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 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). 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. 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 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 (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 ZLB. 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.[1]

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 ZLB. In our baseline, we identify discretionary

[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 (2011).

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 (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 show that our results are a robust feature of the data by conducting a series 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, instrumenting aggregate government spending shocks through military spending data, and using alternative definitions of ZLB states. Moreover, we verify that our main results hold independent of the prevailing exchange rate regime (fixed versus flexible) and do not depend on special circumstances during the world war episodes. Importantly, we also show that our results are not simply a reflection of multipliers being generally higher during periods of economic slack or during financial crises.

We present additional evidence that supports the predictions of the New Keynesian model for an economy stuck at the ZLB, as described by, amongst others, Christiano, Eichenbaum, and Rebelo (2011) and Eggertsson (2011). We observe a significant decline in the real interest rate and a significant rise in private consumption in response to a government spending shock when the monetary authority is constrained by the ZLB. During normal times, by contrast, the real interest rate rises and we do not find evidence for a crowding-in of private consumption.

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 ZLB.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 presents the empirical model. Section 4 presents our main findings concerning the size of the government spending multiplier during ZLB episodes and during normal times as well as the results of several robustness checks. In Section 5, we take a closer look at the underlying transmission mechanism of government spending shocks at the ZLB. Finally, Section 6 concludes.

2 Data

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 in our baseline specification 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

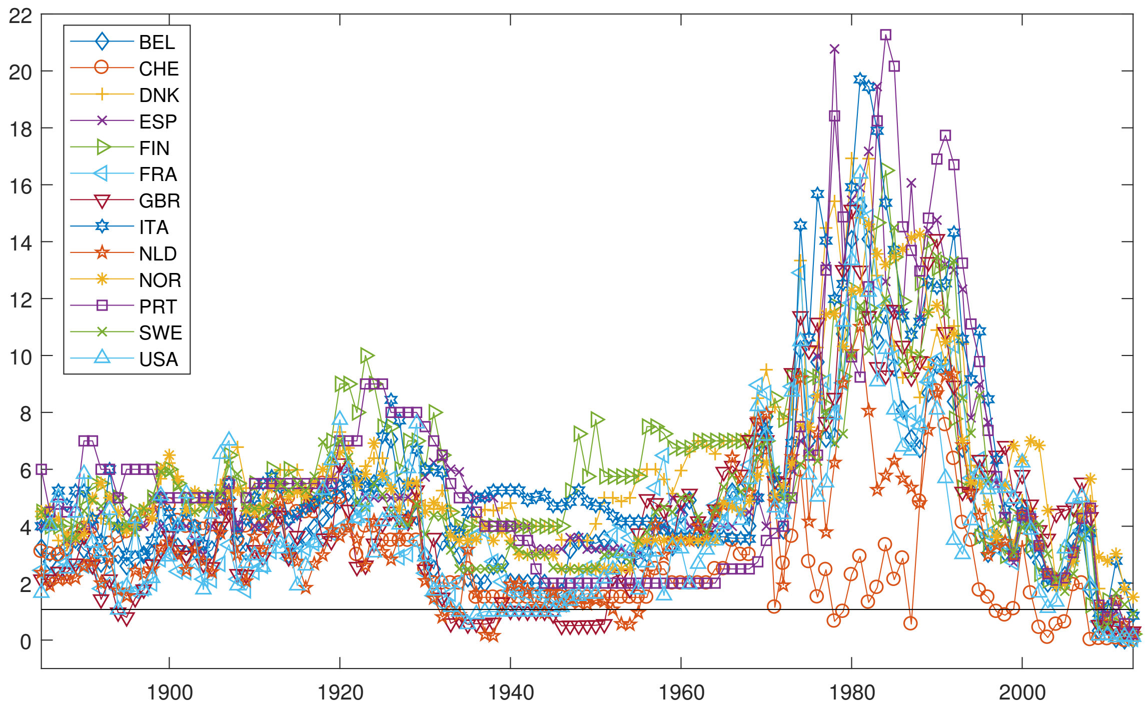
Appendix for details on data construction. Our balanced panel includes 13 countries for the period 1885-2013, resulting in more than 1600 observations. The beginning and the end of the sample are restricted by the data availability for some countries. The countries included are Belgium, Denmark, Finland, 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 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.

The 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.[2] As our baseline, we define ZLB periods as those episodes when the short-term interest rate is smaller than, or equal to, 1 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 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

[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`.

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



is more conservative than Ramey and Zubairy (2017)'s definition of ZLB periods during the late 1940s and early 1950s. 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 1885-2013. 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. 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. While in our baseline definition of ZLB periods, we set a specific threshold to divide between episodes of constrained and unconstrained monetary policy, we verify below that our results are robust when considering a smooth transition between both regimes.

