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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 fiscal multipliers that are around 1.5 during zero lower bound episodes and significantly below unity outside of it. 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 evidence that the government spending multiplier is significantly 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 are significantly below unity during normal times.

The Global Financial Crisis 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).[1] 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 and Braun, Korber, and Waki 2013). In sum, as the theoretical literature provides us with ambiguous results, an empirical evaluation on 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 zero lower bound is a difficult task because ZLB periods are unusual and extremely scare situations. Two strategies to

[1]The transmission mechanism is that higher government spending increases expected inflation, which given constant nominal interest rates at the ZLB, translates into a decrease in the real interest rate, ultimately boosting private demand.

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 (2017) 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 (2017) do not find that the multiplier is generally larger when interest rates are near the zero lower bound. In this paper, we follow the route suggested by Ramey and Zubairy (2017), but make use of a large historical *international* dataset and provide robust evidence that the government spending multiplier is significantly larger during ZLB periods than during normal times.[2]

Our data series are taken from Jordà, Schularick, and Taylor (2017)'s Macrohistory Database, which provides us a balanced panel of 13 advanced economies from 1885 through 2013, including multiple ZLB episodes. In particular, we detect 83 episodes, approximately 5% 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 local projections, as suggested by Jordà (2005). The responses are allowed to vary depending on whether or not interest rates are at, or near, the zero lower bound. We identify discretionary government spending changes by restricting the contemporaneous response of government spending to economic activity. In doing so, we consider a wide range of elasticities of government spending with respect to current output, including the Blanchard and Perotti

[2] 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 Global Financial Crisis. 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 (2011).

(2002) assumption of a zero within-period response of government spending to output as a special case.

We find that multipliers during ZLB periods are significantly larger than multipliers during normal times. When the economy is stuck at the ZLB, multipliers take values around 1.5, whereas estimates are around 0.6 during normal times. We conduct a battery of robustness checks, including showing that our main result of higher multipliers at the ZLB does not depend on the imposed contemporaneous reaction of government spending to current output. Moreover, we verify that our main results hold independent of the prevailing exchange rate regime (fixed versus flexible). Importantly, we also show that our results are not simply a reflection of multipliers being higher during periods of economic slack.

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 time periods. Given the scarcity of ZLB episodes, this disadvantage is off set by the rich historical dataset that enables inference based on more than 80 episodes of interest rates at, or near, the zero lower bound.

The rest of the paper is organized as follows. Section 2 describes the data and the empirical model. Section 3 presents our main findings concerning the size of the government spending multiplier during ZLB episodes and during normal times. In addition, we present results of various robustness checks that verify our main findings. In Section 4, we discuss the role of the business cycle for our results. Section 5 concludes.

2 Data and Empirical Method

For our analysis, we use historical data provided by the Jordà-Schularick-Taylor Macrohistory Database (Jordà, Schularick, and Taylor 2017). The database covers several advanced economies with annual series going back until the 19th century. The variables we use are: real GDP per capita, real government spending per capita (constructed as government 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. Our balanced panel includes 13 countries for the period 1885-2013, resulting in more than 1600 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.

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 scare 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 these data series cover many more periods of constrained monetary policy, which provides a reasonable number of observations for conducting inference.

In following Bonam, de Haan, and Soederhuizen (2017), we define ZLB periods as those episodes when the short-term interest rate is smaller than, or equal to, 1 percent.

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 2013 (the end of our sample); whereas Ramey and Zubairy (2017) 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 (2017)'s definition of ZLB periods during the late 1940s and early 1950s. Overall, our definition implies that 83 periods, or approximately 5% of our sample, are defined as episodes during which the economy was stuck at the ZLB, while the remaining periods are considered to be non-ZLB periods.

We estimate state-dependent government spending multipliers using local projections as proposed by Jordà (2005) and as applied in the fiscal policy literature by, among others, Ramey and Zubairy (2017) 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 expenditure innovation to a reported horizon h, where h captures the time dimension, years in our case. Following Ramey and Zubairy (2017), we estimate a series of regressions for the cumulative multiplier at each horizon $h = 0, \dots, 4$:

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

where $\frac{Y_{i,t+j} - Y_{i,t-1}}{Y_{i,t-1}}$ is the percentage change in real per capita GDP in country i between time $t-1$ and time $t+j$, $\nu_{i,h}$ are country fixed effects, $\delta_{t,h}$ capture time fixed effects, t and t^2 are linear and quadratic time trends, and $X_{i,t-1}$ is a vector of control variables. $\nu_{i,h}$ and $\delta_{t,h}$

are included into the regressions to control for country-specific characteristics and common macro shocks, respectively. We instrument the cumulative change in real per capita government spending, $\sum_{j=0}^{h} \frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}$, by the exogenous (discretionary) component of government spending innovations, $\widetilde{g}_{i,t}$, constructed as

$$\widetilde{g}_{i,t} = g_{i,t} - \mu y_{i,t}, \tag{2}$$

where $g_{i,t}$ is government spending 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 spending to changes in aggregate economic activity. A positive value of μ corresponds to a procyclical behavior of government spending, while a negative value of μ indicates countercyclical government spending. We identify discretionary spending changes by restricting the contemporaneous response of government spending to economic activity, i.e., by calibrating the parameter μ. In our baseline estimation, we set $\mu = 0$ implying that government spending does not react contemporaneously to output (consistent with Blanchard and Perotti (2002)'s recursive identification approach). This assumption requires that government spending does not contain components that automatically fluctuate with the business cycle. Moreover, it requires that policy makers 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. Note, though, that Born and Müller (2012) provide robust evidence that a recursive identification is appropriate for annual post WWII U.S. time-series data.[3] Since both requirements are not *ex ante* assured for our annual historical data, we show in a later exercise that our main findings are robust to imposing a wide range of values of the elasticity of government

[3] In addition, Beetsma and Giuliodori (2011) point out that budget decisions are typically made once a year, and argue that, consequently, annual data provide a more natural way to reconcile discretionary fiscal policy changes.

