

# Government Spending Multipliers in Good Times and in Bad: Evidence from U.S. Historical Data

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22 November 2014

## Abstract

This paper investigates whether U.S. government spending multipliers differ according to two potentially important features of the economy: (1) the amount of slack and (2) whether interest rates are near the zero lower bound. We shed light on these questions by analyzing new quarterly historical U.S. data covering multiple large wars and deep recessions. We estimate a state-dependent model in which impulse responses and multipliers depend on the average dynamics of the economy in each state. We find no evidence that multipliers differ by the amount of slack in the economy. These results are robust to many alternative specifications. The results are less clear for the zero lower bound. For the entire sample, there is no evidence of elevated multipliers near the zero lower bound. When World War II is excluded, some point estimates suggest higher multipliers during the zero lower bound state, but they are not statistically different from the normal state. Our results imply that, contrary to recent conjecture, government spending multipliers were not necessarily higher than average during the Great Recession.

We are grateful to Roy Allen, Alan Auerbach, Graham Elliott, Yuriy Gorodnichenko, Jim Hamilton, Òscar Jordà, Michael Owyang, Garey Ramey, Tatevik Sekhposyan, Harald Uhlig, participants at the Hallfest and the NBER EFG conference, and seminar participants at Berkeley, Bocconi, EUI, EIEF, Rice, University of Arizona, LSE, University of Kentucky and University of Houston for very helpful suggestions. We thank Kate Vermann and Michelle Ramey for excellent research assistance.

# 1 Introduction

What is the multiplier on government purchases? The policy debates during the Great Recession led to an outpouring of research on this question. Most of studies have found estimates of modest multipliers in aggregate data, often below unity. If multipliers are indeed this low, they suggest that increases in government purchases are unlikely to stimulate private activity and that fiscal consolidations that involve spending decreases are unlikely to do much harm to the private sector.

Most of the estimates are based on *averages* for a particular country over a particular historical period. Because there is no scope for controlled, randomized trials on countries, all estimates of aggregate government multipliers are necessarily dependent on historical happenstance. Theory tells us that details such as the persistence of spending changes, how they are financed, how monetary policy reacts, and the tightness of the labor market can significantly affect the magnitude of the multipliers. Unfortunately, the data do not present us with clean natural experiments that can answer these questions. While the recent U.S. stimulus package was purely deficit financed and undertaken during a period of high unemployment and accommodative monetary policy, it was enacted in response to a weak economy and hence any aggregate estimates are subject to simultaneous equations bias.

During the last several years, the literature has begun to explore whether estimates of government spending multipliers vary depending on circumstances. One strand of this literature considers the possibility that multipliers are different during recessions (e.g. Auerbach and Gorodnichenko (2012), Bachmann and Sims (2012), Baum et al. (2012), Auerbach and Gorodnichenko (2013), Fazzari et al. (2013) and Riera-Crichton et al. (2014)). Another strand of the literature considers how monetary policy affects government spending multipliers. New Keynesian DSGE models show that when interest rates are stuck at the zero lower bound, multipliers can be higher than in normal times (e.g. Cogan et al. (2010), Christiano et al. (2011), Coenen and et al. (2012)).

This paper contributes to the empirical literature by investigating whether government spending multipliers differ according to two potentially important features of the economy: (1) the amount of slack in the economy and (2) whether interest rates are near the zero lower bound.<sup>1</sup> Extending the initial analysis in Owyang et al. (2013), we exploit the fact that the entire 20th Century contains potentially richer information than the post-WWII data that has been the focus of most of the recent research. We create a new quarterly data set for the U.S.

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1. We will use government "spending" and "purchases" interchangeably. It should be noted that our multipliers apply only to government purchases, not government transfers.

extending back to 1889. This sample includes episodes of huge variations in government spending, wide fluctuations in unemployment, prolonged periods near the zero lower bound of interest rates, and a variety of tax responses.

This paper extends the small, but growing, literature on state dependence of multipliers in two other ways. First, none of the existing papers that estimate state dependent models consider the zero lower bound on interest as a state.<sup>2</sup> Thus, our paper is the first to analyze state-dependence involving the zero lower bound. Second, we contribute to state-dependent multiplier literature by highlighting some key methodological issues that arise. In particular, we show that some of the most widely-cited findings of higher multipliers during recessions are due to special details of the calculation of multipliers and are not robust to plausible generalizations.

Using Jordà's (2005) local projection method, which we find to be a more robust methodology, we find no evidence that the multiplier on government purchases is higher during high unemployment states. Most estimates of the multiplier are between 0.6 and 1. We perform extensive robustness checks with respect to our measures of state, sample period, identification method and the behavior of taxes and find little change in the estimates. We demonstrate that most of the differences in conclusions between our work and that of Auerbach and Gorodnichenko (2012) lie in subtle, yet crucial, assumptions underlying the construction of impulse response functions on which the multipliers are based. In contrast to linear models, where the calculation of impulse response functions is a straightforward undertaking, constructing impulse response functions in nonlinear models is fraught with complications.

We also find little evidence that the multiplier is higher at the zero lower bound. The only case in which the multiplier noticeably exceeds unity in the zero lower bound state is when we exclude the rationing periods of WWII. However, the estimates are imprecise and not robust to simple generalizations of the specification.

The paper proceeds as follows. We begin by discussing the data construction in Section 2. In Section 3 we introduce the econometric methodology. In Section 4, we present our measures of slack and then present estimates of a model in which multipliers are allowed to vary according to the amount of slack in the economy. We first present baseline results using our new data and methodology. We next conduct various robustness checks and explain why our results are different from the leading estimates in the literature. We also explore possible explanations for our results, such as the behavior of taxes. Section 5 tests theories that

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2. Only two papers have investigated the aggregate multiplier at the zero lower bound, Ramey (2011) for the U.S. and Crafts and Mills (2012) for the U.K. Both of these papers simply estimated multipliers over an episode of the zero lower bound rather than estimating state-dependent models.

predict that multipliers should be greater when interest rates are at the zero lower bound and the final section concludes.

## 2 Data Description

A key contribution of the paper is the construction of a new data set that spans historical periods that involve potentially informative movements of the key variables. In particular, we construct quarterly data from 1889 through 2013 for the U.S. We choose to estimate our model using quarterly data rather than annual data because agents often react quickly to news about government spending and the state of the economy can change abruptly.<sup>3</sup> The historical series include real GDP, the GDP deflator, government purchases, federal government receipts, population, the unemployment rate, interest rates, and defense news.

The data appendix contains full details, but we highlight some of the features of the data here. For the post-WWII sample, we use available published quarterly series. For the earlier periods, we follow Gordon and Krenn (2010) by using various higher frequency series to interpolate existing annual series.<sup>4</sup> In most cases, we use the proportional Denton procedure which results in series that average up to the annual series.

The annual real GDP data combine the series from *Historical Statistics of the U.S.* (Carter et al. (2006)) for 1889 through 1928 and the NIPA data from 1929 to the present. The annual data are interpolated with Balke and Gordon's (1986) quarterly real GNP series for 1889-1938 and with quarterly NIPA nominal GNP data adjusted using the CPI, for 1939-1946. We use similar procedures to create the GDP deflator.<sup>5</sup>

Real government spending is derived by dividing nominal government purchases by the GDP deflator. Government purchases include all federal, state, and local purchases, but exclude transfer payments. We splice Kendrick's (1961) annual series starting in 1889 to annual NIPA data starting in 1929. Following Gordon and Krenn (2010), we use monthly federal outlay series from the NBER Macrobistory database to interpolate annual government spending from 1889 to 1938. We use the 1954 quarterly NIPA data from 1939-1946 to interpolate the modern series. We follow a similar procedure for federal receipts.

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3. For example, the unemployment rate fell from over 10 percent to 5 percent between mid-1941 and mid-1942.

4. Gordon and Krenn (2010) use similar methods to construct quarterly data back to 1919. We constructed our own series rather than using theirs in order to include WWI in our analysis.

5. We also checked the robustness of our results by using alternative series constructed by Christina Romer. See Romer (1999) for a discussion of her data.

Figure 1 shows the logarithm of real per capita government purchases and GDP. We include vertical lines indicating major military events, such as WWI, WWII and the Korean War. It is clear from the graph that both series are quite noisy in the pre-1939 period. This behavior stems from the interpolator series, especially in the case of government spending. Part of this behavior owes to the fact that the monthly data used for interpolation include government transfers and are on a cash (rather than accrual) basis. Fortunately, the measurement errors are less of an issue for us because we identify the shocks using narrative methods.<sup>6</sup>

The unemployment series is constructed by interpolating Weir's (1992) annual unemployment series, adjusted for emergency worker employment.<sup>7</sup> Before 1948 we use the monthly unemployment series available from the NBER Macrohistory database back to April 1929 to interpolate. Before 1929, we interpolate Weir's (1992) annual unemployment series using business cycle dates and the additive version of Denton's method. Our comparison of the series produced using this method with the actual quarterly series in the post-WWII period reveal that they are surprisingly close.

Because it is important to identify a shock that is not only exogenous to the state of the economy but is also unanticipated, we use narrative methods to extend Ramey's (2011) defense news series. This news series focuses on changes in government spending that are linked to political and military events, since these changes are most likely to be independent of the state of the economy. Moreover, changes in defense spending are anticipated long before they actually show up in the NIPA accounts. For a benchmark neoclassical model, the key effect of government spending is through the wealth effect. Thus, the news series is constructed as changes in the expected present discounted value of government spending. The narrative underlying the series is available in Ramey (2014). The particular form of the variable used as the shock is the nominal value divided by one-quarter lag of nominal GDP. We display this series in later sections when we construct the states so that one can see the juxtaposition.

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6. Because our shock is constructed independently from news sources and we regress both government spending and GDP on the shock and use the ratio of sums of coefficients, our method is much less sensitive to measurement error in any of the series. See the appendix of Ramey (2011) and footnote 14 of Mertens and Ravn (2013) for a discussion.

7. Because we use the unemployment series to measure slack, we follow the traditional method and include emergency workers in the unemployment rate.

### 3 Econometric Methodology

#### 3.1 Model Estimation Using Local Projection

We use Jordà's (2005) local projection method to estimate impulse responses and multipliers. Auerbach and Gorodnichenko (2013) were the first to use this technique to estimate state-dependent fiscal models, employing it in their analysis of OECD panel data.<sup>8</sup> The Jordà method simply requires estimation of a series of regressions for each horizon  $h$  for each variable. The linear model looks as follows:

$$(1) \quad x_{t+h} = \alpha_h + \psi_h(L)y_{t-1} + \beta_h shock_t + \text{quartic trend} + \varepsilon_{t+h}, \text{ for } h = 0, 1, 2, \dots$$

$x$  is the variable of interest,  $y$  is a vector of control variables,  $\psi_h(L)$  is a polynomial in the lag operator, and  $shock$  is the identified shock. The shock is identified as the defense news variable scaled by lagged nominal GDP. Our vector of baseline control variables,  $y$ , contains logs of real per capita GDP and government spending. In addition,  $y$  includes lags of the news variable to control for any serial correlation in the news variable.  $\psi(L)$  is a polynomial of order 2.<sup>9</sup> As discussed in Francis and Ramey (2009), it is potentially important to include a quadratic trend in the post-WWII period because of slow-moving demographics. Since our current sample is twice as long, we include a quartic trend. The coefficient  $\beta_h$  gives the response of  $x$  at time  $t + h$  to the shock at time  $t$ . Thus, one constructs the impulse responses as a sequence of the  $\beta_h$ 's estimated in a series of single regressions for each horizon. This method stands in contrast to the standard method of estimating the parameters of the VAR for horizon 0 and then using them to iterate forward to construct the impulse response functions.

The local projection method is easily adapted to estimating a state-dependent model. For the model that allows state-dependence, we estimate a set of regressions for each horizon  $h$  as follows:

$$(2) \quad x_{t+h} = I_{t-1} [\alpha_{A,h} + \psi_{A,h}(L)y_{t-1} + \beta_{A,h}shock_t] \\ + (1 - I_{t-1}) [\alpha_{B,h} + \psi_{B,h}(L)y_{t-1} + \beta_{B,h}shock_t] + \text{quartic trend} + \varepsilon_{t+h}.$$

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8. Stock and Watson (2007) also explore the properties of this method for forecasting.

9. BIC favors 2 lags for both the government spending and GDP equations. AIC also favors 2 lags for the GDP equation, but favors 4 lags for government spending. We decided to go with 2 lags for both since the results are similar to those with 4 lags.

$I$  is a dummy variable that indicates the state of the economy when the shock hits. We allow all of the coefficients of the model (other than deterministic trends) to vary according to the state of the economy. Thus, we are allowing the forecast of  $x_{t+h}$  to differ according to the state of the economy when the shock hit. The only complication associated with the Jordà method is the serial correlation in the error terms induced by the successive leading of the dependent variable. Thus, we use the Newey-West correction for our standard errors (Newey and West (1987)).

Apart from the advantages specific to estimating state-dependent multipliers that we will discuss in the next section, the Jordà method has the advantage that it does not constrain the shape of the impulse response function, so it is less sensitive to mis-specification of the SVAR. Second, it does not require that all variables enter all equations, so one can use a more parsimonious specification. A third advantage is that the left-hand-side variables do not have to be in the same form as the right-hand-side variables. As we will explain below, this is an important advantage over a standard SVAR in this particular context.

The Jordà method does not uniformly dominate the standard SVAR method for calculating impulse responses, though. First, because it does not impose any restrictions that link the impulse responses at  $h$  and  $h + 1$ , the estimates are often erratic because of the loss of efficiency. Second, as the horizon increases, one loses observations from the end of the sample. Third, the impulse responses sometimes display oscillations at longer horizons. Ramey (2012) compares impulse responses estimated using Jordà's method to both a standard VAR and a dynamic simulation (such as the one used by Romer and Romer (2010)), based on military news shocks. The results are qualitatively similar for the first 16 quarters, though the responses using the Jordà method tend to be more erratic. However, at longer horizons, the Jordà method tends to produce statistically significant oscillations not observed in the other two methods. Since we are interested in the shorter-run responses, the long-run estimates are not a concern for us.

### **3.2 Pitfalls in Calculating Multipliers**

We now highlight two potential problems that affect multipliers computed not only from nonlinear VARs but also from all of the standard linear SVARs used in the literature.

The first problem concerns the the conversion of elasticities to multipliers. The usual practice in the literature is to use the log of variables, such as real GDP, government spending, and taxes. However, the estimated impulse response functions do not directly reveal the government spending multiplier because the estimated elasticities must be converted to

dollar equivalents. Virtually all analyses using VAR methods obtain the spending multiplier by using an *ex post* conversion factor based on the sample average of the ratio of GDP to government spending,  $Y/G$ .

We first noticed a potential problem with this method when we extended our sample back in time. In the post-WWII sample,  $Y/G$  varies between 4 and 7, with a mean of 5. In our full sample from 1889-2013,  $Y/G$  varies from 2 to 24 and with a mean close to 8. We realized that we could estimate the same elasticity of output with respect to government spending, but derive much higher multipliers simply because the mean of  $Y/G$  was so much higher.

To determine whether using *ex post* conversion factors can lead to inflated multipliers, we conducted a test based on the following point made by Ramey (2013). If the multiplier exceeds one, then it must be the case that private spending  $Y - G$  rises when  $G$  rises. Thus, one can compare the multipliers estimated the standard way to the response of real private spending to see if there is a contradiction.

