Essays on Identification of Fiscal Policy Shocks

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Abstract

This thesis contributes to the literature by exploring three different but interconnected issues on the identification of fiscal policy shocks. Grounded in various extensions of the structural vector autoregressive (SVAR) framework, the three essays provide empirical analyses of the macroeconomic impact of fiscal policy shocks, as well as their implications for the transitional dynamics of the U.S. economy.

Chapter 2 discusses the use of external instruments or proxy variables in the SVAR model. This method utilizes information from "outside" the VAR to identify the shocks of interest instead of using a recursive structure or constraints on coefficient matrices. We consider a variety of instruments that have been suggested in the literature to identify government spending and tax shocks. We examine their validity individually and use only those which pass relevance and exogeneity tests. This chapter extends the strand of literature on proxy SVAR to the case where multiple instruments are used to identify the structural shocks of interest. Furthermore, we provide results on the size of fiscal multipliers for the U.S. economy using various proxies and model specifications. We show that the calculation of government spending and tax multipliers vary considerably across different identification strategies.

In Chapter 3, we study the impact of government spending shocks on prices in the U.S. using a nonlinear VAR approach. According to conventional wisdom, inflation increases in response to a positive government spending shock. However, a large number of studies provide evidence that a government spending shock is deflationary. This is referred to as the "fiscal price puzzle". Thus, in attempts to delve into the discrepancy between theoretical predictions and empirical evidence, we look at the impact of fiscal spending on inflation under different states of the monetary policy rate. In particular, we analyze the role of prices in the transmission of fiscal spending shocks to real activity by exploiting an interacted VAR (IVAR) model. A nonlinear analysis of this finding would be important because it allows us to gain further insight into how the effects of government spending shocks differ at or away from the zero lower bound (ZLB). We find robust evidence that prices decline persistently and significantly following an increase in fiscal spending in both states of the economy. During periods of active monetary policy, a fiscal spending innovation raises output on impact, whose effect dies down after about three years. However, in presence of the zero interest rate constraint, private consumption and output drop quickly in response to a government spending shock after two quarters

and remain negative for most of the horizons before recovering slowly to pre-shock levels. Government spending multipliers under both regimes are high on impact and decrease to below one after one year. The multiplier in the normal state is more persistent and remains positive in the long run while the multiplier in the ZLB state displays a quick drop and turns negative after two years, although the results indicate no statistical difference between the multipliers.

Chapter 4 studies the macroeconomic effects of government spending shocks when focusing on the unconventional monetary policy measures taken by the Federal Reserve in response to the global financial crisis. Since the onset of the global financial crisis until the pre-COVID-19 period, the Federal Reserve implemented three rounds of large-scale asset purchases (LSAPs), or quantitative easing (QE). Simultaneously, the U.S. government introduced discretionary fiscal stimulus packages to stabilize the economy and boost the aggregate demand. The combination of unconventional monetary policy and fiscal policy is a distinctive feature of policy reaction during the Great Recession. However, given its unusual nature, the challenge is to figure out a suitable econometric framework for assessing the joint effects of unconventional monetary and fiscal policies during both pre- and post-crisis periods. This chapter contributes to the literature by studying the macroeconomic effects of government spending surprises and the interaction between government spending and QE, using a local projections framework. Using monthly data on government spending and real activity in the U.S., we document that the expansionary effects of a government spending shock appear to be more persistent and statistically larger in absence of the ZLB. We find that prices rise significantly in response to a positive government spending shocks in normal times, which aligns with the predictions of theoretical models. There is little evidence of difference in the responses of real activity measures irrespective of the influence of QE programs in the economy. We provide evidence that these results are robust to various estimation methodologies and alternative specifications.

Declaration

This thesis is an original work of my research and contains no material which has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis.

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Chapter 1

Introduction

The macroeconomic effects of fiscal policy shocks and the interactions between fiscal and monetary policies have long been addressed by macroeconomic researchers and policy-makers. Since the wake of the global economic downturn in 2007-2008, these issues have gained new traction. In response to the economic recession and the weak recovery that followed, the Federal Reserve implemented unprecedented measures in the U.S. including keeping the federal funds rate close to zero since late 2008 and engaging in asset purchases, also referred to as quantitative easing (QE). However, as policy rates ultimately hit rock bottom and the effectiveness of conventional monetary policy in boosting economic activity became limited, many advanced governments turned to active fiscal policy as an additional tool to mitigate global economic turbulence.[1] This has led to substantial shifts in certain norms for policy behaviour and raised important questions on the identification of fiscal shocks as well as how monetary-fiscal interactions might affect our understanding of fiscal policy impacts.

Despite the growing number of studies on this topic, no consensus has been reached regarding the impact of fiscal shocks relating to monetary policy stance on macroeconomic aggregates. Existing literature on the subject demonstrates substantial deviations between theoretical models and their empirical counterparts. This long-standing debate is due to variations in underlying assumptions and shock identification methods. In light of this gap in the literature, this thesis is broadly concerned with the macroeconomic effects

[1] The U.S. government implemented a large-scale fiscal stimulus, which comprised a major component of the America Recovery and Reinvestment Act (ARRA) of 2009. The Act allocated approximately $500 billion to government expenditure and $300 billion to tax relief, which accounts for almost 6% of the GDP in the US. Similarly, the European Union proceeded with the European Economic Recovery Plan (EERP) while a wide spectrum of fiscal packages was called for by other developed countries in response to the Great Recession.

and identification of fiscal policy shocks. We divide this thesis into three distinct but related chapters. Each chapter addresses policy-relevant questions regarding contemporary macroeconomic debates and economic developments over the last decade. The first chapter focuses on using different instrumental variables to identify fiscal policy shocks, while the next two chapters focus on the role of nonlinearity in the transmission mechanism of government spending shocks during the financial crisis and the zero effective lower bound of interest rates. We extend the empirical structural vector autoregressive (SVAR) models to perform this analysis in the context of the U.S. economy.

1.1 An examination of proxy SVAR models for identification of fiscal shocks

In chapter 2, we follow a relatively new approach, which identifies the structural shocks using external instruments, or proxies, the so-called proxy SVAR approach (Mertens and Ravn, 2013, Stock and Watson, 2012). This method utilizes information from "outside" the VAR to identify the shocks without relying on recursive structures or constraints on coefficient matrices. This approach has gained rising popularity in the literature recently. In particular, we employ the proxy SVAR technique using various proxies to identify the fiscal spending and tax shocks. Subsequently, we compute the corresponding output multipliers, which are defined as the dollar change in GDP per effective dollar change in fiscal policy in each instance. We consider two scenarios. First, we consider the "single shock" approach, where only one fiscal structural shock is identified using either a single proxy (identification I) or multiple proxies (identification III). Second, we examine the "multiple shocks" approach, where government spending and tax shocks are identified simultaneously using one proxy for each shock (identification II).

In addition to the set of external instruments which have been suggested and used considerably to aid the identification of fiscal shocks in the literature, we propose the use of a measure of consumer sentiment index to address the challenge of "anticipation effect" or "fiscal foresight" in fiscal settings. In this case, the standard VAR model might not mirror the true timing of the fiscal innovations since they are anticipated well before being implemented. Specifically, we use the expected change in one's personal financial situation in a year to identify fiscal shocks. We conduct a Granger causality test, which shows that the expectation measure Granger-causes the net taxes. Interestingly, the relevance test provides evidence of a high correlation between this variable and GDP, yet a close to zero correlation with tax shocks. It implies that the variation of net taxes captured by this measure of expectation might arise due to the positive feedback from output to taxes. Nevertheless, it does not qualify as a valid proxy for fiscal shocks.

Our analysis suggests mixed evidence on the size of fiscal multipliers using different identification approaches. Upon conducting a test for differences in the effects of government spending and tax shocks on GDP, we observe that the change in output in response to changes in spending differs across the estimates and so does the tax multipliers. Furthermore, the difference in the size of fiscal multipliers highlights the importance of anticipation effects. However, the fact that we observe considerably large confidence intervals for tax multipliers indicates a high degree of uncertainty in external instruments.

1.2 Understanding the fiscal price puzzle: Evidence from a nonlinear VAR approach

In chapter 3, we examine the implications of fiscal intervention for macroeconomic variables, focusing on the causal relationship between government spending and inflation. This is motivated by the fact that in the wake of the global financial crisis (GFC) in 2008-2009, interest rates in many countries dropped near the zero lower bound (ZLB) thus limiting the options to stimulate the economy through reductions in the policy rates. Since then, discretionary fiscal stimulus packages have started to serve as additional policy tools in advanced countries and interest has grown anew toward studying the impacts of fiscal policy on output and other key macroeconomic variables. Most studies focus on the impact of government spending shocks on aggregate output, consumption and other macroeconomic variables. However, the literature on how prices respond to fiscal spending shocks remains fairly limited.

As suggested by standard theoretical frameworks, such as the Real Business Cycle or New Keynesian models, a positive government spending shock is likely to raise the price level via increased aggregate demand. However, a large number of academic contributions provide empirical evidence that a government spending shock decreases prices. This is called the "fiscal price puzzle", which does not conform with conventional theories analyzing the economic effects of fiscal policies (Mountford and Uhlig, 2009, Dupor and Li, 2015, Jørgensen and Ravn, 2022, D'Alessandro et al., 2019). We recognize that this finding might be due to the nonlinearity in the monetary-fiscal policy interaction. Thus, in attempts to delve into the discrepancy between theoretical predictions and empirical evidence, we look at the impact of fiscal spending on inflation under different states of the monetary policy rate. A nonlinear analysis of this finding enables us to gain further insight into how the effects of government spending shocks differ at or away from the ZLB. In particular, we analyze the role of inflation in the transmission of fiscal spending shocks to real activity by exploiting an Interacted VAR (IVAR) model using quarterly U.S. data. This model builds on the IVAR model employed by Towbin and Weber (2013) and Caggiano et al. (2017), which augments a standard VAR model by an interaction term between government spending and interest rate variables. This enables us to quantify the varied effects of a government spending shock on all endogenous variables and to capture the possible presence of nonlinearity in the transmission mechanism. The limited number of observations available for the ZLB period makes this method more suitable than alternative regime-switching models as it uses the whole sample of observations. The proposed model leaves us sufficient degrees of freedom to gauge the economy's response conditional on the stance of the monetary policy.

The main results can be summarized as follows. First, we find robust evidence that prices decline significantly and persistently following an increase in government spending in both states of the economy. This observation is supportive of the prevalent finding of the price puzzle among empirical studies on the effects of fiscal policy on inflation. Second, we find that, during periods of active monetary policy, output reacts positively to a fiscal spending surprise on impact. The positive effect dies down after about three years following the shock. However, this pattern changes when the effective rate is at the zero bound. We observe a quick drop in the levels of private consumption and output in response to a government spending shock. They remain negative for most of the horizons before recovering slowly to pre-shock levels. We also find a rapid rise in uncertainty level in response to a jump in government spending during the ZLB and the timing of heightened uncertainty matches well with the drop in real activity variables. Third, estimates obtained from our quarterly IVAR show that the cumulative multipliers under both regimes are high on impact when the fiscal spending shock hits the economy and is less than one after one year. We find that the multiplier in the normal state is more persistent and remains positive in the long run while the multiplier in the ZLB state indicates a quick drop and turns negative after two years following the shock. However, we find no statistical difference between the multipliers.

1.3 Government spending shocks and unconventional monetary policy

In chapter 4, we explore empirically whether and how the macroeconomic effects of government spending shocks vary when unconventional monetary policy (UMP) takes effect during the ZLB period. As the conventional monetary policy rate is constrained at the zero region, the Federal Reserve ought to rely on the unprecedented UMP to support aggregate economic activity. The major UMP tools include large-scale asset purchases (LSAPs) (also known as quantitative easing" (QE) by the financial community and media) and the commitment to keep interest rates low (forward guidance). In addition to the alternative monetary policy tools when federal funds rates were kept near the ZLB, fiscal stimulus packages have also been implemented to enhance economic recovery. Thus, understanding the unfamiliar mechanism of policy interactions arising due to this unusual (un)conventional monetary-fiscal combination has become an important subject for policymakers and researchers.