3 Empirical Method

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 $h = 0, \ldots, 4$:

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

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.

In our baseline specification, 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

$$\widetilde{g}_{i,t} = g_{i,t} - \mu_i y_{i,t}, \qquad (2)$$

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 in country i. A positive value of μ_i corresponds to a procyclical behavior of government spending, while a negative value of μ_i 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_i. In our baseline estimation, we assume that μ_i is common across countries and impose $\mu_i = \mu = 0$ 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.[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 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. Moreover, we allow for variation in the strength of stabilizers across countries; that is, we allow the spending elasticity to current output to differ across countries. Within all these exercises, identification of the fiscal shock is achieved by restricting the systematic response of fiscal policy to output. An alternative identification scheme is to instrument total government spending by military spending that is seen as exogenous with respect to the state of the

[3]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. 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.

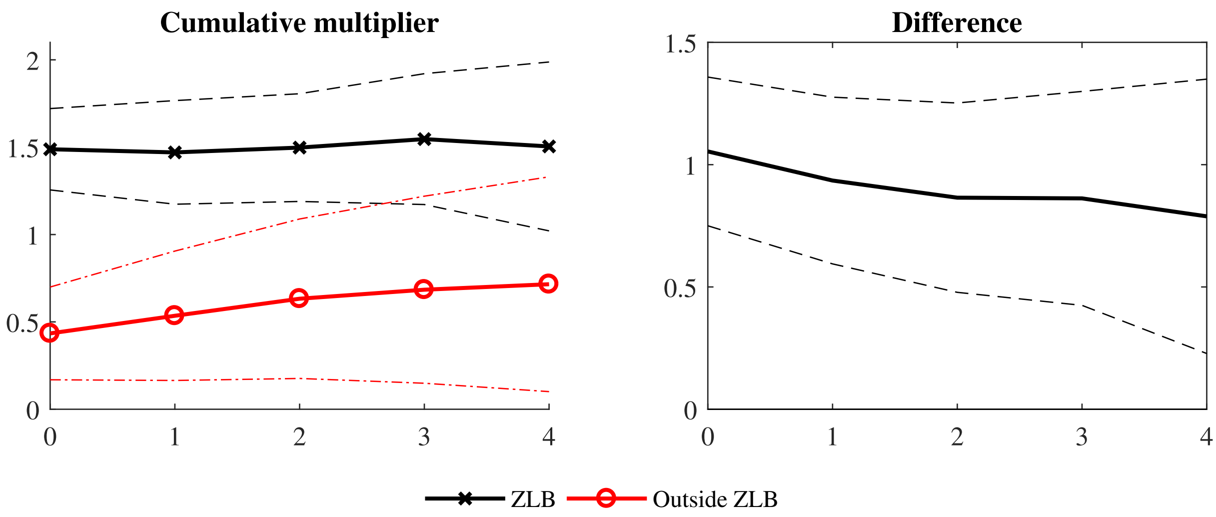
economy, see, e.g., Hall (2009) and Barro and Redlick (2011). In an additional robustness check, we follow this route and use military expenditure data from the Correlates of War Project to predict government spending innovations.

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 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 sample. 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 the log of 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 also incorporate consumer price inflation and the short-term interest rate. Yang (2007) shows that 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).

The vector of control variables $X_{i,t}$ includes, furthermore, two lags of the log of real per capita GDP and two lags of the log of real per capita government spending. 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 2: 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.

4 Results

In this section, we first present estimation results of the baseline model. Afterwards, we show that our main results are a robust feature of the data by presenting results of a series of robustness checks.

4.1 Baseline

The left panel of Figure 2 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 ZLB due to fundamental shocks, suggesting a significant crowding-in of private demand in response to an exogenous increase in government spending.[4] 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. Note that Japan is not part of our sample, which means that our results cannot be driven by the prolonged low interest rate episode of the Japanese economy. 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). 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 2, 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.

4.2 Robustness

Our main finding of significantly 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, changing the lag length, and using stock prices instead of exchange rates as control variable. 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. We also show robustness to normalizing changes in output and government spending by an estimate of potential, or trend, GDP

[4] In a later section, we take a closer look at the transmission mechanism and provide additional evidence that supports the predictions of the New Keynesian model for an economy stuck at the ZLB.

following Gordon and Krenn (2010) and Ramey and Zubairy (2017).[5] Our estimates are also not driven by any key country in the sample. Details on these robustness checks can be found in the Appendix.