To conduct this test, we first estimate a trivariate SVAR with military news, log real per capita government spending, and log real GDP, using four lags and quartic trend, on data from 1889 - 2013. The estimated elasticity at 2 year horizon is around 0.20 (based on the ratio of the cumulative response of  $\ln(Y)$  to the cumulative response of  $\ln(G)$ ). We then multiply the estimated elasticity by the average of  $Y/G$  for the full sample, and obtain an implied multiplier of 1.56. To conduct the comparison, we next estimate a model in which we substitute the log of real private spending for log real GDP, and compute the impulse response functions (using the standard method). These responses show that private spending *falls* when government spending rises, and specifically has a negative response at the 2 year horizon. Thus, these results imply a multiplier that is less than unity. It appears that the practice of backing out multipliers using *ex post* conversion factors can lead to upward biased multiplier estimates in some situations.

In order to avoid this bias, we follow Hall (2009) and Barro and Redlick (2011) and convert GDP and government spending changes to the same units *before* the estimation. In particular, our  $x$  variables on the left-hand-side of Equation 2 are defined as  $(Y_{t+h} - Y_{t-1})/Y_{t-1}$  and  $(G_{t+h} - G_{t-1})/Y_{t-1}$ . The first variable can be rewritten as:

$$\frac{Y_{t+h} - Y_{t-1}}{Y_{t-1}} \approx (\ln Y_{t+h} - \ln Y_{t-1})$$

and hence is analogous to the standard VAR specification. The second variable can be rewritten as:

$$\frac{G_{t+h} - G_{t-1}}{Y_{t-1}} \approx (\ln G_{t+h} - \ln G_{t-1}) \cdot \frac{G_{t-1}}{Y_{t-1}}$$

Thus, this variable converts the percent changes to dollar changes using the value of  $G/Y$  at each point in time, rather than using sample averages. This means that the coefficients from the  $Y$  equations are in the same units as those from the  $G$  equations, which is required for constructing multipliers.

It would be difficult to use this transformation in a standard SVAR, since all the variables on the left and right must be of the same form. It is easy to use it in the Jordà framework since the variables on the right side of the equation are control variables that do not have to be the same as the left-hand-side variables.

An alternative transformation is the one used by Gordon and Krenn (2010). Instead of taking logarithms of the variables, they divide all variables by an estimate of potential GDP. The advantage of their transformation is that the variables can be used in a VAR. We will test the robustness of our results to this alternative transformation.

The second pitfall concerns the definition of the multiplier in a dynamic setting. The original Blanchard and Perotti (2002) paper defined the multiplier as the ratio of the *peak* of the output response to the *initial* government spending shock. Numerous papers have used this same definition, or variations, such as the average of the output response to the initial government shock (e.g. Auerbach and Gorodnichenko (2012), Auerbach and Gorodnichenko (2013)). As argued by Mountford and Uhlig (2009), Uhlig (2010) and Fisher and Peters (2010), multipliers should instead be calculated as the *integral* of the output response divided by the *integral* government spending response. The integral multipliers address the relevant policy question because they measure the cumulative GDP gain relative to the cumulative government spending during a given period. As we will discuss later, the Blanchard-Perotti method tends to bias up the estimates of multipliers relative to the cumulative method.

## 4 Multipliers During Times of Slack

The original Keynesian notion that government spending is a more powerful stimulus during times of high unemployment and low resource utilization permeates undergraduate textbooks and policy debates. Other than the zero lower bound papers, which make a distinct argument that we will discuss below, there is only a limited literature analyzing rigorous models that produces fiscal multipliers that are higher during times of high unemployment. Michailat (2014) is one of the few examples, but his model applies only to government spending on

public employment.<sup>10</sup> Thus, there is still a gap between Keynes' original notion and modern theories.

In this section, we analyze the issue empirically. Section 4.1 discusses our measure of slack and shows graphs of the data and periods of slack. Section 4.2 presents the main results. The later sections conduct robustness checks and analyzes in detail why our results are different from Auerbach and Gorodnichenko (2012).

## 4.1 Measurement of Slack States

There are various potential measures of slack, such as output gaps, the unemployment rate, or capacity utilization. Based on data availability and the fact that it is generally accepted as a key measure of underutilized resources, we use the unemployment rate as our indicator of slack. We define an economy to be in a slack state when the unemployment rate is above some threshold. For our baseline results, we use 6.5 as the threshold based on the U.S. Federal Reserve's choice of that value as a threshold in its recent policy announcements. We also conduct various robustness checks using different thresholds.

Note that our use of the unemployment rate to define the state is different from using NBER recessions or Auerbach and Gorodnichenko's (2012) moving average of GDP growth. The latter two measures, which are highly correlated, indicate periods in which the economy is moving from its peak to its trough. A typical recession encompasses periods in which unemployment is *rising* from its low point to its high point, and hence is not an indicator of a state of slack. Only half of the quarters that are official recessions are also periods of high unemployment.

Figure 2 shows the unemployment rate and the military spending news shocks. The largest military spending news shocks are distributed across periods with a variety of unemployment rates. For example, the largest news shocks about WWI and the Korean War occurred when the unemployment rate was below the threshold. In contrast, the initial large news shocks about WWII occurred when the unemployment rate was still very high.

How informative are our military news shocks for changes in government spending? Because our method for estimation can be reinterpreted as an instrumental variables regression, we can answer this question by conducting tests for weak instruments. According to Staiger and Stock (1997), a first-stage F-statistic below 10 can indicate that the instrument

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10. Numerous papers explore theoretically the possibility of state-dependent multipliers that depend on alternative states, such as the debt-to-GDP ratio, the condition of the financial system, and exchange rate regimes. For example, see Corsetti et al. (2012) for a brief survey of this literature, as well as Canzoneri et al. (2013) and Sims and Wolff (2013).

may have low relevance.<sup>11</sup> Figure 3 shows the first-stage F-statistics.<sup>12</sup> The figure shows these for the full historical sample, the historical sample excluding World War II, and the post-WWII sample, and splits each of these according to whether the unemployment rate is above 6.5 percent. In all cases, the F-statistics are very low at short horizons. This is to be expected since the entire point of Ramey (2011) is that the news about government spending occurs at least several quarters before the government spending actually rises. For the linear model, the F-statistic is high for many horizons in the post-WWII period, but for only several horizons in the historical samples. In contrast, the F-statistic is above 10 for the high unemployment state for most horizons in the full historical sample, but is low at most horizons in the historical sample that excludes WWII, and is always very low for the post-WWII period. The results for the low unemployment rate state look similar to the linear case, although the F-statistics are lower in the full historical sample. These results support our initial conjecture that the post-WWII sample was not sufficiently rich to be able to distinguish multipliers across states using the military news instrument.<sup>13</sup> They also suggest caution about estimates of multipliers during slack states when World War II is excluded.

## 4.2 Baseline Results for Slack States

We now present the main results of our analysis using the full historical sample and the local projections method. We first consider results from the linear model, which assumes that multipliers are invariant to the state of the economy. The top panel of Figure 4 shows the responses of government spending and output to a military news shock in the linear model using the U.S. data. The bands are 95 percent confidence bands and are based on Newey-West standard errors that account for the serial correlation induced in regressions when the horizon  $h > 0$ . After a shock to news, output and government spending begin to rise and peak at around 12 quarters.

In the linear model, the multipliers are derived from the estimated  $\beta_h$ 's from the set of  $Y$  and  $G$  regressions. We compute multipliers over two horizons: the ratio of the cumulative

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11. Olea and Pflueger (2013) show, however, that the thresholds can be higher when the errors are serially correlated.

12. The F-statistics are based on the exclusion of current news (scaled by the previous quarter's nominal GDP) in the regression of the government spending variable at horizon  $h$  on current news, and two lags each of news, log GDP and log government spending and a quartic trend.

13. In contrast, the Blanchard and Perotti (2002) identification scheme is almost guaranteed to produce shocks with high F-statistics at the first few horizons since the shock is identified as the part of current government spending not explained by the other lagged variables in the SVAR. However, we find that the F-statistics for this shock fall to very low values for horizons beyond the first year.

responses of GDP and government spending through two years, and the cumulative responses through four years.<sup>14</sup> It should be noted, however, that by focusing on the shorter horizons, we are most likely neglecting the negative effects due to the eventual increase in distortionary tax, as highlighted by Drautzburg and Uhlig (2011). As indicated in the first column of the top panel of Table 1, the implied multipliers are below one and range from 0.7 to 0.96. The estimates are not statistically different from one at the five percent significance level.<sup>15</sup>

The main question addressed in this paper is whether the multipliers are state-dependent. The impulse response functions and multipliers in the state-dependent case are derived from the estimated  $\beta_{A,h}$  and  $\beta_{B,h}$  for  $Y$  and  $G$  in Equation 2. The bottom panel of Figure 4 shows the responses when we estimate the state-dependent model where we distinguish between periods with and without slack in the economy. Similar to many pre-existing studies (e.g. Auerbach and Gorodnichenko (2012)), we find that output responds more robustly during high unemployment states. Note that government spending also has a stronger response during those high slack periods. Consequently, the larger output response during the high unemployment state does not imply a larger government spending multiplier. In fact, as shown in the second and third columns of Table 1, the implied 2 and 4 year multipliers are slightly lower during the high unemployment state. In no case do we find a statistically different multiplier across states.

One might worry that the multipliers are different at other horizons. Figure 5 shows the cumulative multiplier for each horizon from impact to 5 years out. The top graph shows the linear model multipliers and the bottom graph shows the state dependent multipliers. In the linear case, the cumulative multiplier in the first year is above one but then falls. The reason for the higher initial multipliers after a news shock is given by Ramey (2011): output responds immediately to news about future government spending increases. Since output rises more quickly than government spending, the calculated multiplier looks large.<sup>16</sup> The bottom graph shows that whatever the values, the multipliers in the high unemployment state are below or equal to those in the low unemployment state.

To summarize, in our linear model we find multipliers that are less than 1 in all cases

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14. To further clarify, the cumulative multipliers are constructed as  $\frac{\sum_{i=1}^M \Delta Y_i}{\sum_{i=1}^M \Delta G_i}$  for  $M = 8$  and 16, where  $\Delta$  denotes the difference between the path conditional on the shock versus no shock.

15. The standard errors are computed by estimating all of the regressions jointly as one panel regression and using the Driscoll and Kraay (1998) standard errors to account for correlations in the error terms. We implement this estimation in Stata, using *xtscc* followed by *nlcom*, which uses the delta method to analyze functions of parameters (Hoechle (2007)).

16. When we instead use Blanchard-Perotti SVAR shocks (which do not account for news), we find that multipliers are close to zero in the first year.

(beyond the first couple of quarters). Considering state dependence, we find no evidence of larger multipliers in the periods of slack, and multipliers vary between 0.8 and 1.

### 4.3 Robustness

Our baseline results suggest that there is no difference in multipliers across slack states. These results are potentially sensitive to the numerous specification choices we made that were not guided by theory. Thus, in this section we explore the sensitivity of our findings to these choices.

We first investigate the impact of using a different interpolation method for the data. Recall that our underlying interpolators were quite volatile and led to a lot of jumps in the early data. To investigate the impact, we create alternative data that uses linear interpolation of the annual data in the pre-WWII period. When we re-estimate the model, we find slightly lower multipliers on average, and no difference in multipliers across states of the economy. These results are shown in the first panel of Table 2.

We included the quartic trend in our equations to capture low frequency demographics, such as the Baby Boom. Since times series estimates can be sensitive to trends, we investigate the impact of omitting the quartic trend in our model. As shown in the second panel of Table 2, this specification produces slightly lower multipliers, but no difference in multipliers across states of the economy.

We also consider how the results change when we use Gordon and Krenn's (2010) transformation, which divides all variables by potential GDP.<sup>17</sup> This robustness check also tests whether cyclicalities of  $Y_{t-1}$  can lead to biases in the state-dependent estimates when we use the Hall-Barro-Redlick transformation.<sup>18</sup> As the table shows, the results are very similar to our baseline.

We chose our threshold unemployment rate as 6.5 percent in the baseline specification, but it might be a concern that we are not allowing for the possibility of state-dependence that might arise only for a higher degree of slack in the economy. The third panel in Table 2 shows that when we choose the threshold for the unemployment rate to be higher than 8 percent, our main results are still preserved where there is no evidence of difference in multipliers across states.

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17. See the data appendix for details on how real potential GDP was constructed. We divide all variables by current real potential GDP. The news variable requires a nominal quantity in the denominator, so we multiply real potential by the lagged actual deflator.

18. This concern has been expressed by Yuriy Gorodnichenko in private correspondence and in his NBER discussion.

We also allow for a time-varying threshold, where we consider deviations from trend for a Hodrick-Prescott filtered unemployment rate.<sup>19</sup> This definition of threshold results in about 50 percent of the observations being above the threshold. As shown in Figure 6, this threshold also suggests prolonged periods of slack both in the late 1890s and during the 1930s. There is substantial evidence that the "natural rate" of unemployment displayed an inverted U-shape in the post-WWII period, and this time-varying threshold also helps account for this. Using this time-varying threshold, we find results in line with our baseline findings, with multipliers typically less than one for the state-dependent case and no significant difference between the multipliers in the low unemployment state and the high unemployment state (see Table 2).

Next, we consider a threshold based on the moving average of output growth, as in Auerbach and Gorodnichenko (2012). We construct a smooth transition threshold, where we replace the dummy variable  $I_{t-1}$  in Equation 2 with the function  $F(z_t)$ , where  $z$  is the normalized 7-quarter centered moving average of output growth.<sup>20</sup> Figure 7 shows the function  $F(z)$  along with the NBER recessions for our full sample. Results in the bottom panel of Table 2 show that when we use this weighting function for recessionary regimes in our specification to construct state-dependent multipliers, we still get multipliers less than one for U.S. across both recession and expansion regimes, and do not find any evidence of higher multipliers in expansions versus recessions.

Another point of departure with the pre-existing literature is the fact that most of the papers employ a shorter data sample that spans the post World War II period. As a robustness check we limit our sample to this period, 1947-2013, and employ the Jorda local projection method on this data set. In this shorter sub-sample too, about 30 percent of the observations are above our baseline threshold for unemployment rate, signifying state of slack.<sup>21</sup> As shown in the fourth panel of Table 2, in the linear case, the multipliers for U.S. are still smaller than 1. Looking at state-dependent multipliers, we find that the multiplier in the high unemployment state jumps around. The 2-year integral multiplier in high unemployment state is large and negative at -5 and the four year integral multiplier is large and positive taking a value of 25! The reason for these jumps is that output has a negative response to

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19. We use a very high smoothing parameter of  $\lambda = 1,000,000$ , but even with this the Great Depression and World War II have a big influence. Thus, we fit the HP filter over a split sample, 1889 - 1929 and 1947 - 2013 and linearly interpolate the small gap in trend unemployment between 1929 and 1947.

20. We use the same definition of  $F(z)$  as Auerbach and Gorodnichenko (2012), which is given by  $F(z_{t-1}) = \frac{\exp(-\gamma z_{t-1})}{1 + \exp(-\gamma z_{t-1})}$  and set  $\gamma = 3$ , in order to ensure that  $F(z)$  is greater than 0.8 close to 30 percent of the time for the U.S., which lines up with the total duration of recessions during our full sample starting in 1889.

