−0.023
	(0.336)	(1.229)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

Table A2: Dropping one country at a time (continued).

Country excluded	ZLB	Outside ZLB
Italy		
Impact	2.495**	0.339
	(1.179)	(0.568)
2 Year	1.713**	0.176
	(0.668)	(0.598)
4 Year	1.564**	0.004
	(0.668)	(0.673)
The Netherlands		
Impact	1.618*	0.214
	(0.911)	(0.469)
2 Year	1.223***	−0.334
	(0.373)	(0.459)
4 Year	1.251***	−0.887*
	(0.425)	(0.477)
Norway		
Impact	2.354**	0.399
	(1.159)	(0.597)
2 Year	1.588**	0.329
	(0.667)	(0.698)
4 Year	1.511**	0.077
	(0.667)	(0.789)
Portugal		
Impact	2.106*	0.224
	(1.189)	(0.611)
2 Year	1.608**	0.144
	(0.724)	(0.763)
4 Year	1.522**	−0.272
	(0.722)	(0.898)
Sweden		
Impact	2.171*	0.315
	(1.223)	(0.659)
2 Year	1.604**	0.197
	(0.732)	(0.765)
4 Year	1.519**	−0.095
	(0.716)	(0.793)
United States		
Impact	0.682	0.709
	(1.061)	(0.751)
2 Year	1.715*	0.879
	(0.993)	(1.070)
4 Year	1.964**	0.512
	(0.911)	(1.069)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.

A3 Low versus Constant Interest Rates

To disentangle the effects of the level of the interest rate from the effects of a zero change in interest rates, we estimate an augmented state-dependent local projection instrumental variable model as introduced in Section 4. As before, I_{it}^{A} equals one when the short term interest rate is below, or equal to, one percent, irrespective of the change in the interest rate. The additional state indicator I_{it}^{B} equals one when the change in the short-term interest rate has been approximately zero for two or more consecutive years, irrespective of the level of the interest rate. As Table A3 shows, the additional effect on the fiscal multiplier is large and statistically significant when moving from the neutral state to the low-interest-rate environment. Contrary, the effect is small and insignificant in the case of a constant interest rate. Thus, it seems that the level of the short-term interest rate has a stronger impact on the fiscal multiplier than the change in the interest rate.

Table A3: Additional effect on output multiplier, low vs. constant interest rates

	Low interest rate m_h^A	Constant interest rate m_h^B
Impact	2.357*	0.238
	(1.319)	(0.577)
2 Year	1.665**	0.566
	(0.760)	(0.567)
4 Year	1.541**	0.465
	(0.761)	(0.675)

Notes: The table reports the additional effects on the neutral cumulative multiplier and Driscoll-Kraay standard errors in parentheses. *Significant at 10%; **significant at 5%; ***significant at 1%.