When is the Fiscal Multiplier High? A Comparison of Four Business Cycle Phases*

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Abstract

This paper compares the effect of fiscal spending on economic activity across various phases of the business cycle. We show that the fiscal multiplier is higher when unemployment is increasing than when it is decreasing. Conversely, fiscal multipliers do not depend on whether the unemployment rate is above or below its long-term trend. This result emerges both in the analysis of long time-series at the U.S. national level as well as for a post-Vietnam War panel of U.S. states. Our findings synthesize previous, at times conflicting, evidence on the state-dependence of fiscal multipliers and imply that fiscal intervention early on in economic downturns is most effective at stabilizing output.

Keywords: Fiscal multipliers; countercyclical policy; cross-sectional analysis; local projections

JEL classification: E62; C31; C32.

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*The views expressed in this paper are those of the authors and do not necessarily reflect those of the Federal Reserve Board.

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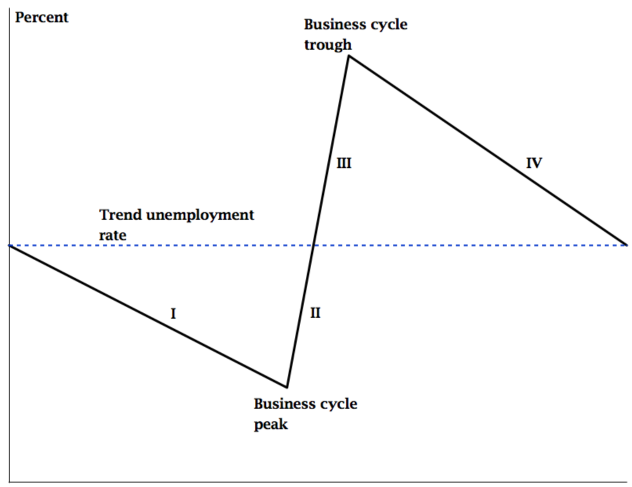
1 Introduction

Fiscal policy aims to stabilize macroeconomic fluctuations. By implementing expansionary policies during downturns, fiscal policymakers hope to reduce variation in economic activity and unemployment. However, the effectiveness of such countercyclical policies remains the subject of debate. In particular, there is a wide range of views on the magnitude and cyclical properties of the 'fiscal multiplier,' the increase in output from a given increase in government spending. If the multiplier exceeds one, an increase in spending stimulates private spending and raises output beyond the government's initial expense whereas a multiplier below one implies that government spending crowds out private spending and is less effective at stimulating activity. Recent research suggests that the size of the multiplier may depend on the state of the economy, exceeding unity during recessions (Auerbach and Gorodnichenko 2012b, Blanchard and Leigh 2013, Nakamura and Steinsson 2014). If so, well-timed fiscal policy can spur economic growth in recessions even if the multiplier is lower than one on average over the business cycle. Recent evidence from an extended historical analysis, however, suggests that multipliers are below one and do not vary over the business cycle (Ramey and Zubairy 2018). That would limit the scope for countercyclical fiscal policy. A reconciliation of these results is needed to allow for future assessments of the benefits of fiscal stimulus.

This paper shows that the effect of fiscal spending on output does depend on the state of the economy. Specifically, we find that fiscal multipliers are higher when the unemployment rate is increasing than when it is decreasing. Our estimated multipliers are also larger than one when unemployment is increasing in almost all specifications. In contrast, we do not find that the fiscal multiplier is higher when the unemployment rate is above its trend compared to when it is below its trend. To obtain these results, we deploy two established approaches from the literature on effectiveness of fiscal spending, and show that existing measures of fiscal multipliers share the prediction that multipliers are higher when unemployment is increasing. We first assess the cyclical properties of fiscal multipliers in the historical data from Ramey and Zubairy (2018), which covers the period 1889–2015. Multipliers are measured as the response of output to changes in fiscal spending that are driven by unexpected news about defense spending. Contrary to Ramey and Zubairy (2018), our measure of government spending is detrended to remove its secular rise over this period, so that our estimates of the multiplier capture the effect of discretionary changes in fiscal spending. We also assess the robustness of our results using the methodology from Nakamura and Steinsson (2014). We use U.S. state-level data from 1976 to 2015 to measure

Figure 1: Stylized behavior of unemployment rate across the business cycle.



Notes: The stylized unemployment rate is purposefully asymmetric across the business cycle, reflecting the fact that unemployment rises much more quickly than it falls. For simplicity we have drawn the trend unemployment rate as time-invariant. Economic booms occur in phases I/II, in phases III/IV the economy is in a slump. The economy is in recession during phases II/III, and in expansion in phases I/IV.

the relative response of state-level output to exogenous changes in relative state-level defense spending. The panel enables the inclusion of time fixed effects to control for, e.g., the state-dependent response of monetary policy.[1] Our results are robust to the use of either method, to a broad series of controls for the state of the economy, as well as to the use of different algorithms to identify the peaks and troughs in the unemployment rate.

Our main innovation is to highlight that fiscal spending has different effects on output across *four* phases of the business cycle. To illustrate, figure 1 depicts a stylized path of the unemployment rate over the business cycle. Initially, the economy is 'running hot,' as the unemployment rate is below its trend rate and falling. After a business cycle peak, the unemployment rate remains below its trend, but economic activity is declining and the unemployment rate rising. The fall in economic activity eventually brings the unemployment rate above its trend. Finally, economic activity is expanding and the unemployment rate is falling, even if it remains above its trend. The figure defines four distinct stages of the business cycle: Phases I and II are a boom since the economy is operating above its trend, whereas phases III and IV are a slump. In contrast, phases I/IV and II/III are the business cycle expansion and recession, respectively.

We show that the simple distinction between boom/slump and expansion/recession can reconcile the conflicting results of past work. We find that the multiplier is higher when the economy is in reces-

[1] For a survey on fiscal multipliers estimated using cross-sectional data see Chodorow-Reich (2019). Auerbach et al. (2020) show that fiscal multipliers are higher in cities with unemployment above 25^{th} percentile.

sion (with a rising unemployment rate) than when the economy is in expansion. That is in line with the manner in which recessions are measured in papers that do find state-dependence in fiscal multipliers (e.g., Blanchard and Leigh 2013, Auerbach and Gorodnichenko 2012b, Nakamura and Steinsson 2014).[2] We do not find different multipliers when the economy is in a slump (with unemployment rates above trend) compared to when it is in a boom, which is the comparison in Ramey and Zubairy (2018). Our results imply that the business cycle decomposition into the four stages in figure 1 can reconcile the conflicting results of past work.

Our results have implications for the optimal response of fiscal policies to economic downturns. Because we find higher multipliers when unemployment is increasing, our results imply that expansionary policies are most effective at stimulating activity early on in recessions. This conclusion contrasts the policy recommendations of state-of-the-art macroeconomic models. Typically, these models produce time-variation in fiscal multipliers by relying on convexity in the aggregate supply curve. In this situation, the fiscal multiplier is larger when the economy is operating below its potential. In Michaillat (2014), for example, the supply curve is convex because it is more costly to hire labor when labor markets are tight. Alternatively, Canzoneri et al. (2016) postulate that financial frictions are smaller when the output gap is small. Barnichon et al. (2020) combine the financial frictions with the downward wage rigidity to explain the asymmetries found in their paper. While these mechanisms are intuitive, they imply that fiscal policies should be expansionary when the output gap is negative or unemployment is high, rather than when unemployment is increasing.

To match our findings that fiscal multipliers are higher when unemployment is increasing, future models could embed loss-aversion utility, where the reference point is consumption in the previous period (external habit), as in Santoro et al. (2014).[3] Thus, the representative agent switches between the "gain" part of the utility function and the "loss" part of the utility function when consumption falls below the external habit, which is around business cycles turning points. This model implies that during recessions the intertemporal elasticity of substitution is higher and the intratemporal marginal rate of substi-

[2]Several other papers study whether the multiplier is higher during recessions. Auerbach and Gorodnichenko (2012a) find evidence of state-dependence using a sample of OECD countries. Other papers that use U.S. data and find evidence of state-dependence in the fiscal multiplier include: Bachmann and Sims (2012), Baum et al. (2012), Shoag (2013), Candelon and Lieb (2013), Fazzari et al. (2015), and Dupor and Guerrero (2017). Overall state-dependence of multipliers is also analyzed in Ilzetzki et al. (2013) and Corsetti et al. (2012). It is worth noting that Ramey and Zubairy (2018) do find evidence that the multiplier is higher when interest rates hit the zero lower bound state, in line with predictions from some macroeconomic models (e.g., Christiano et al., 2011).

[3]Santoro et al. (2014) show that this mechanism generates state-dependent effects of monetary policy shocks over GDP growth cycles, which roughly correspond to increases and decreases in the unemployment rate.

tution between consumption and leisure is higher, causing a convex labor supply function. Loss-averse households increase their labor supply to prevent income losses in recessions, such that expansionary policy does not crowd out private consumption. Furthermore, the model endogenously generates downward wage rigidity during recessions due to higher intratemporal substitutability between leisure and consumption opportunities.[4] Thus, wages do not (fully) adjust downwards in recessions (see Abbritti and Fahr, 2013), causing asymmetries in the effects of demand shifters—like monetary and fiscal policy—over expansions and recessions. Several recent papers put wage rigidities at the center of explaining fiscal multiplier heterogeneities. Theoretical analysis of how fiscal multipliers are impacted by wage rigidities can be found in, for example, Shen and Yang (2018) and Broer et al. (2021), while empirical evidence is in Barnichon et al. (2020) and Jo and Zubairy (2021).

Many other papers have studied the state dependence of fiscal multipliers. The fiscal multiplier is higher when the interest rates are at the zero lower bound (Ramey and Zubairy, 2018), during financial crises (Bernardini et al., 2020), and when consumer debt or household leverage is higher (Klein et al., 2020, Demyanyk et al., 2019). Fiscal multipliers also depend on the way spending is financed (Hagedorn et al., 2019). Furthermore, the multiplier is larger for direct transfers to financially constrained households than for government purchases at the zero lower bound (Giambattista and Pennings, 2017), to name a few other mechanisms explored in the literature. Lastly, Barnichon et al. (2020) present evidence that the sign of the government spending shock is an important determinant of the fiscal multiplier. Negative spending shocks are associated with multipliers of above one, and are largest during periods of economic slack. In contrast, expansionary spending shocks have multipliers below one irrespective of the stage of the business cycle.

Our work is also related to the literature studying regional business cycle differences across U.S. states. Carlino and Defina (1998) examine the differential impact of monetary policy across U.S. states and regions and find that manufacturing regions experience larger reactions to monetary policy shocks than industrially-diverse regions. Furthermore, Blanchard and Katz (1992) study the behavior of wages and employment over regional cycles, and Driscoll (2004) details the effect of bank lending on output across U.S. states. Owyang et al. (2005) and Francis et al. (2018) also use state-level data to evaluate business cycles and countercyclical policy.

[4] For cross-country empirical evidence on downward wage rigidities, see, e.g., Dickens et al. (2007).

The remainder of this paper proceeds as follows. Section 2 describes our strategy to identify the business cycle phases in figure 1, both for national-level data in the United States as well as for state-level data. Section 3 describes the data. Results and robustness checks are presented in section 4, and section 5 concludes.

2 Identifying business cycle phases

This section describes our decomposition of the business cycle into the four phases outlined in figure 1. We do so by identifying peaks and troughs in the unemployment rate at either the state or national level. We use the unemployment rate because it is a highly cyclical measure and because several recent papers indicate that labor market variables meaningfully identify phases of the business cycle.[5] Another advantage of using the unemployment rate is that there are monthly estimates at both the national and state levels, so that we can perform our analysis at different geographical levels using the same methodology.

2.1 Business cycles at the national level

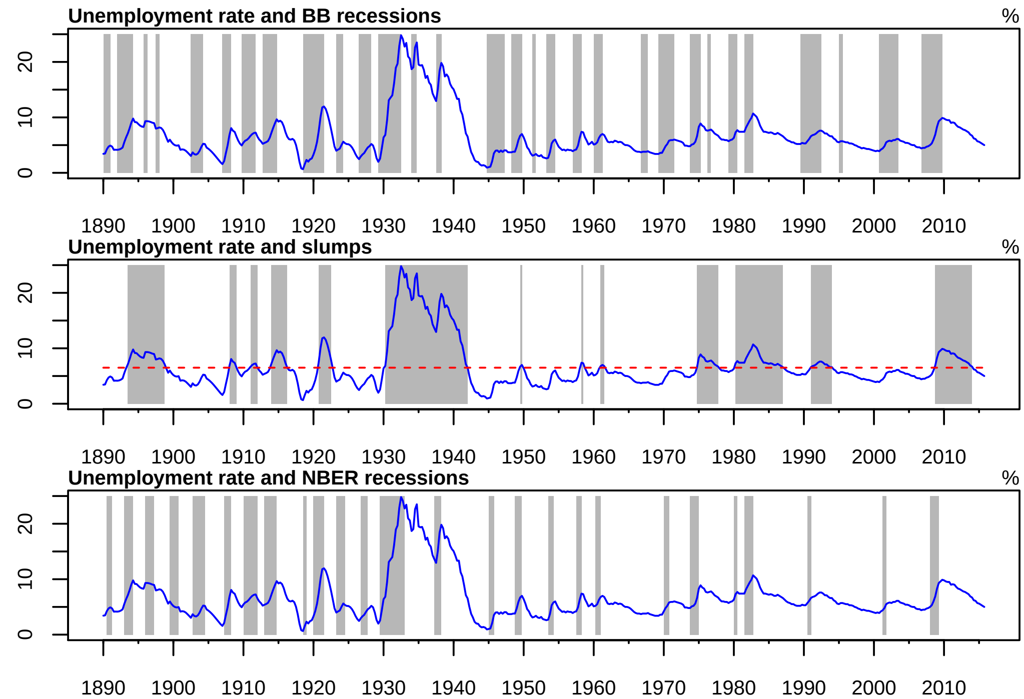
Recessions and expansions are measured by local peaks and troughs in the unemployment rate. To identify these peaks and troughs, we use the Bry and Boschan (1972) algorithm (BB algorithm), which can identify local peaks and troughs in a given series.[6] We add one restriction to an otherwise standard application of the algorithm: we require that the unemployment rate rises at least .5 percentage point during recessions. This restriction is unimportant for the national unemployment rate, but ensures that small movements in state-level unemployment rates—which may be due a relatively large sampling error—are not erroneously identified as turning points in our state-level analysis (Bureau of Labor Statistics, 2017). As a point of comparison, we also perform our analysis using the NBER-defined recession chronology.