In the following, we present, and discuss in more detail, results of further robustness exercises. First, we show that our main results hold for alternative definitions of ZLB states. Second, we show that our main results do not depend on how we calibrate the contemporaneous response of government spending to economic activity. Third, we use military spending as an alternative instrument for exogenous government spending changes. Fourth, we control for exchange rate regimes. Fifth, we leave out the observations pertaining to the world wars. Finally, we explore the role of the business cycle and financial crises.

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, 1 percent for two or more consecutive years. Table 1 presents the results of this exercise and compares them to our baseline case. When relying on this alternative state definition, we again find that the multiplier during ZLB episodes is statistically larger than during normal times. However, we find no evidence that the government spending multiplier changes significantly with the duration of the ZLB episode.

So 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

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

Table 1: Alternative definition of ZLB episodes.

	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)
Prolonged ZLB episode			
Impact	1.375***	0.433***	0.943***
	(0.219)	(0.161)	(0.249)
2 Year	1.376***	0.637***	0.739***
	(0.180)	(0.275)	(0.234)
4 Year	1.303***	0.727***	0.576***
	(0.304)	(0.353)	(0.286)
Smooth transition			
Impact	1.4110***	0.423***	0.988***
	(0.363)	(0.152)	(0.360)
2 Year	1.662***	0.627***	1.036***
	(0.426)	(0.249)	(0.527)
4 Year	1.693***	0.726***	0.967*
	(0.472)	(0.329)	(0.641)

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

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 mean 1% and standard deviation 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 1 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 significantly 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.[6]

[6]Detailed results on these exercises are available from the authors upon request.

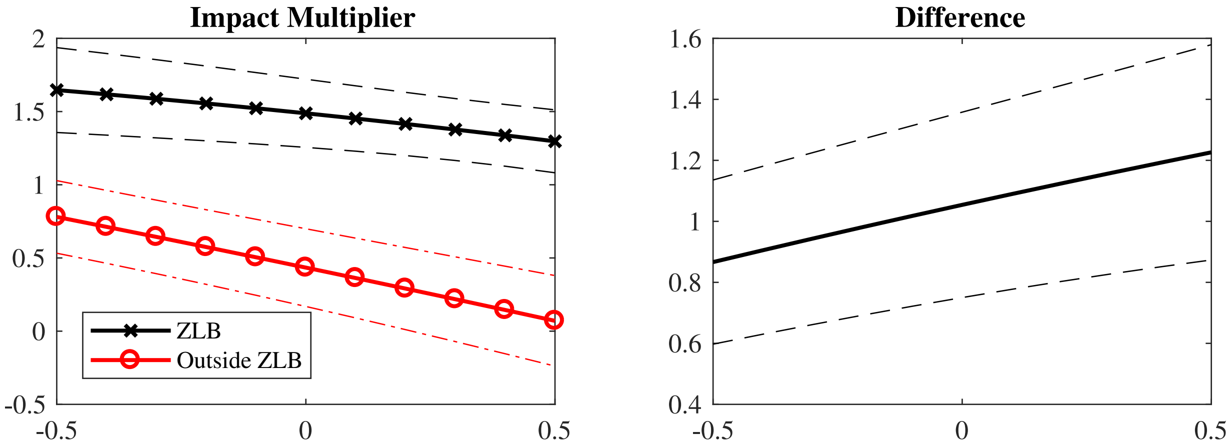
Alternative Calibrations of Spending Elasticity to Current Output. In what follows, we show that our results are robust to alternative calibrations of the elasticity of government spending to current output. In our baseline estimation, we set this elasticity to zero. While this assumption is consistent with a large literature following the seminal contribution of Blanchard and Perotti (2002), it leads to biased estimates when the true elasticity 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 ZLB and normal periods, this may explain the difference in multipliers across states. However, this is not the case, as seen in Figure 3, 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 spending elasticity to current output 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.[7] Thus, our chosen interval of elasticities covers a rather wide range of possible values. As before, solid lines with crosses show multipliers in ZLB states, solid lines with circles show multipliers in normal times.