21. When conducting this sub-sample analysis we change our baseline specification to use a quadratic trend.

the news shock in the high unemployment state, and the government spending response in the high unemployment state also becomes negative after 2-3 years. Since the military news variable has very low instrument relevance during slack periods in the post-WWII period, the impulse responses in this state are very imprecisely estimated. Thus, it is hard to take these state-dependent multipliers for the sub-sample seriously.

Most of the literature that finds evidence of state-dependence in spending multipliers also identifies government spending shocks differently. The commonly used approach is to employ Blanchard and Perotti (2002) identification scheme which is based on the assumption that within quarter government spending does not contemporaneously respond to macroeconomic variables.<sup>22</sup> Table 2 shows that when we identify the government spending shock using Blanchard and Perotti (2002) identification scheme for our full sample, we still find multipliers less than one in the linear and state-dependent case. Also, the multipliers are not statistically different across the high and low unemployment states.

Another potential concern is the role of rationing in World War II. If consumers are constrained in what they can buy, then the traditional Keynesian multiplier might not work to full effect. While most of the World War II period is characterized as a non-slack state by our definition, some of the initial news shocks hit during the slack state and rationing could potentially be depressing the multiplier during the subsequent periods. Official rationing did not start until 1942, but other restrictions were imposed on the U.S. economy starting in the second half of 1941. For example, the Office of Production Management asked for a cutback in automobile production. Gordon and Krenn (2010) carefully document various other capacity constraints that occurred starting in the second half of 1941. In order to determine whether our results are sensitive to the rationing period, we exclude observations when either the dependent variable or the shock occurs in the period 1941q3 through 1945q4.

When we exclude World War II from the sample, the multipliers are indeed a little higher overall. In the linear case, they are 1 and 1.26 for the two and four year integral multipliers, respectively. As the second to last panel in Table 2 shows, the multipliers are larger in the slack and non-slack state as well, relative to our baseline. The 2-year integral multiplier is 0.83 in high unemployment and 0.97 in low unemployment. For the four year integral, the multiplier is slightly higher in the slack state versus low unemployment period, but the difference is again not statistically significant. Therefore, our baseline results are robust to the exclusion of World War II from the sample, where we find no significant difference

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22. However, this type of shock is much more sensitive to potential measurement errors given our historical construction of quarterly government spending and GDP series and is subject to the critique that it is likely to have been anticipated.

between slack and non-slack states.

#### 4.4 Comparison to Auerbach-Gorodnichenko (2012, 2013)

Our finding that multipliers do not differ across slack states stands in contrast to two of the leading studies of state dependence, the two studies by Auerbach and Gorodnichenko (2012, 2013). In this section, we explain the main source of the different results.

We first compare our results to Auerbach and Gorodnichenko (2012) (AG-12). They use a smooth transition VAR (STVAR) model, post-WWII data, the Blanchard-Perotti identification method, and a function of the 7-quarter centered moving average of normalized real GDP growth as their measure of the state. They also include four lags of the 7-quarter moving average growth rate as exogenous regressors in their model.<sup>23</sup> They construct their baseline impulse responses based on two assumptions: (1) the economy remains in an extreme recession or expansion state for at least 5 years; and (2) changes in government spending do not impact the state of the economy.<sup>24</sup> They find multipliers during recessions that are well above two and these results have been cited by those advocating stimulus during the Great Recession (e.g. DeLong and Summers (2012))

To understand the difference between our results and theirs, we begin by taking only one step away from what AG-12 did by using all details of their analysis except the method for estimating and constructing the impulse responses. In particular, we apply the Jordà method to their post-WWII data, using their exact definition of states, logs of variables, estimated government spending shocks from their STVAR model, and inclusion of four lagged values of the centered 7-quarter moving average of output growth as controls.  $F(z)$  is the indicator of the state as a function of the moving average of output ( $z$ ). It varies between a maximum of one (extreme recession) and zero (extreme expansion).

Figure 8 shows the linear responses in the top panel.<sup>25</sup> The government spending response looks similar to the linear case in AG-12, though the GDP response is more erratic and the standard error bands are much wider. The state-dependent responses shown in the lower panel look very different. The Jordà method produces impulse responses in which the response of government spending to a shock is higher in a recession than in an expan-

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23. The published paper does not discuss these additional terms, but the initial working paper version includes these terms in one equation and the codes posted for the published paper include them.

24. These results are shown in Table 1 and Figure 2 of their paper.

25. We have multiplied the log output response by a conversion factor of 5.6, following AG-12. They use a nonstandard measure of government purchases as their measure of  $G$ . As a result their  $Y/G$  ratio used to convert multipliers is higher than the usual one based on total government purchases.

sion, similar to our earlier results, but in opposition to those of AG-12. The response of output differs little across states, in contrast to AG-12 who find that output rises robustly and continuously throughout the 5 years in the recession state but quickly falls toward zero and becomes negative in the expansion state.

The first panel of Table 3 compares AG-12's cumulative 5-year and 2-year multipliers to those we estimated by the Jordà method. For the 5-year horizon, AG find multipliers of 2.24 for recessions and -0.33 for expansions whereas the Jordà method estimates multipliers of 0.84 in recessions and -0.59 in expansions.<sup>26</sup> Thus, the Jordà multipliers are below one in recessions, but similar to AG-12's multipliers in expansions. At the 2-year horizon, AG's estimates imply a recession multiplier of 1.65 and a gap between states of 1.55. In contrast, the Jordà method implies a 2-year multiplier in recessions of 0.24 and a gap between states of -0.12.<sup>27</sup>

Thus, even when we use the same sample period, data, variable definitions, definition of slack, and estimated shocks as AG-12, the Jordà method produces multipliers in recessions that are much lower than those of AG-12. This means that AG-12's high multipliers during recessions are likely due to the method for constructing the impulse responses. As Koop et al. (1996) point out, constructing impulse responses in nonlinear models is far from straightforward since many complexities arise when one moves from linear to nonlinear systems. In a linear model, the impulse responses are invariant to history, proportional to the size of the shock, and symmetric in positive and negative shocks. In a nonlinear model, the response can depend differentially on the magnitude and sign of the shock, as well as on the history of previous shocks. If one estimates the parameters of a nonlinear model and then iterates on those parameters to construct impulse responses, assumptions on how the economy transitions from state-to-state, as well as the feedback of the shocks to the state, are key components of the constructed responses.<sup>28</sup>

In the Jordà method, the impulse responses at each horizon are estimated directly by regressing  $x_{t+h}$  on the shock in period  $t$  and lagged values of other control variables. Since separate regressions are estimated for each horizon  $h$ , no iteration is involved. The estimated

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26. AG-12 also report a recession multiplier of 2.5, but that is based on comparing the peak response of output to the initial government spending shock, a practice we critiqued in a previous section.

27. One should keep in mind, however, that the Jordà estimates are not very precise. We were not able to estimate the standard errors of the multipliers because the `xtscc` command in Stata reported that the variance matrix was nonsymmetric or highly singular.

28. For instance, Caggiano et al. (2014) employ the STVAR approach of AG-12 for a shorter sample, but compute impulse response functions using the generalized impulse response approach advocated by Koop et al. (1996), and find that the spending multipliers in recessions are not statistically larger than in expansions. They only find evidence of nonlinearities when focusing on deep recessions versus strong expansionary periods.

parameters depend on the average behavior of the economy in the historical sample between  $t$  and  $t+h$ , given the shock, the initial state, and the control variables. The parameter estimates on the control variables incorporate the average tendency of the economy to evolve between states. Thus, if the duration of State A is typically short relative to State B, the  $h$  quarter ahead forecast will take this into account. Similarly, the estimate of the coefficient on the shock includes the effects of the shock on the future state of the economy. Thus, the estimates incorporate both the natural transitions and endogenous transitions from state to state that occur on average in the data.

In contrast, Auerbach and Gorodnichenko (2012) calculate their baseline impulse responses under the assumption that the economy stays in its current state for the 20 quarters over which they compute their multiplier. This assumption turns the problem into a linear one, but it is potentially inconsistent with the data and the multipliers actually estimated. As Figure 7 shows, during the post-WWII period the recession states - defined by AG to be quarters when their  $F(z)$  exceeds 0.8 - are much shorter than 20 quarters in duration; the mean duration is only 3.3 quarters. According to their definition of recession, the Great Recession lasted nine quarters, much longer than the average but substantially less than 20 quarters. Even the nine-quarter duration is an overestimate, since AG-12 use only the extreme of  $F(z) = 1$  in their calculations.<sup>29</sup> This inconsistency of their assumption with the data means that the multipliers they calculate for recessions are based on impulse responses that do not represent any episode ever experienced in their sample. Moreover, the assumptions imply that a positive shock to government spending during a recession does not help the economy escape the recession. AG-12 relax this second assumption in a second experiment, and we will describe those results below.

To see the importance of these assumptions, we conduct several experiments. In these experiments, we compute alternative impulse responses by iterating on AG's STVAR parameter estimates under different assumptions about the dynamics of the state of the economy. Since the economy is never literally in an extreme recession or expansion, we focus on the average of "severe" recessions and "severe" expansions, which we define as the few quarters in which  $F(z)$  is above 0.95 or below 0.05, respectively. The few quarters of severe recession occur during the recessions of 1974-75 (two quarters), 1981-82 (one quarter), and 2008-09 (five quarters).

The second panel of Table 3 reports these experiments. For reference, the first line of the second panel shows that AG's baseline 5-year multipliers do not change much when we

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29. The mean value of AG's  $F(z)$  indicator is 0.81 during NBER recessions and 0.42 during NBER expansions.

change  $F(z)$  by a small amount. The second line shows the multiplier calculated assuming a constant state and no feedback, but looking at the 2-year integral multiplier. This calculation requires less drastic assumptions because it only assumes that the state remains constant for 2 years rather than 5 years. Here, the multipliers in severe recessions are not as high and those in severe expansions are not as low, so the difference across states falls from 2.47 to 1.46.

The next experiment, "Actual State Dynamics," assumes that instead of staying constant at an extreme value, the state indicator  $F(z)$  is equal to its actual value at each point in time. In practice the experiment is conducted as follows. We first calculate the paths of government spending and output, assuming that the shocks to the government spending, tax, and output equations take their estimated values. This essentially reproduces the actual path of the economy for all variables including  $F(z)$ . We then calculate an alternative path of government spending, taxes, and output assuming that there is an additional one-time positive shock in the current period to government spending, equal to one standard deviation of the estimated government spending shock (equal to 1.3 percent of government spending). We allow the shock to change the path of spending, taxes, and output, but not the state of the economy,  $F(z)$ , relative to its actual path. The difference between this simulation and the actual values of the variables forms the impulse response functions. Despite the lack of feedback, this experiment is different from AG's baseline experiment because it allows the state of the economy to experience its natural dynamics (i.e.  $F(z)$  is allowed to vary between its extremes as it actually does). The third and fourth lines of the lower panel show the multipliers for this experiment. In severe recessions, the 5-year multiplier is estimated to be 1.4 and the 2-year multiplier is estimated to be 1.1. Thus, allowing the state of the economy to follow its subsequent natural dynamics reduces the constructed multiplier in recessions. The effect is not so big on the expansion multipliers, however. This is to be expected since expansions have a much longer duration than recessions, so the assumption of no change in state is not so at odds with the data.

AG-12 relax one of their baseline assumptions in a second experiment by allowing partial feedback of government spending on the state of the economy, but otherwise not allowing the state to change. They are not able to allow full feedback, though, because of the nature of their state variable.<sup>30</sup> The fifth and sixth lines of the second panel of the table show the average of their multipliers in severe recessions and expansions. Their experiment also

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30. Their state variable is a function of a *centered* moving average of GDP growth. Thus, future values of GDP enter the current state. This formulation makes it impossible to allow full feedback in a logically consistent manner.

lowers the estimated multiplier in severe recessions, to 1.36 for the 5-year multiplier and 1.01 for the 2-year multiplier.

The final two lines of the table show our experiment in which we allow both the natural dynamics of the economy and partial feedback from the government spending shock. The experiment is the same as the "Actual State Dynamics, No Feedback" except that it also allows the  $F(z)$  indicator to change from its actual path in reaction to current changes of output resulting from the government spending shock. As shown in the table, both the 5-year and 2-year multipliers during severe recessions are calculated to be 1.07. During severe expansions, the 5-year multiplier is calculated to be 0.14, which is higher than AG's multiplier of -0.3. As a result, the gap in multipliers across states shrinks.

Thus, even when we use AG's STVAR parameter estimates, we can get very different estimates for the multiplier. Differing assumptions made about transitions between states and the feedback of government spending to the state lead to very different estimates of multipliers. When we compute multipliers allowing for the natural dynamics of the economy we find a much smaller gap across states than AG-12. The gap we do find is not because the multiplier is so high during recessions, but because it is estimated to be so low during expansions when we use AG's data and variable definitions.

As pointed out above, the state variable that AG-12 employ is a function of a *centered* moving average of GDP growth. This suggest that future values of GDP are used to construct the current state of the economy. This formulation not only makes it impossible to allow full feedback in a logically consistent manner, but it in fact plays an important role in driving their results as well. In our exploration of the AG-12 results, we found that if we focus only on the backward moving average terms, and in particular, instead of a 7-quarter centered moving average growth rate, consider a 4-quarter backward moving average growth rate, while keeping all the details of the AG set-up the same, the resulting multiplier is much larger in expansion than in recession.<sup>31</sup> Alloza (2014) conducts a much more systematic analysis of the importance of this two-sided moving average filter and further corroborates our finding. He finds that using a higher order of centered moving average (i.e. using more information about future values of GDP) reduces the effect of the spending shock on output during times of booms and amplifies it during recession periods. He also explores varying the symmetry of the 7-quarter moving average, and shows that when less information about the future is used, then the results suggest a more robust response of output to a spending shock in booms, while the response in recessions becomes negative. This suggests, that the

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31. The 5 year cumulative multiplier is close to 0.7 in recession and 1.8 in expansion.

future information used in constructing the current state variable in AG-12 is also crucial to their results.

But why do we obtain different results from those of Auerbach-Gorodnichenko's second paper? As discussed earlier, the second Auerbach-Gorodnichenko paper (AG-13) also applies the Jordà method to a panel of OECD countries; in fact, AG-13 were the ones who first realized the potential of this method for state-dependent fiscal models. Thus, a key question is why they find higher multipliers during recessions even with this method. There is, of course, the obvious difference in time period and country sample. We believe, however, the most likely reason for the difference is in two details of how they calculate multipliers. First, following the standard practice, they estimate everything in logarithms and then use the *ex post* conversion factor based on average  $Y/G$  during their sample to convert elasticities to multipliers. Second, they follow Blanchard and Perotti (2002) by comparing the path or peak of output to the *impact* of government spending rather than to the peak or integral of the path of government spending. This is a big difference because the effects of a shock to government usually build up for several quarters. As we argued in a previous section, this is not the type of multiplier that interests policy makers because it does not count the average cumulative cost of government spending associated with the path. If we used that same procedure on our baseline estimates, calculating multipliers by dividing the average response of output over the 2-year horizon by the the *initial* shock to government spending, we would produce multipliers of 4.3 in the linear case, 2.2 in the low unemployment state and 8.8 in the high unemployment state! Thus, it is clear that even using the same estimation method and same method for computing impulse responses, details of the calculations of multipliers can make a big difference.