To identify slumps and booms, we follow Ramey and Zubairy (2018) and define slumps as periods when the unemployment rate is above 6.5 percent, and booms as periods when the unemployment rate is below 6.5 percent. The three panels of figure 2 plot the unemployment rate and each business cycle chronology: the BB algorithm is shown in the top panel, the second panel shows periods when the unemployment rate is above 6.5 percent, while the final panel shows the NBER recession dates for comparison.

[5] See, e.g., Hamilton and Owyang (2012), Francis et al. (2018), and Berge and Pfajfar (2019).

[6] For details on the implementation of the algorithm, see Bry and Boschan (1972). Harding and Pagan (2002) and Stock and Watson (2014) provide recent applications of the algorithm.

Figure 2: Various business cycle phases in the United States.



Notes: The blue line in each panel is the U.S. unemployment rate. Grey bars indicate the state of the economy as identified by the BB algorithm, the 6.5 percent threshold (red dashed line), or by the NBER business cycle dating committee. See the text for details.

Table 1 presents summary statistics on the number of cycles and the duration of cycles for each of the three chronologies.

NBER-defined recessions have the shortest duration, as the NBER committee looks across many different indicators to identify the peaks and troughs in economic activity. The Bry-Boschan algorithm produces business cycle peaks that roughly coincide with those from the NBER. However, the BB recessions are somewhat longer in duration than those identified by the NBER, especially in the post-Great Moderation period and the so-called 'jobless recoveries.' Relative to the NBER dates, the BB algorithm produces several brief false positives associated with very small upward movements in the unemployment rate (for example, 1934, 1967, 1977, and 1995), as well as one false negative (1900). There is also one period that the NBER has identified as a double-dip that the BB algorithm identifies as one long recession, 1918–1921. However, on the whole, the two recession chronologies are quite similar. This result gives us confidence that the BB algorithm applied to state-level unemployment rates produces meaningful recession chronologies.[7] In contrast, slumps are clearly quite different from the two recession series, since

[7] In our robustness exercises, we impose additional restrictions on the BB algorithm regarding the duration of business cycles which also produces a recession series that very closely mirrors the NBER's chronology, see figure A.1.

Table 1: Summary statistics of U.S. downturns 1890Q1–2015Q4.

	BB recession	Slump	NBER recession
N. obs	29	13	26
Duration (quarters)			
Mean	7.5	13.9	5.6
Median	7.0	10.0	5.0
Std dev	3.5	13.2	2.5
Min	3	2	3
Max	14	48	15

Notes: Table shows summary statistics for three different business cycle downturns: slumps, defined as periods when the unemployment rate is above 6.5 percent; BB-defined recessions; and NBER-defined recession dates. Sample period 1890Q1–2015Q4, duration measured in quarters. See the text for details.

they measure the presence of economic slack and not simply whether the economy is expanding or contracting. The start of slumps roughly coincide with business cycle peaks, but have much longer duration. Indeed, slumps are only weakly correlated with NBER recessions, whereas BB recessions largely coincide with NBER recession dates.

Table 2 summarizes the behavior of the unemployment rate, conditional on each phase. While the unemployment rate is about flat over slumps and booms, it clearly increases during recessions and falls during expansions, whether defined by the BB algorithm or the NBER. It is worth noting that the minimum unemployment rate occurred in 1918Q3, a quarter defined as a business cycle peak by both the BB algorithm and the NBER. It's maximum occurred in 1932, the height of the Great Depression.

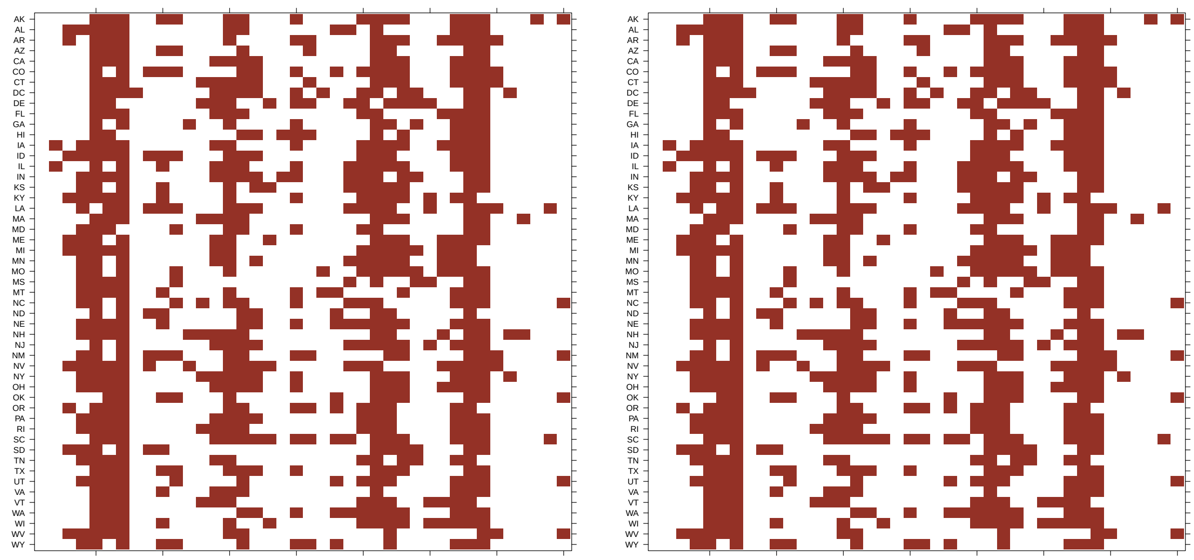
Finally, because the BB algorithm produces several very brief false positive recession events, we compute two alternative BB chronologies. In the first, we impose that the duration of the complete cycle has to be at least 7 quarters. In the second, we impose that complete business cycle has duration of at least 16

Table 2: Summary statistics of U.S. unemployment rate by business cycle phase.

	BB rec.	BB exp.	Slump	Boom	NBER rec.	NBER exp.
N. phases	29	28	13	12	26	25
Behavior of unemployment rate						
Mean change	0.5	-0.3	0.1	0.0	0.6	-0.3
Mean	6.4	6.8	10.3	4.6	7.1	6.4
Std dev	4.0	4.0	4.6	1.2	4.5	3.8
Min	0.6	0.8	6.5	0.6	0.6	0.8
Max	24.8	24.1	24.8	6.4	24.8	23.5

Notes: Table shows summary statistics of the U.S. unemployment rate, in percent, conditional on each business cycle phase. Sample period 1890–2015. See text for details.

Figure 3: State-level recession chronologies from the BB algorithm and state-level slumps using HP filter.



(a) Recessions

(b) Slumps

Notes: Each row denotes a U.S. state, by time. Red shaded area denotes recession as determined by Bry-Boschan algorithm (left panel) or slumps as determined by the HP filter (right panel). See text for details.

quarters. We denote these two alternatives as "prolonged (7)" and "prolonged (16)" cycles. We also provide an alternative measure of slumps and booms by comparing the unemployment rate to its HP filter trend. These chronologies are shown in figure A.1 and summarized in table A.2.

2.2 State-level business cycles

The methodology described in the previous section is also applied to state-level unemployment rate data to define state-level business cycle chronologies.[8,9] However, while we use monthly data to determine the U.S. state-level chronologies our regression analysis requires the use of annual data. We define a given state-year as recession if more than six months in a given year are identified as a recession. We again require that business cycle troughs correspond to a cumulative rise in the unemployment rate of at least 0.5 percentage point from the previous peak. Panel (a) of figure 3 shows Bry-Boschan state-level recession chronologies. In panel (b) we show our measure of slumps. Because we do not wish to impose the same level of the natural rate across states, we define slumps as periods when the state's actual unemployment

[8] Summary statistics are provided in table A.3 and A.4 in the Appendix.

[9] As a robustness check, we have also generated recession chronologies using a state-level coincident index as our measure of economic activity. We find that our chronologies are qualitatively unchanged.

rate is above its HP-filtered trend. Again, we identify a year as a slump if more than 6 months within that year are slumps.

3 Fiscal spending data

We use both historical U.S. national data and U.S. state-level data on fiscal spending to calculate fiscal multipliers. Ramey and Zubairy (2018) collect a long time-series of U.S. quarterly data, from 1889 through 2015. The data includes nominal GDP, the GDP deflator, government purchases, federal government receipts, population, the unemployment rate, interest rates, and news about defense spending. The news shocks are the present value of changes in expected defense spending divided by trend nominal GDP. The creation of the series for news shocks are detailed in Ramey (2011b); we use the extended series from Ramey and Zubairy (2018). The data is shown in figures A.2 and A.3. Details on the underlying sources of this data, as well as the treatment applied to create consistent series is provided in Ramey and Zubairy (2018).

Turning to the state-level data, annual state-level real GDP growth is obtained from the Bureau of Economic Analysis (BEA), and is available over the post-1976 period. We obtain military spending from Nakamura and Steinsson (2014) up to 2007 and extend their data to 2015, thus our sample for the analysis at the state level is 1977–2015. Our data covers prime military procurement, which consists of all contracts valued over $25,000. The Bureau of Labor Statistics (BLS) also provides state-level unemployment rates, which we use to measure business cycle phases.[10]

We are also able to include a large set of control variables in our state-level regressions. To control for state heterogeneity in the labor market, we add controls for labor market dynamism, firm size, union power, and minimum wages. Dynamism is measured through the reallocation rate, defined as the sum of job destruction and job creation rates. Firm size is measured by the average number of employees per firm. We account for differences in state minimum wages with the ratio of minimum to median wages, which are compiled using data from the BLS. Collins (2014) provides data on union power, which is defined by the absence of *right-to-work* laws in a state. The other control variables relate to the structure of the economy. The share of workers employed by the government is included since government expenditures are relatively insensitive to shocks. The share of workers employed in services controls for

[10] See https://www.bls.gov/web/laus/laumstrk.htm. We obtain our data from the FRED database, https://research.stlouisfed.org/pdl/337.

Table 3: Summary statistics for state-level data and controls.

	Mean	SD	Obs.	Min.	Max.	Source
Biannual state GDP growth (%)	5.4	5.2	1,886	-13.8	36.2	BEA
Military spending shocks						
Growth in prime military exp. - state (% of GDP p.c.)	0.00	0.74	1,886	-7.5	8.0	NS
Growth in prime military exp. - national (% of GDP p.c.)	0.01	0.03	37	-0.5	0.7	NS
State control variables						
Labor market dynamism	27.9	4.63	1,886	17.6	53.2	BDS
Firm size	20.2	3.53	1,886	10.8	31.6	BDS
Minimum state wage/ median state wage	0.46	0.07	1,886	0.30	0.74	CPS/BLS
Union power	0.45	0.50	1,886	0.00	1.00	Collins
Share services (% employment)	0.70	0.05	1,733	0.55	0.83	CPS
Share government (% employment)	0.07	0.03	1,733	0.03	0.27	CPS

Notes: This data is yearly, except where stated. BEA is Bureau of Economic Analysis; NS stands for Nakamura and Steinsson (2014); Collins stands for Collins (2014); CPS is Current Population Survey; BLS is Bureau of Labor Statistics; BDS indicates the Business Dynamics Statistics of the Census Bureau.

sectoral composition: certain industries may be more vulnerable to demand fluctuations. The data is summarized in table 3.