In the previous chapter, we analyze the impact of government spending shocks on real activity and prices in the presence of the ZLB relative to normal times using quarterly data. This is a standard exercise in the literature since data on many macro-aggregate variables, such as government spending or gross domestic product (GDP) are only available at quarterly or even annual frequencies. However, these low frequencies present us with key challenges of identifying government spending shock and limiting statistical power due to the limited number of observations available for the ZLB period. This is even more so when we are specifically interested in the periods with active QE policy. We address these issues by adopting monthly-frequency data on U.S. government spending.

We extract the monthly fiscal spending series from the monthly statements released by the Bureau of the Fiscal Service. Following Auerbach and Gorodnichenko (2012), we employ the local projections approach developed by Jordà (2005) to compute the impulse response functions. The goal of this chapter is twofold. First, we analyze the state-dependent impact of fiscal spending shocks in light of the ZLB. Next, we scrutinize the state-dependent evidence at the binding ZLB with regard to QE policy. Particularly, we look at the ZLB periods with and without QE separately. To the best of our knowledge, this is the first empirical study that investigates the state-dependent impacts of government spending shock with the joint presence of the ZLB and QE programs using monthly-frequency data.

Our main finding is that government spending shocks exhibit asymmetric effects on real activity contingent upon the monetary policy at work. Specifically, an expansionary government spending shock boosts GDP and private consumption in normal times. In contrast, a surprise increase in government spending fails to produce any boost in confidence or any inflationary effect when the ZLB is in place. Turning to the three-regime model, the exercises show that there is little evidence of state-dependent response based on the presence of QE policy when the interest rate is at the ZLB. We also find evidence of inflationary response of prices to a government spending expansion, which can be reconciled with the predictions of standard New Keynesian models. This is at odds with the result of the previous chapter which is based on quarterly data, thus opening up an interesting avenue for future research.

Chapter 2

An examination of proxy SVAR models for identification of fiscal shocks

2.1 Introduction

Following the global financial crisis in 2007-2008 and the reduction of interest rates to near zero region, the effectiveness of monetary policy in boosting economic activity became more limited. Thus interest grew anew towards studying the impacts of fiscal policy. The U.S. government implemented a large-scale fiscal stimulus, which comprised a major component of the America Recovery and Reinvestment Act (ARRA) of 2009.[1] Similarly, the European Union proceeded with the European Economic Recovery Plan (EERP) while a wide range of fiscal packages was called for by other developed countries in response to the Great Recession. Despite the growing number of studies on this topic, no consensus has been reached regarding the size of spending and tax multipliers as well as the qualitative effects of fiscal shocks on macroeconomic aggregates. This long-standing debate is due to variations in methods of identifying underlying fiscal shocks.[2] Another challenge is that identifying the effects of fiscal shocks on macro activities is not straightforward due to

[1] The Act allocated approximately $500 billion in government expenditure and $300 billion in tax relief, which accounts for almost 6 percent of GDP in the US.

[2] Among the prominent methods are the recursive approach introduced by Sims (1980), the structural VAR approach proposed by Blanchard and Perotti (2002) and extended by Perotti (2002), the sign-restriction approach by Uhlig (2005), the narrative approach developed by Ramey (2011) to investigate the impact of large surprise changes in government spending.

inherent endogeneity, thus the literature tackles this issue by exploiting series of exogenous changes in policy instruments.

Relatively recent contributions by Stock and Watson (2012) and Mertens and Ravn (2013) suggest a promising approach to tackle this identification challenge. They consider an SVAR model which uses external "instruments" or "proxies"[3], the so-called proxy SVAR. This proxy SVAR approach utilizes information from "outside" the VAR to identify the shocks of interest instead of using recursive structures or constraints on the coefficient matrices of the VAR regressions. The proxy is considered valid if it is correlated with the structural shock of interest and uncorrelated with other shocks in the system, the so-called "*relevance*" and "*exogeneity*" conditions. A common identification method involves the narrative measures of shocks derived from textual analysis of historical events, such as Romer and Romer (2010) proxy for tax shocks and Ramey (2011) proxy for federal spending shocks. Mertens and Ravn (2013) decompose the narrative measure of shocks in federal tax liabilities proposed by Romer and Romer (2010) into changes in personal and corporate income taxes. They also account for inherent delays in the legislative system by retaining only tax changes where implementation occurs in less than one quarter after becoming law and use this measure as an instrument for the conventional SVAR tax shocks. Stock and Watson (2012) employ the proxy SVAR method to study the effect of various structural shocks including fiscal policy shocks. In particular, they use a measure of federal defense spending news (Ramey, 2011) and excess returns on stocks of military contractors (Fisher and Peters, 2010) as instruments for federal government spending changes and Romer and Romer (2010)'s measure of exogenous tax changes relative to GDP as an instrument for tax shocks. In a related study, Mertens and Ravn (2014b) attempt to reconcile the difference between narrative estimates of the effects of tax shocks (Romer and Romer, 2010) with the estimates obtained by the standard identification developed by Blanchard and Perotti (2002) using unanticipated tax changes as an external instrument.

In this chapter, we implement a proxy SVAR approach that allows for joint use of instruments for both fiscal spending and tax shocks. We make several contributions to the literature. First, we investigate the validity of different external instruments by conducting comprehensive tests on their relevance and exogeneity within our sample period. In addition, we provide evidence on cross-correlations among the instruments and estimated structural shocks. Second, we employ an empirical strategy that allows one to accommodate situations in which: i) a single shock is identified using one or multiple instruments; ii) multiple shocks are jointly identified. Such an approach enables us to achieve a flexible identification of tax revenue and government spending shocks. Then, we compute the tax

[3]The terms "instrument" and "proxy" are used interchangeably from here onwards.

and government spending multipliers correspondingly using the valid instruments from the previous stage.

The main contribution of this chapter, which is to evaluate the robustness of the fiscal multipliers using alternative instruments, is something the recent literature rarely mentions. By doing so, we are able to dig deeper and understand what is behind the conflicting results in the extant literature, therefore synthesizing the results and validating the econometric analysis. For this purpose, we propose a wide spectrum of instruments that the existing literature has assumed to be relevant for the identification of fiscal shocks. As instruments for government spending shock, we use the Ramey (2011)'s defense spending news; one-quarter-ahead and four-quarter-ahead forecast errors which are computed as the differences between actual federal government spending and its one-quarter-ahead and four-quarter-ahead forecasts by the Survey of Professional Forecasters (SPF), respectively; the unanticipated spending shocks as in Blanchard and Perotti (2002), the excess stock returns on defense stocks proposed by Fisher and Peters (2010), revisions of expectations produced by Gambetti (2012) and defense spending news proposed by Ben Zeev and Pappa (2017). Turning to tax shocks, we use the exogenous tax changes introduced by Romer and Romer (2010), the unanticipated tax changes adopted by Mertens and Ravn (2012) and the implicit tax rate developed by Leeper et al. (2013).

We also consider the use of expected change in personal financial situation in a year, extracted from the University of Michigan's Survey of Consumers. One is aware that examining the effects of policy shocks can be problematic in the context of fiscal intervention. Various studies have pointed out that the decision lags (the time for policy to be passed as a law in response to macro aggregate shocks) and implementation lags (the period over which policy changes come into force), which are inherent in fiscal policy, can pose some challenges to the identification of shocks (Blanchard and Perotti, 2002). Problems arise because private agents receive signals about these changes when information is circulated among economists, policy-makers and news media, and much of this happens well before the fiscal measures are implemented. Thus, the impulse responses obtained in the standard VAR model might not mirror the true timing of the fiscal innovations since they are anticipated by the private sector. This phenomenon is often referred to as the "anticipation effect" or "fiscal foresight". For this reason, it might be crucial to explore the relevant impacts of consumer confidence on changes in fiscal policy instruments. We address this question by using the expected change in personal financial situation to identify the fiscal shocks.[4]

[4]Details for all series that are used as proxies are given in the Appendices.

The list of instrumental variables that we employ in this chapter is not exhaustive, nevertheless, we limit our selection to those that feature substantially in studies of fiscal spending and tax shocks. We examine the validity of these instruments by testing their relevance and exogeneity, and subsequently conduct our exercises by using the valid instruments, which include: (1) the anticipated defense spending shocks estimated by Ramey (2011), (2) the one-quarter-ahead and (3) four-quarter-ahead federal spending forecast errors based on Survey of Professional Forecasters (4) the unanticipated spending shocks proposed by Blanchard and Perotti (2002), (5) the exogenous tax shocks estimated by Romer and Romer (2010) and (6) the unanticipated tax shocks estimated by Mertens and Ravn (2011). The first four instruments are used to identify federal spending shock, while the last two instruments are used for the identification of tax shock.

Most of the existing studies on proxy SVAR are restricted to using one instrument at a time to identify a single structural shock, in isolation from all other shocks in the system (Stock and Watson, 2012, Olea et al., 2021). Among the exceptions are Mertens and Ravn (2013) and Mertens and Montiel Olea (2018) who decompose the narrative tax series of Romer and Romer (2010) into exogenous changes in personal income and corporate tax revenues to identify the corresponding structural tax shocks. They argue that when dealing with more than one external instrument, additional constraints yielded by the instruments to complement the original restrictions imposed on the errors of the VAR are needed for the identification of shocks. Mertens and Ravn (2013) obtain these constraints from a Cholesky decomposition of a covariance matrix.

To our knowledge, the number of studies that use proxy SVARs to jointly identify government spending and tax shocks remains relatively scarce. Some of the notable works in this strand of literature include Caldara and Kamps (2017), who implement a proxy SVAR strategy that identifies all shocks of a five-variable system. They use three non-fiscal instruments including Fernald (2012)'s technology series, the oil shocks constructed by Hamilton (2003) and the monetary policy shocks by Romer and Romer (2004) to identify three non-fiscal shocks (output, inflation and monetary policy) and simultaneously identify tax and spending shocks under the restrictions that government spending does not react on impact to taxes. Furthermore, Miyamoto et al. (2019) examine the effects of fiscal shocks identified by proxies on the open economy using panel data for a large number of countries. They use military expenditures as an external instrument for exogenous government spending shocks and find a negative movement in the current account in response to a positive shock in government expenditures. Klein and Linnemann (2019) consider the use of external instruments to identify tax and spending shocks and study their effects on the current account and budget deficit. They use the US narrative tax shocks by Romer and Romer (2010) as an instrument for tax shocks and the growth rate of military spending per head of population to identify the government spending shocks. Another recent contribution is Angelini and Fanelli (2019) who consider cases in which multiple instruments are used for multiple target shocks. They propose a novel method which allows full identification of fiscal and non-fiscal shocks.

The closest paper to ours is Angelini et al. (2022). They conduct a proxy SVAR analysis that jointly employs fiscal and non-fiscal instruments to estimate the fiscal spending and tax multipliers in the U.S., using the Augmented-Constrained SVAR (AC-SVAR) model developed by Angelini and Fanelli (2019). They find that the estimate of tax multiplier appears sensitive to the assumption of orthogonality between the fiscal shock and non-fiscal instrument, whereas the spending multiplier is robustly larger than unity across various choices of instrument. There are substantial differences between Angelini et al. (2022) and our study. First, they focus on quantifying the magnitude of fiscal multipliers by dedicating one instrument for a particular shock, i.e., they use Mertens and Ravn (2011)'s unanticipated tax shocks and Auerbach and Gorodnichenko (2012)'s unanticipated spending shocks to identify the fiscal shocks; and Fernald (2012)'s total factor productivity series and Hamilton (2003)'s oil shocks to identify the non-fiscal shocks. Differently, we assess the robustness of fiscal multipliers using a variety of instruments for each type of fiscal shock. Second, we provide empirical evidence on the validity of each potential instrument by implementing a variety of tests on their relevance and exogeneity over data availability.