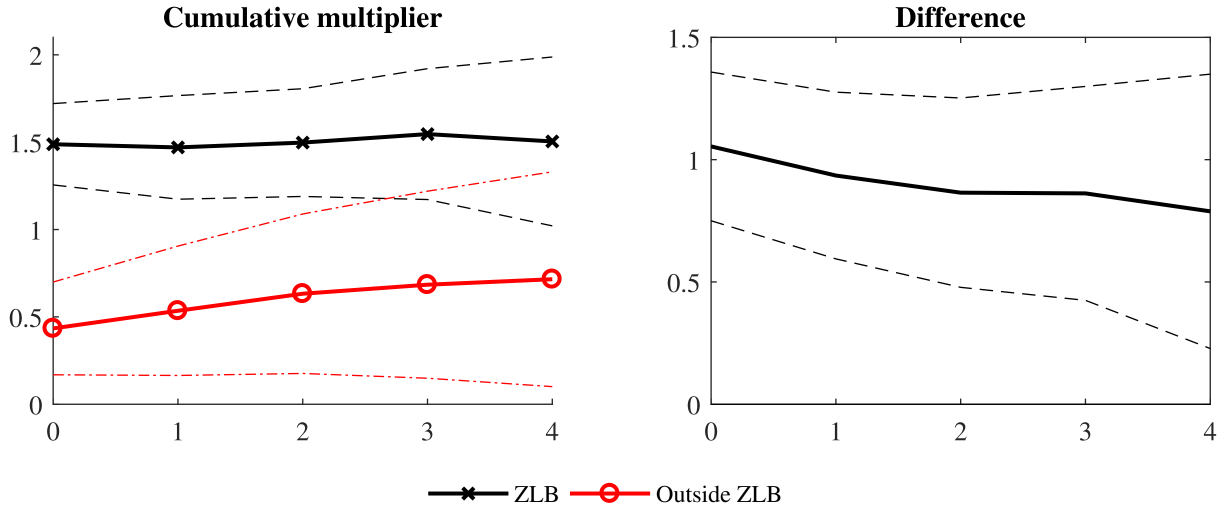
spending with respect to current output μ, following Nguyen, Sergeyev, and Miyamoto (2017), Beetsma and Giuliodori (2011), and Beetsma, Giuliodori, and Klaassen (2008). We also show that our results hold if we allow the parameter μ to be state-dependent; that is, to take different values during ZLB periods and during normal times.

The dummy variable $I_{i,t}$ captures the state $\{A, B\}$ of the economy prior to the 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. 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.

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 2011 and Fisher and Peters 2010). Another approach to account for fiscal foresight is to add a series of government spending forecasts to the set of control variables (see, e.g., Auerbach and Gorodnichenko 2012). Both approaches are not feasible in our case because the Jordà-Schularick-Taylor Macrohistory Database does not provide us with the required information. The literature, though, suggests other approaches for addressing these anticipation problems. One is to use annual data, as we do (see, e.g., Beetsma and Giuliodori 2011 and Ramey 2011). 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 anticipation problems 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 two lags of the log of real per capita GDP, log of real per capita government spending, consumer price inflation, the short term interest rate, and the log of the exchange rate. Yang (2007) shows that including short-term interest rates and prices include information about future fiscal policy shocks. Besides accounting for fiscal foresight, these variables also 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). We also incorporate the exchange rate following the suggestion of Forni and Gambetti (2010) to include a financial market variable that, due its forward-looking nature, helps to account for fiscal foresight. In a robustness exercise, we alternatively include stock prices. We do not include them in our baseline specification because our dataset contains stock prices just for a shorter time span.

We estimate equation (1) using a fixed effects estimator. Standard errors are computed 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.

Figure 1: Fiscal multipliers across monetary policy regimes.



Notes: Left panel: Cumulative output multiplier across different horizons in ZLB states (solid line with crosses) and in normal states (solid line with circles). Right panel: Difference in cumulative multipliers between ZLB and non-ZLB states. Dashed lines show 90% confidence bands.

3 Results

In this section, we first present estimation results of the baseline model. Afterwards, we discuss results of several modifications of the baseline setting; most importantly, we show that our main results do not depend on how we calibrate the contemporaneous response of government spending to economic activity.

The left panel of Figure 1 displays the cumulative government spending multiplier for each horizon from impact to four years after the fiscal shock. The solid line with crosses shows the multiplier during ZLB periods, whereas the solid line with circles 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 significantly greater than unity with point estimates of about 1.5 in ZLB periods, it is as small as between 0.5 and 0.6 in normal times. The estimated multiplier in ZLB periods is in line with the predictions of the New Keynesian model for an economy stuck at the zero lower bound due to fundamental shocks, suggesting a significant crowding-in of private demand in response to an exogenous increase in government spending. In terms of magnitude, our estimate for the 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.[4] Our estimated multiplier during normal times is in the ballpark of linear (state-independent) estimates based on U.S. historical data (see, e.g., Ramey 2011 and Barro and Redlick 2011). It is also consistent with the canonical New Keynesian model predicting that an expansion of government spending crowds out private economic activity when monetary policy is unconstrained. Importantly, the difference in multipliers across states is not only quantitatively important, but it is also statistically significant, as seen in the right panel of Figure 1, showing the difference in multipliers across states. The difference between both multipliers is strongest on impact and becomes smaller, but remains statistically significant, at the end of the forecast horizon.

Our results are 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, changing the lag length, and using stock prices instead of exchange rates as control variable. Our estimates are also not driven by any key country in the sample. Moreover, our results prove 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

[4] Recall that Japan is not included in our dataset.

fixed exchange rate regimes. However, this rationale is not supported by the data. First, out of the 83 periods defined as ZLB episodes, only 40 periods are also classified as fixed exchange rate regimes, while the remaining 43 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. Details on all these robustness checks are in the Appendix.

Importantly, our results are robust to alternative calibrations of the elasticity of government spending to current output, μ. In our baseline estimation, we set this elasticity to zero. While this is assumption is consistent with a large literature following the seminal contribution of Blanchard and Perotti (2002), it leads to biased estimates when the true elasticity, μ, is non-zero, i.e., when government spending reacts systematically to output changes within a given period. If the bias has not the same sign and approximately the same size across the ZLB and normal periods, this may explain the difference in multipliers across states. However, this is not the case, as seen in Figure 2, which 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 elasticity μ ranging from -0.5 (highly countercyclical government spending, a 1% increase in output lowers government expenditures by 0.5%) to 0.5 (highly procyclical government spending, a 1% increase in output increases government expenditures by 0.5%). To put these values into perspective, Caldara and Kamps (2017) find a mildly countercyclical behavior of government spending in post-WWII U.S. data ($\mu = -0.13$). By contrast, Fatás and Mihov (2012) find a procyclical behavior of government expenditure for a panel of OECD countries ($\mu = 0.28$). When estimating a simple fiscal rule in the spirit of Fatás and Mihov (2012), we find a value of 0.17 for

μ in our sample.[5] Thus, our chosen interval of elasticities covers a rather wide range of possible values for μ. As before, solid lines with crosses show multipliers in ZLB states, solid lines with circles show multipliers in normal times.