4 Revisiting state-dependence of the fiscal multiplier

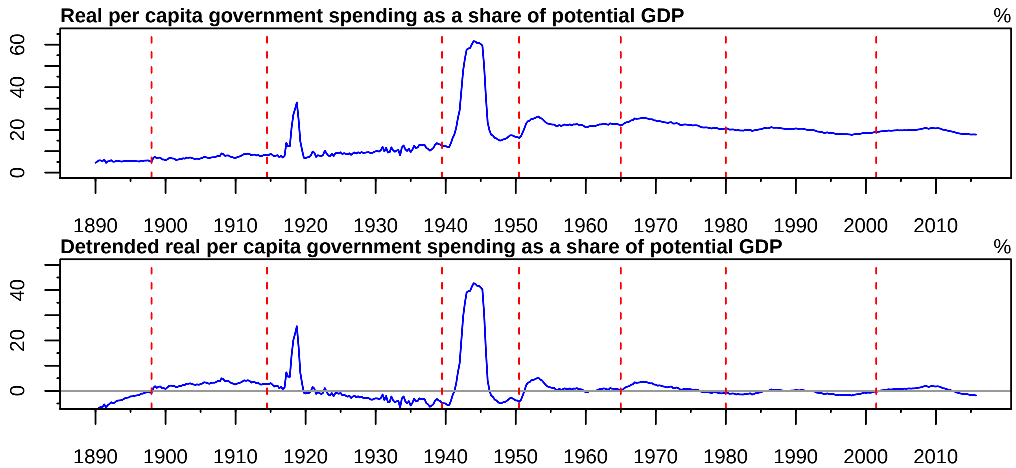
We now estimate the fiscal multiplier across the phases of the business cycle described in section 2. We do so in two different ways. First, we use a local projection-instrumental variable (LP-IV) approach, using the national military news shocks of Ramey (2011b) and Ramey and Zubairy (2018) as an instrument for government expenditures. Secondly, we estimate the fiscal multiplier using a panel data approach at the U.S. state level, following Nakamura and Steinsson (2014). We present each approach in turn.

4.1 Estimating fiscal multipliers with historical time-series

4.1.1 Empirical approach

Let y_{t+h} denote the cumulative change in GDP between t and $t+h$, normalized by potential GDP in the initial period as in Gordon and Krenn (2010); $y_{t+h} = Y_{t+h}/Y_t^p$. Let g_{t+h} represent the cumulative change in detrended government spending: $g_{t+h} = (G_{t+h} - G_{t+h}^p)/Y_t^p$, where G^p is the trend in government expenditures, computed by regressing G on time trends up to the fourth power, where coefficients are estimated by excluding the WWII period. We note that our definition of g is somewhat different from other papers in this literature in that we subtract the trend of government expenditures. We do so because the ratio of government spending to potential GDP has a secular trend, and failing to account for this trend

Figure 4: Real government expenditures before and after controlling for its secular trend.



Notes: Figure shows the raw and the detrended measure of real government expenditures per capita in the United States. Vertical dashed lines denote start of various wars (Spanish-American, WWI, WWII, Korean, Vietnam, response to Soviet invasion of Afghanistan, and Sept 11, 2001). Detrended series are the residuals from a regression including time trends up to the fourth power. See text for details.

in the econometric specification will bias the ultimate estimate of the fiscal multiplier.[11] The multipliers that result from our definition can be thought of as capturing the discretionary part of government spending. Figure 4 illustrates the difference between the original and the detrended series.[12]

Understanding the fiscal multiplier requires that we trace the response of real GDP and real government spending to an identified fiscal policy innovation. If one had an exogenous fiscal policy shock, $shock_t$, then the set of local projections trace out the responses of output and government expenditure:

$$y_{t+h} = \alpha_{y,h} + \beta_{y,h} shock_t + \gamma_{y,h} z_{t-1} + \epsilon_{y,t+h} \tag{1}$$

$$g_{t+h} = \alpha_{g,h} + \beta_{g,h} shock_t + \gamma_{g,h} z_{t-1} + \epsilon_{g,t+h}. \tag{2}$$

The $\beta_{i,h}$ coefficients in equations (1)–(2) give the average response of output or government expenditure in period $t+h$ to a fiscal spending shock in t. z is a set of control variables. The fiscal multiplier m over an h-quarter horizon is:

$$m_h = \sum_{j=1}^{h} \beta_{y,j} / \sum_{j=1}^{h} \beta_{g,j}. \tag{3}$$

Of course, the validity of the approach requires an identified exogenous fiscal spending shock, which is not observed. Thus, in our applications below we will instrument observed government spending with

[11] In the state-level panel analysis we remove time trends through the inclusion of time fixed effects.

[12] We provide a number of robustness check regarding the detrending method in section 4.1.4.

exogenous news shocks for fiscal policy. Further, in our regressions below we follow Ramey and Zubairy (2018) and estimate the fiscal multiplier m via one set of LP-IV regressions instead of two. Specifically, we instrument cumulated government spending at each horizon, $\sum_{j=0}^{h} g_{t+j}$. To estimate the fiscal multiplier conditional on the business cycle phase we further interact government expenditure with a dummy variable indicating the phase of the business cycle, and calculate the multiplier from the set of LP-IV regressions:

$$\sum_{j=0}^{h} y_{t+j} = I_{t-1}\big(\alpha_{1,h} + m_{1,h}\sum_{j=0}^{h} g_{t+j} + \gamma_{1,h} z_{t-1}\big) + \big(1 - I_{t-1}\big)\big(\alpha_{0,h} + m_{0,h}\sum_{j=0}^{h} g_{t+j} + \gamma_{0,h} z_{t-1}\big) + \omega_{t+h}, \qquad (4)$$

where I_{t-1} is the indicator function for the state of the business cycle in period $t-1$. The respective multipliers are then $m_{1,h}$ and $m_{0,h}$.

4.1.2 Results

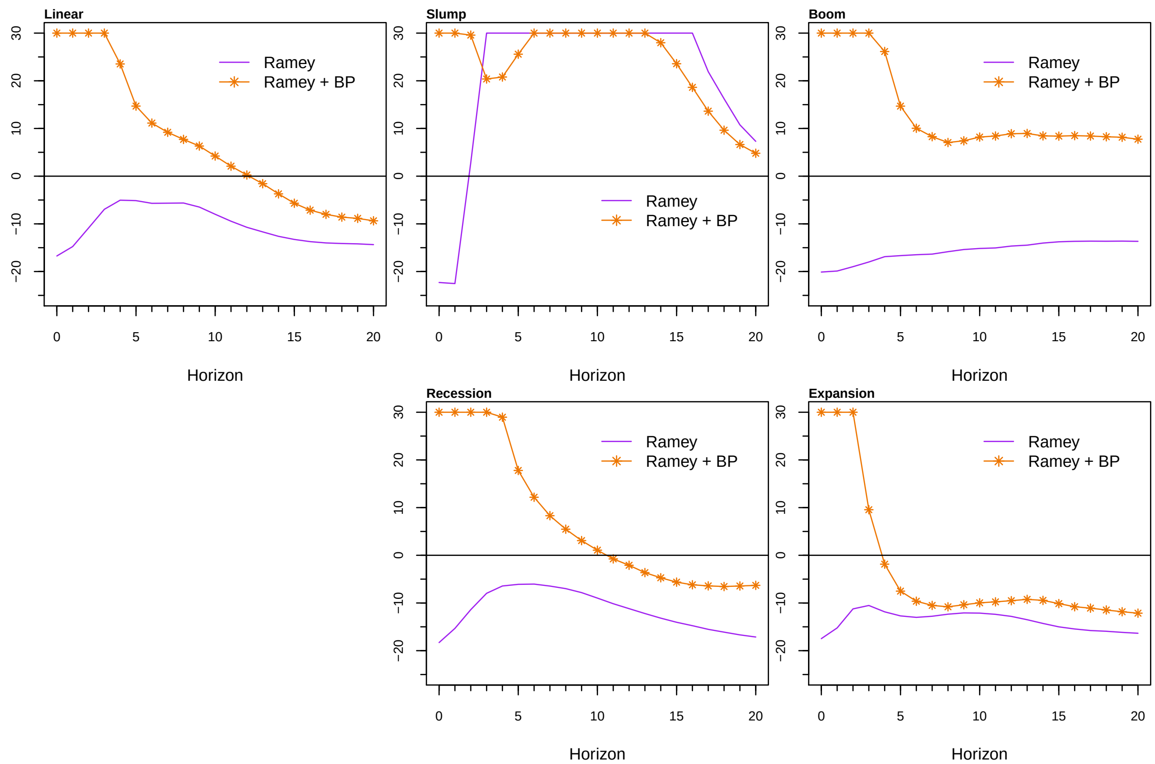
We begin by examining instrument relevance. The first stage of our LP-IV regressions projects cumulated real government spending $\sum_{j=0}^{h} g_{t+j}$ onto the instruments at period t. We consider two sets of instruments: the Ramey fiscal news shocks described in Section 3 (Ramey 2011b), and the Ramey news shocks with the Blanchard and Perotti (2002) shocks.[13] The set of controls for our baseline specification includes four lags of $shock$, y, and g. Figure 5 plots the difference between the first-stage effective F-statistics and the thresholds computed in Montiel Olea and Pflueger (2013).[14] The figure suggests that military news shocks have high relevance only during slumps. In contrast, when we use both shocks to instrument government spending, the first-stage F-statistic tends to be above the relevant threshold, especially within the first two to three years of the shock.

Table 4 presents estimates of the fiscal multiplier. The blocks of the table present different regression specifications, and for each we cumulate the fiscal multiplier over a two- and four-year period. The baseline specification, block one, uses the same controls as Ramey and Zubairy (2018) (but recall that our transformation of government expenditures is different). Specification 2 adjusts the regression for average tax rates and inflation. In specifications 3 and 4 we use both military news shocks and Blanchard

[13]The Blanchard-Perotti shocks are from a Cholesky-identified vector autoregression with government spending, GDP and taxes. Results that use only Blanchard-Perotti shocks as instruments are in appendix Table A.5.

[14]Note that instrument validity will depend on the horizon, h, as we calculate cumulative multipliers, see eq.(4).

Figure 5: Montiel Olea and Pflueger tests of instrument relevance.



Notes: Lines show the difference between the first-stage F-statistic and the 5 percent level threshold from Montiel Olea and Pflueger (2013). Purple line uses Ramey news variable as the instrument; asterisked orange line uses Ramey news and BP shocks. See equation (4) and text for details.

and Perotti (2002) shocks as instruments for fiscal spending, both for the full sample and for the sample excluding WWII.[15]

There is little evidence of state dependence when we compare the fiscal multiplier across slumps and booms when we instrument using only the military spending shock, but there are some signs of state dependence when we use both shocks as instruments. During periods when the unemployment rate is above 6.5 percent the cumulative two-year multiplier in the baseline specification is .76, compared to its estimated value of .57 periods when the unemployment rate is low. The null hypothesis the two estimates are the same cannot be rejected using any standard threshold. Relative to Ramey and Zubairy (2018), our estimated multipliers during slumps and the linear multipliers are a touch higher due to our slightly different transformation of government spending. When we add additional controls for taxes and inflation, the estimated multiplier increases, especially during slumps, but remains statistically indistin-

[15]In Appendix A1 we report results using threshold VARs. Fiscal multipliers estimated using TVARs suggest little difference across the business cycle phases at the two year integral, although there is some evidence of asymmetry at a four year horizon.

Table 4: Historical time-series estimates of fiscal multipliers.

	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Military spending shock (baseline specification)					
2 year integral	0.72	0.76	0.57	1.60	0.64†
	(0.09)	(0.11)	(0.10)	(0.42)	(0.11)
4 year integral	0.78	0.76	0.63	1.93	0.74†
	(0.06)	(0.05)	(0.10)	(0.57)	(0.08)
2. Military spending shock, taxes and inflation as additional controls					
2 year integral	0.74	0.86	0.63	1.28	0.67
	(0.09)	(0.17)	(0.10)	(0.33)	(0.09)
4 year integral	0.79	0.82	0.66	1.48	0.78
	(0.07)	(0.08)	(0.12)	(0.46)	(0.06)
3. Military spending shock + BP shocks					
2 year integral	0.50	0.83	0.42†	0.88	0.54
	(0.08)	(0.18)	(0.08)	(0.28)	(0.11)
4 year integral	0.71	0.75	0.56†	1.37	0.69†
	(0.06)	(0.05)	(0.08)	(0.41)	(0.09)
4. Military spending shock + BP shocks, excluding WWII					
2 year integral	0.47	1.94	0.33†	0.57	0.42
	(0.16)	(0.83)	(0.13)	(0.37)	(0.26)
4 year integral	0.77	1.67	0.59	1.20	0.59
	(0.35)	(0.71)	(0.31)	(0.48)	(0.52)

Notes: Table presents estimates using equation (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). The baseline set of controls, z_{t-1}, includes four lags of $shock$, y, and g. Specification 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

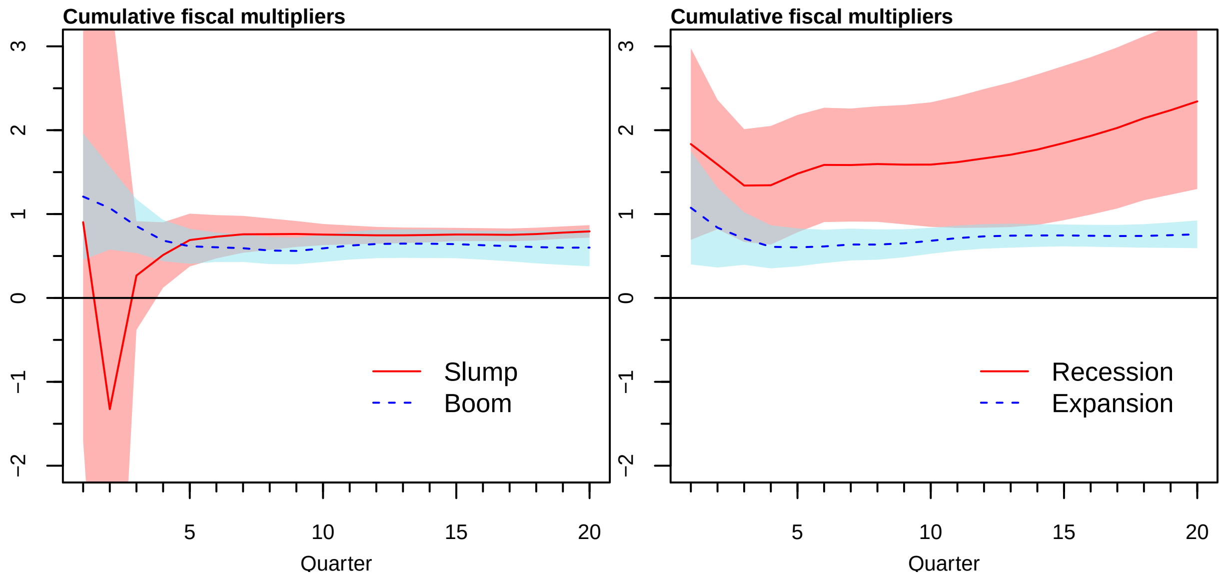
guishable from the boom-time multiplier. Finally, the estimates of the fiscal multiplier are below one; the only specification where slump-specific multiplier is larger than one occurs when we exclude WWII.