In the empirical part of the chapter, we employ a standard fiscal policy-only VAR model using data on tax revenue, federal government spending and gross domestic product (GDP) for the U.S. as a starting point of our analysis. In addition, to control for the role of monetary policy and the possible impact of anticipated fiscal shocks, we consider a fiscal-monetary policy mixed framework. In this extended VAR model, we include variables that tend to react to signals about future changes in policy such as inflation and interest rate. The two scenarios considered are the "single shock" scenario, where only one fiscal structural shock is identified using either a single instrument (identification I) or multiple instruments (identification III), and the "multiple shocks" scenario, where government spending and tax shocks are identified jointly using one instrument for each type of shock (identification II).

Our findings suggest mixed evidence on the size of fiscal multipliers using different identification approaches. Upon conducting a test for differences among the effect of government spending on GDP, we find that the multiplier estimates vary when taking into account the role of anticipation effects. In particular, the spending multiplier computed using the anticipated fiscal spending shocks by Ramey (2011) is considerably larger than the ones obtained by other unanticipated proxies. Likewise, the tax multipliers differ remarkably in magnitude when one accounts for the anticipation effects. This highlights the relevance of anticipations for the identification of fiscal shocks. Interestingly, our analysis shows that response of GDP is more modest and might even turn negative when controlling for the role of monetary policy using multiple instruments to identify government spending and tax shocks simultaneously.

The remainder of this chapter is organized as follows. Section 2.2 introduces our methodology and the different identification approaches considered. Section 2.3 provides details of our dataset and external instruments employed. The results of validity tests are illustrated in Section 2.4. Section 2.5 exhibits empirical results on the impact of fiscal shocks using proposed model identifications. Section 2.6 concludes and finally, Section 2.7 contains the appendices.

2.2 Methodology

2.2.1 Proxy VAR model

Our starting point is a standard vector autoregression (VAR):

$$\mathbf{X}_t = \mathbf{A}_1\mathbf{X}_{t-1} + \mathbf{A}_2\mathbf{X}_{t-2} + \cdots + \mathbf{A}_p\mathbf{X}_{t-p} + \mathbf{u}_t \tag{2.1}$$

where $\mathbf{X}_t$ is an $n \times 1$ vector of endogenous variables; $\mathbf{A}_j, j = 1, \ldots, p$, are $n \times n$ coefficient matrices and $\mathbf{u}_t$ is an $n \times 1$ vector of VAR innovations. We exclude deterministic terms from the set-up in (2.1) for simplicity. Let $\boldsymbol{\varepsilon}_t$ be an $n \times 1$ vector of structural shocks that are assumed to be white noise, i.e., $E(\boldsymbol{\varepsilon}_t) = 0$, $E(\boldsymbol{\varepsilon}_t\boldsymbol{\varepsilon}_s') = 0$ for t $\neq$ s, with unit variances and independent across equations, i.e., $E(\boldsymbol{\varepsilon}_t\boldsymbol{\varepsilon}_t') = I$. We call $\mathbf{u}_t$ and $\boldsymbol{\varepsilon}_t$ the reduced-formed residuals and the structural shocks. Then, $\mathbf{u}_t$ are related to $\boldsymbol{\varepsilon}_t$ by

$$\begin{aligned} \mathbf{u}_t &= \mathbf{B}\boldsymbol{\varepsilon}_t \\ \text{where} \quad E(\mathbf{u}_t\mathbf{u}_t') &= \Sigma_{uu'} = \mathbf{B}\mathbf{B}'. \end{aligned} \tag{2.2}$$

Suppose that we are interested in the first k structural shocks, where $k < n$. We decompose $\boldsymbol{\varepsilon}_t$ into $\boldsymbol{\varepsilon}_t = [\boldsymbol{\varepsilon}_{1,t},\, \boldsymbol{\varepsilon}_{2,t}]$ where $\boldsymbol{\varepsilon}_{1,t}$ is a $k \times 1$ vector containing the shocks of interest and $\boldsymbol{\varepsilon}_{2,t}$ is an $(n-k) \times 1$ vector of other structural shocks. The estimated covariance matrix of $\mathbf{u}_t$ in Equation (2.2) only provides $n(n+1)/2$ restrictions for the n^2 unknown elements of $\mathbf{B}$. In order to bring in additional restrictions that aid identification, Stock and Watson (2012) and Mertens and Ravn (2013) propose the "external instrument", or "proxy SVAR", approach, which exploits information outside the VAR model as proxy measures for the latent shocks to aid identification. Let $\mathbf{m}_t$ be a $l \times 1$, $(l \geq k)$ vector of instruments for $\boldsymbol{\varepsilon}_{1,t}$,[5] we assume that $\mathbf{m}_t$ satisfy the following conditions:

$$E(\mathbf{m}_t \boldsymbol{\varepsilon}'_{1,t}) = \Gamma \neq 0, \quad \text{rank}(\Gamma) = k \tag{2.3}$$

$$E(\mathbf{m}_t \boldsymbol{\varepsilon}'_{2,t}) = 0 \tag{2.4}$$

The former condition states that the external instruments are contemporaneously correlated with structural shocks of interest, where Γ is an $l \times k$ full column rank matrix. The latter condition requires the proxies to be uncorrelated with other structural shocks. Conditions (2.3) and (2.4) are referred to as *relevance* and *exogeneity* conditions of instruments, respectively. It is theoretically infeasible to assess the relevance and exogeneity conditions of the proxies as the structural shocks are unobservable. However, one can still test for the validity of instruments approximately by examining the relationship between these instruments and VAR innovations (Gertler and Karadi, 2015).

Let us consider the decomposition of $\mathbf{u}_t$ and $\mathbf{B}$ corresponding to the partitioning of the structural shocks. The relationship between the VAR innovations and structural shocks can be respecified as:

$$\begin{bmatrix} \mathbf{u}_{1,t} \\ (k \times 1) \\ \mathbf{u}_{2,t} \\ (n-k \times 1) \end{bmatrix} = \begin{bmatrix} \boldsymbol{\beta}_{11} & \boldsymbol{\beta}_{12} \\ (k \times k) & (k \times n-k) \\ \boldsymbol{\beta}_{21} & \boldsymbol{\beta}_{22} \\ (n-k \times k) & (n-k \times n-k) \end{bmatrix} \begin{bmatrix} \boldsymbol{\varepsilon}_{1,t} \\ (k \times 1) \\ \boldsymbol{\varepsilon}_{2,t} \\ (n-k \times 1) \end{bmatrix} \tag{2.5}$$

where $\boldsymbol{\beta}_{11}$ and $\boldsymbol{\beta}_{22}$ are non-singular matrices. Since $\boldsymbol{\beta}_1 = [\boldsymbol{\beta}'_{11}, \boldsymbol{\beta}'_{21}]'$ is a matrix of coefficients that correspond to the structural shocks of interest, our goal is to identify $\boldsymbol{\beta}_1$ and consequently, to perform impulse response functions using $\boldsymbol{\beta}_1$.

The set-up in (2.5) combined with the validity conditions in (2.3) and (2.4) imply that

$$E(\mathbf{m}_t \mathbf{u}'_{1,t}) = \Gamma \boldsymbol{\beta}'_{11} \quad \text{and} \quad E(\mathbf{m}_t \mathbf{u}'_{2,t}) = \Gamma \boldsymbol{\beta}'_{21}$$

so that

$$\boldsymbol{\beta}_{21} \boldsymbol{\beta}_{11}^{-1} [E(\mathbf{m}_t \mathbf{u}'_{1,t})]' = [E(\mathbf{m}_t \mathbf{u}'_{2,t})]' \tag{2.6}$$

[5] Mertens and Ravn (2013) assume that $\mathbf{m}_t$ and $\boldsymbol{\varepsilon}_{1,t}$ are of the same dimension k, i.e., $l = k$.

Then, estimation of the proxy SVAR follows a three-step procedure. First, we estimate a standard VAR model by least squares. Next, we regress reduced-form VAR innovations on the proxy $\mathbf{m}_t$. Third, we use the restrictions obtained in the second step to identify the SVAR model and in conjunction with additional constraints if necessary. It is common in the literature that data of external instruments $\mathbf{m}_t$ might span shorter period than the endogenous variables $\mathbf{X}_t$. As in Gertler and Karadi (2015), it is not always immediate to find available instrument $\mathbf{m}_t$ that matches the span of $\mathbf{X}_t$. However, it is possible to obtain the reduced-form system using the full length of $\mathbf{X}_t$ and regress the reduced-form residuals of interest on $\mathbf{m}_t$ only for the period when the proxy is available (Stock and Watson, 2018). They argue that using the longer period for the VAR enhances efficiency at all horizons. We follow in this fashion and estimate the reduced-form VAR using a full span of data regardless of the availability of the proxy. In the same vein, Noh (2017) argues that potential challenges when using external instruments including shorter span and measurement errors have minor consequence on the identification of structural shocks.[6]

Regarding confidence bands, in a series of papers related to the proxy SVAR framework, Mertens and Ravn (2011, 2013, 2014b) compute these using a residual-based wild bootstrap. This approach has been popular for providing inference including impulse responses and forecast error variance decomposition. However, as discussed in Lunsford (2015), the wild bootstrap generates pseudo-residuals and pseudo-instruments by changing the sign of the estimated VAR reduced-form errors and the instruments at randomly selected periods. This implies that the relationship between residuals and instruments is identical in every bootstrap replication and thus, accounts very little for sampling variation in the relationship between the instrument and the shocks. Hence, Jentsch and Lunsford (2019) modify the moving block bootstrap proposed by Brüggemann et al. (2016)[7]and show that it is asymptotically valid for proxy SVAR. For this reason, we present confidence bands produced from moving block bootstrap in the application of this chapter.[8]

[6]Alternatively, they show that one is able to evaluate the validity of the instrument with the longer data of $\mathbf{X}_t$ by assigning zero for missing data in $\mathbf{m}_t$.

[7]Jentsch and Lunsford (2019) take into account the censoring in the narrative proxy variables by applying the centering to non-zero observations and keeping the censored observations with values of zero.

[8]Details of these bootstrap algorithms can be found in the Appendices.

2.2.2 Identification

2.2.2.1 Identification scheme I - Identification of one policy shock with one instrument

In a simple case where one instrument is employed to identify a single structural shock, i.e $k = 1$ and $\boldsymbol{\varepsilon}'_{1,t}$ contains only one shock of interest, then the correlation between the external instrument and structural shock, Γ, and $\boldsymbol{\beta}_{11}$ are scalars. We can identify the impact coefficients and $\boldsymbol{\varepsilon}_{1,t}$ up to a sign convention on $E(\mathbf{m}_t\mathbf{u}_{1,t})$.