As seen in Figure 3, the multiplier estimates depend on the calibration of the spending elasticity to current output. Similar to Caldara and Kamps (2017), we find that the mul-

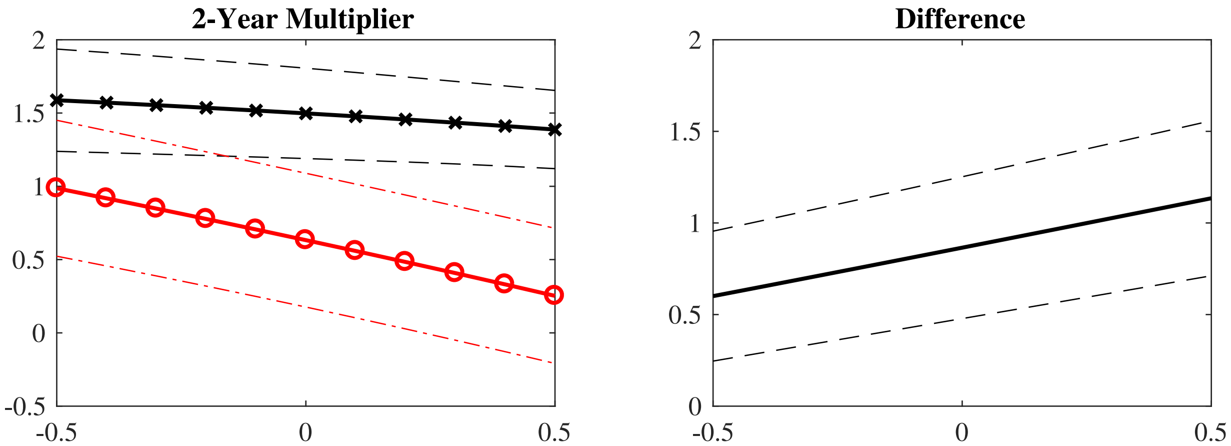
[7] In the estimation, we regress the log of real per capita government spending on the log of contemporaneous real per capita GDP, two lags of log real per capita government spending, a linear and a quadratic time trend, as well as country and time fixed effects.

Figure 3: Fiscal multipliers as function of spending elasticity to output.

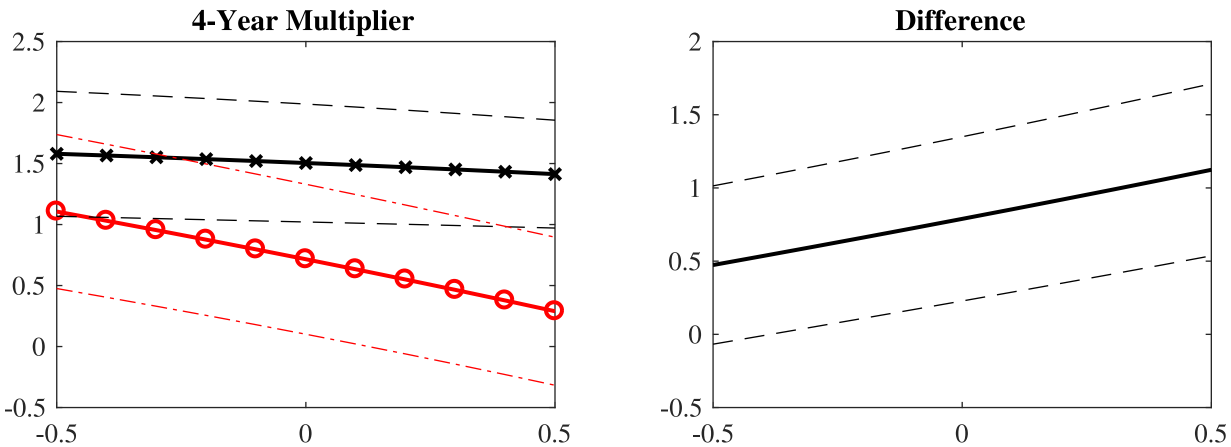
(a) $h = 0$



(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.

tiplier 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 regimes. Fiscal policy is estimated to be significantly more effective during ZLB episodes than during normal times, irrespective of how we calibrate μ.

Thus far, we assume that the elasticity of government spending to current output 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 3), this calibration works diametrically opposed to the hypothesis that multipliers are larger at the ZLB. The upper part of Table 2 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.

Our results so far are based on the assumption that the elasticity of government spending to economic activity is common across countries. While this is somewhat in line with our panel approach of estimating average effects, it is important to check whether our results hold when we allow for country-specific elasticities. To do so, we estimate, for each country in our sample, a simple fiscal rule in the spirit of Fatás and Mihov (2012). In the estimation, we regress the log of real per capita government spending on the log of

Table 2: Different calibrations of spending elasticity to current output.