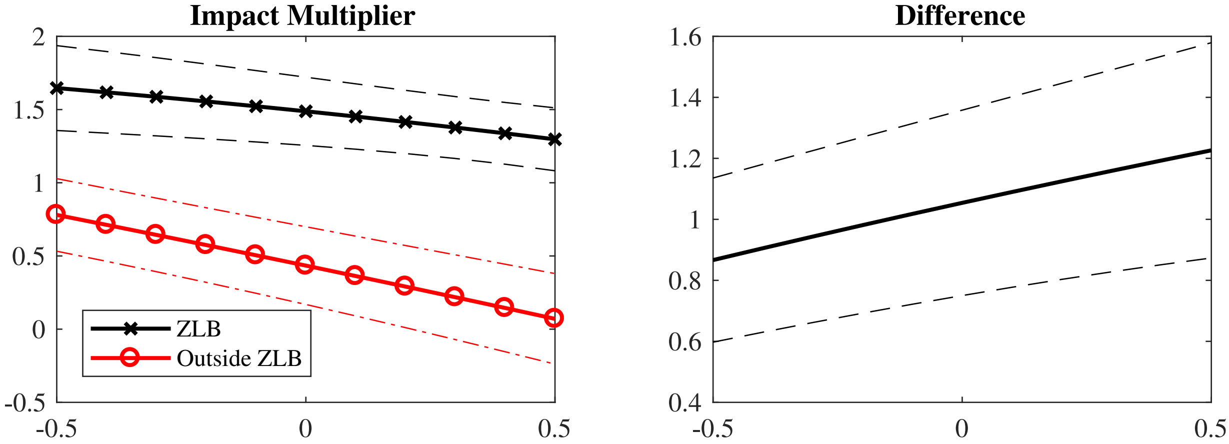
As seen in Figure 2, the multiplier estimates depend on the calibration of μ. Similar to Caldara and Kamps (2017), we find that the multiplier is larger when government spending displays a countercyclical behavior, whereas procyclical government spending reduces the estimated multiplier. For example, the impact multiplier in normal times is around 0.6 when $\mu = -0.5$, whereas it is approximately zero when $\mu = 0.5$. Most importantly, though, this estimation bias does not significantly impact the relative effectiveness of fiscal policy across monetary regimes. Fiscal policy is estimated to be significantly more effective during ZLB states than during normal times, irrespective of how we calibrate μ.

Thus far, we assume that the elasticity of government spending to current output μ is independent of whether the economy is at the ZLB or not. We now check whether our main results hold if we allow the elasticity to differ across monetary regimes. To do so, we assume that government spending behaves strongly procyclical during ZLB periods (i.e., we set $\mu^A = 0.5$) and strongly countercyclical during normal times (i.e., we set $\mu^B = -0.5$). Given that the multiplier decreases in μ (see Figure 2), this calibration works diametrically opposed to the hypothesis that multipliers are larger at the ZLB. Figure 3 shows that – even under this calibration – the output multiplier is found to be significantly larger during ZLB periods than during normal times. Thus, our main result also is confirmed for a state-dependent contemporaneous response of government spending to economic activity.

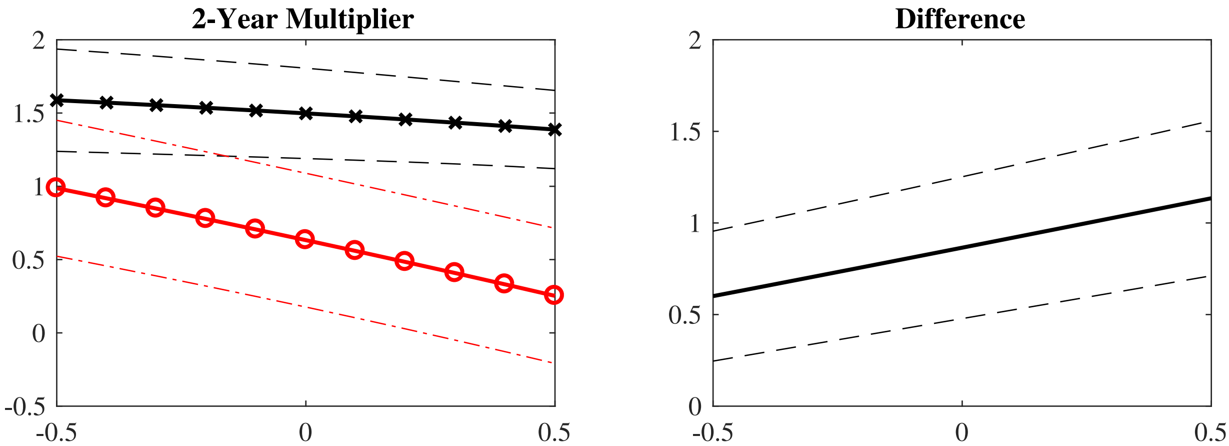
[5] In the estimation, we regress the log of real per capita government spending on the log of contemporaneous real per capita GDP, two lags of log real per capita government spending, a linear and a quadratic time trend, as well as country and time fixed effects.

Figure 2: Fiscal multipliers as function of spending elasticity to output.