2.2.2.2 Identification scheme II - Identification of two policy shocks with two instruments

When using proxies to identify more than one policy shock of interest, additional restrictions are required. Mertens and Ravn (2013) provide a very comprehensive study for cases dealing with identification of two structural shocks of interest using two external instruments simultaneously. We present their identification scheme by considering the relationship between the VAR reduced-form errors, $\mathbf{u}_t$, and structural shocks, $\boldsymbol{\varepsilon}_t$, as follows

$$\mathbf{u}_{1,t} = \boldsymbol{\eta}\mathbf{u}_{2,t} + \mathbf{S}_1\boldsymbol{\varepsilon}_{1,t}$$
$$\mathbf{u}_{2,t} = \boldsymbol{\zeta}\mathbf{u}_{1,t} + \mathbf{S}_2\boldsymbol{\varepsilon}_{2,t}$$

Reconcile these expressions with (2.1) to yield:

$$\begin{pmatrix} \boldsymbol{\beta}_{11} & \boldsymbol{\beta}_{12} \\ \boldsymbol{\beta}_{21} & \boldsymbol{\beta}_{22} \end{pmatrix} = \begin{pmatrix} \mathbf{I} & -\boldsymbol{\eta} \\ -\boldsymbol{\zeta} & \mathbf{I} \end{pmatrix}^{-1} \begin{pmatrix} \mathbf{S}_1 & \mathbf{0} \\ \mathbf{0} & \mathbf{S}_2 \end{pmatrix} = \begin{pmatrix} \mathbf{I} + \boldsymbol{\eta}(I - \boldsymbol{\zeta}\boldsymbol{\eta})^{-1}\boldsymbol{\zeta} & \boldsymbol{\eta}(\mathbf{I} - \boldsymbol{\zeta}\boldsymbol{\eta})^{-1} \\ (\mathbf{I} - \boldsymbol{\zeta}\boldsymbol{\eta})^{-1}\boldsymbol{\zeta} & (\mathbf{I} - \boldsymbol{\zeta}\boldsymbol{\eta})^{-1} \end{pmatrix} \begin{pmatrix} \mathbf{S}_1 & \mathbf{0} \\ \mathbf{0} & \mathbf{S}_2 \end{pmatrix}$$

The restrictions in Equations (2.2) and (2.7)[9] produce estimates of $\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}, \boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11}$ and $\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}$, such that:

$$\begin{aligned}
&(A1) \qquad \boldsymbol{\beta}_{11}\mathbf{S}_1^{-1} = (\mathbf{I} - \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})^{-1} \\
&(A2) \qquad \boldsymbol{\beta}_{21}\mathbf{S}_1^{-1} = \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}(\mathbf{I} - \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})^{-1} \\
&(A3) \qquad \mathbf{S}_1\mathbf{S}'_1 = (\mathbf{I} - \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})\boldsymbol{\beta}'_{11}\boldsymbol{\beta}_{11}(\mathbf{I} - \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})',
\end{aligned}$$

[9] See the Appendices for derivations of these expressions.

With these solutions in hand, it is possible to identify the structural shocks, $\boldsymbol{\varepsilon}_{1,t}$. In addition to the relevance and exogeneity conditions, it is essential to determine the cross-correlation between instruments and other structural shocks of interest. If Γ is diagonal, each element of $\mathbf{m}_t$ can be used in isolation as in the case of $k = 1$, which is sufficient to identify the impulse responses of the structural shocks up to a sign convention. When Γ is not diagonal, Mertens and Ravn (2013) suggest the use of a Cholesky decomposition of $\mathbf{S}_1\mathbf{S}_1'$ to provide the additional restriction. This imposes a causal ordering on the variables $\mathbf{X}_t$. In particular, the authors assume that there is no immediate impact from one variable of interest to another where they discriminate the impact of average personal income tax rates (APITR) and average corporate income tax rates (ACITR) in a single system, i.e., ACITR does not affect APITR contemporaneously, hence APITR is ordered before ACITR in the VAR setting. Indeed, imposing orthogonality between the narrative personal income (corporate income) tax changes and structural corporate (personal) tax shocks appears unreasonable given that these two types of taxes are highly correlated in practice. The assumption implies that the ACITR reacts contemporaneously to an APITR shock directly through $\boldsymbol{\varepsilon}_{1,t}$ as well as indirectly through $\mathbf{u}_{2,t}$, whereas an ACITR shock can only impact the APITR indirectly through $\mathbf{u}_{2,t}$.

2.2.2.3 Identification scheme III - Identification of one policy shock with multiple instruments

Since it is not clear which instrument outperforms the others and is more likely to deliver shocks that are exogenous to other macroeconomic aggregates, we extend the analysis by considering the joint use of several proxies for a single shock of interest. As argued by Mertens and Ravn (2013), using different variables simultaneously as external instruments in the VAR framework mitigates the impact of noise in the instruments. It is also conceivable that identifying the effects of fiscal policy shocks using more than one instrument might exploit the information content of several approaches.

We adopt the two-stage least squares (2SLS) method to obtain identification by estimating $n - k$ auxiliary regressions of the non-fiscal residuals $u_{i,t}$ in $\mathbf{u}_{2,t}$ on fiscal VAR residuals $u_{j,t}$ in $\mathbf{u}_{1,t}$:

$$u_{i,t} = \gamma_i u_{j,t} + v_{i,t} \qquad \forall i \neq j$$

and instrumenting the fiscal residuals with the valid instruments where over-identification occurs, i.e., $l \geq k$. We test for over-identifying restrictions using Sargan statistics.[10] Similarly to other identification schemes, we examine the joint validity of the instruments by an F-test of overall significance for k auxiliary regressions of fiscal residuals in $\mathbf{u}_{1,t}$ on the instruments in $\mathbf{m}_t$.

2.2.3 Multipliers

The key issue in our study is to compute the fiscal multipliers. Following the large part of literature, we define the multiplier as the dollar response of output to a fiscal shock of size one dollar (see, for example, Blanchard and Perotti (2002), Auerbach and Gorodnichenko (2012), Caldara and Kamps (2017), Angelini et al. (2022)). The multiplier is calculated as follows:

$$\mathcal{M}_h^F = \frac{\Delta GDP_h}{\Delta F_0} \approx \frac{\Delta \ln (GDP_h)}{\Delta \ln (F_0)} \times \overline{\left(\frac{GDP}{F}\right)}$$

where F stands for either the fiscal spending G or taxes T (in levels), i.e., $F = \{G, T\}$ and GDP be the level of output. $\overline{\left(\frac{GDP}{F}\right)}$ is a policy shock-specific scaling factors for the fiscal shocks, i.e., we use $\overline{\left(\frac{GDP}{G}\right)}$ for the unexpected change in fiscal spending and $\overline{\left(\frac{GDP}{T}\right)}$ for the unexpected change in tax revenues. Since the fiscal variables and output enter the SVAR in logs, the responses are re-scaled by the sample average of output to the fiscal variable of interest to convert elasticities into dollar terms. Specifically, this amounts to an output multiplier corresponding to a government spending increase and a tax cut of one dollar. In our sample period, the scaling factors for the innovations in fiscal spending and tax revenues are 0.21 and 0.18, respectively.[11] As it is conventional for quarterly analysis, we compute the impulse responses and multipliers for a horizon of $H = 20$ quarters.

2.3 Data

In the empirical part of this chapter, our baseline model is a fiscal-only model. Specifically, we estimate a VAR model containing the following endogenous variables: net taxes,

[10]See Olea et al. (2021) for a comprehensive theoretical study on the use of more than one instrument for identification of a single shock.

[11]We adopt this definition of fiscal multipliers for the comparability of our results with those reported in the empirical literature. See Ramey and Zubairy (2018) for an alternative measure of fiscal multipliers.

government spending and GDP. These variables are expressed as log of real per capita federal government revenue or net taxes (τ_t), defined as the sum of federal tax receipts and contributions for social insurance net of transfers payments to persons; log of real per capita federal government spending (g_t), defined as the sum of federal government consumption and investment and log of real per capita GDP (y_t). Thus $\mathbf{X}_t = (\tau_t,\, g_t,\, y_t)'$ is the vector of endogenous variables in our baseline specification. We construct the variables as in Blanchard and Perotti (2002) but we use data at the federal level instead of total government since most of the instruments pertain to changes in federal government spending and net taxes. The dataset is taken from FRED and spans the period from 1950Q1 to 2019Q4 at a quarterly basis. We end the sample period in 2019Q4 to include the ZLB episodes. Although there are more recent data available, we choose to avoid observations related to the COVID-19 period since they can bias the VAR responses (Lenza and Primiceri, 2022). The model includes four lags for each endogenous variable.[12]

In addition to the fiscal-only model, we consider an alternative specification to control for the impact of monetary policy. A growing body of literature has provided evidence on how failing to take into account the interaction between systematic monetary policy and fiscal policy can yield misleading results (Rossi and Zubairy, 2011, Leeper and Leith, 2016). Indeed, it is conceivable that policymakers rely on a fiscal-monetary mix to stabilize financial markets and stimulate economic activity in general, hence studying monetary or fiscal policy in isolation could attribute macroeconomic fluctuations to incorrect sources. For this reason, we study to what extent the results change when using a fiscal-monetary model. This extended model includes five variables: net taxes, government spending, GDP, inflation and a measure of monetary policy stance. The additional variables are the inflation rate calculated from the consumer price inflation, π_t, defined as the four-quarter growth of consumer price index; and the 3-month T-bill rate, i_t.

Working with the proxy SVAR method, we identify government spending and tax shocks using a selection of instruments, which have featured substantially in discussions on fiscal shocks. The following section provides a description of these instruments. In the remainder of this chapter, a government spending shock and a tax shock refer to an exogenous spending increase and a tax cut, respectively. As far as the scale of responses is concerned, we normalize the shock size to one in each case.

[12]We select the lag length for our VAR estimations based on the Akaike Information Criterion (AIC) and as suggested by a large body of the literature. Schwarz Information Criterion (SC) suggests three lags but the results remain qualitatively similar.

2.3.1 Instruments for government spending shocks

The first instrument of government spending shocks is the federal defense spending news by Ramey (2011). This variable estimates the expected discounted value of defense spending due to foreign political events relative to the lagged value of nominal GDP. Ramey (2011) also proposes an alternative measure of government spending shocks which is drawn from the Survey of Professional Forecasters (SPF) from the Philadelphia Fed to compute forecast errors as the difference between forecasts and the actual series of government spending growth rate for the same period. During the period 1968Q4-1981Q2, SPF produce forecasts of nominal defense spending while predict real federal spending from 1981Q3 to the present. Ramey utilises the forecast errors instead of forecasts to get the data with a reasonably long sample period. The use of forecast errors also does not require rebasing or employing vintage data. Indeed, one problem with SPF data is that the base year of relevant variables changes several times during the sample and the SPF forecast does not reflect such changes (Perotti, 2011). Following Ramey's method, we obtain two series of forecast errors for one quarter ahead and four quarters ahead using the SPF forecasts made one quarter and four quarters earlier, respectively. However, to avoid the caveat of combining two distinctive time series, the sample period is limited to 1981Q3-2019Q4 so that the news shock variables refer only to federal spending.[13]

We also employ Blanchard and Perotti (2002) unanticipated fiscal spending shock, which is obtained from a Cholesky decomposition in VAR with government spending ordered first. The variable is then estimated as the part of contemporaneous government spending not explained by lagged values of control variables. Another instrument for government spending shocks is taken from Fisher and Peters (2010). They construct the series using excess returns to military contractors on defense stocks for the period starting in 1958. Recent work on the identification of government spending shocks also includes a measure of news to defense spending proposed by Ben Zeev and Pappa (2017). They build on Barsky and Sims (2011) and use the medium-horizon identification method to identify defense spending news as a shock that is orthogonal to defense spending and which maximally explains future variation in defense spending over a horizon of five years. The last proxy for government spending shocks is suggested by Gambetti (2012). This measure is estimated by summing up revisions of expectations, that is $\sum_{j=1}^{3}(E_t g_{t+j} - E_{t-1} g_{t+j})$, where $E_t g_{t+j}$ is the SPF forecast made at time t of real government spending growth rate from period $t+j-1$ to period $t+j$. Table 2.9 in the Appendices provides a summary of proxies for government spending shocks.

[13]We find very little difference in results using the full sample period 1968Q4-2019Q4 for the forecast errors.