	ZLB	Outside ZLB	Difference
State-dependent μ			
Impact	1.465***	0.779***	0.686***
	(0.136)	(0.152)	(0.139)
2 Year	1.483***	0.983***	0.500***
	(0.173)	(0.284)	(0.216)
4 Year	1.517***	1.104***	0.413
	(0.277)	(0.385)	(0.325)
Country-specific μ			
Impact	2.483***	0.641***	1.842***
	(0.388)	(0.239)	(0.411)
2 Year	3.329***	0.834***	2.495***
	(0.631)	(0.378)	(0.605)
4 Year	2.011***	0.901*	1.110*
	(0.411)	(0.605)	(0.677)

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

contemporaneous real per capita GDP, two lags of log real per capita government spending, as well as a linear and a quadratic time trend. As the lower part of part of Table 2 indicates, our main findings also go through when using our country-specific estimates of government spending elasticities. Compared to our baseline case, the multiplier during ZLB episodes is larger and the difference to the multiplier during normal times becomes more pronounced.

Military Spending as Instrument. Next, we show that our results are robust when relying on two alternative series as instruments for government spending. In particular, we use the log of real per capita military spending as well as military personnel as a fraction of total population to predict changes in aggregate public expenditures. Hall (2009), Barro and Redlick (2011), and Ramey (2011), amongst others, also use military spending data to identify exogenous government spending shocks. This approach rests on the idea that defense spending is not correlated with the state of the economy and thereby do not face the possible endogeneity problem associated with the Blanchard and Perotti (2002) approach. Our second instrument, military personnel as a fraction of total population, takes into account that, in many countries, a significant component of military spending is directed to foreign goods, and thus has no direct effect on aggregate government spending in the home economy.

The military data series are taken from the National Material Capabilities database of the Correlates of War Project. This database contains historical series for population, military personnel, and military expenditures in US dollars of several economies. For our countries of interest, those data are available just for a shorter period, namely 1918-2012. We use the exchange rate series provided by the Macrohistory database to convert the military expenditure series into local currencies units.

Table 3 presents the results when using these military instruments for aggregate government spending. The estimates show that our baseline findings are robust to this alternative identification approach. When nominal interest rates are at, or near, the ZLB, government spending multipliers are above unity. In contrast, when the monetary authority is not constrained by the ZLB, multipliers are considerably smaller and take values below unity. On average, irrespective which of the alternative instruments is used, the multiplier is twice as large when monetary policy is constrained compared to situations in which the ZLB is not binding. Compared to our baseline specification, the point estimates are generally more uncertain, turning estimates of outside-ZLB multipliers insignificant. While the difference between multipliers is not statistically significant when using military expenditures, the difference is estimated to be significant when relying on military personnel to instrument aggregate government spending. Overall, this exercise indicates that our baseline findings are robust to alternative ways of identifying government spending shocks. Given that we lose a significant amount of information (around 27%) when using the military spending data, we rely on our baseline approach in what follows.

Table 3: Military spending as instrument.

Instrument	ZLB	Outside ZLB	Difference
Military expenditures			
Impact	2.189*	0.344	1.843
	(1.550)	(0.569)	(1.531)
2 Year	1.418***	0.619	0.799
	(0.736)	(0.742)	(0.870)
4 Year	1.428***	0.715	0.714
	(0.736)	(0.724)	(0.851)
Military personnel			
Impact	2.232***	0.738	1.494***
	(0.600)	(0.572)	(0.664)
2 Year	2.387***	0.959	1.428*
	(0.654)	(1.045)	(1.00)
4 Year	2.507***	0.148	2.359*
	(0.835)	(1.521)	(1.621)

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

Controlling for Exchange Rate Regimes. In this section, we show that our result of larger multipliers at the ZLB 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 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.

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:

$$\begin{aligned}\sum_{j=0}^{h}\frac{Y_{i,t+j}-Y_{i,t-1}}{Y_{i,t-1}} = I^{S}_{A,i,t-1}&\left[M^{SA}_{h}\sum_{j=0}^{h}\frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}+\phi^{SA}_{h}(L)X_{i,t-1}\right]\\ &+I^{S}_{B,i,t-1}\left[M^{SB}_{h}\sum_{j=0}^{h}\frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}+\phi^{SB}_{h}(L)X_{i,t-1}\right]\\ &+I^{S}_{O,i,t-1}\left[M^{SO}_{h}\sum_{j=0}^{h}\frac{G_{i,t+j}-G_{i,t-1}}{Y_{i,t-1}}+\phi^{SO}_{h}(L)X_{i,t-1}\right]\\ &+\nu^{S}_{i,h}+\delta^{S}_{t,h}+\psi^{S}_{1}t+\psi^{S}_{2}t^{2}+\varepsilon^{S}_{i,t+h},\quad \text{for } S\in\{C,D\}.\end{aligned}\tag{3}$$

$I^S_{A,i,t}$ 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 4: 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)
Excluding Euro Area			
Impact	1.447***	0.441***	1.01***
	(0.183)	(0.162)	(0.219)
2 Year	1.424***	0.637***	0.787***
	(0.215)	(0.278)	(0.254)
4 Year	1.526***	0.711**	0.814***
	(0.289)	(0.379)	(0.336)

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

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.