In summary, we have shown that the differences between our results and those of AG-12 and AG-13 owe little to the differences in the method for estimating the underlying parameters. Instead, the differences are due mostly to the details of how those underlying estimates are translated to impulse response functions and how those impulse response functions are in turn translated into multipliers. We argue that AG's methods tend to bias upward the multipliers in recessions resulting in the large differences in multipliers in their papers.

## 4.5 The Behavior of Taxes

Our analysis so far has ignored the responses of taxes. Romer and Romer's (2010) estimates of tax effects indicate very significant negative multipliers on taxes, on the order of -2 to -3. Thus, it is important for us to consider how the increases in government spending are

financed in order to interpret our multiplier results.

To analyze how taxes and deficits behave, we re-estimate our basic model augmented to include deficits and tax rates so that we can distinguish increases in revenues caused by rising output versus rising rates. Average tax rates are computed as the ratio of federal receipts to nominal GDP. The deficit is the real total deficit. We include two lags of these two new variables along with GDP and government spending as controls in all of the regressions, and we estimate this system for the full sample using the Jordà method.

Figure 9 shows the results from the linear case. The responses of government spending and GDP, as well as the multipliers, are almost identical to the baseline case. The middle panels show that both average tax rates and the deficit increase in response to news shock. Taking the ratio of the cumulative response of deficit to cumulative response of government spending at various horizons, we estimate a sharp rise in the share of government spending financed with the deficit during the first year. The deficit fraction of government spending then plateaus at 60 percent .

From a theoretical perspective, the fact that tax rates increase steadily during the first two years has significant implications for the multiplier. If all taxes are lump-sum taxes, news about a future increase in the present discounted value of government spending leads to an immediate jump in hours and output because of the negative wealth effect. In a neoclassical model, the effect is the same whether the taxes are levied concurrently or in the future. In contrast, the need to raise revenues through distortionary taxation can change incentives significantly. As Baxter and King (1993) show, if government spending is financed with current increases in tax rates, the multiplier can become negative in a neoclassical model.

The situation changes considerably when tax rates are slow to adjust, but agents anticipate higher future tax rates. To see this, consider the case of labor income taxes and a forward-looking household:

$$(3) \quad 1 = \beta E_t \left[ \frac{u_{n,t+1}}{u_{n,t}} \frac{(1 - \tau_t)w_t}{(1 - \tau_{t+1})w_{t+1}} (1 + r_t) \right]$$

where  $u_n$  is the marginal utility of leisure,  $\tau$  is the tax on labor income,  $w$  is the real wage rate,  $r$  is the real interest rate, and  $E_t$  is the expectation based on period  $t$  information. In expectation, the household should vary the growth rate of leisure inversely with the growth rate of after-tax real wages. This means that if  $\tau_{t+1}$  is expected to rise relative to  $\tau_t$ , households have an incentive to substitute their labor to the present (when it is taxed less) and their leisure to the future.

It is easy to show in a standard neoclassical model that the delayed response of taxes, such as we observe in the estimated impulse responses, results in a multiplier that is higher in the short-run but lower in the long-run relative to the lump-sum tax case. We have also conducted this experiment in the Gali et al. (2007) model where 50 percent of the households are rule-of-thumb consumers. We found the same effect in that model as well. Drautzburg and Uhlig (2011) analyze an extension of the Smets-Wouters model and also find that the timing of distortionary taxes is very important for the size of the multiplier. Given the impulse response of tax rates, and with these theoretical results in mind, it is very possible that our estimated multipliers are greater than we would expect if taxation were lump-sum.

Nevertheless, our finding that multipliers do not vary across states could be due to differential financing patterns. To determine whether this is the case, Figure 10 shows the state-dependent results. As we showed before, both government spending and GDP rise more if a news shock hits during a slack state, even after adjusting the initial size of the shock.<sup>32</sup> The bottom panels show that tax rates and deficits also rise more during recessions, but there are other interesting differences in the patterns. When we study the ratio of the cumulative deficit to cumulative government spending at each point in time along the path, we find that more of government spending is financed with deficits when a shock hits during a slack state.<sup>33</sup> For example, at quarter seven the ratio of the cumulative deficit to cumulative government spending is 73 percent if a shock hits during a slack state but only 49 percent if the shock hits during a non-slack state. Thus, on average short-run government spending is financed more with deficits if the shock hits during a slack state. In addition, tax rates rise with a delay during the slack state relative to non-slack state. This would imply that the multiplier should be greater during times of slack. In fact, our estimates imply that it is not.

## 5 Multipliers at the Zero Lower Bound

We now investigate whether government spending multipliers differ when government interest rates are near the zero lower bound or are being held constant to accommodate fiscal policy. Some New Keynesian models suggest that government spending multipliers will be substantially higher (e.g. above 2) when the economy is at the zero lower bound.<sup>34</sup> This

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32. The implied multipliers are very similar to the baseline case as well.

33. This is true with the exception of the second quarter. This can be explained by the fact that initially government spending and deficits rise slowly in response to a news shock and for the initial two quarters, deficits fall very slightly before rising.

34. See, for example, Eggertsson (2011) and Christiano et al. (2011). The relationship between government spending multipliers and the degree of monetary accommodation, even outside zero lower bound has been

view has been challenged by a series of new papers, some of which construct models in which multipliers are *lower* at the zero lower bound.<sup>35</sup> Thus, the literature now provides a number of plausible theories that predict both higher and lower multipliers at the zero lower bound. For this reason, it is useful to provide empirical evidence on this issue.

Very few papers have attempted to test the predictions of the theory empirically in aggregate data. As far as we know, only two examples exist. Ramey (2011) estimates her model for the U.S. over the sub-sample from 1939 through 1951 and shows that the multiplier is no higher during that sample. Crafts and Mills (2012) construct defense news shocks for the U.K. and estimate multipliers on quarterly data from 1922 through 1938. They find multipliers below unity even when interest rates were near zero.<sup>36</sup>

## 5.1 Defining States by Monetary Policy

The bottom panel of Figure 11 shows the behavior of three-month Treasury Bill rates from 1920 through the present, where the shortened sample is based on data availability. This interest rate was near zero during much of the 1930s and 1940s, as well as starting again in the fourth quarter of 2008. To indicate the degree to which interest rates were pegged (either by design or the zero lower bound), we compare the behavior of actual interest rates to that prescribed by the Taylor rule. We use the standard Taylor rule formulation:

$$(4) \quad \text{nominal interest rate} = 1 + 1.5 \text{ year-over-year inflation rate} + 0.5 \text{ output gap}$$

Figure 13 shows the behavior of inflation and the output gap, which were quite volatile during the early period.<sup>37</sup> The last panel of Figure 13 compares the behavior of actual interest rates to the Taylor rule. This graph makes clear that there were large deviations of interest rates from those prescribed by the Taylor rule briefly during the early 1920s and in a sustained way during most of the 1930s and 1940s.

In many theoretical models, it is not the zero lower bound per se, but rather the fact that nominal interest rates stay constant rather than following the Taylor rule that amplifies the stimulative effects of government spending. Thus, to assess whether multipliers are greater

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explored by many others, including Davig and Leeper (2011) and Zubairy (2014).

35. See, for example, Mertens and Ravn (2014), Aruoba and Schorfheide (2013), Braun et al. (2013) and Kiley (2014).

36. Bruckner and Tuladhar (2013) focus on local not aggregate multipliers for Japan, and find that the effects of local spending are larger in the ZLB period, but only modestly.

37. The output gap for the earlier period is constructed similarly to Gordon and Krenn (2010). See the data appendix for details.

in these situations we can include periods in which the nominal interest rate is relatively constant despite dramatic fluctuations in government spending.

For our baseline, we define ZLB or extended monetary accommodation times to be 1932q2 - 1951q1 and 2008q4 - 2013q4 (the end of our sample). We do not include the early 1920s as a ZLB episode since the episode was so brief and theory tells us that the multiplier depends on the (expected) length of the spell. Also, while the deviation from the Taylor rule widens starting in 1930, we do not include the early 1930s in our ZLB state. This is because the T-bill rate was fluctuating during this period, potentially responding to the state of the economy, and was as high as 2.5 percent in 1932q1 before falling to 0.5 percent in 1932q2 and staying low from then onwards. We will call these periods "ZLB states" for short, recognizing that they also include periods of monetary accommodation of fiscal policy. We end the early spell in 1951q1 because the Treasury Accord, which gave the Fed more autonomy, was signed in March 1951.

The top panel of Figure 11 shows the behavior of our defense news series over the states defined this way. The main shocks during these states occur after the start of WWII and at the start of the Korean War (in June 1950). There is essentially no information gained from the 1930s, unfortunately.<sup>38</sup> Figure 12 shows the F-tests for the periods split into ZLB periods and normal periods. The F-statistics tend to be higher for horizons 4 through 8. For the full sample, the F-statistics are above 5 but below 10. However, when we exclude WWII, the ZLB periods have F-statistics above 10 for many horizons. Thus, in contrast to the case in which the state is defined by the unemployment rate, the first stage F-statistics for the ZLB state rise when we exclude WWII.

## 5.2 Results

To determine whether multipliers are different in ZLB states, we estimate our baseline state-dependent model, but now allowing the state to be defined by monetary policy. We consider our full sample spanning 1889-2013.<sup>39</sup> Figure 14 shows the impulse responses. The results suggest that government spending responds more slowly, but more persistently during ZLB states than in normal states. The difference in GDP responses follow this pattern, but in a muted way. Table 4 shows the cumulative multipliers in each state for the different horizons

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38. An advantage of the Crafts and Mills (2012) analysis of UK data is that it has more defense news shocks during the 1930s.

39. Even though the 3 month T-bill rate was not available before 1920, we still consider the earlier period and call it a non-ZLB state, based on narrative evidence and data on commercial paper rate for which monthly data is available starting 1857.

of two and four years, respectively. For the 2-year integral, the multipliers in the ZLB state are slightly higher than normal times, and for the 4-year integral, the multiplier is higher in the normal state. However, the multiplier is less than one in both these cases and in no case do we find evidence of significantly higher multipliers during periods at the zero lower bound or constant interest rates. Figure 15 shows the cumulative multiplier for the ZLB and normal state at various different horizons along with 95% confidence bands. The multiplier for both states is high on impact when the news shock hits the economy and is less than one after one year, but the multipliers across the two states are never significantly different.

We explore the robustness in several ways. First, we redefined the ZLB state as periods where the T-bill rate was less than or equal to 50 basis points. We have data for the 3-month T-bill rate starting in 1920, but we assume that prior to 1920 there was no monetary accommodation, and classify 1890-1919 as non-ZLB period. As shown in the first lines of Table 5, this re-definition of the monetary state results in multipliers close to or slightly above 1 in the non-ZLB state and lower in the ZLB state. Thus again, we do not find any evidence of higher multipliers in the ZLB state.

We also check the robustness of our results under Blanchard and Perotti (2002) identification.<sup>40</sup> The next set of results in Table 5 show that under this alternative identification scheme, the linear multiplier is less than one for both two and four year horizons. The state dependent multipliers are also less than one: the 2 year multiplier under ZLB is 0.65 and 0.47 in normal times, whereas at four year horizon the multiplier is 0.78 under ZLB and slightly higher at 0.85 in normal times. Thus, again we do not find any evidence of significantly higher multipliers near the zero lower bound than normal times.

We consider the role of financing by controlling for taxes, by adding 2 lags of tax revenues as controls in our baseline specification. In this case, the multipliers are slightly lower in the ZLB state and higher in the normal state relative to the baseline case, for both 2 and 4 years. The difference between the multiplier in ZLB and normal states at 2 year is negligible and at 4 years, if anything, the multiplier is higher when the shock hits during a normal state than the ZLB state (see third panel of Table 5).

We also consider the robustness of the results to the addition of inflation as a control, by adding 2 lags of GDP deflator inflation in our baseline specification. In this case, as shown in Table 5, we find similar multipliers to the baseline in the ZLB state and slightly higher in the normal state relative to the baseline. The difference across the two states is not significant in this case either. In addition, we consider the response of GDP deflator inflation during

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40. We should note, however, that while the F-statistics for the BP shock are very high for the first four quarters, they quickly fall toward 0.

the ZLB and normal state. We find no evidence of a larger response for inflation in the ZLB state, as suggested by new-Keynesian models (not shown).<sup>41</sup>

A major concern is that an important part of the ZLB state was during the rationing period of WWII. If rationing depressed multipliers, and all of the rationing occurred during the ZLB state, then this could explain why we find no differences across periods. To determine whether our results are sensitive to the rationing period, we exclude observations from the estimation if either the shock or the dependent variable occurred in any quarter from 1941q3 through 1945q4. Table 6 shows that this change results in multipliers slightly higher in the linear case, somewhat above 1. Considering the state dependent multipliers, it is apparent that exclusion of World War II does not impact the multiplier in the normal state, but results in larger multipliers at the 2-year horizon in the ZLB state. The difference in multipliers is driven by the impulse response function of government spending across the two different states, where it rises more robustly and faster in normal times than the ZLB state, when we exclude WWII. The response of output is statistically not different across the two states. Notably, the multiplier at the 2-year horizon in the ZLB state is 1.59 versus 0.57 in the normal state, and the difference in the multipliers has a p-value of 0.126. This is one case, however, where the multipliers calculated for near horizons give different answers. During a few of the quarters of the second year, the cumulative multipliers are 2 or above and the p-value on the differences across states are below 0.05. Thus, these results support the notion that the multiplier is higher at the ZLB.

We also conduct robustness checks for this sample. The Blanchard and Perotti (2002) identification also suggests higher multipliers at the ZLB at both the 2-year and 4-year horizons. The differences across states are not statistically significant, though, because the standard errors are so much bigger. The third set of results shows the effects of controlling for taxes. In this case, the multipliers at the 2-year horizon are 1.2 at the ZLB and 0.7 in normal times. This flips, though, so that at the 4-year horizon, the multipliers are only 0.8 at the ZLB but 1.36 during normal times. In no case, though, are the multipliers statistically different across states. Thus, the finding of higher multipliers in the ZLB is not robust to including taxes in the specification. The fourth set of results shows the effects of including inflation (but not taxes). These results are more similar to the baseline results, indicating elevated multipliers at the ZLB. However, if we also include taxes, these differences disappear as well (not shown).

Thus, we do not find robust results in support of the New Keynesian model prediction

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41. This evidence is also in line with the findings of Dupor and Li (2013).

that multipliers are greater at the zero lower bound. While the key to those predictions lie in the behavior of inflationary expectations, we think it unlikely that expectations would have behaved in a way to change the results. Thus, neither our results nor those of Crafts and Mills (2012) for the UK are consistent with the predictions of the New Keynesian model at the ZLB.<sup>42</sup>

## 6 Conclusion

In this paper, we have investigated whether government spending multipliers vary depending on the state of the economy. In order to maximize the amount of variation in the data, we constructed new historical quarterly data spanning more than 120 years in the U.S. We considered two possible indicators of the state of the economy: the amount of slack, as measured by the unemployment rate, and whether interest rates were being held constant close to the zero lower bound. Using a more robust method for estimating state-dependent impulse responses and better ways of calculating multipliers from them, we provided numerous estimates of multipliers across different specifications.

Our results can be summarized as follows. We find no evidence of significant differences in multipliers when the U.S. economy is experiencing substantial slack as measured by the unemployment rate. Most multipliers are slightly below unity with a few slightly above unity. Our numerous robustness checks suggest that our results are not sensitive to variations in our specification. We also conducted a detailed analysis of why our results are so different from those of Auerbach and Gorodnichenko (2012). We found the key source of differences are the specialized assumptions Auerbach and Gorodnichenko (2012) use to calculate their impulse response functions.