2.3.2 Instruments for tax shocks

The first proxy for tax shocks is a measure of exogenous tax changes by Romer and Romer (2010). They construct several measures of tax changes based on presidential speeches and congressional reports and distinguish those series according to their motivation. Following Mertens and Ravn (2014b), we use the series of changes in tax liabilities that are not motivated by current or prospective short-run economic conditions relative to nominal value of GDP, i.e., the variable $EXOGENRRATIO$ in Romer's dataset. To address the issue of anticipation or fiscal foresight of economic shocks, we use the unanticipated shocks provided by Mertens and Ravn (2011), who split the measure of exogenous tax changes (Romer and Romer, 2010) into anticipated and unanticipated shocks based on the time difference between the passing date of the legislation and when it was implemented. They retain the tax shocks for which the implementation lag is less than 90 days as unanticipated shocks.[14] We also exploit the implicit tax rate implied by municipal bond spreads by Leeper et al. (2013) as one of the potential instruments. Data availability and calculation procedure of the instruments for tax shocks are shown in Table 2.10 in the Appendices.

To assess the robustness of fiscal multipliers to using forward-looking variables as instruments for fiscal shocks, we also consider the consumer sentiment index and its sub-indices from the University of Michigan's Survey of Consumers. In particular, our focus is on the expected change in one's personal financial situation in a year, which can be denoted as $PFE_{t|t-4}$ (where $PFE_{t|s}$ represents the expectations at time t collected at time s). For ease of notation, henceforth we use PFE_t in our specifications. The series is measured via responses to the following question: "Now looking ahead – do you think that a year from now you (and your family living there) will be better off financially, worse off, or just about the same as now?". The variable is calculated as the relative scores (the percent giving favorable responses minus the percent giving unfavourable responses, plus 100), and then this is divided by the base period value of 1966 to form the index. We choose this over another series spanning a similar horizon which quantifies expected

[14]As argued by Mertens and Ravn (2014), an instrument for unanticipated tax shocks should not contain any predictable components that are independent of surprise tax movements. Thus, they propose a measure of anticipation adjusted proxy estimated as residuals in regression of the non-zero observations of tax narrative on four lags of the implicit tax rate based on municipal bond spreads with the maturity of one year by Leeper et al. (2013). As a robustness check, we use the refined tax narrative taken from Mertens and Ravn (2012) and the original Romer series for exogenous tax changes to construct the proxy and refer to these as anticipation-adjusted Mertens-Ravn and anticipation-adjusted Romer-Romer series, respectively. We yield similar results to those obtained using the variables prior to transformations. The results are available upon request.

change in business conditions as a whole. As pointed out by Cochrane (1994), individuals tend to be more accurate in assessing their own financial position than in forming expectations regarding national economy. Along the same lines, Dominitz and Manski (2004) argue that the responses to questions about national business conditions are likely to be more volatile and less informative in comparison with those about personal finances. Nevertheless, the correlation between these two series exceeds 85 percent, and thus results using either of these alternative expectation variables are not qualitatively distinct.

2.4 Validity of the instruments

Relevance. We first check the relevance of each instrument for the structural shock that it intends to identify by assessing the relationship between the instrument and VAR innovations. The rationale of the test lies in that if the instruments are relevant for approximating exogenous changes in fiscal policy, the reduced-form residuals and the instruments should demonstrate a significant correlation.

Table 2.1 reports the results of the relevance test under identification scheme I using our baseline model by computing F-statistics from regressions of each reduced-form residual on the instruments. As Stock and Yogo (2005) show, a first-stage F-statistics below 10 could indicate the presence of weak instruments. Thus, we consider 10 as the recommended benchmark for the relevance test. Another concern related to fiscal policy shocks is the automatic feedback from economic activity to tax revenues. Given the unique nature of the relation between taxes and output, the surprise movements in economic activity or output are also reflected in the unexpected movements in tax revenues. For this reason, following Blanchard and Perotti (2002), we construct the "cyclically adjusted" reduced-form tax residual $u_t^{\tau *}$ by regressing the original reduced-form tax residual u_t^{τ} obtained from the first step on the GDP residual u_t^{y}. We report the results for both u_t^{τ} and $u_t^{\tau *}$ in Table 2.1.

Table 2.1: Relevance of the instruments using a fiscal-only model.

3-variable VAR, $\mathbf{u}_t = [u_t^\tau, u_t^g, u_t^y]$	u_t^τ	$u_t^{\tau *}$	u_t^g	u_t^y
Instruments for spending shocks				
Ramey Defense News	0.019	1.488	*11.967******	0.999
1-Quarter SPF Forecast Errors	0.005	0.158	*332.462******	4.069**
4-Quarter SPF Forecast Errors	5.839**	5.580***	*15.233******	1.236
Blanchard-Perotti Shocks	2.016	0.000	–	8.998***
Revisions of Expectations	4.534**	4.448***	1.834	0.531
Fisher-Peter Shocks	0.091	0.042	0.057	0.781
Ben Zeev-Pappa Shock	*15.297******	7.624***	0.843	9.546***
Instruments for tax shocks				
Romer-Romer Tax Shocks	9.428***	*14.681******	1.191	0.876
Mertens-Ravn Tax Shocks	4.828**	*10.934******	0.344	3.508*
Financial Expectations	0.021	0.021	0.880	*28.270******
Implicit Tax Rate	1.695	1.695	0.155	0.954

Notes: The values in the table refer to the F-statistics yielded from the regressions of the reduced-form residuals on each proxy from a three-variate VAR model which contains net taxes, government spending and GDP. The results in column $u_t^{\tau *}$ are obtained from regressions of u_t^τ on each instrument, adjusted for impact of surprise movements in output on taxes. The estimation follows two steps: (1) Regress the VAR tax shocks and the instruments on GDP shocks and obtain the residuals from these equation, we call these auxiliary residuals r_t^τ and r_t^m, respectively; (2) Regress r_t^τ on r_t^m to obtain the F-statistics. *,** and *** indicate significance at 10, 5 and 1-percent level, respectively.

The F-statistics exceeding 10 are italicized in the table. Regarding the instruments for fiscal spending shocks, the one-quarter-ahead SPF shocks and four-quarter-ahead SPF shocks constructed by Ramey (2011) are strong instruments with the F-statistics of 332.462 and 15.233, respectively. These values are well above the conventional threshold of 10, thus indicating that the VAR innovations are to a large extent predicted by the forecast errors. The strength of the defense spending news constructed by Ramey (2011) is less pronounced, with F-statistics standing at 11.967. The F-statistics reported for revisions of expectations are significant at 5-10 percent for tax shocks but they are well below the threshold of 10. The relevance test for Fisher and Peters (2010) shocks suggests these variables might not be valid proxies for spending shocks. The F-statistics for the Blanchard and Perotti (2002) shocks are not available as they are perfectly correlated with the government spending residual u_t^g by construction.

Turning to tax revenues, the first column of Table 2.1 shows that most of the instruments display F-statistics well below 10. The measure of spending news by Ben Zeev and Pappa (2017) appears to be a relevant proxy for tax shock with an F-statistic of 15.297. Nevertheless, the F-statistics computed from the regression of output residual on the same variable (9.546) indicates that this variable is also relevant for output. This result implies that the spending news by Ben Zeev and Pappa (2017) might not be an appropriate proxy for the fiscal shocks in this study. The second column of Table 2.1 reports F-statistics for regressions of VAR tax shocks on the external instruments when controlling for the effects of GDP on tax revenue. Interestingly, the F-statistics from regressions of tax shocks on the tax narrative by Romer and Romer (2010) and Mertens and Ravn (2013) exceed the threshold of 10, thus suggesting these narrative series are relevant to the tax shocks when the effect of GDP innovations is absent from surprise changes in tax revenue. On the other hand, the personal finance expectation variable could be a potentially strong instrument for GDP, with an F-statistic equal to 28.270 for the baseline model. The Leeper et al. (2013) implicit tax rate appears to be uncorrelated with all structural shocks as can be seen in Table 2.1.

Exogeneity. Next, we provide empirical evidence on the exogeneity test. Caldara and Kamps (2017) suggest a test for the exogeneity condition of the instruments by regressing them on proxies for non-policy shocks associated with output and inflation. Proxies are

Fernald (2012) technology series, which measures total factor productivity adjusted for changes in factor utilization, m_{tfp}, the oil shock constructed by Hamilton (2003), m_{oil}, and the monetary policy shocks by Romer and Romer (2004), $m_{monetary}$. Note that the monetary shocks series is extended through 2006 by Barakchian and Crowe (2013).[15] A summary of the non-fiscal shocks is presented in Table 2.11 in the Appendices.

In Table 2.2, we document the results of the exogeneity test for the candidate instruments. Based on the F-tests constructed, we fail to reject the null hypotheses of insignificant regression coefficients for most of the proxies, implying that these proxies are uncorrelated with contemporaneous movements in external instruments for other shocks. Only the F-statistic from regression of the defense news by Ben Zeev and Pappa (2017) on non-policy proxies is greater than 10. The regression in which personal financial expectation enters as dependent variable also yields a statistically significant F-statistic equal to 6.336.

Correlations among estimated structural shocks and instruments. Looking at results of the relevance and exogeneity tests, it appears that there might exist more than a single valid instrument for the shocks of interest. To provide additional evidence on the feasibility of using multiple valid instruments for the same shock and if some instruments may be valid for both spending shock and tax shock, we look at the correlations among estimated structural shocks and the valid instruments. We consider a variety of relevant correlation checks and provide the rationale for these checks one-by-one. The outcome of these exercises is documented in Table 2.3.

First, we address the question whether the valid instruments for the same shock are highly correlated. In case they do not exhibit remarkably high correlation, it is conceivable to expect that using some or all of them together enables us to enrich the identification of fiscal shocks. Panel A in Table 2.3 displays correlations among the valid instruments employed in our analysis. The results suggest that there are low correlations between instruments for government spending shocks and tax shocks. These are evident with the values ranging between -0.100 to 0.070. We also observe close to zero correlations

[15] Stock and Watson (2012) test the exogeneity assumption using a similar approach. They compute the correlation between instruments for the structural shocks of interest and each estimated structural shock is obtained as the predicted value from a regression of its instrument on the reduced-form residuals. The results based on this approach are in line with our findings.

Table 2.2: Exogeneity of the instruments

	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RE}	m_t^{FP}	m_t^{BZP}	m_t^{RR}	m_t^{MR}	m_t^{PFE}	m_t^{LWY}
F-statistic	1.789	0.825	1.703	1.418	0.162	1.988	10.086	1.905	0.192	6.336	0.699
p-value	(0.152)	(0.483)	(0.881)	(0.242)	(0.922)	(0.118)	(0.000)	(0.131)	(0.902)	(0.000)	(0.396)

Notes: The table reports F-statistics and corresponding p-values for the regressions of each instrument for the fiscal shocks on the non-fiscal proxies which include the technology shocks, oil shocks and monetary policy shocks. Each regression includes a constant. The instruments are of the following order:

- Government spending shocks: Ramey (2011) spending news (m_t^R), one-quarter-ahead SPF error (m_t^{SPF1}), four-quarter-ahead SPF error (m_t^{SPF4}), Blanchard and Perotti (2002) spending shocks(m_t^{BP}), revisions of expectations (m_t^{RE}), Fisher and Peters (2010) excess return (m_t^{FP}) and Ben Zeev and Pappa (2017) news shocks (m_t^{BZP}).
- Tax shocks: tax narrative by Romer and Romer (2010) (m_t^{RR}), unanticipated tax narrative by Mertens and Ravn (2012) (m_t^{MR}), personal financial expectation (m_t^{PFE}), Leeper et al. (2013) implicit tax rate (m_t^{LWY}).

between instruments used to identify spending shocks, except for the correlation between one-quarter-ahead and four-quarter-ahead forecast errors.