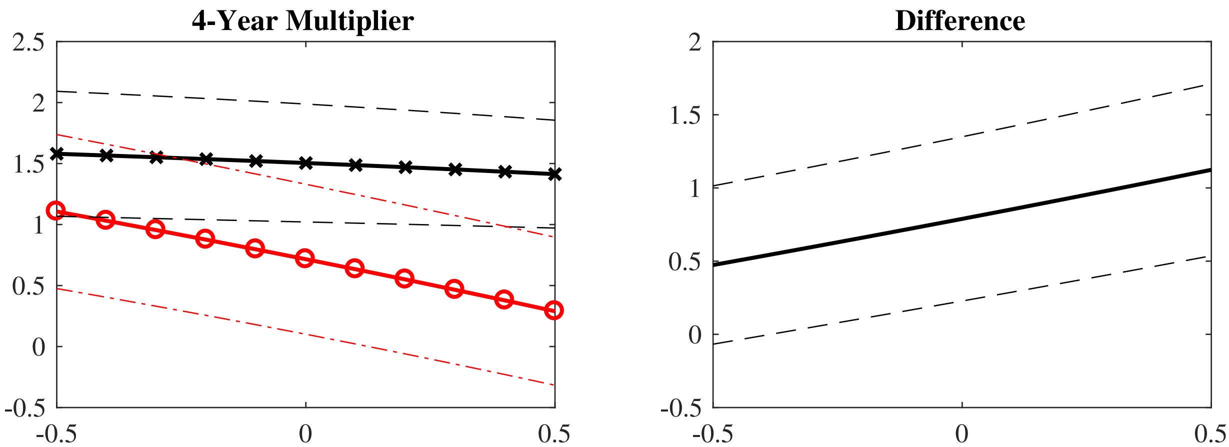
(a) $h = 0$



(b) $h = 2$

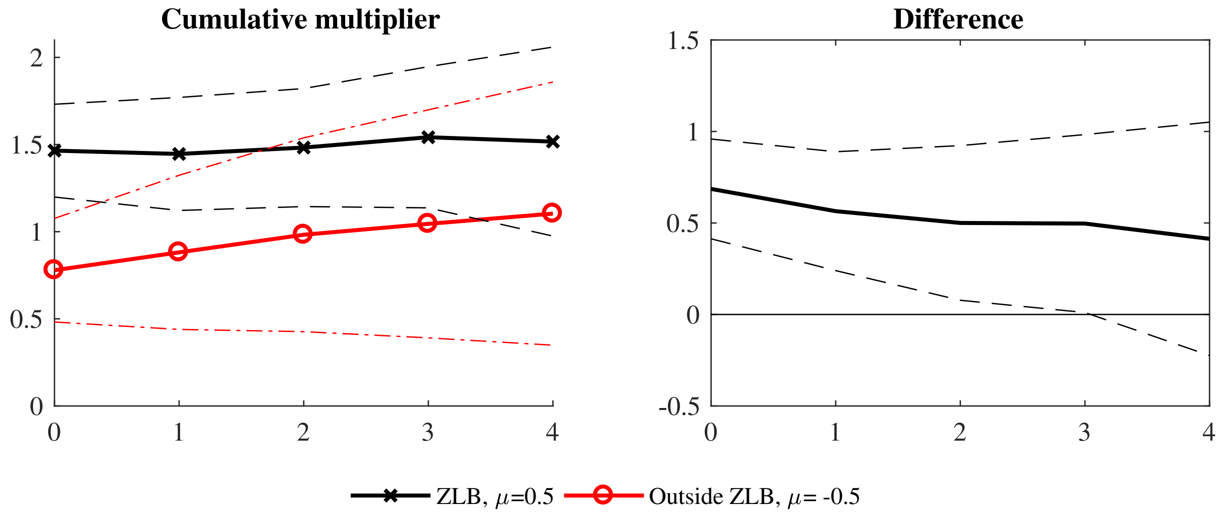


(c) $h = 4$



Notes: Cumulative output multipliers and difference in multipliers for different horizons h across alternative values of the elasticity of government spending with respect to current output μ. Solid lines with crosses show multipliers in ZLB states, solid lines with circles show multipliers in non-ZLB states. Dashed lines show 90% confidence bands.

Figure 3: State-dependent spending elasticity to output.



Notes: Left panel: Cumulative output multiplier across different horizons in ZLB states (solid line with crosses) and in normal states (solid line with circles). Right panel: Difference in cumulative multipliers between ZLB and non-ZLB states. Dashed lines show 90% confidence bands.

4 The Role of the Business Cycle

In the previous analysis, we provide robust 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).[6] Both states are obviously not mutually exclusive as ZLB episodes arise when central banks cut rates during periods of severe economic downturns (the Global Financial Crisis is a recent example). In fact, in our sample 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, however, that

[6] Based on U.S. historical data, the finding of higher fiscal multipliers during recessions is, however, disputed by Owyang, Ramey, and Zubairy (2013) and Ramey and Zubairy (2017).

our estimated multiplier in the ZLB period cannot be attributed to the large effects of government spending in periods of economic slack.

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 real GDP per capita from its long-run HP-trend, where we set the smoothing parameter to 100.[7] We then estimate state-dependent effects of fiscal policy across booms and slumps. In order to separate potential business-cycle effects from that of ZLB periods, we focus on slump periods that do not coincide with ZLB-periods. This procedure implies that out of the 1651 periods included in the sample, 739 or 44% are detected as (non-ZLB) slump periods. We then compare the effects of a fiscal expansion implemented during those (non-ZLB) slump periods to the effects of a fiscal expansion implemented during all other periods (which we call booms for convenience).

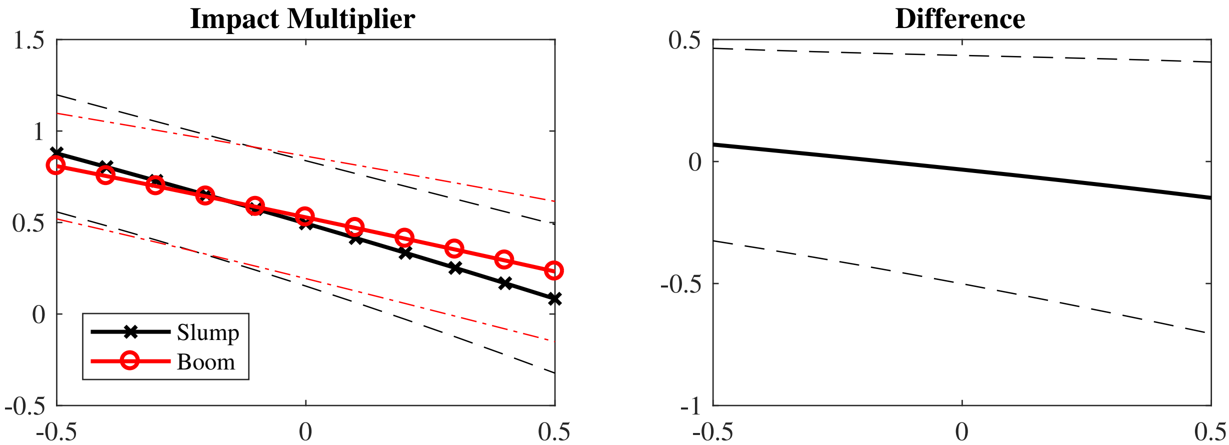
Figure 4 displays estimation results by showing the impact multiplier, the 2-year cumulative multiplier, and the 4-year cumulative multiplier during slumps and booms for different values of μ. The solid lines with crosses show multipliers in periods of economic slack, the solid lines with circles indicate multipliers during boom periods. As shown, there is no evidence of significant differences in the output multiplier across states of the business cycle. This finding holds for different values assigned to μ and all forecast horizons h considered. Within all specifications, the estimated difference between the multiplier in slump states and boom states is statistically indistinguishable from zero.