In our analysis of multipliers in zero lower bound interest rate states, we found no evidence that multipliers are greater at the zero lower bound in the full sample. The results are mixed, however, when we exclude World War II from the sample. Several specifications suggest that multipliers are higher at some horizons at the ZLB. However, these results are not robust to simple generalizations, such as the inclusion of taxes. Thus, we cannot claim that we find definitive evidence for higher multipliers at the zero lower bound.

Of course, our results come with many caveats. As discussed in the introduction, we are forced to use data determined by the vagaries of history so we do not have a controlled

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42. In separate work, Wieland (2013) tests the New Keynesian prediction that negative supply shocks are expansionary at the ZLB, and also finds evidence contrary to that prediction.

experiment. Because we use news about future military spending as our identified shock, our results do not inform us about the size of multipliers on transfer payments or infrastructure spending. Moreover, because the episodes we studied were characterized by certain paths of taxes, the results are not immediately applicable to the case of deficit-financed stimulus packages or fiscal consolidations.

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## Data Appendix

### GDP and GDP deflator:

*1947 - 2013:* Quarterly data on chain-weighted real GDP, nominal GDP, and GDP deflator from BEA NIPA (downloaded from FRED, March 27, 2014 revision).

*1889 - 1946:* Annual data from 1929 - 1946 from BEA NIPA (downloaded from FRED, December 20, 2012 version). For 1889 - 1928, series Ca9 and Ca13 from Table Ca9-19 in Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition, Carter et al. (2006). These series are based on the work of Kuznets, Kenrick, Gallman and Balke-Gordon.

*1939 - 1946:* We used seasonally adjusted quarterly nominal data on GNP from National Income, 1954 Edition, A Supplement to the Survey of Current Business and seasonally unadjusted CPI (all items, all urban consumers) from FRED.

*1889 - 1938:* Quarterly data on real GNP and GNP deflator. Source: Balke and Gordon (1986). Data available at: <http://www.nber.org/data/abc/>

*Data adjustment:* For 1939-1946, we used a simplified version of the procedure used by Valerie Ramey, "Identifying Government Spending Shocks: It's All in the Timing", Quarterly Journal of Economics, February 2011. We used the quarterly nominal GNP series published in *National Income, 1954 Edition, A Supplement to the Survey of Current Business* to interpolate the the modern NIPA annual nominal GDP series, and the quarterly averages of the CPI to interpolate the NIPA annual GDP price deflator using the proportional Denton method. We took the ratio to construct real GDP to use as a second round interpolator. We spliced this quarterly real GDP series to the Balke-Gordon quarterly real GNP series from 1889 - 1938 and used the combined series to interpolate the annual real GDP series (described above) using the proportional Denton method. This method insures that all quarterly real GDP series average to the annual series. We used the Balke-Gordon deflator to interpolate the annual deflator series from 1889 - 1938 and combined it with the CPI-interpolated series from 1939-1946. Finally, we linked the earlier series to the modern quarterly NIPA series from 1947 to the present.

### Potential GDP:

Real potential GDP was constructed by splicing the February 2014 CBO estimates of real potential GDP from 1949 to the present with an estimated cubic trend through real GDP from 1889-1950, excluding 1930 - 1946 from the estimation. Our method of constructing real potential GDP is similar to the method advocated by Gordon and Krenn (2010). They illustrate the problems that arise when one uses standard filters to estimate trends during samples that involve the

Great Depression and World War II, and advocate instead using a piecewise exponential trend based on benchmark years. Our procedure is a smoothed version of theirs. To derive nominal potential GDP, we multiplied real potential GDP by the actual price level. To derive the output gap for the Taylor rule, we used the difference between log actual real GDP and log potential.

### **Government Spending:**

*1947 - 2013:* Quarterly data on nominal "Government Consumption Expenditures and Gross Investment," from BEA NIPA (downloaded from FRED, March 27, 2014 revision).

*1889 - 1946:* NIPA annual nominal data from 1929 - 1946 (BEA Table 1.1.5, line 21) is spliced to annual data from 1889-1928, Source: Kendrick (1961) Table A-II.

*1939 - 1946:* Quarterly data on nominal government spending from *National Income, 1954 Edition, A Supplement to the Survey of Current Business* is used to interpolate the modern annual NIPA values.

*1889 - 1938:* Monthly data on federal budget expenditures. Source: NBER MacroHistory Database.

<http://www.nber.org/databases/macrophistory/contents/chapter15.html>

m15005a U.S. Federal Budget Expenditures, Total 01/1879-09/1915

m15005b U.S. Federal Budget Expenditures, Total 11/1914-06/1933

m15005c U.S. Federal Budget Expenditures, Total 01/1932-12/1938

*Data adjustment:* The monthly series are spliced together (using a 12-month average at the overlap year) and seasonally adjusted in Eviews using X-12. This series includes not just government expenditures but also transfer payments, and so the monthly interpolator series is distorted by large transfer payments in different quarters. Thus, rather than using the series directly, we use it as a monthly interpolator for the annual series which excludes transfers. Following Gordon and Krenn (2010), to find these quarters, we calculated the monthly log change in the interpolator, and whenever a monthly change of +40 percent or more was followed by a monthly change of approximately the same amount with a negative sign (and also symmetrically negative followed by positive), we replaced that particular observation by the average of the preceding and succeeding months. These instances occurred for the following months: 1904:5, 1922:11, 1931:2, 1931:12, 1932:7, 1934:01, 1936:06, and 1937:06. In addition, the first quarter of 1917 was adjusted. The jump in spending was so dramatic in 1917q2 that the interpolated series showed a decline in spending in 1917q1 even though the underlying expenditure series showed an increase of 16 percent in that quarter relative to the previous one. Thus, we replaced the value of 1917q1

with a value 16 percent higher than the previous quarter. Note that our use of the proportional Denton method creates a bumpier series than an alternative that uses the additive Denton method. However, the additive Denton method leads to series that behave very strangely around large buildups and build-downs of government spending, so we did not use it. On the other hand, the alternative series gave very similar results for the multiplier.

### **Military News:**

The narrative underlying the series is available in Ramey (2014).

### **Population:**

*1890-2013:* Annual population data, based on July of each year, were taken from Historical Statistics of the United States Millennial Edition Online, Carter et al. (2006) We used total population, including armed forces overseas for all periods where available (during WWI and 1930 and after); otherwise we used the resident population. For 1952 through the present we used the monthly series available on the Federal Reserve Bank of St. Louis FRED database, "POP."

*Data adjustment:* For 1890 through 1951, we linearly interpolated the annual data to obtain monthly series so that the annual value was assigned to July. We then took the averages of monthly values to obtain quarterly series. We did the same to convert the monthly FRED data from 1952 to the present.

### **Tax Revenues:**

*1947-2013:* Quarterly data on nominal "Federal Government Current receipts," BEA Table 3.2, line 1, March 27, 2014 version. Note that all NIPA BEA data is on an accrual basis.

*1879-1938:* Monthly data on federal budget receipts. Source: NBER Macro-History Database  
<http://www.nber.org/databases/macrohstory/contents/chapter15.html> . These data are on a cash basis.

m15004a U.S. Federal Budget Receipts, Total 01/1879-06/1933

m15004b U.S. Federal Budget Receipts, Total 07/1930-06/1940

m15004c U.S. Federal Budget Receipts, Total 07/1939-12/1962

*1939-1946:* Quarterly data on nominal federal receipts from *National Income, 1954 Edition, A Supplement to the Survey of Current Business* is used to interpolate the modern annual NIPA values. We construct the quarterly federal receipts

interpolator from federal personal taxes + total corporates taxes + total indirect taxes.

*1889-1928:* Annual data on federal receipts. Source: Historical Statistics - fiscal year basis (e.g. fiscal year 1890 starts July 1, 1889).

*1929-1946:* Annual data on nominal "Federal Government Current receipts," BEA Table 3.2, line 1, March 27, 2014 version.

*Data adjustment:* The monthly series are strung together (with the most recent series used for overlap periods) and seasonally adjusted in Eviews using X-12. The annual series is interpolated using the monthly data with the Denton proportional method.

### **Unemployment rate:**

*1948-2013:* Monthly civilian unemployment rate. Source: Federal Reserve Bank of St. Louis FRED database, UNRATE  
<http://research.stlouisfed.org/fred2/series/UNRATE>

Data adjustment: Quarterly series is constructed as the average of the three months.

*1890-1947:* Annual civilian unemployment rate. Source: Weir (1992). We adjusted the Weir series from 1933-1943 to include emergency workers from Conference Board (1945).

*1890-1929:* NBER-based monthly recession indicators. Source: Federal Reserve Bank of St. Louis FRED database, USREC  
<http://research.stlouisfed.org/fred2/series/USREC>.

*1930-1946:* Monthly civilian unemployment rate (including emergency workers). Source: NBER MacroHistory Database  
<http://www.nber.org/databases/macrophistory/contents/chapter08.html>

m08292a U.S. Unemployment Rate, Seasonally Adjusted 04/1929-06/1942  
m08292a U.S. Unemployment Rate, Seasonally Adjusted 01/1940, 03/1940-12/1946

*1947:* Monthly civilian unemployment rate (including emergency workers, seasonally adjusted) Source: Geoffrey Moore, Business Cycle Indicators, Volume II, NBER p. 122

*Data adjustment:* Monthly NBER recession data are used to interpolate annual data using the Denton interpolation from 1890-1929. For 1930-1947 onwards we use the monthly unemployment rate series to interpolate annual data using the Denton proportional interpolation.

### **Interest rate:**

1934-2013: Monthly 3 month Treasury bill. Source: Federal Reserve Bank of St. Louis FRED database, TB3MS

<http://research.stlouisfed.org/fred2/series/TB3MS>.

1920-1933: Monthly 3 month Treasury bill. Source: NBER MacroHistory Database

<http://www.nber.org/databases/macroeconomy/contents/chapter13.html>

m13029a U.S. Yields On Short-Term United States Securities, Three-Six Month Treasury Notes and Certificates, Three Month Treasury 01/1920-03/1934

m13029b U.S. Yields On Short-Term United States Securities, Three-Six Month Treasury Notes and Certificates, Three Month Treasury 01/1931-11/1969

*Data adjustment:* Quarterly series is constructed as the average of the three months.

**Table 1. Estimated Multipliers Across States of Slack**

	Linear Model	High Unemployment	Low Unemployment	P-value for difference in multipliers across states
<b>U.S.</b>				
2 year integral	0.76 (0.102)	0.69 (0.094)	0.78 (0.187)	0.631
4 year integral	0.84 (0.092)	0.76 (0.060)	0.96 (0.218)	0.331

Note: The values in brackets under the multipliers give the standard errors.

**Table 2. Robustness Checks of Multipliers Across States of Slack**

	Linear Model	High Unemployment	Low Unemployment
<b>Using linearly interpolated data</b>			
2 year integral	0.64	0.58	0.67
4 year integral	0.76	0.65	0.84
<b>Omitting the quartic trend</b>			
2 year integral	0.66	0.52	0.71
4 year integral	0.74	0.63	0.82
<b>Using Gordon-Krenn transformation</b>			
2 year integral	0.77	0.62	0.64
4 year integral	0.85	0.75	0.76
<b>Considering 8% unemployment rate threshold</b>			
2 year integral	0.76	0.78	0.76
4 year integral	0.84	0.77	0.96
<b>HP filtered time-varying threshold (with <math>\lambda = 10^6</math>)</b>			
2 year integral	0.76	0.73	0.78
4 year integral	0.84	0.82	0.92
<b>Subsample: 1947-2013</b>			
2 year integral	0.89	-5.52	1.05
4 year integral	0.47	24.97	0.62
<b>Blanchard-Perotti identification</b>			
2 year integral	0.54	0.70	0.49
4 year integral	0.78	0.87	0.75
<b>Exclude World War II</b>			
2 year integral	1.01 (0.376)	0.83 (1.484)	0.97 (0.325)
4 year integral	1.26 (0.365)	1.25 (1.456)	1.18 (0.352)
	Linear	Recession	Expansion
<b>7 qtr. moving avg. output growth, <math>F(z)</math></b>			
2 year integral	0.76	0.55	0.77
4 year integral	0.84	0.61	0.85

Note: The values in brackets under some multipliers give the standard errors.

**Table 3. Comparison to Auerbach-Gorodnichenko (2012) Multipliers**

<b>Direct Comparisons</b>	Extreme Recession ( $F(z)=1$ )	Extreme Expansion ( $F(z)=0$ )	Difference
<b>AG's Estimates, Constant State</b>			
5 year integral	2.24 (0.24)	-0.33 (0.20)	2.57
2 year integral	1.65	0.10	1.55
<b>Jordà Method Applied to AG Specification</b>			
5 year integral	0.84	-0.59	1.43
2 year integral	0.24	0.36	-0.12
<b>Alternative Multipliers using AG's STVAR Estimates</b>	Severe Recession ( $F(z) \geq 0.95$ )	Severe Expansion ( $F(z) \leq 0.05$ )	Difference
<b>Constant State, No Feedback</b>			
5 year integral	2.16	-0.31	2.47
2 year integral	1.56	0.10	1.46
<b>Actual State Dynamics, No Feedback</b>			
5 year integral	1.41	0.19	1.22
2 year integral	1.13	0.15	0.97
<b>AG Partial Feedback</b>			
5 year integral	1.36	-0.04	1.40
2 year integral	1.01	0.15	0.86
<b>Actual State Dynamics, Partial Feedback</b>			
5 year integral	1.07	0.14	0.93
2 year integral	1.07	0.12	0.95

Note: STVAR denotes the Smooth Transition Vector Autoregression used by AG-12. Impulse responses are calculated based on the VAR parameter estimates and auxiliary assumptions. The values in brackets under the multipliers give the standard errors.  $F(z)$  is AG's indicator of the state of the economy.  $F(z) = 1$  indicates the most severe recession possible and  $F(z) = 0$  indicates the most extreme boom possible.

"Constant state" means that the impulse responses are calculated assuming that the economy remains in its current state for the duration of the multiplier. "Feedback" means that the estimates allow government spending to change the state of the economy going forward.

**Table 4. Estimated Multipliers Across Monetary Policy Regimes**

	Linear Model	Near Zero Lower Bound	Normal	P-value for difference in multipliers across states
2 year integral	0.76 (0.102)	0.81 (0.139)	0.57 (0.191)	0.356
4 year integral	0.84 (0.092)	0.76 (0.073)	0.85 (0.501)	0.860

Note: The values in brackets under the multipliers give the standard errors.