Next, we check the correlations among the estimated structural shocks. As discussed by Stock and Watson (2012), if there are more than one valid proxies that can be used to identify the same shock, the shocks identified by these proxies should be perfectly correlated in population. Likewise, the proxies used to identify distinct shocks are considered valid if the shocks produced by these proxies are uncorrelated in population. This is implausible, however, in a finite sample. While the correlations being high confirms the validity of instruments, they also point that using all instruments together may give us a better estimate of the structural shock. The estimated structural shocks are obtained as the predicted value from regressions of the instruments on the VAR residuals.[16] We report these results in panel B of Table 2.3. The estimated structural shocks are obtained as the predicted value from regressions of the instruments on relevant VAR residuals. We find high correlations among the estimated spending shocks identified using the instruments within this category with degree of correlation exceed 0.70 in most pairwise combinations. Interestingly, the correlation between the estimated shocks identified using the one-quarter-ahead forecast errors and Blanchard and Perotti (2002) shocks is as large as 0.988. Similarly, we observe a high degree of correlation between the estimated tax shocks identified by Romer and Romer (2010) and Mertens and Ravn (2012) narrative tax series.

Panel B of Table 2.3 also reports that when spending and tax shocks are estimated separately each using one instrument, the cross-correlation between the estimated fiscal spending and tax shocks are almost always negative; and the magnitude of these correlations are quite large in some cases. Specifically, the correlations between the spending shock identified using Ramey (2011) instrument and the tax shocks identified using Romer and Romer (2010) and Mertens and Ravn (2012) instruments (-0.416 and -0.315,

[16] Since the VAR residuals are linear combinations of the structural shocks, their column space is the same as the column space of the structural shocks, so the orthogonal projection of each instrument in the column space of the VAR residuals is the same as the orthogonal projection of that instrument in the column space of the structural shocks. Since the structural shocks are independent of each other, this orthogonal projection is the sum of the orthogonal projections of the instrument on each of the structural shocks. This, together with equations (2.3) and (2.4) imply that the orthogonal projection of the instrument m_t in the column space of the structural shocks is simply the orthogonal projection of m_t on $\varepsilon_{1,t}$. Hence, the predicted value of the regression of m_t on VAR residuals would be proportional to $\varepsilon_{1,t}$.

Table 2.3: Correlations among estimated structural shocks and instruments using a fiscal-only model.

A. Correlations among instruments

	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RR}	m_t^{MR}
Instruments for spending shocks						
m_t^R	1.000					
m_t^{SPF1}	0.197	1.000				
m_t^{SPF4}	0.060	0.510	1.000			
m_t^{BP}	-0.04	0.495	0.082	1.000		
Instruments for tax shocks						
m_t^{RR}	-0.008	0.007	-0.022	-0.058	1.000	
m_t^{MR}	-0.011	0.054	0.012	-0.044	0.685	1.000

B. Correlations among estimated structural shocks

	ε_t^R	ε_t^{SPF1}	ε_t^{SPF4}	ε_t^{BP}	ε_t^{RR}	ε_t^{MR}
Estimated spending shocks						
ε_t^R	1.000					
ε_t^{SPF1}	0.727	1.000				
ε_t^{SPF4}	0.571	0.754	1.000			
ε_t^{BP}	0.764	0.988	0.679	1.000		
Estimated tax shocks						
ε_t^{RR}	-0.416	-0.155	-0.356	-0.145	1.000	
ε_t^{MR}	-0.315	0.061	-0.133	-0.066	0.934	1.000

C. Correlations between estimated structural shocks and instruments

	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RR}	m_t^{MR}
Estimated spending shocks						
ε_t^R	–	–	–		-0.052	-0.036
ε_t^{SPF1}	–	–	–		-0.071	0.059
ε_t^{SPF4}	–	–	–		-0.112	-0.053
ε_t^{BP}	–	–	–		-0.161	0.037
Estimated tax shocks						
ε_t^{RR}	-0.108	-0.058	-0.095	-0.056	–	–
ε_t^{MR}	-0.068	-0.015	-0.017	0.015	–	–

Notes: The table reports the cross-correlations among instruments and estimated structural shocks over their data availability. The estimated structural shocks are produced as the predicted values from regressions of the instruments on reduced-form residuals obtained from a three-variable VAR with taxes, government spending and GDP. The instruments are of the following order:

- Government spending shocks: Ramey (2011) spending news (m_t^R), one-quarter-ahead SPF error (m_t^{SPF1}), four-quarter-ahead SPF error (m_t^{SPF4}) and Blanchard and Perotti (2002) spending shocks (m_t^{BP}).
- Tax shocks: tax shocks by Romer and Romer (2010) (m_t^{RR}) and unanticipated tax narrative by Mertens and Ravn (2012) (m_t^{MR}).

respectively). There is no compelling reason to justify the negative correlation between structural spending and tax shocks. In fact, such correlation makes the interpretation of impulse responses and multipliers troublesome since it is not feasible to have a unit shock in spending while keeping the tax shock at its unconditional mean of zero. Hence, one might want to identify both shocks simultaneously, imposing that they should be orthogonal. This can be done by using the two instruments for the two structural shocks with $E(\mathbf{m}_t \varepsilon_{1t}') = \Gamma$ with rank 2, but without imposing that Γ must be diagonal.

Last, we examine the cross-correlation between estimated structural shocks and proxies when using jointly one single proxy for government spending shock and one single proxy for tax shock, following the same procedure in Stock and Watson (2012). We report these correlation coefficients in panel C of Table 2.3. The results provide no evidence of cross-correlation for all combinations of external instruments.[17] This finding is as expected since instruments for each structural shock are supposed to be orthogonal to other structural shocks. The negligible cross-correlations imply that we can use each of the two instruments in isolation and the structural shocks can be identified up to sign convention as in the case of $k = 1$. Nevertheless, we follow the same procedure by Mertens and Ravn (2013) which allows for non-zero cross-correlations.[18]

Following Mertens and Ravn (2013), we test for Granger causality between lagged endogenous variables and available proxies but did not detect any statistical significance.[19] Having assessed the validity of the instruments when they are used independently to identify fiscal shocks, we now proceed with the identification strategies by retaining only those which simultaneously satisfy relevance and exogeneity conditions. The instruments for government spending shocks include the Ramey defense spending news (m_R), the one-quarter forecast errors (m_{SPF1}), the four-quarter forecast errors (m_{SPF4}) (Ramey, 2011) and the spending shocks by Blanchard and Perotti (2002) (m_{BP}). The instruments selected for identification of tax shocks include Romer and Romer's narrative tax shocks

[17]The signs are negative since they refer to the association between tax revenues and government spending. Although the correlations between the Mertens and Ravn (2012) and some of the instruments for government spending shocks appear positive, the magnitude of these coefficients is minimal, i.e., less than 0.05, thus imposing no problem for our analysis.

[18]We also notice that, since these series are estimated entities from the system, computing exact standard errors for these correlations would be complicated. Therefore, we use $\pm 1.96/\sqrt{T}$ as a threshold for statistical significance.

[19]Results of the Granger causality test are reported in the Appendices.

(m_{RR}) (Romer and Romer, 2010) and Mertens and Ravn's unanticipated narrative tax shocks (m_{MR}) (Mertens and Ravn, 2014b). These variables are shown in panels A and B of Figure 2.9 in the Appendices, respectively. We now discuss the impulse responses of the endogenous variables to fiscal shocks for each identification strategy.

2.5 Results

2.5.1 Baseline model

In this section, we present the results obtained under various identification schemes for our baseline specification, which corresponds to a trivariate fiscal-only model containing government spending, net taxes and output.

Identification scheme I. We begin our analysis by exploring the basic proxy SVAR identification method which uses one instrument to identify a single structural shock. Figure 2.1 presents the impulse responses 20 periods ahead for the baseline model with three variables: net taxes, government spending and output.[20] The results are obtained using the identification strategy I which identifies structural fiscal spending and tax shocks using one single proxy at a time. As can be seen from Figure 2.1, output reacts positively on impact to a positive shock to government spending using different instruments. Noticeably, the results produced using Ramey (2011)'s anticipated defense spending series as an instrument for government spending shock is substantially larger and significantly different from those obtained from other instruments. We observe that regardless of the choice of instruments, output peaks after around one year, before it subsequently declines and bottoms after ten quarters. At longer horizons, the proxy SVAR model predicts that output effects eventually die out in response to government shocks.

Turning to the tax revenue shocks, the responses of output following a tax cut of one percent obtained from the proxy SVAR using Romer and Romer (2010) and Mertens and Ravn (2014b) are reproduced and reported in Figure 2.2. A negative tax shock raises output by 0.1 and about 0.3 on impact and to a maximum of 0.2 and 0.5 after six to eight

[20] Confidence intervals are not shown to keep the graph uncluttered. Nevertheless, we provide the confidence bands to the IRFs in Figure 2.10 and 2.11.

quarters, before reverting to trend when using Romer and Romer (2010) and Mertens and Ravn (2014b) series, respectively.

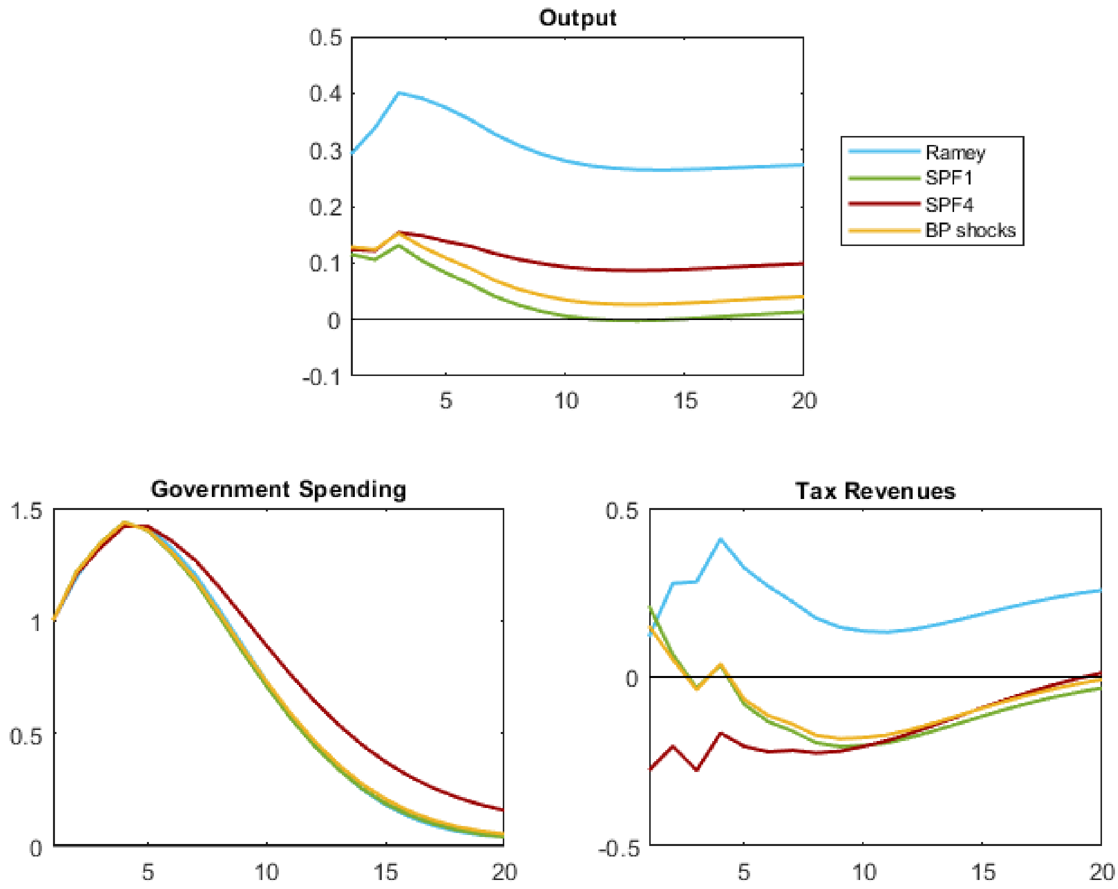


Figure 2.1: Impulse responses to a one percent increase in government spending shock under identification scheme I. Model with three variables including taxes, government spending and GDP. Ramey: Ramey (2011)'s defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks.