As Table 4 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 suggests that government spending multipliers are larger under flexible exchange rate regimes. Note, though, that the difference in multipliers between exchange rate regimes is insignificant.

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 (2017) 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, 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 4 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 significantly 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.

Excluding the World Wars from the Sample. Ramey and Zubairy (2017) deal with confounding effects of WWII during the ZLB period in the U.S. like rationing and military conscription by excluding the sample from their analysis for a robustness check. We follow their approach and focus on non-WWI and/or non-WWII periods. Table 5 presents the results of this exercise. It turns out that our main finding of significantly larger multipliers during periods of constrained monetary policy is robust to excluding major war episodes. This result holds irrespective of whether excluding both world wars separately or together. Thus, our main findings do not seem to be driven by special events during the world war periods.

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

Table 5: Excluding world wars.

	ZLB	Outside ZLB	Difference
Excluding World War I			
Impact	1.516***	0.438***	1.078***
	(0.153)	(0.189)	(0.199)
2 Year	1.511***	0.704***	0.807***
	(0.195)	(0.336)	(0.285)
4 Year	1.496***	0.817**	0.679*
	(0.299)	(0.426)	(0.427)
Excluding World War II			
Impact	1.993***	0.451***	1.542***
	(0.313)	(0.139)	(0.293)
2 Year	1.625***	0.544***	1.081***
	(0.287)	(0.197)	(0.323)
4 Year	1.527***	0.583***	0.944***
	(0.366)	(0.273)	(0.471)
Excluding both world wars			
Impact	2.026***	0.481	1.544***
	(0.316)	(0.185)	(0.322)
2 Year	1.655***	0.602***	1.053***
	(0.323)	(0.271)	(0.324)
4 Year	1.565***	0.659***	0.906***
	(0.413)	(0.332)	(0.446)

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

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).[8] 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, 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 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

[8]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).

Table 6: Multipliers across states of the business cycle.

	Slump	Boom	Difference
Baseline			
Impact	0.502***	0.595***	−0.093
	(0.247)	(0.193)	(0.291)
2 Year	0.859***	0.663***	0.196
	(0.429)	(0.214)	(0.468)
4 Year	0.975***	0.721***	0.254
	(0.443)	(0.225)	(0.431)
Dummy out ZLB periods			
Impact	0.469**	0.439***	0.031
	(0.249)	(0.134)	(0.235)
2 Year	0.828**	0.464***	0.364
	(0.443)	(0.196)	(0.429)
4 Year	0.875**	0.545***	0.330
	(0.502)	(0.266)	(0.409)
Deep slumps			
Impact	0.503***	0.393***	0.106
	(0.251)	(0.157)	(0.224)
2 Year	1.099***	0.422*	0.678
	(0.519)	(0.259)	(0.559)
4 Year	1.146***	0.517*	0.629
	(0.555)	(0.357)	(0.572)
Financial Crises			
Impact	1.299**	0.462***	0.874
	(0.672)	(0.167)	(0.742)
2 Year	0.229	0.636***	−0.407
	(0.904)	(0.282)	(0.913)
4 Year	−0.097	0.715**	−0.812
	(0.815)	(0.376)	(0.816)

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

deviation of real GDP per capita from its long-run HP-trend, where we set the smoothing parameter to $10,000$.[9] In a first step, we estimate state-dependent effects of fiscal policy across booms and slumps but in doing so ignoring monetary policy constraints.

The upper block of Table 6 displays estimation results by showing the impact multiplier, the 2-year cumulative multiplier, and the 4-year cumulative multiplier during slumps and booms. The left columns display multipliers in periods of economic slack, the middle columns display multipliers during boom periods, and the right columns display the difference in multipliers. As shown, there is no evidence of significant differences in the output multiplier across states of the business cycle. This finding holds for all forecast horizons h considered.

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

This also holds true when we cleanly separate potential business-cycle effects from that of ZLB periods. To do so, we focus only on non-ZLB periods and compare the results of fiscal policy across booms and slumps occurring when monetary policy is not constrained. The second block of Table 6 (labeled 'dummy out ZLB periods') displays cumulative fiscal multipliers for different horizons h when excluding all ZLB episodes from our sample. The estimates reveal that there is no evidence for business-cycle dependent fiscal multipliers. The next block of Table 6 presents the multiplier estimates when considering 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. As before, we dummy out all ZLB periods. The results show that multipliers tend to be higher during deep economic slumps. However, for all forecast horizons, the estimated difference in multipliers is statistically indistinguishable from zero.