We do see some differences in the multiplier across recessions and expansions. In the baseline specification, the two-year cumulative multiplier is 1.6 in recessions but .6 during expansions, a statistically relevant difference at the 5 percent level. The difference between the estimated multiplier in a recession versus an expansion is typically not statistically different in the other specifications, although the estimated multiplier is always higher in recessions. Indeed, the standard errors of the recession multipliers are larger than those from the slump/boom chronologies, reflecting a relative paucity of data.

Figure 6 shows the full local projection multipliers for the baseline specification. The left panel compares booms and slumps, while the right panel shows recessions versus expansions. This figure shows a clear state-dependence in the multiplier when comparing recessions to expansions: the multiplier is always higher when a shock occurs during recession, and this difference is significant at several horizons. In contrast, the multiplier is very similar across booms and slumps

To further clarify these fiscal multipliers, figure 7 shows the impulse responses of real government spending and GDP to a news shock equivalent to one percent of GDP and under the baseline estima-

Figure 6: Cumulative fiscal multipliers.

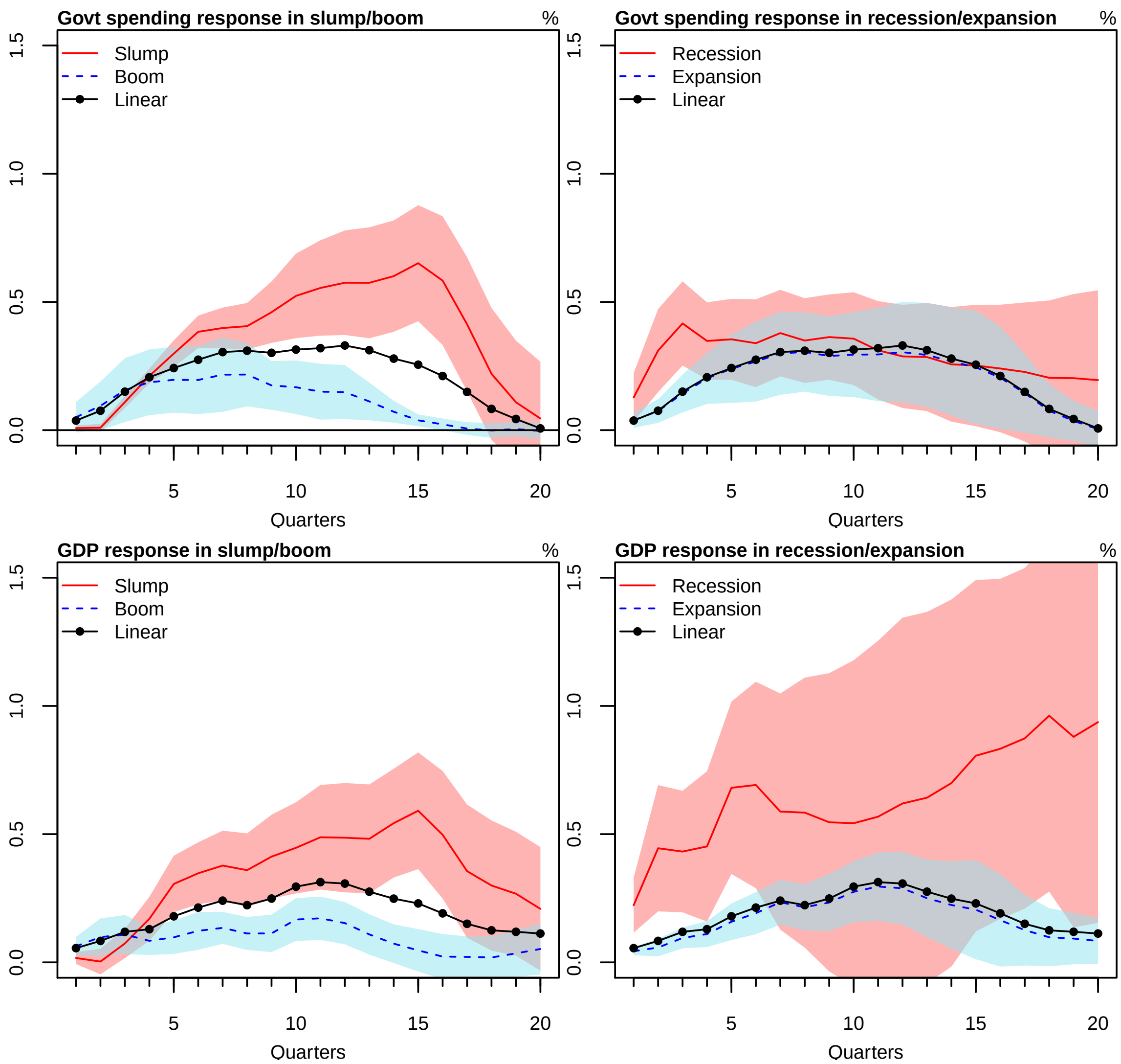


Notes: Figures show cumulative fiscal multipliers conditional on business cycle phase. Results are from the Baseline specification of table 4. Blue line represents multipliers in booms/expansions, while red line shows estimated multiplier in slumps/recession. Shaded areas denote 90 percent confidence intervals. See text for details.

tion. The top row shows the estimated response of government expenditure to the news shock, and the response of output is in the bottom panels. The two left panels compare booms and slumps, while the right panels show the results comparing expansions and recessions. The same linear multiplier is added to each graph as a reference.

The figures reveal large differences in the response of government expenditure to a military spending news shock. During slumps, the response of government expenditure to a news shock is delayed—actual government expenditure peaks four years after the shock. Further, the standard errors in the first two years after the news shock are quite narrow. These results run counter to the case studies in Ramey (2011a) and Ramey and Zubairy (2018), which point to significant heterogeneity in the response of g. In contrast, during recessions the peak in spending happens after just three quarters. When Ramey (2011a) studies the timing of shocks in detail, she argues that it takes a few quarters after the military spending news before the military spending actually materializes, although Ramey and Zubairy (2018) present case studies where the response is further delayed, between one and two years. The three quarter peak we find during recessions is consistent with the event study for both the the Korean and the Vietnam wars. During the First and Second World Wars, government spending increased immediately following the news shocks, and peaked six to eight quarters after. In addition, during recessions government spending re-

Figure 7: Phase-specific response of government spending and GDP to news shocks: historical time-series.



Notes: Panels show state-specific response of government expenditure (top row) and GDP (bottom row) to a military expenditure news shock scaled to one percent of GDP. Regression is the baseline specification in table 4. Red lines show the response in slumps (left) or recessions (right). Blue line is the response in booms (left) or expansions (right). Black lines are the response from the linear model. Shaded areas denote 90 percent confidence intervals.

mains at the elevated level for several years. This is in line with the case studies of several wars mentioned above. All told, while there is substantial heterogeneity in the response of government spending after the news, we believe that the response during recessions is more consistent with the event studies mentioned above than the response shown for slumps.

Putting the responses of government expenditure and output together, one can reconcile the multipliers from figure 6 by mentally applying equation 3. In recessions, the response of government expenditure is front-loaded and peaks at a smaller level than the response during slumps. At the same time, the response of output is cumulatively larger in recessions than in expansions, especially in the first two years after the shock. Because there are few news shocks during our identified recessions, the responses of both government expenditure and output are very uncertain. In contrast, as we can see by the response of government expenditure during slumps, the bulk of government expenditure is quite delayed from the news shock itself. Given that the average recession in our sample lasts just over 1.5 years, it is unlikely that government expenditure actually occurs during periods of severe economic distress. The response of output itself is also ultimately smaller. Overall, we view these results as supporting the idea that fiscal multipliers are larger during periods of economic distress, but emphasize that the period of time in which the multiplier is relatively large may be quite short.

4.1.3 Pairwise versus four-phase multiplier estimates

Thus far, we have presented pairwise comparisons of business cycle phases; i.e., we compared booms to slumps, or expansions to recessions. Table 5 presents the results for each of the four phases of the business cycle we identified in figure 1. Most specifications suggest that multipliers are higher in phases II and IV of the business cycle—although the difference is not always statistically meaningful—while they tend to be smaller in phase I. Phase III, where the unemployment rate is rising and above its trend, is very imprecisely estimated. In Appendix figure A.4 we plot the news shock in each phase of the business cycle. The figure shows that there are only six shocks in phase III, which are also small in magnitude. Furthermore, the identification in other phases tends to rely on a few larger shocks, which are mostly positive in phase II and mostly negative in phase IV (see Barnichon et al., 2020). Because the national-level data contains insufficient government spending shocks in each phase, our preferred results are the pairwise comparisons rather than separate estimates for each phase.[16]

4.1.4 Robustness

We next perform a series of checks to assess the robustness of the aforementioned results. We first test whether the results are robust to the use of the alternative business cycle chronologies detailed in figure A.1. We test one alternative chronology for the boom/slump series, where we define booms and busts by comparing the unemployment rate to its HP trend. The multipliers associated with this chronology are

[16] There are sufficient government spending shocks in all phases at the state level. Hence, we focus on phase-by-phase analysis in Section 4.2, where we present the state-level results.

Table 5: Historical time-series estimates of fiscal multipliers: 4 stages of the business cycle.

	Linear	(1)		(2)	
	All	Phase I	Other phases	Phase II	Other phases
1. Military spending shock (baseline specification)					
2 year integral	0.72	0.51	0.84	1.02	0.63
	(0.09)	(0.11)	(0.14)	(0.33)	(0.10))
4 year integral	0.78	0.57	0.83	1.58	0.73
	(0.06)	(0.12)	(0.10)	(0.55)	(0.06)
2. Military spending shock, taxes and spending as additional controls					
2 year integral	0.74	0.53	0.89†	1.00	0.65
	(0.09)	(0.07)	(0.14)	(0.35)	(0.09)
4 year integral	0.79	0.66	0.86	1.43	0.76
	(0.07)	(0.11)	(0.09)	(0.52)	(0.06)
3. Military spending shock + BP shocks					
2 year integral	0.50	0.12	0.79	0.63	0.56
	(0.08)	(0.04)	(0.12)	(0.25)	(0.09)
4 year integral	0.71	0.16	0.85	1.23	0.71
	(0.06)	(0.10)	(0.12)	(0.34)	(0.07)
4. Military spending shock + BP shocks, excluding WWII					
2 year integral	0.47	0.30	0.88	0.31	0.58
	(0.16)	(0.18)	(0.38)	(0.37)	(0.29)
4 year integral	0.77	0.28	1.43†	1.06	0.83
	(0.35)	(0.41)	(0.46)	(0.35)	(0.41)

	Linear	(3)		(4)	
	All	Phase III	Other phases	Phase IV	Other phases
1. Military spending shock (baseline specification)					
2 year integral	0.72	-0.24	0.69	0.86	0.76
	(0.09)	(1.45)	(0.09)	(0.09)	(0.16)
4 year integral	0.78	0.49	0.76	0.81	0.87
	(0.06)	(2.27)	(0.07)	(0.03)	(0.17)
2. Military spending shock, taxes and spending as additional controls					
2 year integral	0.74	-1.37	0.75	1.20	0.79†
	(0.09)	(1.99)	(0.09)	(0.17)	(0.19)
4 year integral	0.79	-1.32	0.79	0.94	0.86
	(0.07)	(3.51)	(0.07)	(0.04)	(0.18)
3. Military spending shock + BP shocks					
2 year integral	0.50	1.60	0.49	0.91	0.41†
	(0.08)	(1.43)	(0.10)	(0.14)	(0.08)
4 year integral	0.71	1.82	0.69	0.76	0.60
	(0.06)	(1.24)	(0.06)	(0.05)	(0.10)
4. Military spending shock + BP shocks, excluding WWII					
2 year integral	0.47	1.60	0.36	2.76	0.43†
	(0.16)	(1.43)	(0.14)	(1.01)	(0.16)
4 year integral	0.77	1.19	0.61	1.46	0.76
	(0.35)	(2.03)	(0.30)	(0.43)	(0.31)

Notes: Table presents estimates using equation (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). The baseline set of controls, z_{t-1}, includes four lags of $shock$, y, and g. Specification 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

presented in the first two columns of Appendix table A.6. The results are qualitatively similar to those for slumps and booms based on a fixed threshold of 6.5 percent. The multiplier is now always estimated to be higher in slumps than in booms, but as before, the difference is small and not statistically meaningful.