This also holds true when we do not control for ZLB periods and instead compare the results of fiscal policy during booms and slumps, irrespective of the monetary policy regime. We also find no evidence that the fiscal multiplier is higher in bad times when we consider only deep economic slumps. To do so, we re-define slumps as only those output

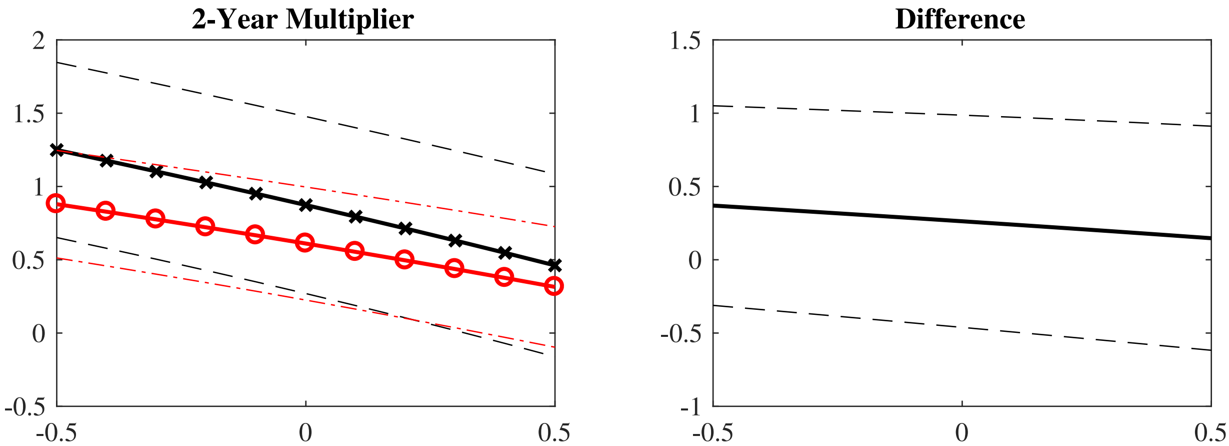
[7]Note that the results are robust to different values of the smoothing parameter.

Figure 4: Fiscal multipliers across states of the business cycle.

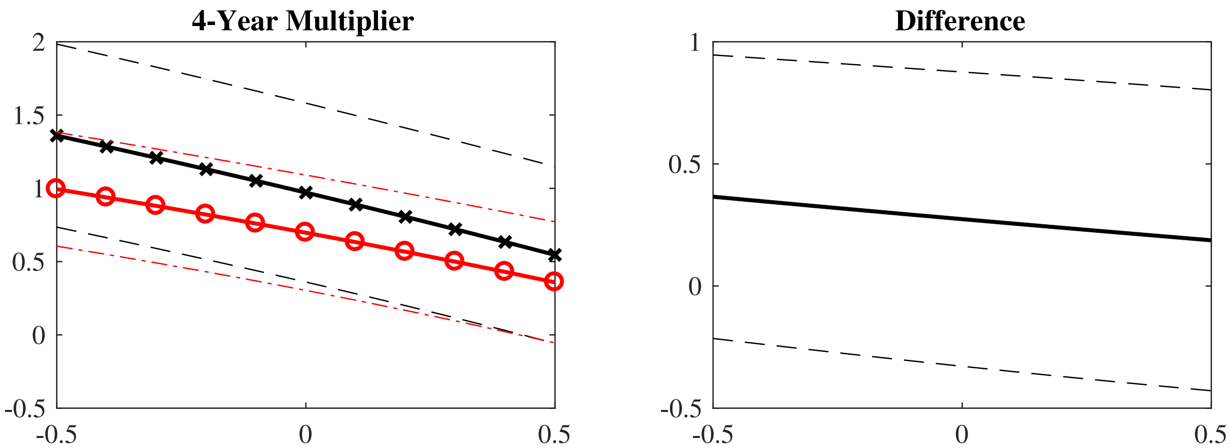
(a) $h = 0$



(b) $h = 2$



(c) $h = 4$



Notes: Cumulative output multiplier for different horizons h across alternative values of the elasticity of government spending with respect to current output μ. Solid lines with crosses show multipliers in slump states, solid lines with circles show multipliers in boom states. Dashed lines show 90% confidence bands.

deviations from trend that are larger than the country-specific mean negative deviations from trend. Details on these exercises are in the Appendix.

From these results, we conclude that the documented nonlinear fiscal policy effects across monetary regimes are not simply a reflection of nonlinear effects across the business cycle.

5 Conclusion

Using historical panel data for 13 advanced countries, we provide robust evidence that the output effects of fiscal policy are significantly larger during ZLB periods than during normal times. This finding is in line with the predictions of the standard New Keynesian model of an economy stuck at the ZLB. From a policy perspective, our findings suggest that the large fiscal stimulus programs undertaken in several countries whose nominal interest rate were 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 Global Financial Crisis.

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Appendix

A1 Data Definitions and Sources

The baseline sample covers the period 1885-2013 and the countries Belgium, Denmark, Finland, France, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and the United States.

All series are taken from the Macrohistory Database. The definition and construction of the respective variables is 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.
- **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.

A2 Robustness

Alternative specifications of 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 together with the difference in multipliers across monetary regimes 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 three lags of the control variables; and v) using stock prices instead of exchange rates as control variable. Due to data availability, the estimation using stock prices is restricted to the period 1915-2013 and the countries of Belgium, Denmark, Finland, France, Italy, the Netherlands, Norway, Spain, Sweden, Switzerland, United Kingdom, and the United States. 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.

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

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

$I^S_{A,i,t}$ 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 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 ± 2%. All other episodes are classified as flexible exchange rates. Based on this definition, Eurozone countries are included as having fixed exchange rates. As the lower part of Table A1 indicates, our main findings are robust to controlling for the exchange rate regime. The government spending multiplier is estimated to be significantly larger during ZLB episodes, irrespective of the specific exchange rate regime. Comparing the point estimates across exchange rate regimes suggest that government spending multipliers are larger under flexible exchange rate regimes. Note, though, that the difference in multipliers between exchange rate regimes is insignificant.

Role of the business cycle. Figure A1 shows cumulative fiscal multipliers for different horizons h across alternative values of the elasticity of government spending with respect to current output μ when considering all periods of economic slack and not just those that do not coincide with ZLB-episodes. The estimates reveal that there is no evidence for business-cycle dependent fiscal multipliers also when applying this approach to define periods of economic slack. Figure A2 presents the multiplier estimates when only considering deep economic slumps. In that case, we define slumps as only those periods in which the negative output gap deviation from trend is larger than the country specific negative deviation from trend. The results show no clear evidence of fiscal multipliers being significantly larger during deep economic slumps.