In Table 2.4, we report the fiscal multipliers together with the 68 percent confidence bands. We find that the on-impact spending multipliers are larger than one regardless of the instruments. This is aligned with the finding in Ramey and Zubairy (2018) and Angelini et al. (2022). We observe that the spending multipliers estimated by using Ramey (2011) defense spending series are noticeably larger than the ones obtained using other instruments. Specifically, these estimates remain persistent and statistically significant in the long run. On the contrary, the fiscal spending multipliers computed using the forecast

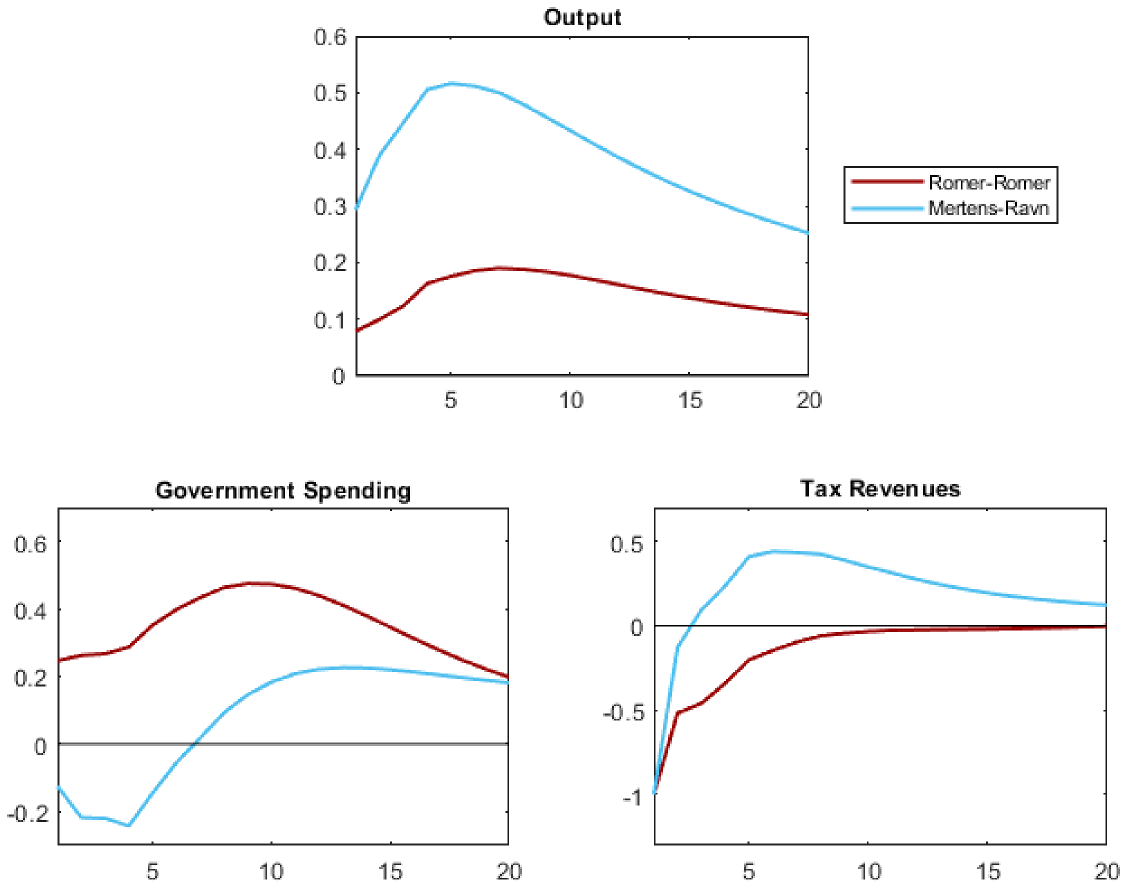


Figure 2.2: Impulse responses to a one percent decrease in tax shock under identification scheme I. Model with three variables including taxes, government spending and GDP. Romer-Romer: Romer and Romer (2010) exogenous tax shock, Merten-Ravn: Mertens and Ravn (2014b) unanticipated narrative tax shock.

errors and Blanchard and Perotti (2002) shocks as the instruments fall below one and become insignificant around three years after the shock occurs.

Looking at the fiscal tax multipliers, we find a substantial difference between the estimates computed using the Romer and Romer (2010) tax shocks and Mertens and Ravn (2013) series as the instrument for tax revenue. In particular, the tax multiplier computed using the Romer and Romer (2010) tax shocks is less than one on impact and becomes insignificant after four years.

We test the differences between these output multipliers and report the pairwise p-values at horizons from one to five years for the three-variable model in Table 2.5. We obtain several observations here. First, the response of output to spending shocks produced by the Ramey (2011) military news is statically significantly different from the others. Second, there is little evidence of significant differences across the spending multipliers obtained by the forecast errors and the Blanchard and Perotti (2002) shocks. These findings are consistent with our empirical results. We document the result on the test of the difference in tax multipliers in the lower panel of Table 2.5. There appears a statistically significant difference between the tax multipliers computed using Mertens and Ravn (2014b) and Romer and Romer (2010), thus implying the importance of incorporating anticipation effects into estimation.

Identification scheme II. We then consider the scenario that allows for joint identification of the government spending and tax shocks using a single instrument for each type of shock. For each combination of instruments for fiscal shocks, we present the estimates in response to a one-dollar increase in government spending and a one-dollar cut in tax revenue for the baseline VAR model. The results are reported in Figure 2.3.

Figure 2.3 shows that identifying both fiscal shocks simultaneously by imposing orthogonality has non-negligible consequences. When we pair each of the four instruments for spending shocks with each of the instruments for tax shocks, the magnitude of the spending multiplier becomes much smaller relative to those computed under identification scheme I. The multiplier even becomes negative after one period for the combination of Mertens-Ravn shock and SPF4. The tax multipliers on output remains persistently positive though declining over the long run, although the magnitude of the multiplier indicates that output response to a tax cut is remarkably sensitive to the selection of instruments at work. What we conclude from this analysis is that the significant negative correlation between the shocks reported in Table 2.3 is evidence that the model is misspecified, and the shocks that are estimated are not what we expect them to be. Moving to identification scheme II to force zero correlation is, therefore, not a satisfactory solution, it only rotates the shocks to absorb this negative correlation in one of the shocks.

Table 2.4: Fiscal multipliers: Identification I using a fiscal-only model.

	Spending Multipliers				Tax Multipliers	
Horizon	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RR}	m_t^{MR}
1	2.88	1.13	1.22	1.26	0.44	1.63
	[2.32, 3.35]	[1.03, 1.21]	[0.71, 1.24]	[1.17, 1.38]	[0.32, 0.64]	[1.34, 2.02]
4	3.85	1.02	1.46	1.26	0.91	2.82
	[2.74, 4.65]	[0.41, 1.44]	[0.34, 1.64]	[0.67, 1.77]	[0.60, 1.21]	[2.09, 3.28]
8	3.04	0.25	1.05	0.53	1.05	2.67
	[1.89, 4.07]	[-0.57, 0.97]	[-0.27, 1.51]	[-0.25, 1.34]	[0.48, 1.24]	[1.50, 2.78]
12	2.63	-0.01	0.86	0.27	0.90	2.15
	[1.56, 3.80]	[-0.85, 0.87]	[-0.49, 1.48]	[-0.54, 1.21]	[0.20, 0.93]	[0.74, 1.95]
16	2.63	0.04	0.89	0.31	0.73	1.72
	[1.52, 3.83]	[-0.87, 0.98]	[-0.52, 1.60]	[-0.56, 1.32]	[0.05, 0.67]	[0.32, 1.43]
20	2.69	0.13	0.98	0.40	0.60	1.40
	[1.55, 3.87]	[-0.79, 1.04]	[-0.46, 1.69]	[-0.49, 1.39]	[-0.01, 0.51]	[0.14, 1.10]

Notes: The table reports multipliers under identification scheme I for a three-variable VAR with taxes, government spending and GDP. Log-values of the GIRFs of output rescaled by the sample mean of output over public spending (both taken in levels) to convert percent changes into dollars. The bands are 68 percent confidence bands computed using 10,000 moving block bootstrap replications.

Table 2.5: Differences in multipliers: Identification I using a fiscal-only model.

	1 qrt	4 qrts	8 qrts	12 qrts	16 qrts	20 qrts
Spending Multipliers						
$m_t^R - m_t^{SPF1}$	0.007	0.002	0.008	0.002	0.000	0.010
$m_t^R - m_t^{SPF4}$	0.003	0.055	0.464	0.262	0.103	0.116
$m_t^R - m_t^{BP}$	0.001	0.000	0.001	0.640	0.692	0.517
$m_t^{SFP1} - m_t^{SPF4}$	0.402	0.461	0.987	0.196	0.134	0.516
$m_t^{SFP1} - m_t^{BP}$	0.349	0.326	0.519	0.633	0.684	0.732
$m_t^{SFP4} - m_t^{BP}$	0.549	0.655	0.704	0.115	0.089	0.116
Tax Multipliers						
$m_t^{MR} - m_t^{RR}$	0.067	0.085	0.008	0.082	0.010	0.020

Notes: The table reports p-values associated from testing the significance of the differences in multipliers computed using available instruments one at a time for the relevant shock. Model with three variables including taxes, government spending and output. The results are obtained from 10,000 moving block bootstrap replications.

Identification scheme III. We now turn to the case in which multiple instruments are used to identify one shock. Table 2.6 reports the strength of jointly using multiple instruments for government spending shocks including Ramey (2011)'s defense spending news, the one-quarter-ahead and four-quarter-ahead SPF forecast errors and Blanchard and Perotti (2002) shocks for the baseline three-variable VAR model. Results tabulated in the table suggest that these instruments are jointly sufficiently strong as they all exceed 10 (F-statistics exceeding 10 are italicized in the table). The Sargan statistics also provide evidence that we cannot reject the validity of the overidentifying restrictions for all combinations.