Next, we evaluate three alternative definitions of recession/expansion: the NBER chronology, and the BB-algorithm chronologies with a minimum cycle duration of 7 quarters ("prolonged 7") and 16 quarters ("prolonged 16"). The resulting estimates are in the remaining columns of table A.6. Our results are on the whole robust to the alternative recession/expansion chronologies. For each chronology and regression specification, we find that the multiplier is higher in recession than in expansion, although the difference is not always statistically relevant. The estimated multiplier in expansions is typically around 0.5, while in recessions the estimated multiplier often, but not always, exceeds one. Since the NBER business cycle chronology is quite similar to the chronology based on the Bry-Boschan algorithm, it is not surprising that the results using the NBER's chronology are by and large similar to those already presented. The results of the two prolonged BB chronologies are also similar to the original results, and if anything, show more pronounced differences between expansions and recessions.

We also assess the effect of alternative methods of detrending government expenditure. Recall that we calculate fiscal multipliers from government spending variable g_{t+h}, defined along:

$$g_{t+h} = (G_{t+h} - G^p_{t+h})/Y^p_t,$$

where G^p_{t+h} is the trend government expenditure level, computed by taking the fitted values from a regression of government expenditure on a fourth-degree polynomial of time, where we exclude the World War II period from the estimation. To assure that our results are robust to the detrending methodology, we have alternatively detrended the government spending series using the Hodrick-Prescott (HP) filter as well as the Christiano and Fitzgerald (2003) (CF) band pass filter. Estimates of the fiscal multiplier under these different definitions of government expenditures are presented in Appendix tables A.7 and A.8, and are qualitatively similar to the main results in table 4. Fiscal multipliers are higher in recessions than in expansions, but evidence that multipliers are higher in slumps than in booms remains scant. Point estimates for the linear multipliers, which measure the average effect of fiscal spending over the cycle, are typically within a decimal of the estimates in table 4, further indicating that our results are robust to the differently detrended measure of government expenditure.

We next assess whether our results are robust to the exclusion of various historical periods. A serious limitation of the preceding analysis is its dependence on shocks around large wars. Hall (2009) has noted that any estimate of fiscal multipliers from historical data on military spending "comes under suspicion for understating the multiplier because the bulk of evidence comes from the command economy of World War II." Our previous estimates have shown that our results are reasonably robust to the exclusion of World War II, but our results still rely heavily on shocks around significant military episodes. Indeed, Appendix figure A.4 shows that the largest spending shocks occur around World Wars I and II. To highlight the importance of these large events for our results, we re-estimate our multipliers on a rolling window of observations. We use 50-year windows to have a sufficient number of observations given the large set of control variables and relatively few shocks, all of which are interacted with the economy's state variables. Results are presented in Appendix figure A.5. To summarize, in line with Hall (2009), we find that results strongly depend on the inclusion of war episodes. In particular, we estimate fairly stable multipliers for slumps and expansions as long as either World War I or World War II is included. Estimates for expansions and recessions are stable provided that the sample includes World War II, in line with the fact that shocks during recessions occur principally in those years (figure A.4).

The sensitivity of our results to war sampling does not prevent our paper from reconciling evidence on state-dependence in the literature that relies on historical data. It does, however, limit the degree of inference about modern-day state dependence of multipliers. We will show, however, that the results at the national-level from historical data are mirrored strongly by results at the level of U.S. states in modern data, and that state-level results *are* robust to the analysis of subsamples. We present this analysis in the next section.

4.2 State-level analysis using military spending shocks

We next show that we also find evidence that the fiscal multiplier varies across the business cycle when we follow the approach of Nakamura and Steinsson (2014). Nakamura and Steinsson identify exogenous variation in state-level fiscal policy by assuming that the federal government does not alter national spending in response to the relative performance of the U.S. states.[17] This approach has the advantage that it introduces a panel element to the data, which may improve the precision of the estimates of the fiscal multiplier. By allowing for the inclusion of time-fixed effects, the panel structure furthermore allows us to control for potentially state-dependent responses of monetary policy. Since we produce business

[17] Compared to the analysis in Nakamura and Steinsson (2014), we start our sample later; we exclude the Korean war, as advocated by Dupor and Guerrero (2017), but extend Nakamura and Steinsson (2014) military spending data to 2015.

cycle chronologies at the state level, we can now more reliably test whether the multiplier differs across the four business cycle phases.

Nakamura and Steinsson (2014) estimate a two-stage instrumental variables regression. In the first stage, the change in military spending at the state level is regressed onto the change in national military spending and controls:

$$\Delta\mu_{s,t} = \beta_s \Delta\mu_{nat,t} + I_{s,t-1}\big(\alpha_{1,s} + \xi_{1,s}(L)z_t\big) + \big(1 - I_{s,t-1}\big)\big(\alpha_{0,s} + \xi_{0,s}(L)z_t\big) + \Phi_s' c_{s,t} + \epsilon_{s,t}, \tag{5}$$

where μ_s and μ_{nat} are biannual changes in state and federal military expenditure as a percentage of GDP, z is a vector of controls, and c are fixed effects. I_{st} is the dummy variable that indicates the state of the business cycle in state s at period t. The second stage regression regresses the fitted values from the first stage onto state-level GDP:

$$\Delta y_{s,t} = I_{s,t-1}\big(\alpha_{0,s} + \psi_{0,s}(L)z_t + \gamma_0 \Delta\widehat{\mu}_{s,t}\big) + \big(1 - I_{s,t-1}\big)\big(\alpha_{1,s} + \psi_{1,s}(L)z_t + \gamma_1 \Delta\widehat{\mu}_{s,t}\big) + \phi_s' c_{s,t} + \eta_{s,t}, \tag{6}$$

where Δy measures biannual growth in state GDP while $\widehat{\mu}$ denotes the fitted value of equation 5.[18] The parameters γ_0 and γ_1 capture the phase-dependent multipliers. It is worth emphasizing that these equations estimate an open economy relative multiplier for federal spending, which quantifies increases in state GDP relative to others after increases in military expenditure. Thus, caution should be used when comparing these multipliers to those calculated in section 4.1.[19]

4.2.1 Results

Table 6 present estimates of the open-economy multiplier. The first line of the table presents regression estimates that include only time fixed effects. Again, each subsequent row presents alternate specifications. Lines 2-5 in table 6 show that the results are broadly robust to various regression specifications. The regressions in row 2 control for the level of military expenditure as a percent of state GDP, since cyclically sensitive industries are likely particularly sensitive to defense spending. Line 3 adjusts the regressions

[18] By including constants $\alpha_{0,s}$ and $\alpha_{1,s}$ we control for endogeneity in the second stage. Omitting the constants would bias our multipliers if there is serial correlation in economic growth, because the fitted values of military expenditure are (by design) correlated with the lagged state of the economy.

[19] In the Nakamura and Steinsson (2014) dataset, the dates that military contracts were awarded are available but the exact timing of the actual expenditure is not known. Because the exact timing of the fiscal spending shocks is unclear, we are not able to calculate local projections. Instead, we calculate the multipliers at the horizon of two years, similar to our analysis using national data. This specification is consistent as long as the majority of allocated funds is spent within two years of assignment.

Table 6: Open-economy fiscal multipliers by business cycle phase: Evidence from U.S. states.

	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Baseline (year fixed effects only)					
Two year integral	2.01	2.07	1.94	2.68	1.31
	(0.83)	(0.76)	(0.97)	(0.91)	(0.93)
2. Year fixed effects; size of military					
Two year integral	0.29	0.31	0.29	1.20	-0.59†
	(0.80)	(0.71)	(1.00)	(0.88)	(1.02)
3. Year and state fixed effects					
Two year integral	3.05	2.13	1.92	2.79	0.84†
	(1.51)	(0.86)	(1.05)	(0.91)	(1.05)
4. Year and state fixed effects; size of military; labor market/industry					
Two year integral	1.58	1.56	1.35	2.20	0.68
	(0.73)	(0.64)	(1.31)	(0.84)	(0.94)
5. Year and state fixed effects; size of military; labor market/industry; lagged dep. var.					
Two year integral	1.55	1.64	0.99	2.13	0.80
	(0.73)	(0.54)	(1.00)	(0.68)	(0.85)

Notes: Table presents estimates using eq. (6). Standard errors clustered by state and time. Number of observations varies from 1,682 to 1,886.
† indicates that the difference across phases is statistically significant at 10 percent level.

with state fixed effects. In line 4, we add controls for state labor market institutions and the sectoral composition. Finally, the last specification adjusts for a lagged dependent variable.

The linear regression estimates a fiscal multiplier between 0.3 and 3.0. These values imply that a 1 percent increase of relative military spending as a percentage of state GDP increases its GDP relative to other states by 0.3–3 percent within two years of the increase in spending. Turning to the phase-dependent estimates, we find little evidence that the open-economy fiscal multiplier differs across slumps and booms. In contrast, we find some evidence that the multiplier varies depending on whether the state is in recession or expansion. The point estimate of the fiscal multiplier in recession is notably higher, ranging from 1.2 to 2.8, whereas in expansions, the multiplier is often below one. However, the standard errors of these estimates tend to be large, such that we usually cannot reject the null hypothesis that the multiplier is the same in the two phases of the business cycle.

Table 7 reports results for each individual business cycle phase described in figure 1. We find that point estimates of the fiscal multiplier in phase II and III of the cycle—periods when the unemployment rate is increasing—are always higher than the other phases, although the differences are not always statistically meaningful. The table also shows why the multipliers are not different between slumps and booms, since each of these periods is comprised of periods in time with increasing or decreasing unemployment rate, and therefore high and low multipliers.

Table 7: Estimated open economy fiscal multipliers by phase of business cycle: Evidence from U.S. states.

	Linear	(1)		(2)	
	All	Stage I	Other stages	Stage II	Other stages
1. Baseline (year fixed effects only)					
Two year integral	2.01	1.64	2.10	3.43	1.71
	(0.83)	(1.16)	(0.84)	(1.23)	(0.81)
2. Year fixed effects; size of military					
Two year integral	0.29	-0.56	0.49	1.78	-0.05†
	(0.80)	(1.35)	(0.75)	(1.09)	(0.81)
3. Year and state fixed effects					
Two year integral	3.05	1.41	2.16	3.64	1.54†
	(1.51)	(1.16)	(0.91)	(1.35)	(0.92)
4. Year and state fixed effects; size of military; labor market/industry					
Two year integral	1.58	0.70	1.73	2.76	1.35
	(0.73)	(1.36)	(0.75)	(1.76)	(0.71)
5. Year and state fixed effects; size of military; labor market/industry; lagged dep. var.					
Two year integral	1.55	0.65	1.73	1.99	1.44
	(0.73)	(1.20)	(0.57)	(1.39)	(0.58)
	Linear	**(3)**		**(4)**	
	All	**Stage III**	**Other stages**	**Stage IV**	**Other stages**
1. Baseline (year fixed effects only)					
Two year integral	2.01	2.93	1.57	2.17	2.00
	(0.83)	(0.84)	(0.89)	(0.85)	(0.78)
2. Year fixed effects; size of military					
Two year integral	0.29	1.25	-0.09	0.01	0.46
	(0.80)	(0.98)	(0.88)	(0.79)	(0.87)
3. Year and state fixed effects; size of military					
Two year integral	3.05	3.04	1.44†	2.11	2.02
	(1.51)	(0.79)	(1.02)	(1.09)	(0.94)
4. Year and state fixed effects; size of military; labor market/industry					
Two year integral	1.58	1.95	1.16	1.51	1.48
	(0.73)	(0.64)	(0.95)	(0.84)	(0.88)
5. Year and state fixed effects; size of military; labor market/industry; lagged dep. var.					
Two year integral	1.55	1.94	1.11	1.45	1.48
	(0.73)	(0.54)	(0.77)	(0.73)	(0.67)

Notes: Table presents estimates using eq. (6). Table reports estimates from the 2SLS estimator in equations 5 and 6. Phases correspond to those labeled in figure 1. Numbers in parentheses are standard errors, clustered by time and state. Number of observations varies from 1,682 to 1,886.
† indicates that the difference across phases is statistically significant at 10 percent level.