Table A1: Alternative specifications of baseline model

	ZLB	Outside ZLB	Difference
Baseline			
Impact	1.488***	0.434***	1.054***
	(0.141)	(0.161)	(0.185)
2 Year	1.498***	0.633***	0.865***
	(0.187)	(0.277)	(0.235)
4 Year	1.504***	0.716**	0.789***
	(0.294)	(0.374)	(0.341)
Excluding time trends			
Impact	1.504***	0.456***	1.048***
	(0.143)	(0.164)	(0.181)
2 Year	1.473***	0.685***	0.788***
	(0.183)	(0.286)	(0.219)
4 Year	1.460***	0.801***	0.657**
	(0.312)	(0.389)	(0.343)
Country-specific time trends			
Impact	1.489***	0.428***	1.061***
	(0.124)	(0.163)	(0.182)
2 Year	1.531***	0.623***	0.908***
	(0.157)	(0.278)	(0.235)
4 Year	1.593***	0.698**	0.895***
	(0.271)	(0.382)	(0.318)
Excluding additional controls			
Impact	1.474***	0.411***	1.063***
	(0.169)	(0.168)	(0.187)
2 Year	1.483***	0.583***	0.900***
	(0.212)	(0.292)	(0.238)
4 Year	1.571***	0.623**	0.948***
	(0.366)	(0.387)	(0.340)
One lag of control variables			
Impact	1.558***	0.476***	1.082***
	(0.186)	(0.184)	(0.233)
2 Year	1.451***	0.681***	0.769***
	(0.202)	(0.275)	(0.235)
4 Year	1.375***	0.739**	0.635**
	(0.307)	(0.433)	(0.372)
Three lags of control variables			
Impact	1.520***	0.436***	1.08***
	(0.127)	(0.162)	(0.181)
2 Year	1.546***	0.632***	0.914***
	(0.177)	(0.271)	(0.230)
4 Year	1.716***	0.700**	1.016***
	(0.282)	(0.369)	(0.377)
Controlling for stock prices			
Impact	1.554***	0.457***	1.09***
	(0.195)	(0.195)	(0.231)
2 Year	1.637***	0.824***	0.813***
	(0.277)	(0.402)	(0.356)
4 Year	1.688***	0.929**	0.759
	(0.436)	(0.522)	(0.611)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 16%; **significant at 10%; ***significant at 5%.

Table A2: Dropping one country at a time

Country excluded	ZLB	Outside ZLB	Difference
None (Baseline)			
Impact	1.488***	0.434***	1.054***
	(0.141)	(0.161)	(0.185)
2 Year	1.498***	0.633***	0.865***
	(0.187)	(0.277)	(0.235)
4 Year	1.504***	0.716**	0.789***
	(0.294)	(0.374)	(0.341)
Belgium			
Impact	1.432***	0.368***	1.064***
	(0.145)	(0.181)	(0.232)
2 Year	1.415***	0.470**	0.944***
	(0.190)	(0.262)	(0.262)
4 Year	1.433***	0.494	0.939***
	(0.293)	(0.369)	(0.349)
Switzerland			
Impact	1.466***	0.426***	1.040***
	(0.159)	(0.163)	(0.198)
2 Year	1.506***	0.625***	0.881***
	(0.214)	(0.281)	(0.265)
4 Year	1.565***	0.694**	0.870***
	(0.189)	(0.379)	(0.331)
Denmark			
Impact	1.528***	0.474***	1.054***
	(0.160)	(0.159)	(0.206)
2 Year	1.539***	0.687***	0.853***
	(0.198)	(0.271)	(0.242)
4 Year	1.551***	0.792***	0.759***
	(0.300)	(0.367)	(0.346)
Spain			
Impact	1.522***	0.450***	1.072***
	(0.161)	(0.156)	(0.191)
2 Year	1.565***	0.646***	0.917***
	(0.202)	(0.275)	(0.235)
4 Year	1.608***	0.721**	0.888***
	(0.334)	(0.378)	(0.352)
Finland			
Impact	1.536***	0.391***	1.144***
	(0.175)	(0.175)	(0.196)
2 Year	1.529***	0.602***	0.927***
	(0.201)	(0.298)	(0.261)
4 Year	1.534***	0.672**	0.861***
	(0.304)	(0.398)	(0.372)
France			
Impact	1.477***	0.468***	1.009***
	(0.146)	(0.182)	(0.223)
2 Year	1.474***	0.663***	0.811***
	(0.184)	(0.326)	(0.296)
4 Year	1.495***	0.729**	0.766**
	(0.282)	(0.436)	(0.411)
Great Britain			
Impact	1.484***	0.364***	1.119***
	(0.135)	(0.162)	(0.181)
2 Year	1.533***	0.539**	0.994***
	(0.191)	(0.286)	(0.262)
4 Year	1.417***	0.612*	0.805**

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 16%; **significant at 10%; ***significant at 5%.

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

Country excluded	ZLB	Outside ZLB	Difference
Italy			
Impact	1.469***	0.375***	1.095***
	(0.159)	(0.172)	(0.198)
2 Year	1.484***	0.511**	0.973***
	(0.205)	(0.286)	(0.243)
4 Year	1.477***	0.542	0.936***
	(0.318)	(0.385)	(0.344)
The Netherlands			
Impact	1.305***	0.485***	0.819***
	(0.114)	(0.164)	(0.139)
2 Year	1.325***	0.682***	0.643***
	(0.165)	(0.284)	(0.197)
4 Year	1.243***	0.782***	0.461*
	(0.187)	(0.375)	(0.289)
Norway			
Impact	1.527***	0.512***	1.015***
	(0.149)	(0.151)	(0.183)
2 Year	1.519***	0.714***	0.805***
	(0.203)	(0.274)	(0.226)
4 Year	1.558***	0.807***	0.751***
	(0.314)	(0.370)	(0.339)
Portugal			
Impact	1.501***	0.436***	1.064***
	(0.155)	(0.156)	(0.200)
2 Year	1.532***	0.652***	0.881***
	(0.196)	(0.269)	(0.238)
4 Year	1.558***	0.751***	0.807***
	(0.306)	(0.369)	(0.349)
Sweden			
Impact	1.500***	0.445***	1.055***
	(0.163)	(0.174)	(0.206)
2 Year	1.499***	0.632***	0.866***
	(0.224)	(0.284)	(0.260)
4 Year	1.547***	0.707**	0.839***
	(0.307)	(0.381)	(0.361)
United States			
Impact	1.419***	0.407***	1.013***
	(0.306)	(0.189)	(0.215)
2 Year	1.443***	0.699***	0.745***
	(0.460)	(0.299)	(0.367)
4 Year	1.589***	0.848***	0.742*
	(0.583)	(0.367)	(0.477)

Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 16%; **significant at 10%; ***significant at 5%.