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 simple 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 and estimate state-dependent fiscal multipliers during times of financial crises and during non-crises times. To isolate the effects of financial turmoil from those of constrained monetary policy, we again exclude all ZLB episodes from our sample and focus on financial crises that do not coincide with ZLB periods. As Table 6 shows, we indeed find that the fiscal multiplier is larger during periods of financial turmoil. However, this result holds only on impact, while at longer horizons the multiplier during financial crises becomes considerably smaller and is estimated to be insignificant. Importantly, the multiplier during episodes of financial turmoil is never significantly larger than the multiplier during tranquil times.

Of course, since our identified ZLB periods mainly occur during the Great Depression and the Great Recession, we can not rule out completely that our result of fiscal multipliers being significantly larger during ZLB episodes is a reflection of circumstances other than constrained monetary policy that are associated with extreme 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.

5 Empirical Facts versus Theory

Our results of government spending multipliers being significantly larger during ZLB episodes is in line with the predictions of a standard New Keynesian model for an economy stuck at the ZLB due to fundamental shocks. This model predicts a crowding-in of private economic activity in response to a government spending expansion at the ZLB. The rationale is that a fiscal stimulus 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. In this section, we test the New Keynesian model's transmission mechanism by investigating the effects of government spending shocks on output, consumption, inflation, and the real interest rate both during normal times and during ZLB episodes. Specifically, we estimate state-dependent impulse responses from the local projections

$$\begin{aligned} Z_{i,t+h} - Z_{i,t-1} = \nu_{i,h} + \delta_{t,h} + \psi_1 t + \psi_2 t^2 + I_{i,t-1}\left[\beta_h^A g_{i,t} + \phi_h^A(L) X_{i,t-1}\right] \\ + (1 - I_{i,t-1})\left[\beta_h^B g_{i,t} + \phi_h^B(L) X_{i,t-1}\right] + \varepsilon_{i,t+h} \; , \end{aligned} \tag{4}$$

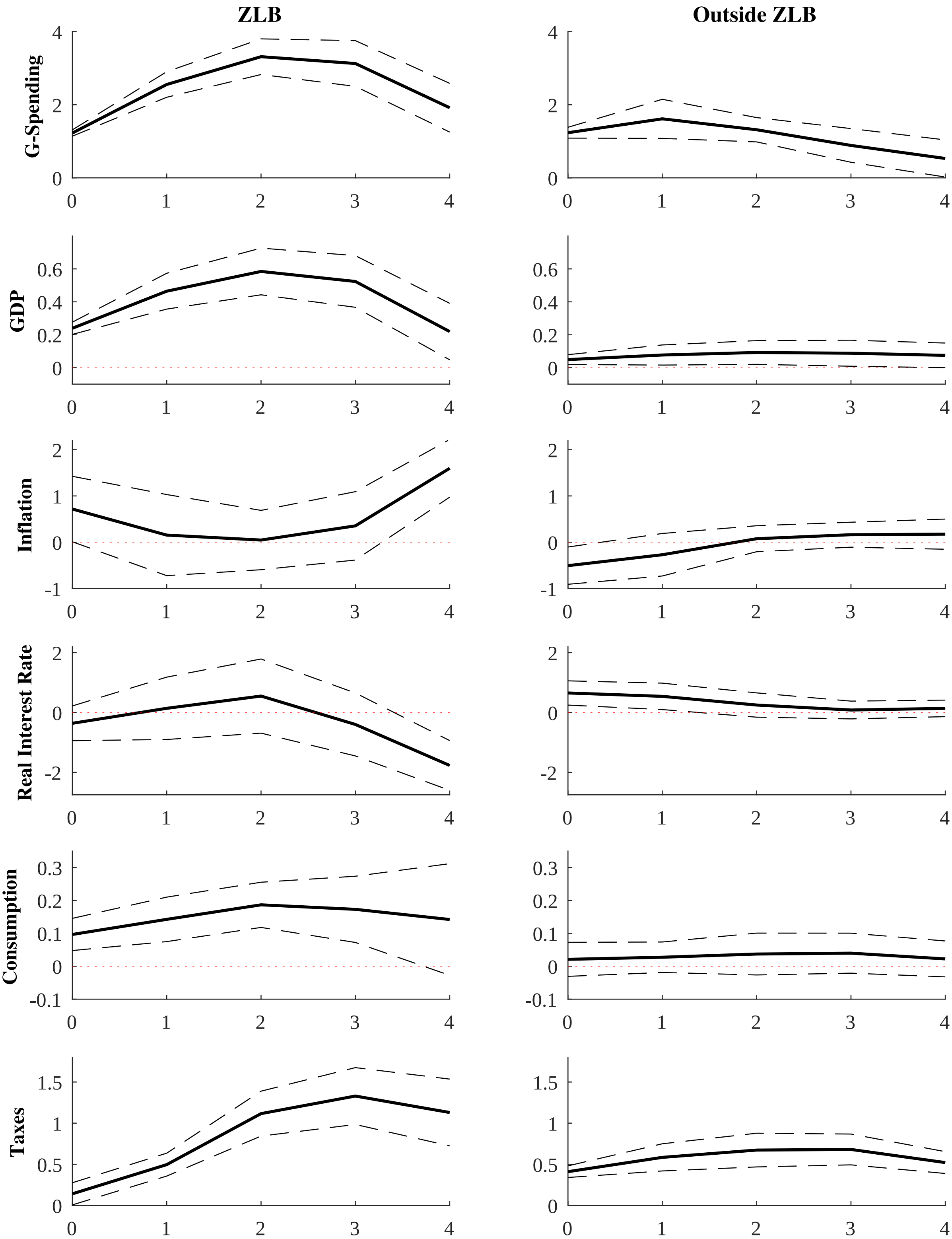
where the dependent variable Z_t is, respectively, government spending, GDP, consumption, inflation, and the real interest rate. In case of government spending, GDP, and consumption being the variable of interest, the dependent variable is normalized by its respective lagged value, i.e. the left-hand side is divided by $Z_{i,t-1}$. The identified government spending shock is given by log real per capita government spending $g_{i,t}$ (consistent with $\mu_i = 0$ for all i). The local projections for government spending, output, and inflation include our baseline set of control variables, e.g. two lags of government spending, GDP, inflation, the nominal interest rate, and the exchange rate. In the regressions for consumption, we also include two lags of consumption, whereas the regressions for the real interest rate include two lags of government spending, GDP, the real interest rate, and the exchange rate as controls.[10]