4.2.2 Robustness checks

As before, we check for the robustness using a different definition of recessions. We do not present results for an alternative slump/boom chronology, since we use the HP filter to define our baseline slumps and booms. Results using the alternative recession chronologies—the NBER dates and the two prolonged Bry-Boschan chronologies—are reported in table A.9. For the national-level NBER chronology, we find that multipliers are *smaller* in recessions for some of the regression specifications. This is at odds with the results using the national data (table A.6), where NBER-recessions had higher multipliers. This result suggests that state-level multipliers are only higher when the local economy is in recession, not the national economy. In contrast, alternative BB algorithms again show evidence that multipliers differ across recessions and expansions. Indeed, the difference between recessions and expansions is often more pronounced under these alternative chronologies and more often statistically significant at standard levels.

As a second robustness check we repeat the analysis of time sub-samples at the state-level. Given that our full sample lasts from 1977 to 2015, we use 25-year windows in the sub-sample analysis. Results are presented in Appendix figure A.6. The figure shows that state-level results are significantly more robust to the analysis of sub-samples than the national level results. For slumps and booms we consistently find similar multipliers. Multipliers in recessions are consistently higher than multipliers in expansions, although the magnitude of the difference becomes smaller from end-year 2014 onwards.

As a third robustness check, we estimate the state-dependent effect of fiscal spending on employment as an alternative dependent variable. For the historical analysis this is not possible because there is no high-quality data available for earlier years. We repeat the main analysis at the state level using the two-year percentage change in the employment rate as the dependent variable. Appendix table A.10 presents the results, which strongly supports our previous results. On average, a one percentage point increase spending relative to GDP in a state leads to an increase in that state's employment rate of roughly .35 percentage point after two years. As before, we find no notable difference between the multipliers in slumps and booms. In contrast, we find that the effect of fiscal spending on employment rates is both economically and statistically significantly greater in recessions than in expansions. For example, the estimated multipliers in the preferred specification with time and state fixed effects (row 3) imply that the effect of fiscal spending during expansions on employment is zero, while a one percentage point increase in spending over GDP raises the employment rate by 0.8 percentage points during recessions.[20]

5 Conclusion

This paper revisits the question of whether fiscal multipliers are larger in recessions than in expansions. By separating the business cycle into four phases, we are able to reconcile conflicting results in previous work. We view the bulk of the evidence presented as supporting the idea that the fiscal spending multiplier is larger in recessions than expansions. In contrast, there is scant evidence that the multiplier varies

[20] To the extent that labor force participation is also affected by increased demand, these estimates may understate the impact of this spending.

when the unemployment rate is above or below its trend. We interpret these results as a synthesis of the often conflicting results found in the literature.

Our results imply that policies that aim to reduce the volatility of economic activity and unemployment are most effective in recessions characterized by increasing unemployment. Even when unemployment is low and the output gap is positive, fiscal policy has the ability to cause a disproportionate increase in output. When unemployment is falling, however, fiscal policy is less effective. Multipliers are below 1, even when the level of unemployment remains high. Our results have implications for the optimal timing of spending, as policymakers should base their decisions about expansionary policies on the direction of change rather than on the level of the unemployment rate. This implies that earlier interventions, at the onset of recessions, are potentially more effective than those when the unemployment rate has already started to recover.

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A Online appendix

A.1 Estimates from a threshold VAR

We also employ a threshold VAR approach, as in Auerbach and Gorodnichenko (2012b) and section 6 of Ramey and Zubairy (2018). We write the threshold VAR in reduced-form:

$$Y_t = I_{t-1}\Psi_1(L)Y_{t-1} + \left(1 - I_{t-1}\right)\Psi_0(L)Y_{t-1} + u_t, \tag{7}$$

where I indicates the phase of the economy, $\Psi(L)$ is a lag polynomial of VAR coefficients, $u_t \sim N(0, \Omega)$, and $\Omega = I_{t-1}\Omega_1 + \left(1 - I_{t-1}\right)\Omega_0$.

Military news shocks are identified using a Choleski decomposition with the following ordering $Y = [news_t, g_t, y_t]$. Our measures of government spending, g_t, and output, y_t, are as in the main text.

Table A.1 presents the results. Each panel gives the estimated multiplier using a particular estimated business cycle chronology. The top row gives our baseline results, using the 6.5 percent threshold and the BB algorithm, respectively. The middle and bottom rows present results using the alternative chronologies.

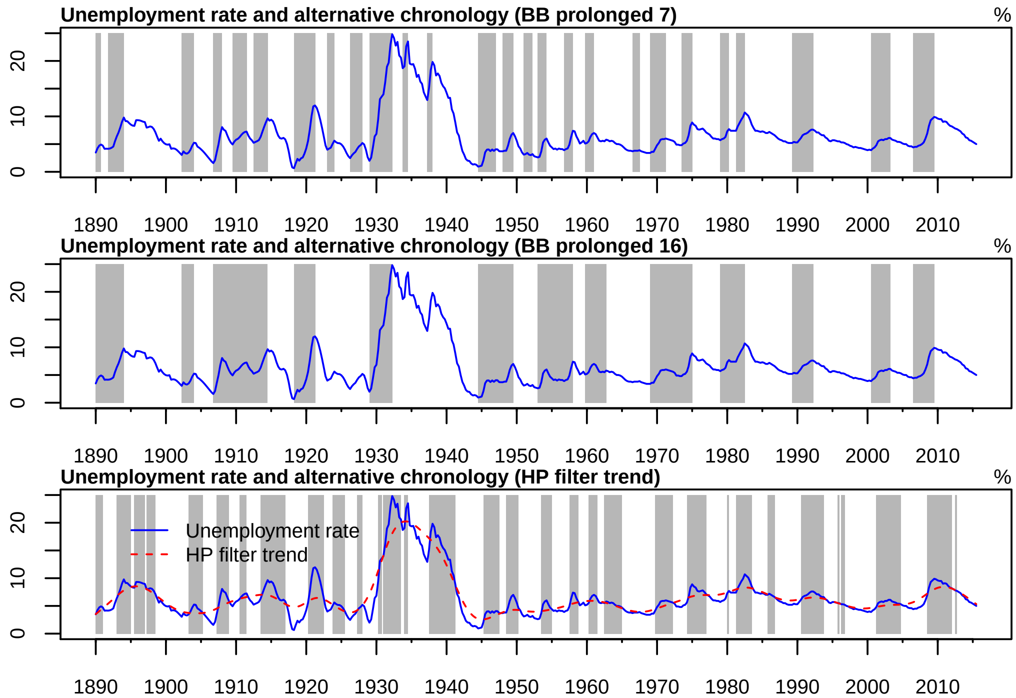
Table A.1: Historical time-series estimates of fiscal multipliers: Evidence from a TVAR

	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
2 year integral	0.66	0.81	0.55	1.04	0.60
4 year integral	0.79	1.68	0.60	1.35	0.63
	Linear	NBER Business Cycle		BB prolonged 16	
	All	Recession	Expansion	Recession	Expansion
2 year integral	0.66	1.26	0.55	1.96	0.61
4 year integral	0.79	1.51	0.65	2.39	0.64
	Linear	Above/Below Trend(HP filter)		BB prolonged 7	
	All	Recession	Expansion	Recession	Expansion
2 year integral	0.66	0.72	0.77	1.05	0.61
4 year integral	0.79	1.28	0.68	1.34	0.64

Notes: Table gives estimated fiscal multipliers from a threshold VAR (eq. (7)). Top row gives results from our baseline slump/boom and recession/expansion chronologies. Middle and bottom rows give results from alternative chronologies.

A.2 Additional figures and tables

Figure A.1: Alternative business cycle chronologies.



Notes: The blue line in each panel is the U.S. unemployment rate, and is the same across panels. Each panel's grey bars indicate the business cycle phase as determined by: BB algorithm with prolonged phases; BB algorithm with prolonged complete cycle; unemployment rate relative to HP filter trend (red dashed line). See the text for details.

Table A.2: Summary statistics of U.S. downturns 1890–2015, alternative definitions.

	BB rec. (prolonged 7)	BB rec. (prolonged 16)	HP filter trend
N. phases	26	12	32
Duration (quarters)			
Mean	7.2	15.7	7.1
Median	6.0	12.5	7.0
Min	3	7	1
Max	13	31	15

Notes: Table shows summary statistics for three alternative business cycle downturns: HP filter Slumps, the BB algorithm requiring the complete sample to last at least 16 quarters and the BB algorithm requiring the complete cycle to last at least 7 quarters. Sample period 1890–2015, duration measured in quarters. See the text for details.

Table A.3: Summary statistics for state-level recessions and expansions.

	Recession					Expansion				
State	Count	Median	Std dev.	Min	Max	Count	Median	Std dev.	Min	Max
AK	10	17	11	8	40	7	37	18	7	61
AL	5	25	16	17	55	5	69	29	12	89
AR	4	27	13	14	39	4	66	47	47	147
AZ	6	21	7	11	30	6	54	34	10	101
CA	4	39	6	33	46	4	71	23	39	94
CO	8	19	13	10	43	8	25	19	10	62
CT	6	37	19	11	57	6	41	13	18	58
DC	8	24	14	11	48	8	24	21	6	65
DE	7	24	10	11	42	7	28	25	7	70
FL	4	41	11	22	46	4	68	20	49	96
GA	9	11	10	8	36	9	23	23	6	70
HI	6	26	13	10	44	6	42	37	7	101
IA	5	39	17	12	54	5	46	36	17	107
ID	6	29	5	22	37	6	44	33	6	92
IL	7	20	15	13	55	7	34	21	6	71
IN	5	25	8	21	40	5	70	46	7	126
KS	7	20	14	11	55	7	43	26	10	75
KY	6	20	21	12	66	6	37	38	14	107
LA	9	29	15	8	49	9	16	19	8	63
MA	4	33	9	26	47	4	67	24	52	108
MD	6	26	12	12	43	6	45	16	34	70
ME	5	29	7	22	40	5	69	41	8	115
MI	5	27	12	19	47	5	74	40	10	105
MN	5	26	14	19	55	5	74	35	11	93
MO	5	22	20	11	58	5	70	42	10	105
MS	6	20	14	11	47	6	37	37	19	115
MT	6	19	14	14	46	6	42	24	19	84
NC	7	23	10	11	36	6	47	25	11	83
ND	7	18	5	12	26	7	54	27	12	79
NE	6	27	20	16	65	6	36	21	19	74
NH	4	31	16	19	52	4	71	25	55	111
NJ	6	25	14	10	41	6	47	37	8	92
NM	8	25	8	12	36	7	44	19	8	59
NV	4	48	10	38	57	4	63	16	48	87
NY	5	35	16	13	53	5	43	32	16	101
OH	5	40	16	13	54	5	61	21	30	74
OK	8	17	9	8	32	7	35	25	10	82
OR	6	25	10	11	39	6	42	29	9	86
PA	4	38	5	36	46	4	71	19	48	93
RI	4	42	8	31	48	4	71	21	44	95
SC	7	32	14	14	53	7	21	24	7	73
SD	5	23	18	14	59	5	33	70	7	179
TN	7	24	14	8	47	7	36	30	7	78
TX	7	27	9	9	32	7	41	23	10	74
UT	7	17	17	9	55	6	48	17	18	64
VA	5	34	11	14	38	5	59	31	18	102
VT	5	37	14	14	48	5	63	40	6	105
WA	4	36	16	30	64	4	67	19	46	92
WI	4	33	18	11	49	4	80	19	56	98
WV	5	23	17	12	57	4	74	28	49	113
WY	6	21	9	12	35	6	50	39	6	114

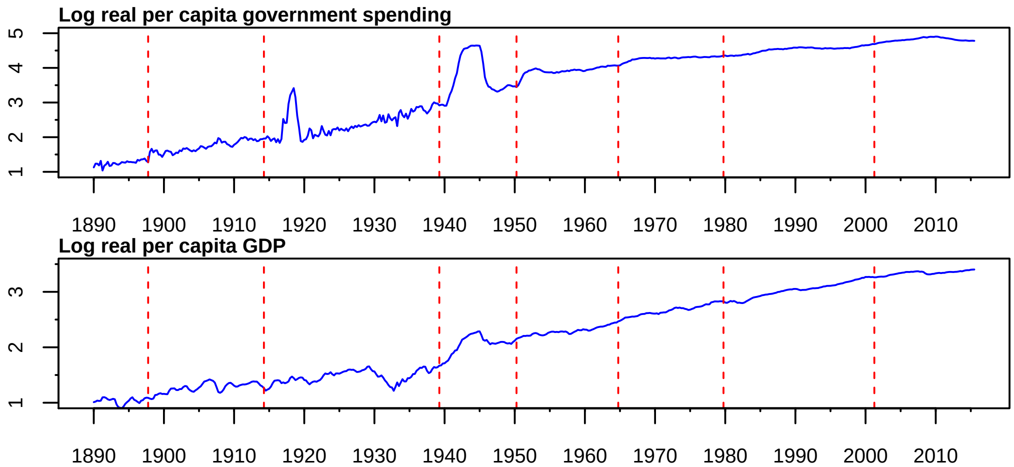
Notes: Median, standard deviation, minimum and maximum summarize phase duration in months, January 1976–December 2015. Expansions and recessions defined via Bry-Boschan algorithm. See text for details.