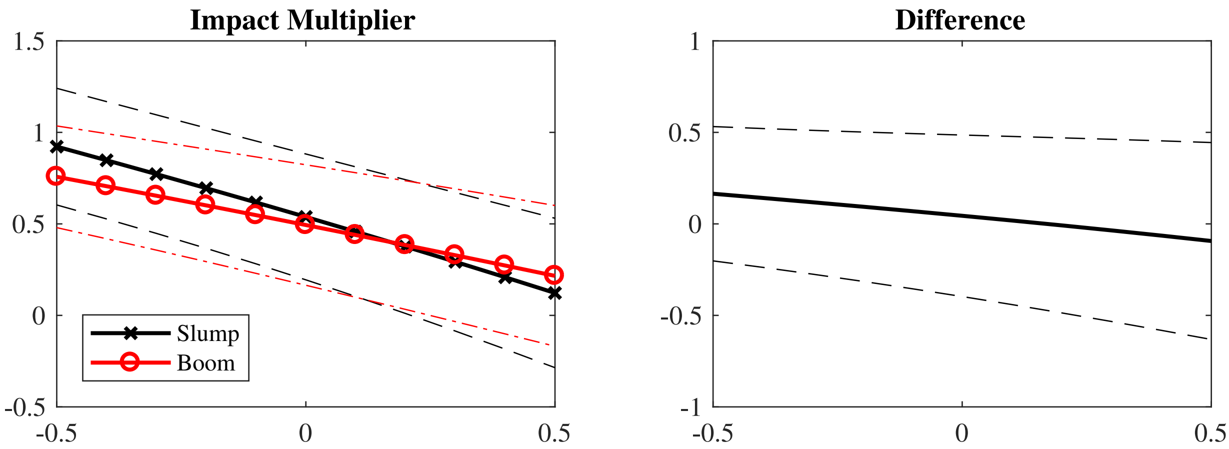
Table A3: Controlling for exchange rate regime

	ZLB	Outside ZLB	Difference
Fixed exchange rate regime			
Impact	1.573***	0.206	1.366***
	(0.147)	(0.177)	(0.203)
2 Year	1.553***	0.334**	1.219***
	(0.104)	(0.201)	(0.304)
4 Year	1.564***	0.546**	1.018***
	(0.462)	(0.304)	(0.429)
Flexible exchange rate regime			
Impact	2.062***	0.689***	1.372***
	(0.371)	(0.233)	(0.339)
2 Year	1.834***	1.012***	0.822***
	(0.510)	(0.404)	(0.348)
4 Year	1.903***	1.014***	0.889
	(0.874)	(0.428)	(0.841)

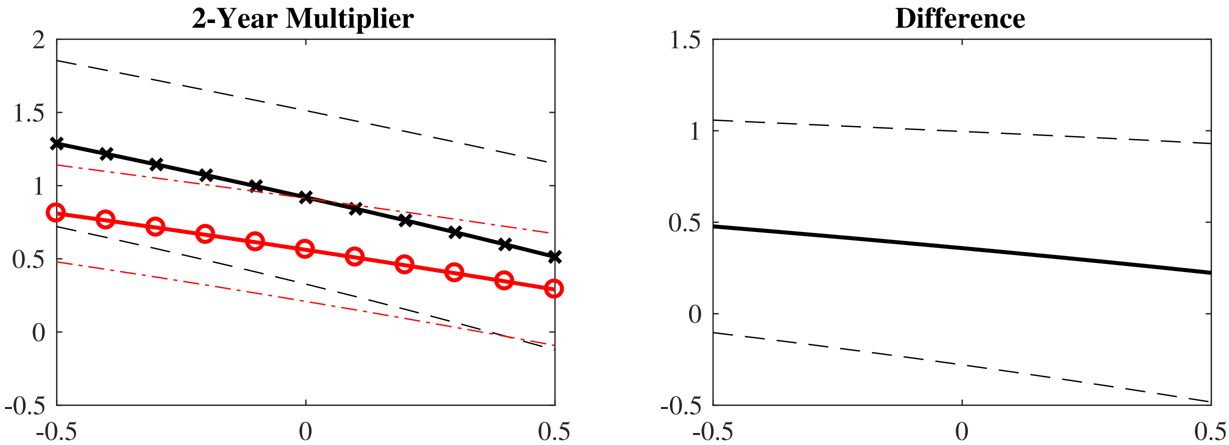
Notes: The table reports cumulative multiplier estimates and Driscoll-Kraay standard errors in parentheses. *Significant at 16%; **significant at 10%; ***significant at 5%.

Figure A1: Fiscal multipliers across states of the business cycle, alternative definiton of boom/slump states.