**Table 5. Robustness Checks of Multipliers Across Monetary Policy Regimes**

	Linear Model	Near Zero Lower Bound	Normal	P-value for difference in multipliers across states
<b>Defining ZLB as T-bill rate <math>\leq 0.5</math></b>				
2 year integral	0.76 (0.104)	0.70 (0.092)	1.02 (0.389)	0.442
4 year integral	0.84 (0.097)	0.72 (0.059)	1.30 (0.455)	0.225
<b>Blanchard-Perotti identification</b>				
2 year integral	0.54 (0.087)	0.65 (0.079)	0.47 (0.191)	0.411
4 year integral	0.78 (0.112)	0.78 (0.091)	0.85 (0.579)	0.897
<b>Including taxes</b>				
2 year integral	0.74 (0.103)	0.73 (0.131)	0.71 (0.259)	0.925
4 year integral	0.82 (0.106)	0.71 (0.065)	1.36 (0.541)	0.245
<b>Including inflation</b>				
2 year integral	0.78 (0.090)	0.80 (0.129)	0.69 (0.144)	0.592
4 year integral	0.85 (0.088)	0.76 (0.063)	1.02 (0.383)	0.511

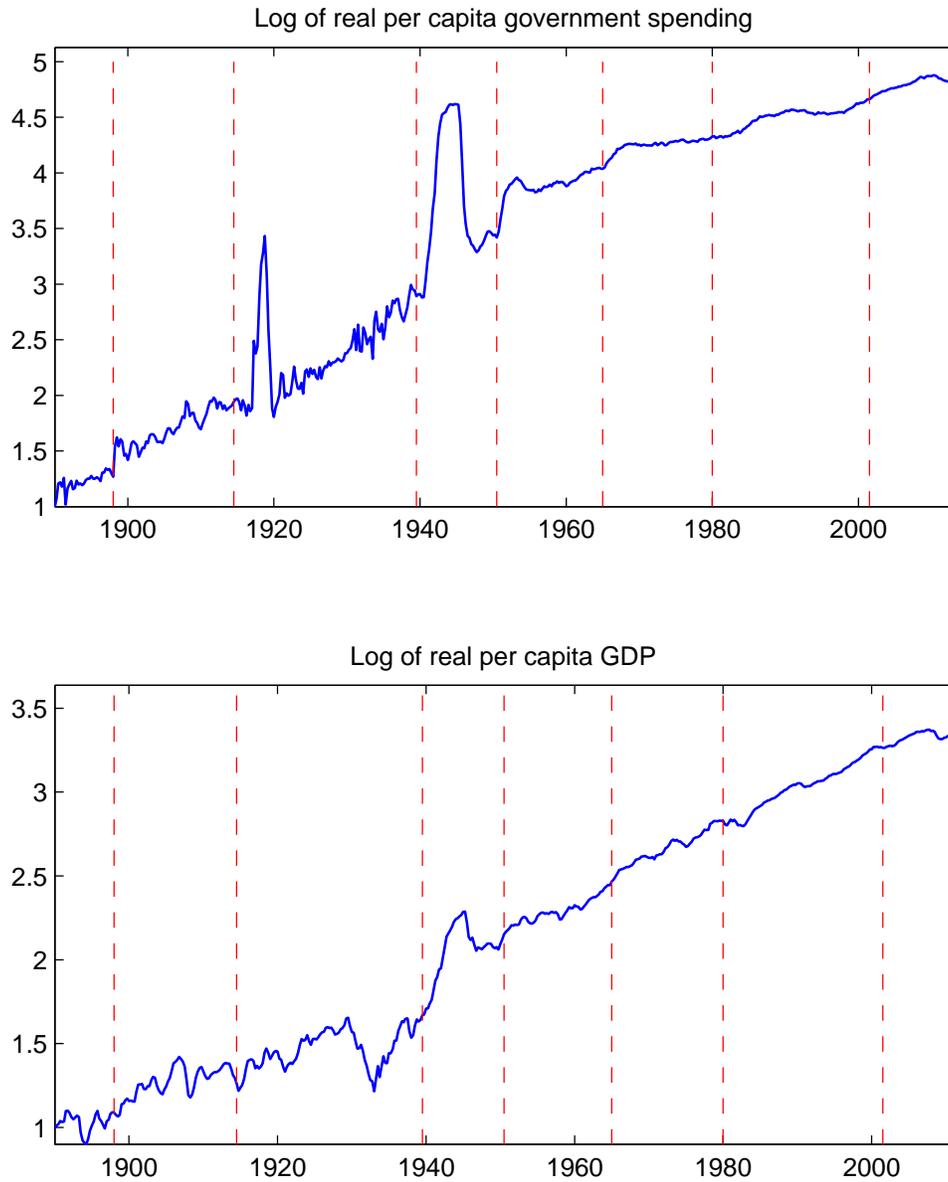
Note: The values in brackets under the multipliers give the standard errors.

**Table 6. Robustness Checks of Multipliers Across Monetary Policy Regimes: Excluding WWII**

	Linear Model	Near Zero Lower Bound	Normal	P-value for difference in multipliers across states
<b>Baseline specification</b>				
2 year integral	1.01 (0.376)	1.59 (0.532)	0.57 (0.192)	0.126
4 year integral	1.26 (0.365)	1.06 (0.364)	0.85 (0.501)	0.790
<b>Blanchard-Perotti identification</b>				
2 year integral	0.49 (0.190)	1.88 (1.078)	0.47 (0.193)	0.198
4 year integral	0.78 (0.461)	1.70 (1.084)	0.86 (0.573)	0.469
<b>Including taxes</b>				
2 year integral	0.98 (0.266)	1.19 (0.266)	0.71 (0.261)	0.240
4 year integral	1.29 (0.272)	0.79 (0.301)	1.36 (0.542)	0.388
<b>Including inflation</b>				
2 year integral	1.06 (0.326)	1.53 (0.496)	0.69 (0.144)	0.140
4 year integral	1.28 (0.316)	1.00 (0.356)	1.01 (0.379)	0.987

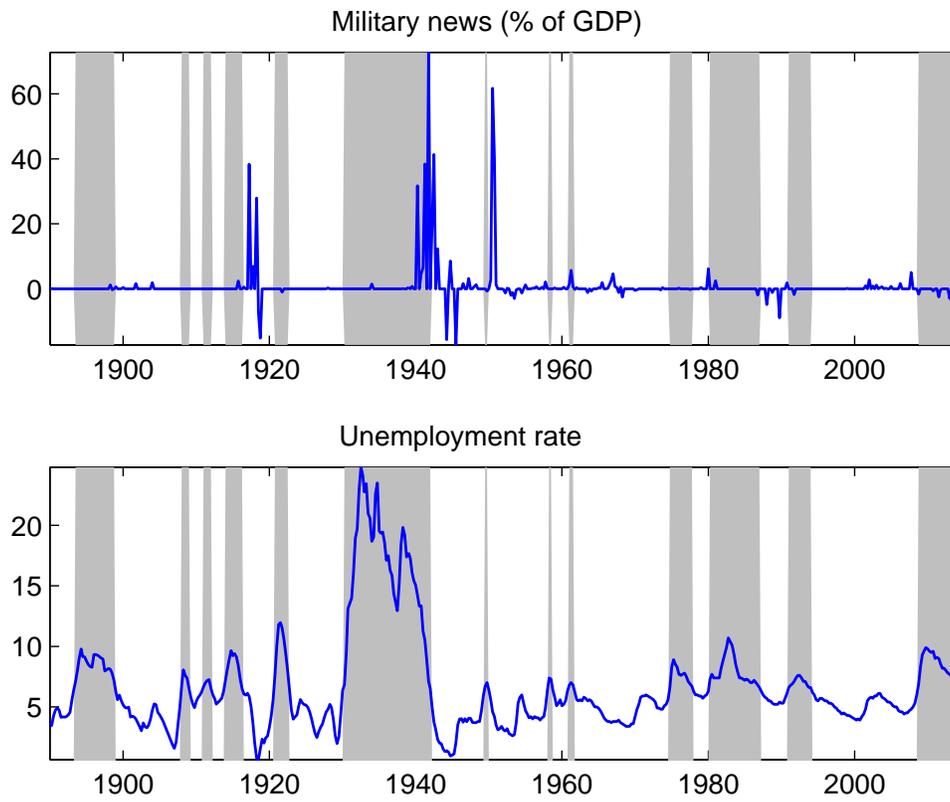
Note: The values in brackets under the multipliers give the standard errors.

**Figure 1. Government Spending and GDP**



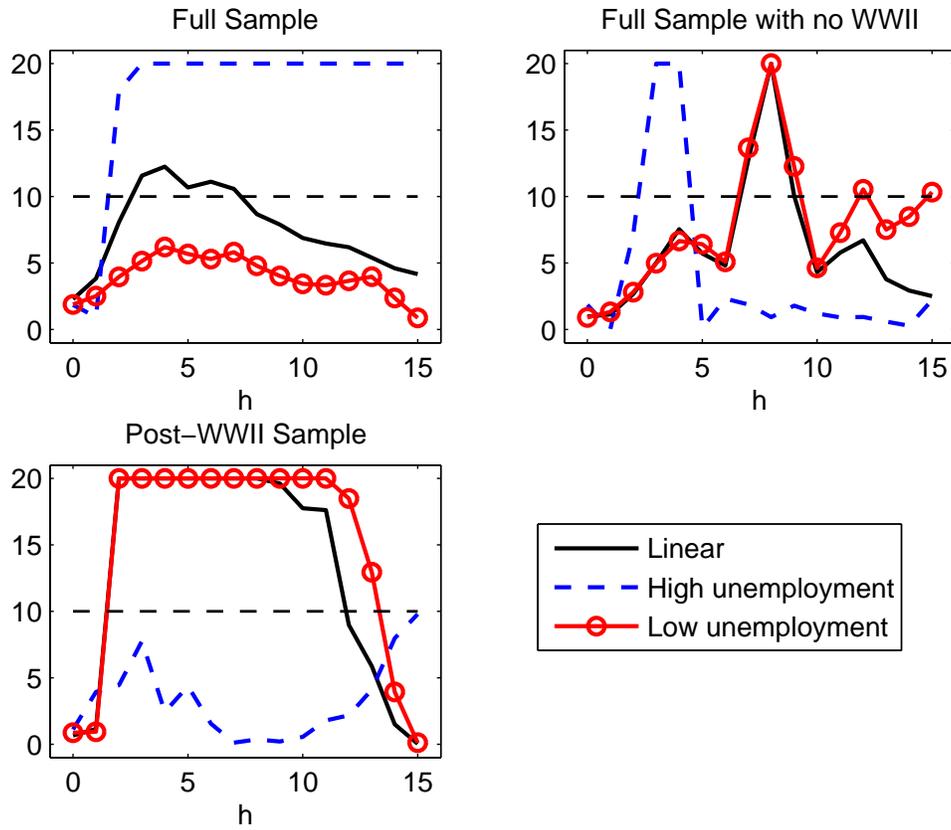
Note: The vertical lines indicate major military events: 1898q1(Spanish-American War), 1914q3 (WWI), 1939q3 (WWII), 1950q3 (Korean War), 1965q1 (Vietnam War), 1980q1 (Soviet invasion of Afghanistan), 2001q3 (9/11).

**Figure 2. Military spending news and unemployment rate**



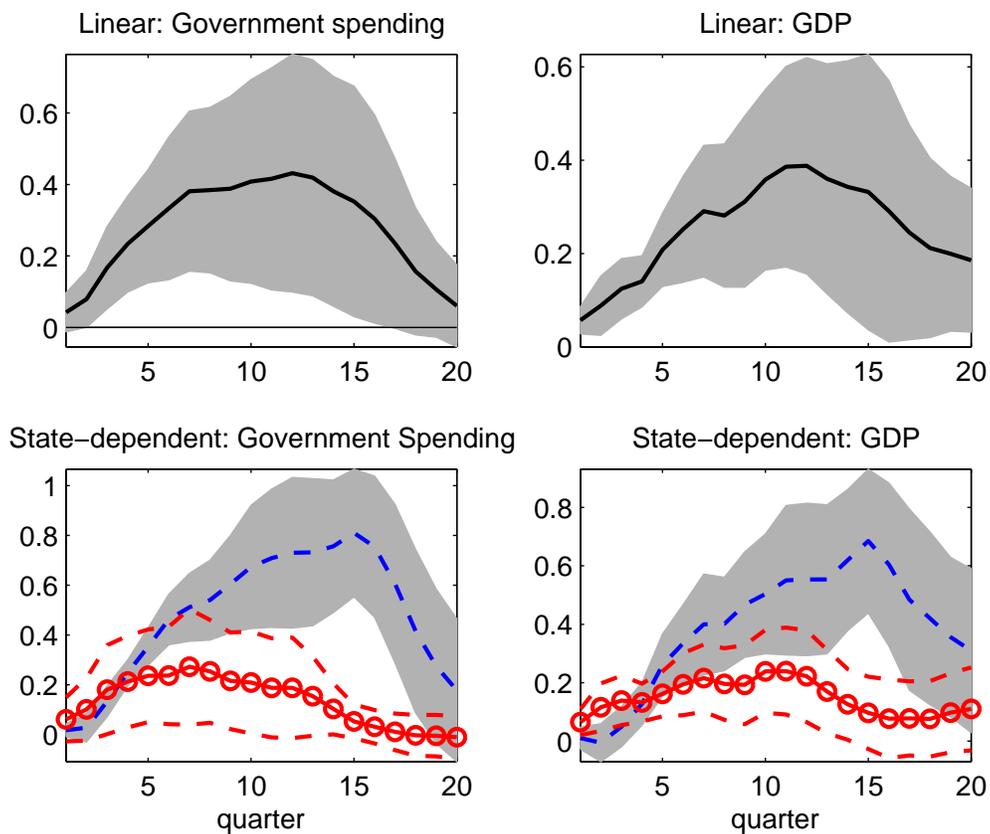
Note: Shaded areas indicate periods when the unemployment rate is above the threshold of 6.5%.

**Figure 3. Tests of Instrument Relevance Across States of Slack**



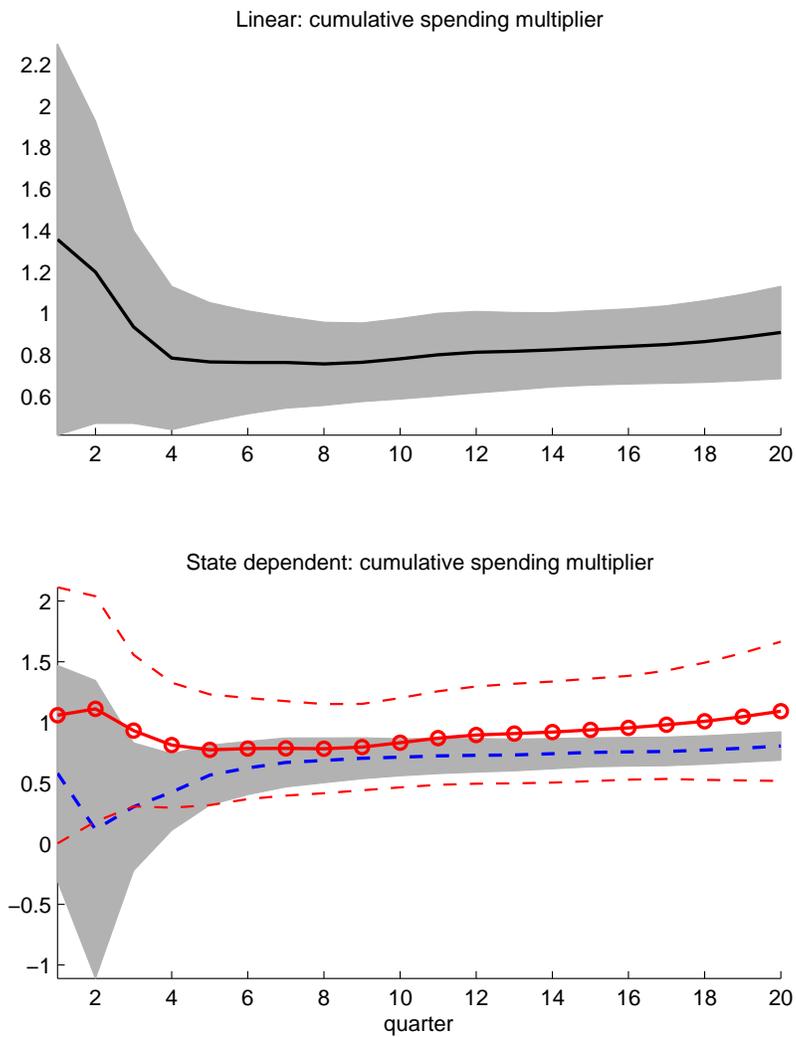
Note: "Slack" is when the unemployment rate exceeds 6.5 percent. The lines show the F-statistic on the news variable for each horizon in the case of the linear model (solid black lines), high unemployment state (blue dashed lines) and the low unemployment state (lines with red circles). Statistics are capped at 20. The full sample is 1890:1-2013:4, and the post-WWII sample spans 1947:3 - 2013:4.