Table A.4: Summary statistics for state-level booms and slumps.

	Boom					Slump				
State	Count	Median	Std dev.	Min	Max	Count	Median	Std dev.	Min	Max
AK	20	9	42	1	192	19	12	10	1	32
AL	19	12	42	1	193	18	14	11	1	45
AR	21	7	42	1	199	21	6	10	1	29
AZ	22	11	40	1	196	21	7	10	1	31
CA	19	8	43	1	192	18	13	13	1	43
CO	18	15	43	1	192	17	10	12	1	39
CT	20	9	45	1	208	19	12	11	1	36
DC	17	17	44	1	192	17	11	11	2	42
DE	22	10	40	1	192	21	10	10	1	35
FL	18	8	45	1	195	17	12	15	1	44
GA	25	8	38	1	195	25	7	8	1	29
HI	26	9	38	1	199	26	8	8	1	27
IA	24	6	40	1	199	24	5	11	1	35
ID	18	10	43	1	192	17	12	11	1	34
IL	20	12	42	1	196	19	12	11	1	36
IN	24	8	39	1	197	23	9	6	1	26
KS	21	10	41	1	192	21	11	12	1	37
KY	18	13	46	1	207	17	13	9	1	25
LA	22	9	40	1	192	22	8	9	1	35
MA	16	15	51	1	213	15	16	12	1	38
MD	17	12	44	3	192	16	17	12	3	34
ME	22	10	39	1	192	22	9	10	1	34
MI	20	12	46	2	214	19	10	10	1	32
MN	18	11	44	1	195	17	16	12	1	48
MO	20	12	41	1	192	20	7	11	3	42
MS	21	8	41	1	194	20	13	10	1	33
MT	18	11	44	1	192	17	12	11	1	33
NC	15	16	47	2	195	14	14	9	5	33
ND	23	7	39	1	192	23	11	8	1	28
NE	23	10	40	1	195	22	9	10	1	36
NH	19	8	43	1	192	19	13	11	1	35
NJ	19	13	42	1	192	19	10	12	1	39
NM	13	18	49	5	192	12	22	11	3	37
NV	18	11	43	1	192	17	13	12	1	38
NY	21	7	41	1	192	21	11	11	1	34
OH	21	9	44	1	209	20	9	11	1	37
OK	21	10	40	1	192	20	12	10	1	27
OR	20	8	45	1	210	19	12	11	1	37
PA	21	9	41	1	192	20	10	11	1	36
RI	17	17	45	1	192	17	13	12	1	31
SC	17	15	45	1	195	17	13	9	3	32
SD	24	10	38	1	194	24	9	8	1	32
TN	21	9	41	1	193	20	10	9	1	30
TX	21	10	41	1	192	20	8	12	1	44
UT	14	15	55	1	215	13	15	14	1	41
VA	22	7	40	1	193	22	8	10	1	35
VT	20	5	46	1	210	19	12	13	1	35
WA	19	6	43	1	192	18	14	11	1	35
WI	25	8	38	1	197	24	6	10	1	35
WV	18	11	45	1	199	18	11	9	2	28
WY	20	12	45	1	210	19	14	9	1	28

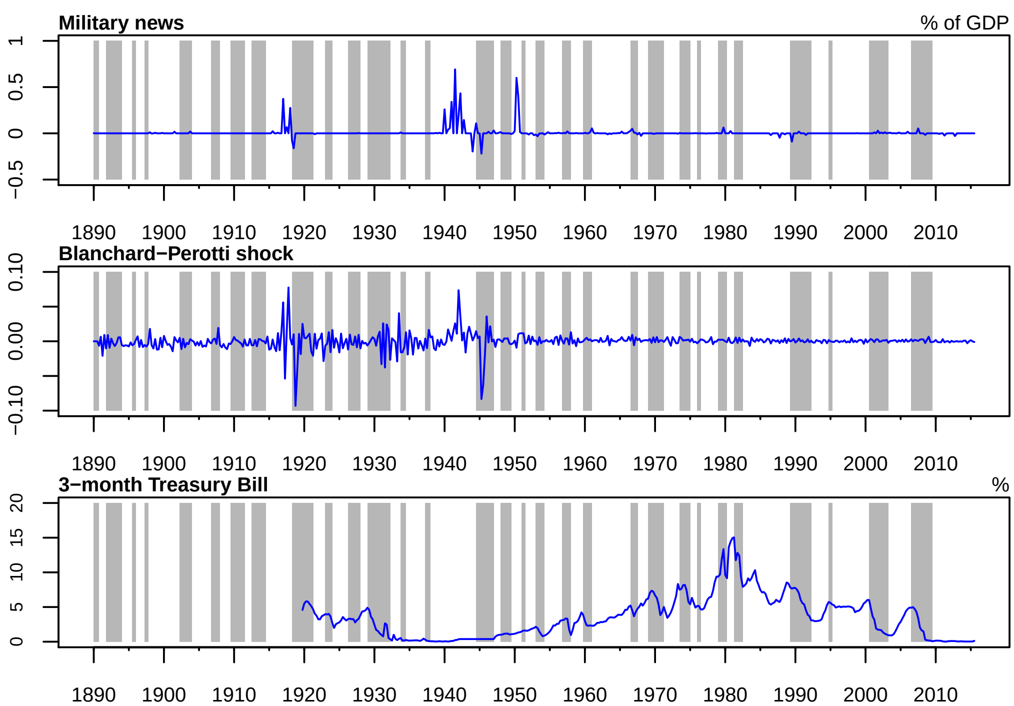
Notes: Median, standard deviation, minimum and maximum summarize phase duration in months, January 1976–December 2015. Boom and slumps defined by comparing state-level unemployment rate to its HP-filtered trend. See text for details.

Figure A.2: Real per capita output and government expenditure.



Notes: Figure shows raw data from Ramey and Zubairy (2018). Vertical dashed lines denote start of various wars (Spanish-American, WWI, WWII, Korean, Vietnam, response to Soviet invasion of Afghanistan, and Sept 11, 2001).

Figure A.3: Military spending news, Blanchard-Perotti shock, and Treasury bill.



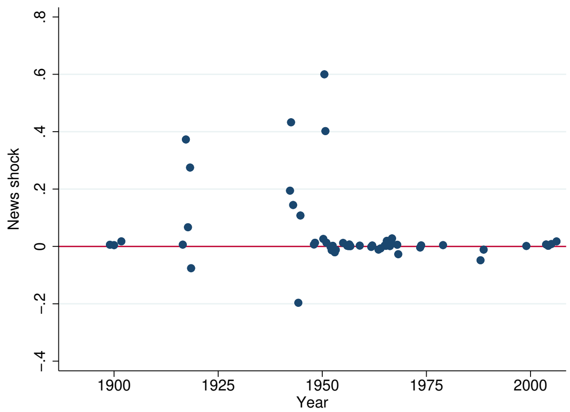
Notes: Figure shows raw data from Ramey and Zubairy (2018). Gray shaded bars denote baseline BB-defined recessions.

Table A.5: Historical time-series estimates of fiscal multipliers: BP shocks.

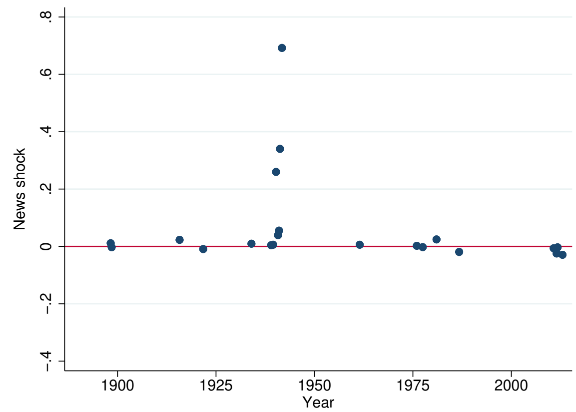
	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Baseline (BP shock)					
2 year integral	0.44	0.69	$0.38^{\dagger}$	0.32	0.48
	(0.10)	(0.11)	(0.09)	(0.27)	(0.10)
4 year integral	0.58	0.79	$0.50^{\dagger}$	0.94	0.57
	(0.09)	(0.09)	(0.10)	(0.38)	(0.12)
2. BP shocks, taxes and inflation as additional controls					
2 year integral	0.46	0.72	0.44	0.41	0.67
	(0.08)	(0.16)	(0.06)	(0.24)	(0.09)
4 year integral	0.62	0.85	0.60	1.08	0.72
	(0.07)	(0.15)	(0.07)	(0.32)	(0.06)
3. Baseline (BP shock), excluding WWII					
2 year integral	0.31	1.91	$0.22^{\dagger}$	0.56	0.24
	(0.15)	(0.91)	(0.14)	(0.29)	(0.17)
4 year integral	0.56	2.52	$0.46^{\dagger}$	1.25	$-0.12^{\dagger}$
	(0.33)	(1.16)	(0.33)	(0.40)	(0.48)
4. BP shocks, taxes and inflation as additional controls, excluding WWII					
2 year integral	0.50	2.80	$0.40^{\dagger}$	0.50	0.62
	(0.18)	(1.01)	(0.17)	(0.36)	(0.29)
4 year integral	0.93	9.31	0.83	1.31	0.88
	(0.24)	(12.0)	(0.25)	(0.38)	(0.48)

Notes: Table presents estimates using eq. (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). Specifications 3 and 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

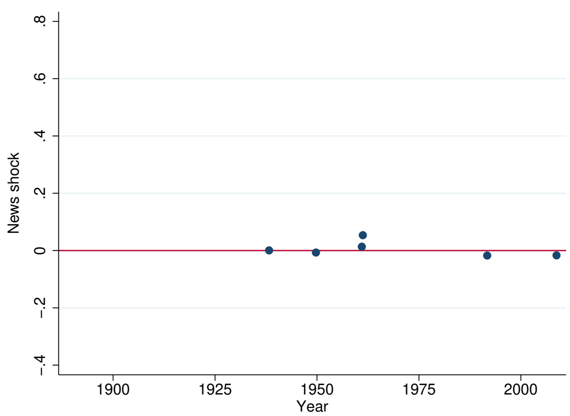
Figure A.4: Distribution of government spending shocks over time and across business cycle phases



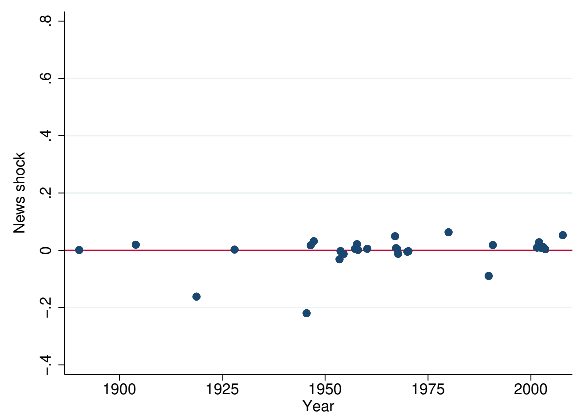
(a) Phase I



(b) Phase II



(c) Phase III



(d) Phase IV

Notes: The figure plots government spending news shocks as a fraction of potential GDP. Each figure presents shocks for that phase, where scatters are only plotted in case a year is classified as the respective phase and the government news shock was different from 0.

Table A.6: Historical time-series estimates of fiscal multipliers: alternative chronologies.

	Linear	Above/below HP filter trend		NBER chronology		BB (prolonged 7)		BB (prolonged 16)	
	All	Slump	Boom	Recession	Expansion	Recession	Expansion	Recession	Expansion
1. Military spending shock (baseline specification)									
2 year integral	0.72	0.73	0.60	1.36	0.59	1.88	0.62†	1.65	0.64†
	(0.09)	(0.25)	(0.10)	(0.56)	(0.13)	(0.63)	(0.12)	(0.44)	(0.11)
4 year integral	0.78	0.77	0.65	1.98	0.70†	2.30	0.73†	2.00	0.74†
	(0.06)	(0.20)	(0.16)	(0.69)	(0.10)	(0.70)	(0.08)	(0.58)	(0.08)
2. Military spending shock, taxes and spending as additional controls									
2 year integral	0.74	0.94	0.74	1.20	0.67	1.50	0.67	1.29	0.67
	(0.09)	(0.34)	(0.12)	(0.52)	(0.08)	(0.49)	(0.09)	(0.34)	(0.09)
4 year integral	0.79	0.93	0.76	1.64	0.77	1.75	0.76†	1.48	0.78
	(0.07)	(0.26)	(0.17)	(0.74)	(0.06)	(0.57)	(0.06)	(0.46)	(0.06)
3. Military spending shock + BP shocks									
2 year integral	0.50	0.63	0.42	0.95	0.50	1.02	0.54	0.88	0.54
	(0.08)	(0.21)	(0.09)	(0.44)	(0.12)	(0.31)	(0.11)	(0.27)	(0.11)
4 year integral	0.71	0.78	0.53	1.60	0.64†	1.50	0.69†	1.38	0.69†
	(0.06)	(0.21)	(0.13)	(0.50)	(0.11)	(0.40)	(0.09)	(0.40)	(0.09)
4. Military spending shock + BP shocks, excluding WWII									
2 year integral	0.47	0.80	0.35	1.02	0.44	0.50	0.43	0.53	0.41
	(0.16)	(0.41)	(0.17)	(0.70)	(0.29)	(0.36)	(0.30)	(0.37)	(0.25)
4 year integral	0.77	1.17	0.53	1.77	0.57	1.07	0.63	1.13	0.54
	(0.35)	(0.51)	(0.38)	(0.62)	(0.52)	(0.44)	(0.52)	(0.48)	(0.51)

Notes: Table presents estimates using eq. (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). Alternative chronologies calculated as: above/below HP filter trend; NBER recession chronology; BB algorithm with minimum duration of seven quarters; BB algorithm with minimum cycle of 16 quarters. The baseline set of controls, z_{t-1}, includes four lags of $shock$, y, and g. Specification 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

Table A.7: Historical time-series estimates of fiscal multipliers: Alternative detrending of government expenditures using the HP filter.