Figure 4 shows the impulse responses to a government spending shock occurring during normal times (see right column of Figure 4) and during ZLB episodes (see left column of Figure 4). As seen in the figure, the responses are well in line with the predictions of the baseline New Keynesian model for an economy stuck at the ZLB. An expansionary fiscal policy shock puts upward pressure on inflation and induces a decline in the real interest rate (note, though, that in contrast to the textbook model, the responses become significant only with a delay). The decline in the real interest rate induces households to bring forward consumption. Private consumption increases significantly. During normal times, by contrast, a government spending expansion is associated with a significant increase in the real interest rate. Consequently, we do not observe a significant increase in private consumption expenditures.

An additional channel that can rationalize our findings is a more accommodating tax policy during ZLB episodes. In particular, if tax rates rise less strongly in response to a

[10]Due to data availability, the regressions for consumption are restricted to the period 1913-2013.

Figure 4: Impulse responses to government spending shock.



Notes: Left panel: Impulse responses in ZLB states. Right panel: Impulse responses in non-ZLB states. Dahsed lines show 90% confidence bands.

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. However, as the last row of Figure 4 indicates, this story is not supported by the data. Here, we plot the state-dependent responses of the tax rate, defined as the ratio between tax revenues and GDP, to a government spending shock. In both states, a spending expansion induces a significant increase in the tax rate. The rise is even more pronounced when monetary policy is constrained by the ZLB. Thus, there is no evidence of a more accommodative tax policy following a government spending shock during ZLB periods.

6 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 Great Recession.

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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.

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

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; 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); vi) controlling for tax responses by including tax revenues as control variable (due to data availability, the estimation using taxes excludes Denmark); 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: 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)

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

Table A1: Alternative specifications of baseline model (continued).

	ZLB	Outside ZLB	Difference
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)
Controlling for tax response			
Impact	1.551***	0.489***	1.062***
	(0.156)	(0.157)	(0.203)
2 Year	1.475***	0.719***	0.755***
	(0.187)	(0.267)	(0.253)
4 Year	1.507***	0.842***	0.665***
	(0.213)	(0.358)	(0.306)
Normalization using potential GDP			
Impact	1.176***	0.022	1.154***
	(0.411)	(0.100)	(0.439)
2 Year	1.231***	0.109	1.122***
	(0.208)	(0.188)	(0.339)
4 Year	1.435***	0.023	1.412***
	(0.138)	(0.297)	(0.357)

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%.