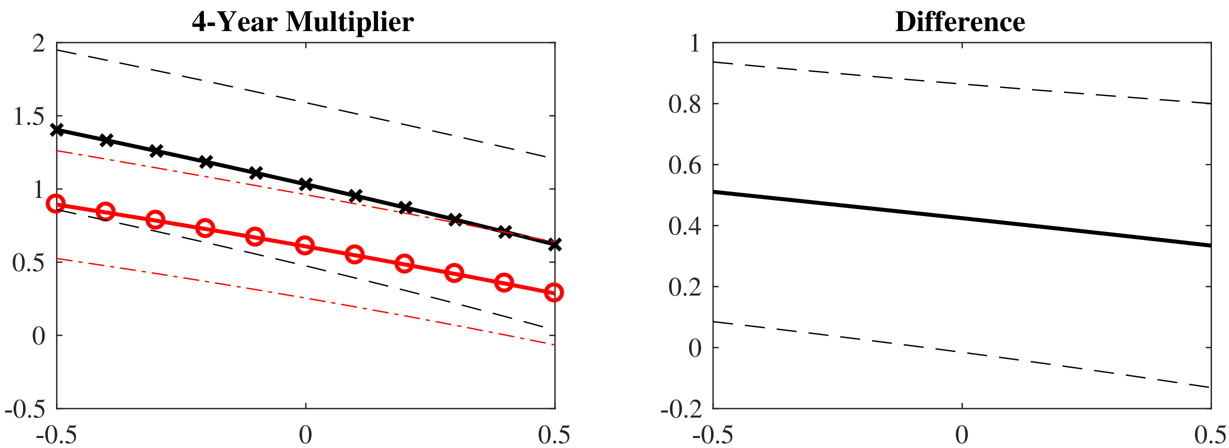
(a) $h = 0$



(b) $h = 2$



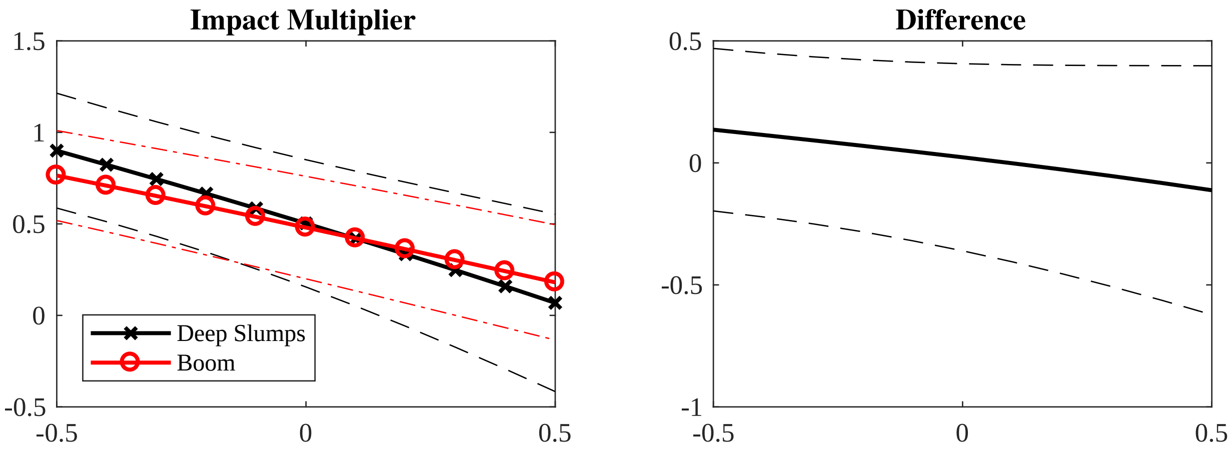
(c) $h = 4$



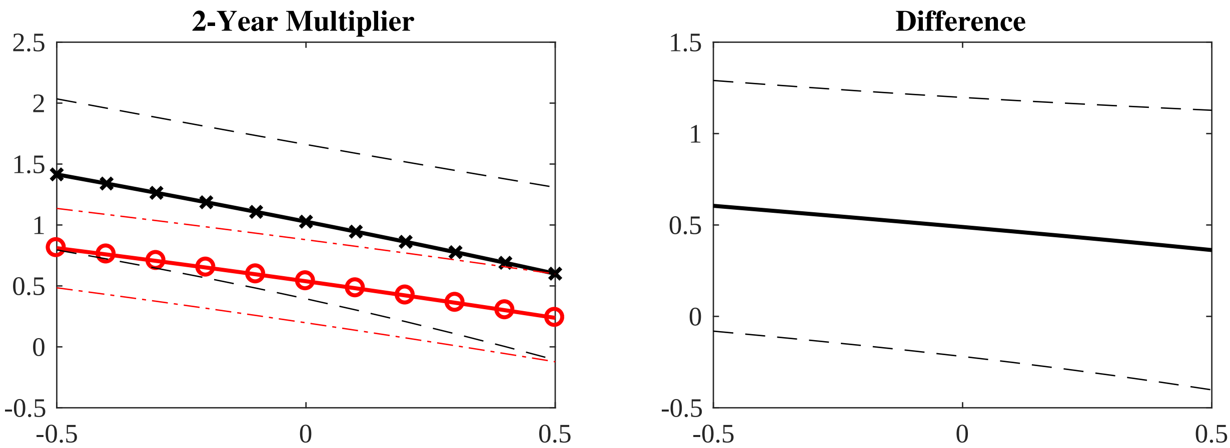
Notes: Cumulative output multipliers and difference in multipliers for different horizons h across alternative values of the elasticity of government spending with respect to current output μ. Solid lines with crosses show multipliers in slumps, solid lines with circles show multipliers in booms. Dashed lines show 90% confidence bands.

Figure A2: Fiscal multipliers across states of the business cycle, deep slumps/booms.

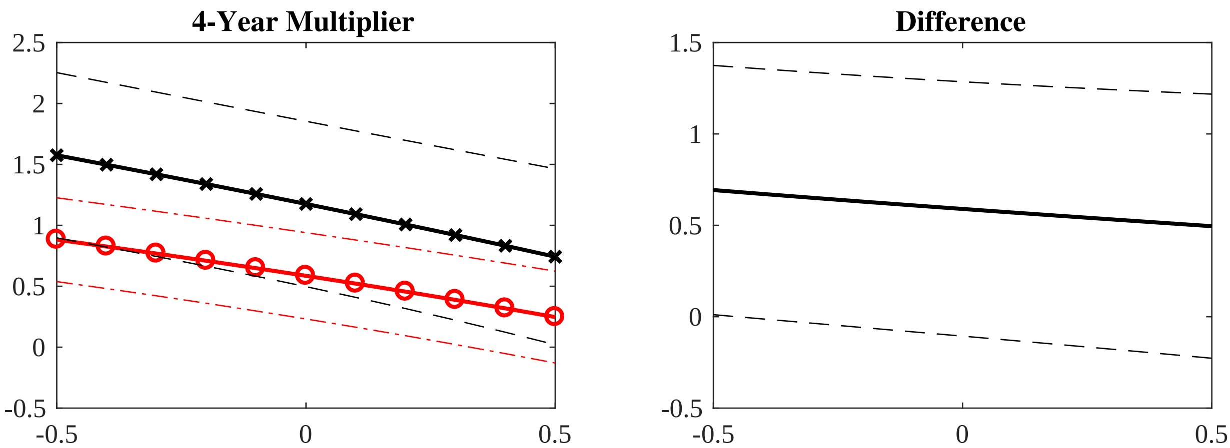
(a) $h = 0$



(b) $h = 2$



(c) $h = 4$



Notes: Cumulative output multipliers and difference in multipliers for different horizons h across alternative values of the elasticity of government spending with respect to current output μ. Solid lines with crosses show multipliers in deep slumps, solid lines with circles show multipliers in booms. Dashed lines show 90% confidence bands.