	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Military spending shock, HP filter					
2 year integral	0.77	0.83	0.65	1.20	0.70
	(0.11)	(0.17)	(0.14)	(0.46)	(0.13)
4 year integral	0.85	0.83	0.77	1.37	0.82†
	(0.09)	(0.09)	(0.17)	(0.60)	(0.10)
2. Military spending shock, taxes and inflation as additional controls, HP filter					
2 year integral	0.81	0.99	0.73	1.04	0.73
	(0.13)	(0.20)	(0.13)	(0.35)	(0.11)
4 year integral	0.87	0.96	0.81	1.18	0.86
	(0.09)	(0.10)	(0.20)	(0.48)	(0.09)
3. Military spending shock + BP shocks, HP filter					
2 year integral	0.51	0.86	0.43†	0.73	0.57
	(0.08)	(0.21)	(0.08)	(0.32)	(0.12)
4 year integral	0.75	0.83	0.59	1.06	0.74
	(0.07)	(0.09)	(0.11)	(0.47)	(0.10)
4. Military spending shock + BP shocks, excluding WWII, HP filter					
2 year integral	0.46	1.75	0.31†	0.39	0.51
	(0.18)	(0.81)	(0.12)	(0.45)	(0.31)
4 year integral	0.75	1.43	0.53	0.85	0.88
	(0.30)	(0.73)	(0.34)	(0.55)	(0.52)

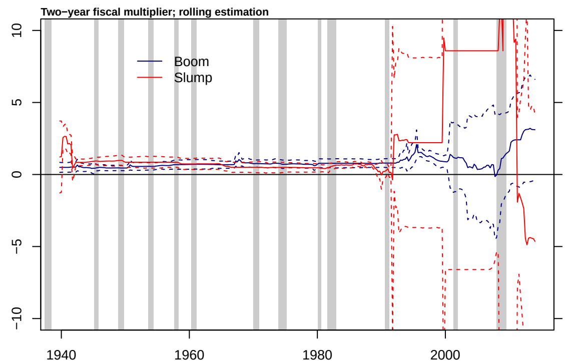
Notes: Government expenditures are detrended with HP filter with $\lambda = 1,500,000$. Table presents estimates using equation (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). The baseline set of controls, z_{t-1}, includes four lags of $shock$, y, and g. Specification 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

Table A.8: Historical time-series estimates of fiscal multipliers: Alternative detrending of government expenditures using the CF filter.

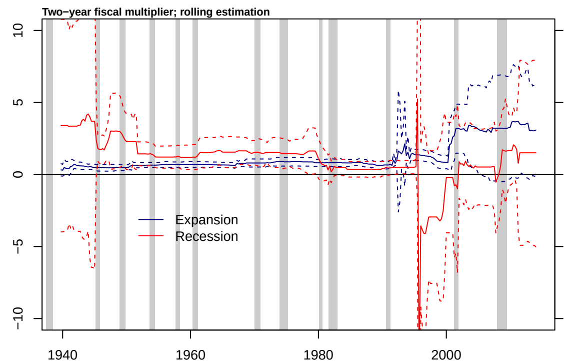
	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Military spending shock, CF filter					
2 year integral	0.78	0.82	0.70	1.21	0.69
	(0.12)	(0.19)	(0.18)	(0.54)	(0.15)
4 year integral	0.85	0.81	0.84	1.38	0.81
	(0.10)	(0.09)	(0.23)	(0.71)	(0.11)
2. Military spending shock, taxes and inflation as additional controls, CF filter					
2 year integral	0.82	0.96	0.83	0.96	0.73
	(0.16)	(0.22)	(0.16)	(0.40)	(0.13)
4 year integral	0.87	0.89	0.92	1.05	0.87
	(0.11)	(0.09)	(0.28)	(0.55)	(0.10)
3. Military spending shock + BP shocks, CF filter					
2 year integral	0.44	0.78	0.44	0.56	0.49
	(0.12)	(0.21)	(0.13)	(0.33)	(0.14)
4 year integral	0.70	0.80	0.61	0.96	0.68
	(0.08)	(0.09)	(0.16)	(0.50)	(0.12)
4. Military spending shock + BP shocks, excluding WWII, CF filter					
2 year integral	0.25	1.12	0.31	0.14	0.30
	(0.13)	(1.03)	(0.16)	(0.42)	(0.25)
4 year integral	0.51	1.34	0.53	0.64	0.57
	(0.36)	(0.95)	(0.36)	(0.62)	(0.58)

Notes: Government expenditures are detrended with CF filter where frequency is set between 3 and 160 quarters. Table presents estimates using equation (4). Newey-West standard errors in parentheses. BP denotes Blanchard and Perotti (2002). The baseline set of controls, z_{t-1}, includes four lags of $shock$, y, and g. Specification 4 excludes observations from 1941Q3 to 1945Q4. See text for details.
† indicates that the difference across phases is statistically significant at 10 percent level.

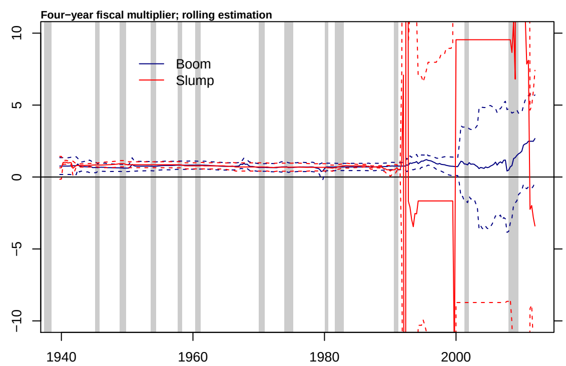
Figure A.5: Sub-sample analysis: 50-year rolling window estimation at the national level



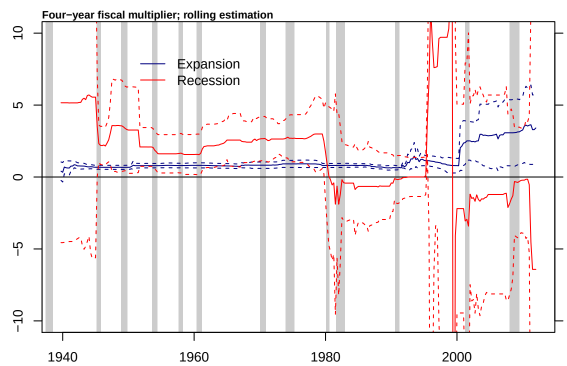
(a) Two-year multiplier, boom versus slump



(b) Two-year multiplier, expansion versus recession



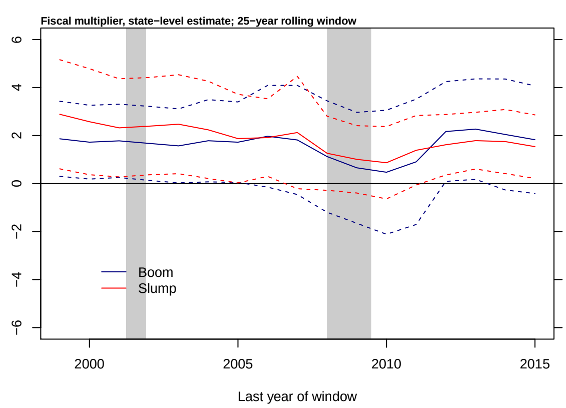
(c) Four-year multiplier, boom versus slump



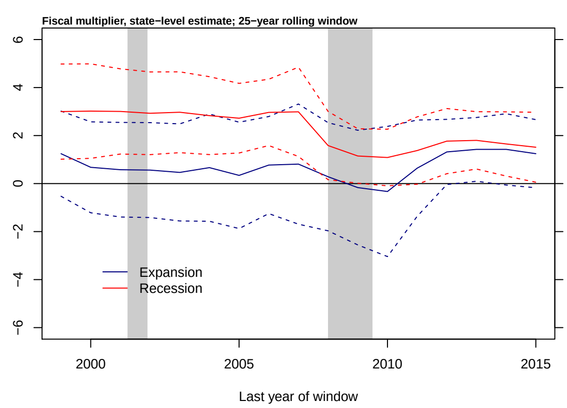
(d) Four-year multiplier, expansion versus recession

Notes: The figures plot the estimated two- and four-year fiscal multipliers for a rolling window sample. The horizontal axis denotes the final year for the rolling windows. The left figures compare slumps and booms while the right figures compare recessions and expansions. Dashed lines indicate 90 percent confidence intervals. Estimates are obtained from the baseline specification that includes controls for four lags of GDP and government expenditure, two lags of the ratio of GDP over potential GDP, two lags of the ratio of government spending over trend, and lagged instruments, all interacted with the state variables. See text for details.

Figure A.6: Sub-sample analysis: 25-year rolling window estimation at the state level



(a) Slump versus boom



(b) Recession versus expansion

Notes: The figures plot the estimated two-year fiscal multipliers at the state-level for a rolling window sample. The x-axis denotes the final year for the 25-year rolling window. The left figure compares slumps and booms while the right figure compares recessions and expansions. Dashed lines indicate 90 percent confidence interval. Estimates are obtained from regressions that include year and state fixed effects, in line with specification 3 in Table 8 of the original manuscript. See text for details.

Table A.9: Estimated open economy fiscal multipliers, alternative chronologies: Evidence from U.S. states.

	Linear	NBER		BB (prolonged 7)		BB (prolonged 16)	
	All	Recession	Expansion	Recession	Expansion	Recession	Expansion
1. Baseline (year fixed effects only)							
Two year integral	2.01	2.18	1.99	2.27	1.60	2.24	1.44
	(0.83)	(0.97)	(1.01)	(0.91)	(1.17)	(0.91)	(1.24)
2. Year fixed effects; size of military							
Two year integral	0.29	0.36	0.06	1.25	-0.86†	1.28	-1.03†
	(0.80)	(1.00)	(0.92)	(0.91)	(1.17)	(0.90)	(1.21)
3. Year and state fixed effects; size of military							
Two year integral	3.05	2.15	2.02	2.36	1.18	2.60	0.68
	(1.51)	(0.84)	(1.15)	(0.88)	(1.39)	(0.90)	(1.45)
4. Year and state fixed effects; size of military; labor market/industry							
Two year integral	1.58	0.02	1.81	2.06	0.60	2.57	-0.00†
	(0.73)	(0.96)	(0.93)	(0.74)	(1.08)	(0.71)	(1.07)
5. Year and state fixed effects; size of military; labor market/industry; lagged dep. var.							
Two year integral	1.55	-0.06	1.83†	2.12	0.42	2.56	0.02†
	(0.73)	(0.96)	(0.70)	(0.58)	(0.97)	(0.59)	(0.99)

Notes: Table presents estimates using eq. (6). Table reports estimates from the 2SLS estimator in equations 5 and 6. Phases correspond to those labeled in figure 1. Numbers in parentheses are standard errors, clustered by time and state. Number of observations varies from 1,223 to 1,325.
† indicates that the difference across phases is statistically significant at 10 percent level.

Table A.10: State-dependence of multipliers for employment: Evidence from U.S. states.

	Linear	Above/below trend		Peak to trough (BB alg)	
	All	Slump	Boom	Recession	Expansion
1. Baseline (year fixed effects only)					
Two year integral	0.36	0.42	0.29	0.76	-0.01†
	(0.15)	(0.18)	(0.14)	(0.22)	(0.16)
2. Year fixed effects; size of military					
Two year integral	0.21	0.16	0.16	0.35	-0.00
	(0.13)	(0.15)	(0.09)	(0.19)	(0.10)
3. Year and state fixed effects					
Two year integral	0.50	0.47	0.34	0.76	-0.10†
	(0.22)	(0.20)	(0.15)	(0.23)	(0.23)
4. Year and state fixed effects; size of military; labor market/industry					
Two year integral	0.32	0.30	0.23	0.54	-0.02†
	(0.12)	(0.11)	(0.16)	(0.21)	(0.15)
5. Year and state fixed effects; size of military; labor market/industry; lagged dep. var.					
Two year integral	0.32	0.29	0.06	0.54	-0.03†
	(0.12)	(0.12)	(0.17)	(0.21)	(0.16)

Notes: Table presents estimates using the two-year percentage change in the employment rate, defined as the ratio of employment over the labor force. Standard errors clustered by state and time. Number of observations varies from 1,682 to 1,886.
† indicates that the difference across phases is statistically significant at 10 percent level.