**Figure 4. Government spending and GDP responses to a news shock across slack states**



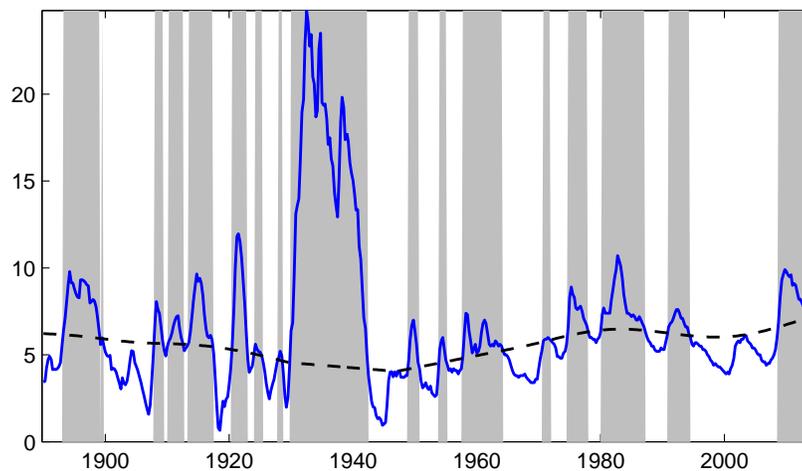
Note: Response of government spending and GDP to a news shock equal to 1% of GDP. The top panel shows the responses in the linear model. The bottom panel shows the state-dependent responses where the blue dashed lines are responses in the high unemployment state and the lines with red circles are responses in the low unemployment state. 95% confidence intervals are shown in all cases.

**Figure 5. Cumulative multipliers across slack states**



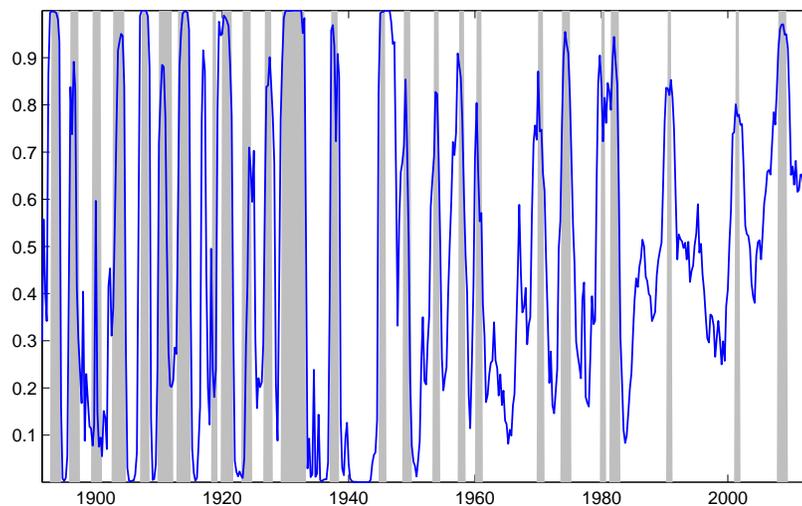
Note: Cumulative spending multipliers across different horizons. The top panel shows the cumulative multipliers in the linear model. The bottom panel shows the state-dependent multipliers where the blue dashed lines are multipliers in the high unemployment state and the lines with red circles are multipliers in the low unemployment state. 95% confidence intervals are shown in all cases.

**Figure 6. Robustness check: New threshold of unemployment rate based on time-varying trend**



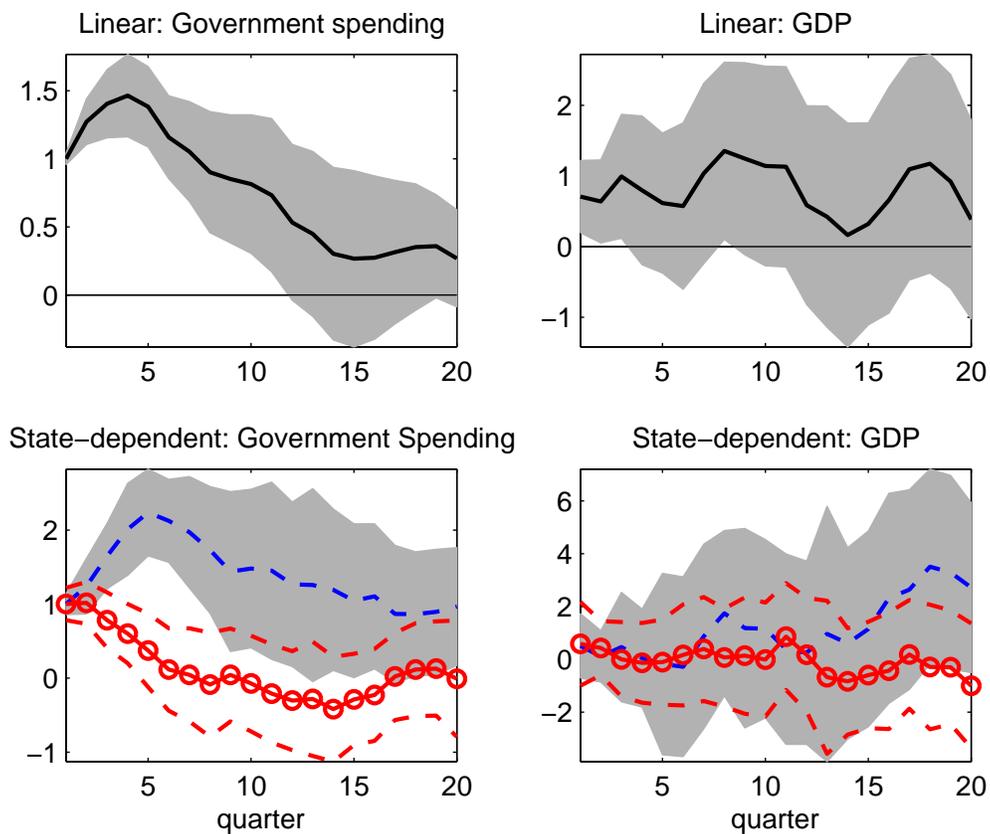
Note: Unemployment rate with a time-varying trend. The solid line is the unemployment rate and the black dashed line shows the time varying trend based on HP filter with  $\lambda = 10^6$ , over a split sample, 1889 - 1929 and 1947 - 2013 and linearly interpolated for the small gap in trend unemployment between 1929 and 1947. Shaded areas indicate periods when the unemployment rate is above the time-varying trend.

**Figure 7. Robustness check: New smooth transition threshold based on moving average of output growth**



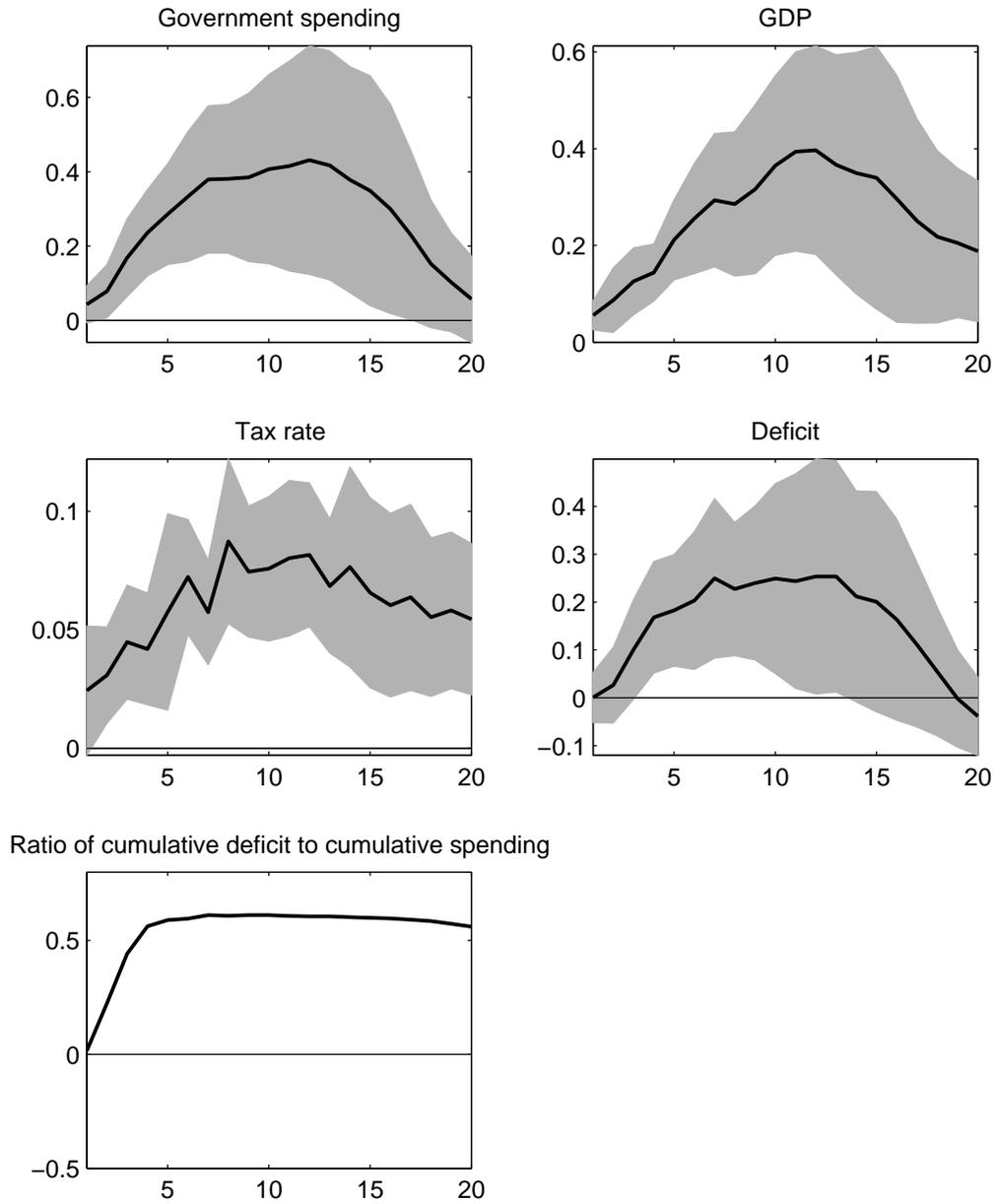
Note: The figures shows the weight on a recession regime,  $F(z)$  and the shaded areas indicate recessions as defined by NBER.

**Figure 8. Estimating Auerbach and Gorodnichenko (2012) with the Jorda method**



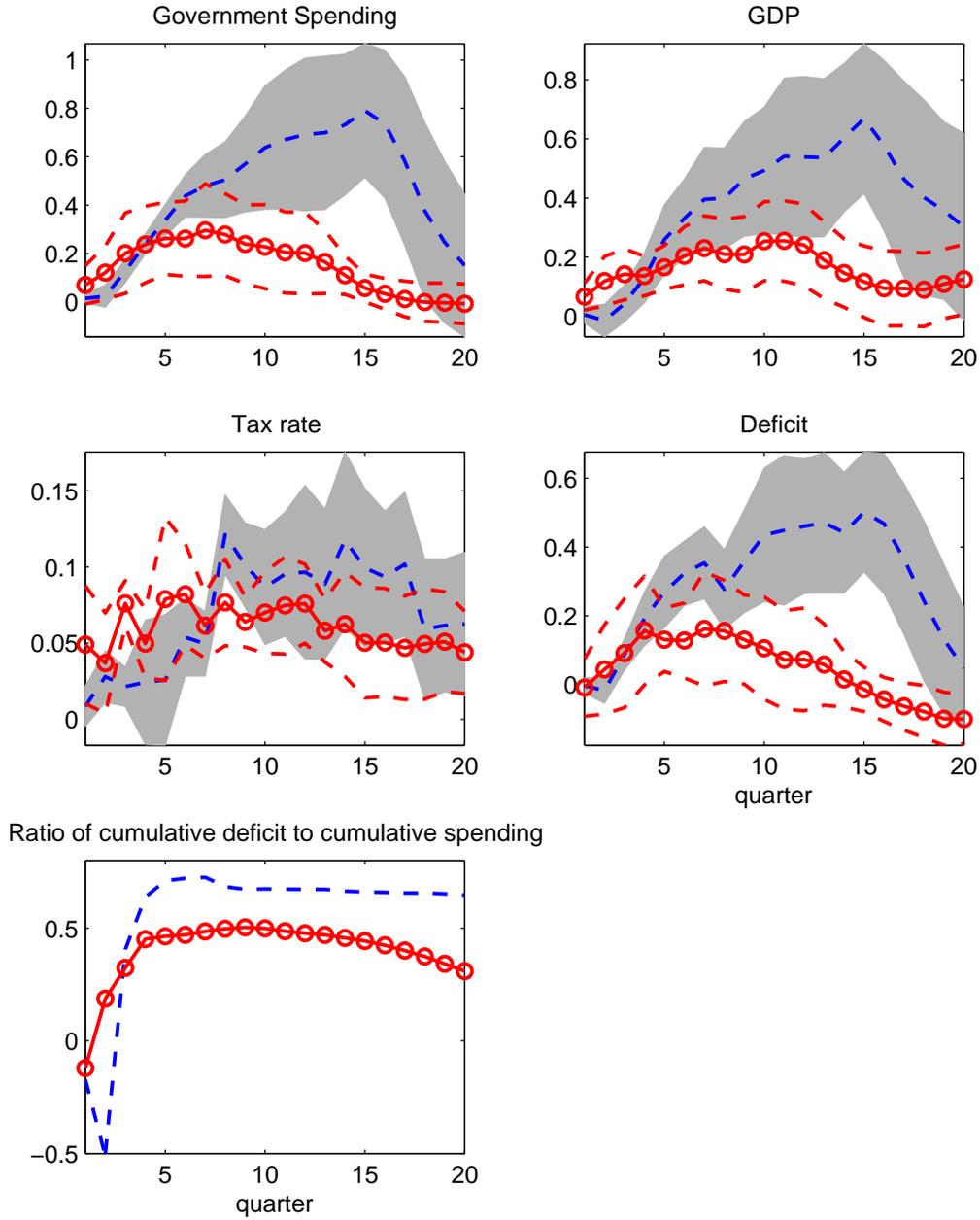
Note: Response of government spending and GDP to a government spending shock equal to 1% of GDP, with the same data, identification scheme and threshold definition as Auerbach and Gorodnichenko (2012). The top panel shows the responses in the linear model. The bottom panel shows the state-dependent responses where the blue dashed lines are responses in recession and the lines with red circles are responses in expansions. 95% confidence intervals are shown in all cases.

**Figure 9. Responses of taxes and deficits**



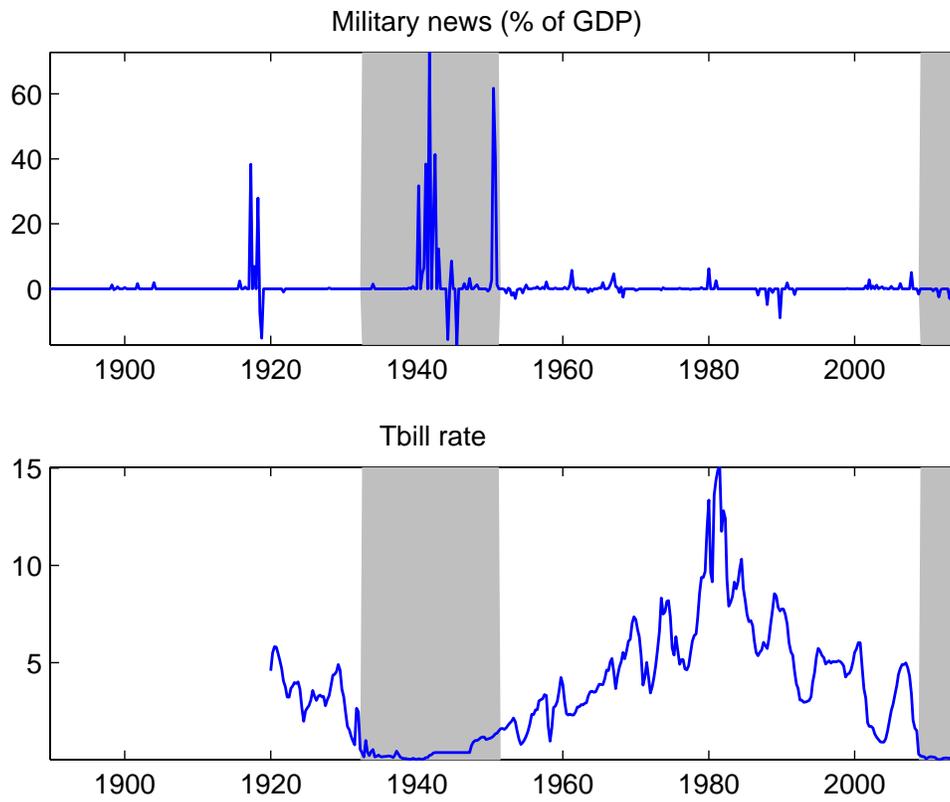
Note: These are responses for taxes and deficits in the linear model. The shaded areas indicate 95% confidence bands.

**Figure 10. State-dependent responses of taxes and deficits: Considering slack state**



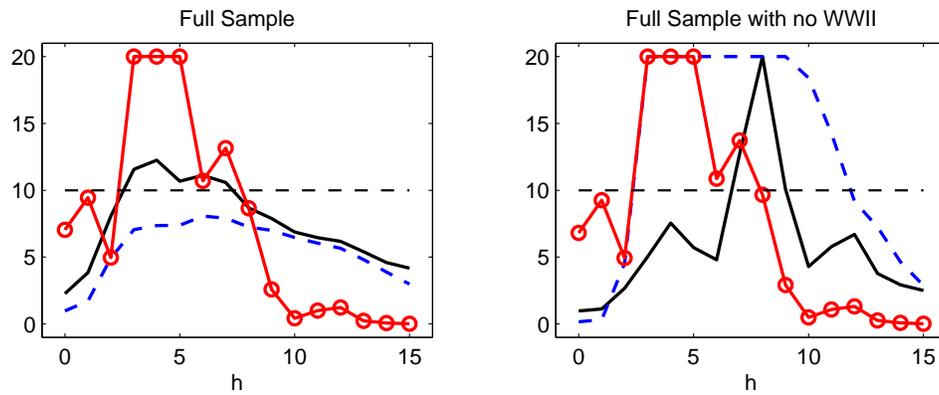
Note: These are state-dependent responses for taxes and deficits, where the black solid lines are responses in the high unemployment state and the lines with red circles are responses in the low unemployment state. 95% confidence intervals are also shown.

**Figure 11. Military spending news and interest rate**



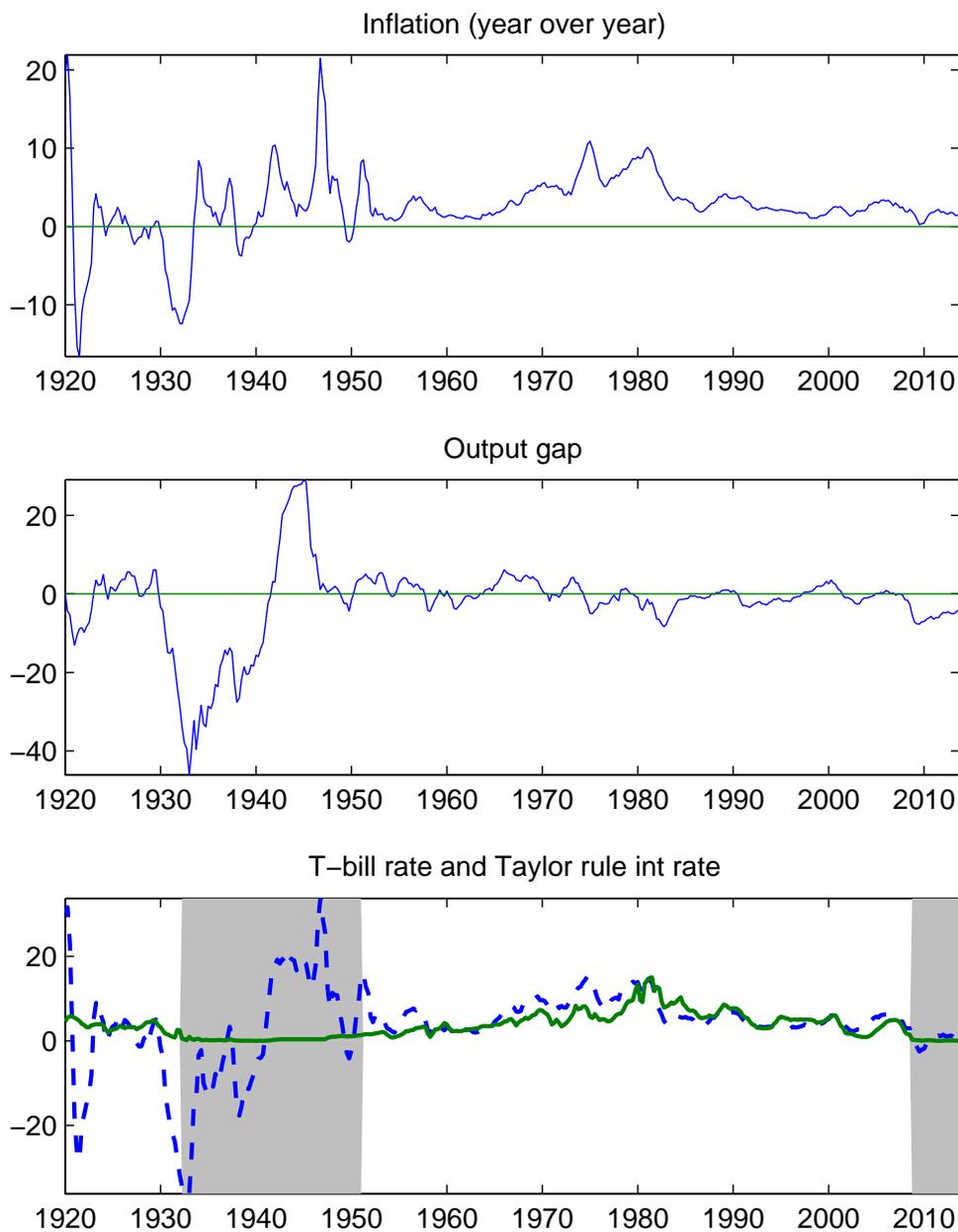
Note: Shaded areas indicate periods which we classify as the zero lower bound period for interest rate.

**Figure 12. Tests of Instrument Relevance Across Monetary Policy Regimes**



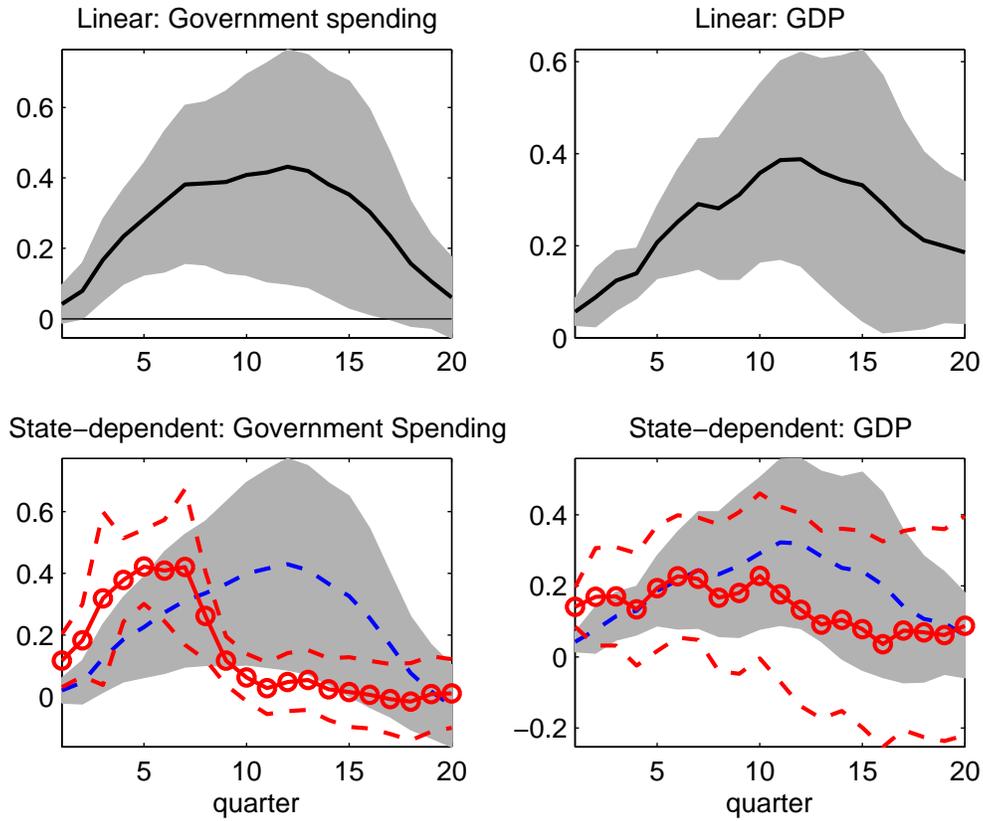
Note: "ZLB" is when interest rates are near the zero lower bound or the Fed is being very accommodative of fiscal policy (1932q1-1951q1, 2008q4-2013q4). The lines show the F-statistic on the news variable for each horizon in the case of the linear model (solid black lines), ZLB state (blue dashed lines) and the normal state (lines with red circles). Statistics are capped at 20. The full sample is 1890:1-2013:4, and the post WWII sample spans 1947:3 - 2013:4.

**Figure 13. Inflation, output gap and Taylor rule implied interest rate**



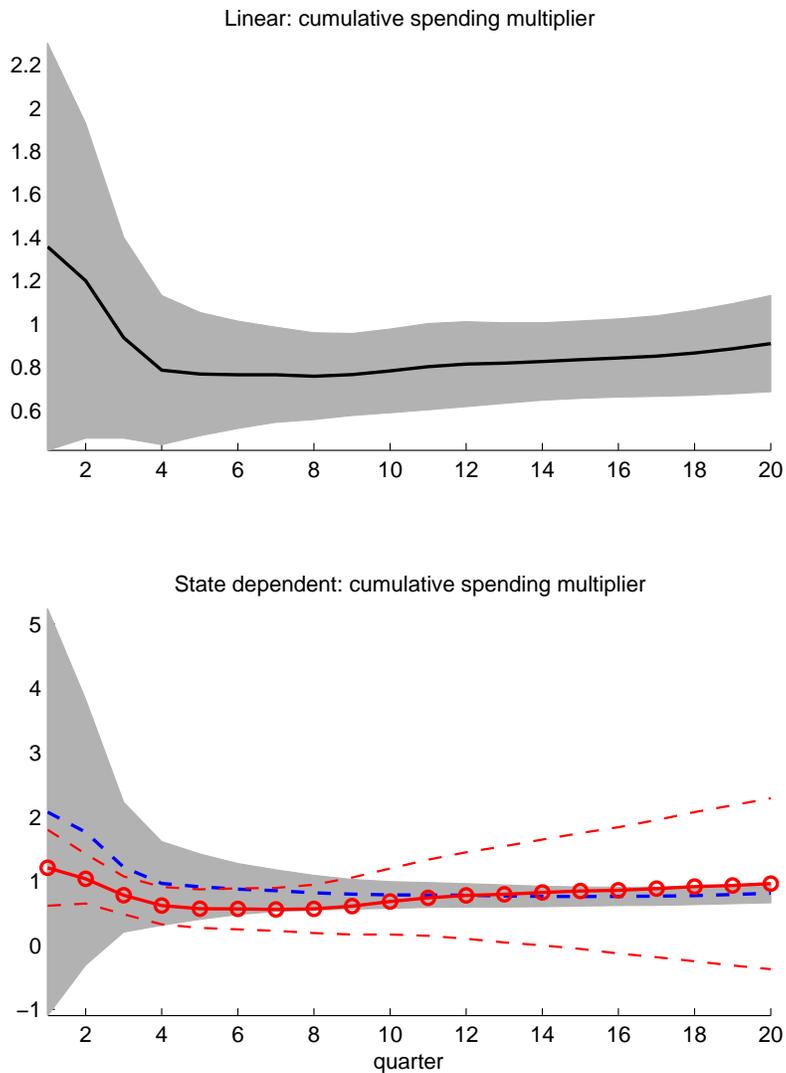
Note: The top panel shows the year-over-year GDP deflator inflation rate and the second panel shows the output gap, which is constructed as the percentage deviation between real GDP and potential GDP. In the last panel, the solid line shows the data for the 3-month T-bill rate, and the dashed line shows the Taylor-rule implied nominal interest rate, where shaded areas indicate periods which we classify as the zero lower bound period for interest rate.

**Figure 14. Government spending and GDP responses to a news shock: Considering zero lower bound**



Note: Response of government spending and GDP to a news shock equal to 1% of GDP. The top panel shows the responses in the linear model. The bottom panel shows the state-dependent responses where the blue dashed lines are responses in the near zero-lower bound state and the lines with red circles are responses in the normal state. 95% confidence intervals are shown in all cases.

**Figure 15. Cumulative multipliers: Considering zero lower bound**



Note: Cumulative spending multipliers across different horizons. The top panel shows the cumulative multipliers in the linear model. The bottom panel shows the state-dependent multipliers where the blue dashed lines are multipliers in the near zero-lower bound state and the lines with red circles are multipliers in the normal state. 95% confidence intervals are shown in all cases.