We report results for the baseline three-variable VAR in Figure 2.4 where we use jointly the instruments for spending shocks including Ramey (2011)'s defense spending news, the one-quarter-ahead SPF shock, four-quarter-ahead shock constructed by Ramey (2011) and Blanchard and Perotti (2002) unanticipated spending shock. In general, the joint use of these instruments suggests a spending multiplier that stands at 1.5 on impact, peaks at 1.8 after four quarters and reverts the trend in the long run. The two instruments for tax shocks are Romer and Romer (2010) exogenous tax shock and Mertens and Ravn (2012) unanticipated tax shock, which is a subset of the Romer-Romer shock. This explains the high correlation between these two variables, approximately 0.7. Hence, our exercise

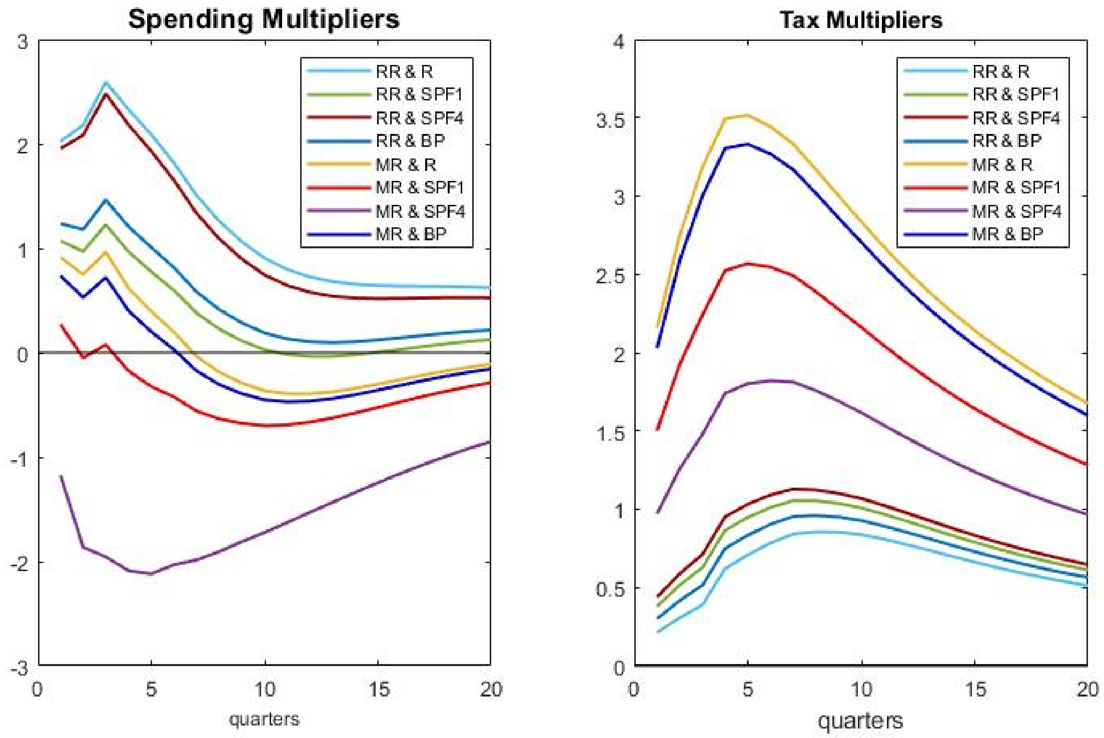


Figure 2.3: Responses of output to a one dollar increase in government spending and one dollar cut in taxes under identification scheme II. A three-variable model with taxes, government spending and output. Ramey: Ramey (2011) defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks, RR: Romer and Romer (2010) exogenous tax shock, MR: Mertens and Ravn (2014b) unanticipated narrative tax shock. The response is expressed as a multiplier.

in which multiple instruments for fiscal shocks are used simultaneously to estimate the output multiplier is limited to government spending shock.

2.5.2 Fiscal-monetary policy model

In this section, we show the results obtained using the extended model which accounts for the role of monetary policy. As described in Section 2.3, the fiscal-monetary policy model augments the baseline model by adding the consumer price inflation and a measure of monetary policy stance, i.e., the 3-month T-bill rate. Similar to the previous section, we check the validity of the instruments in this context and investigate three different identification schemes accordingly.

Table 2.6: Tests on the strength of multiple instruments for government spending shocks: Identification III using a fiscal-only model.

	u_t^{τ}	$u_t^{\tau *}$	u_t^{g}	u_t^{y}
Instruments: Ramey news, 1-quarter SPF forecast error, 4-quarter SPF forecast error, Blanchard-Perotti shocks				
F-statistic	2.940	4.103	*106.005*	3.082
p-value	(0.023)	(0.002)	(0.000)	(0.018)
Sargan p-value	0.295	0.295	-	0.276
Instruments: Ramey news, 1-quarter SPF forecast error, Blanchard-Perotti shocks				
F-statistic	3.930	5.164	*118.195*	4.071
p-value	(0.010)	(0.001)	(0.000)	(0.008)
Sargan p-value	0.347	0.473	-	0.766
Instruments: Ramey news, 4-quarter SPF forecast error, Blanchard-Perotti shocks				
F-statistic	0.147	3.324	*130.691*	4.071
p-value	(0.930)	(0.012)	(0.000)	(0.008)
Sargan p-value	0.860	0.356	-	0.532
Instruments: 1-quarter SPF forecast error, 4-quarter SPF forecast error, Blanchard-Perotti shocks				
F-statistic	4.021	5.081	*142.339*	3.339
p-value	(0.009)	(0.001)	(0.000)	(0.019)
Sargan p-value	0.721	0.849	-	0.601
Instruments: Ramey news, 1-quarter SPF forecast error				
F-statistic	0.092	4.365	*158.075*	2.653
p-value	(0.912)	(0.006)	(0.000)	(0.074)
Sargan p-value	0.532	0.569	-	0.437
Instruments: 1-quarter SPF forecast error, Blanchard-Perotti shocks				
F-statistic	0.121	4.162	*152.075*	2.027
p-value	(0.017)	(0.007)	(0.000)	(0.136)
Sargan p-value	0.418	0.696	-	0.698

Notes: Results are reported for a three-variable VAR model which contains government spending, net taxes and GDP. The results in column $u_t^{\tau *}$ are obtained from regressions of u_t^{τ} on the instruments, adjusted for impact of surprise movements in output on taxes. For each panel of the table, the three-variable VAR residuals are regressed on a different combination of instruments for government spending shocks. The F-statistics and the corresponding p-values refer to the overall significance tests of these equations. Over-identifying restrictions are tested using Sargan test, in which the estimated equation is $u_t^i = \phi u_t^g + v_t^i$, where u_t^i is the non-government spending VAR innovation indicated in the column of this table and ϕ is estimated using a two-stage least squares approach with proposed instruments. The Sargan p-values refer to the overall significance tests from regressions of $\hat{v}_t^i$ on the instruments.

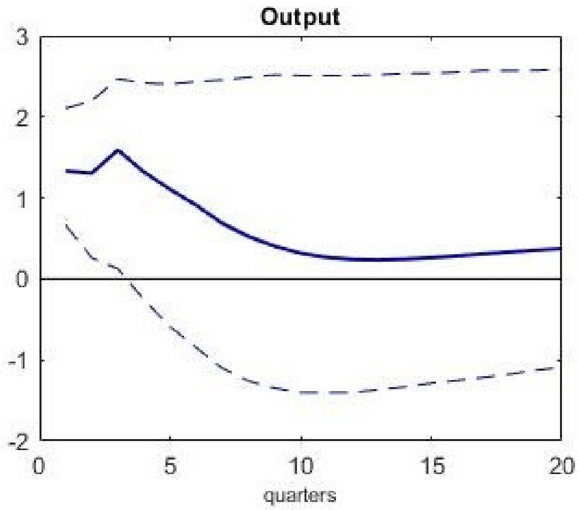


Figure 2.4: Responses of output to a one dollar increase in government spending under identification scheme III. The baseline three-variable model using government spending, net taxes and GDP. The external instruments are Ramey (2011)'s defense spending news, one-quarter-ahead and four-quarter-ahead SPF forecast errors and Blanchard and Perotti (2002) news as proxies for government spending shocks. The dashed lines show the 68 percent confidence bands based on 10,000 moving block bootstrap replications. The response is expressed as a multiplier.

Validity of the instruments. We conduct the relevance test and document the results in Table 2.7. In general, the relevance condition tests show little difference between the three-variable and five-variable VARs. The table demonstrates that the one-quarter-ahead SPF shocks and four-quarter-ahead SPF shocks constructed by Ramey (2011) are strongly relevant instruments with the F-statistics of 245.599 and 13.498, respectively. The defense spending news constructed by Ramey (2011) exhibits an F-statistics of 12.246, thus is also a relevant instrument. The F-statistics reported for revisions of expectations are well below the threshold of 10, although they are significant at 5-10 percent for tax shocks, whereas the Fisher and Peters (2010) shocks indicate little relevance to the government spending shock.

We obtain qualitatively similar findings to the fiscal-only model. The F-statistic obtained with spending news by Ben Zeev and Pappa (2017) exceeds the recommended threshold of 10, however, their F-statistics computed from regressions of other reduced-form residuals are also well above 10. This indicates that this variable may not be informative as an instrument for the fiscal shocks in our analysis. The results obtained for

Table 2.7: Relevance of the instruments using a fiscal-monetary model.

5-variable VAR, $\mathbf{u}_t = [u_t^\tau, u_t^g, u_t^y, u_t^\pi, u_t^i]$						
	u_t^τ	$u_t^{\tau *}$	u_t^g	u_t^y	u_t^π	u_t^i
Instruments for spending shocks						
Ramey Defense News	1.157	2.907	*12.246****	0.986	0.699	0.060
1-Quarter SPF Forecast Errors	0.156	0.578	*245.599****	4.078**	0.259	0.244
4-Quarter SPF Forecast Errors	6.351***	5.949***	*13.498****	0.168	3.108*	0.028
Blanchard-Perotti Shocks	1.190	0.042	–	*16.919****	1.465	0.065
Revisions of Expectations	2.842*	3.449**	1.627	0.161	0.642	0.028
Fisher-Peter Shocks	0.243	0.082	0.019	0.268	1.220	0.408
Ben Zeev-Pappa Shocks	*17.120****	7.388***	0.769	*13.710****	7.060***	*39.208****
Instruments for tax shocks						
Romer-Romer Tax Shocks	4.741**	8.329***	0.002	1.437	0.342	2.703
Mertens-Ravn Tax Shocks	3.397**	*10.119****	0.306	4.680**	0.527	0.144
Financial Expectations	0.217	0.131	0.673	8.491***	*11.797****	0.003
Implicit Tax Rate	2.954	0.540	0.152	5.649**	1.259	2.007

Notes: The values in the table refer to the F-statistics yielded from the regressions of the reduced-form residuals on each proxy from a five-variate VAR model which contains government spending, net taxes, GDP, consumer price inflation and 3-month T-bill rate. The results in column $u_t^{\tau *}$ are obtained from regressions of u_t^τ on each instrument, adjusted for impact of surprise movements in output on taxes. The estimation follows two steps: (1) Regress the VAR tax shocks and the instruments on GDP shocks and obtain the residuals from these equation, we call these auxiliary residuals r_t^τ and r_t^m, respectively; (2) Regress r_t^τ on r_t^m to obtain the F-statistics. *,** and *** indicate significance at 10, 5 and 1-percent level, respectively.

the cyclically adjusted reduced-form tax residual show that the tax narrative by Romer and Romer (2010) and Mertens and Ravn (2013) are more relevant instruments for tax shock when accounting for the surprise impacts of GDP on taxes. The personal finance expectation variable, interestingly, can serve as an instrument for GDP with an F-statistic equal to 8.491 when adding money market variables.

Correlations among estimated structural shocks and instruments. The estimates on the correlations for the five-variable model is reported in Table 2.13 in the Appendices. Similar to the trivariate model, the results indicates high degree of correlations among the estimated spending shocks identified using the valid instruments, as shown in panel A of the table. Similar to the fiscal-only model, the estimated structural spending and tax shocks are negatively correlated in most cases. Specifically, there is a large negative correlation between spending shocks ε_t^R and the tax shocks. Panel B of Figure 2.13 displays the correlations between the estimated structural shocks and instruments. We observe low, negative cross-correlation in most combinations as expected.

Identification scheme I. Figure 2.5 and Figure 2.6 plot the responses of the macroeconomic variables to innovations in government spending and taxes computed from the extended model.[21] As reported in Figure 2.5, the responses of output are robust to the inclusion of monetary policy-related variables except for the one obtained identified by the four-quarter-ahead forecast errors. In this case, output displays little-to-no reaction to an expansionary spending innovation. Figure 2.6 shows that even though output rises on impact to a tax shock, the responses turn negatively after two years when using Romer and Romer (2010) tax shock, whereas the one obtained using Mertens and Ravn (2012)'s unanticipated tax shock reverts to equilibrium after four years.

The fiscal multiplier estimates of the extended model are illustrated in Table 2.8. Similar to the baseline results, the spending multipliers computed using Ramey (2011)'s defense spending news are considerably larger than the estimates obtained using the other instruments. However, we notice that the spending multipliers computed using the professional forecast errors are less than one on impact when taking into account the monetary policy stance. Interestingly, the impulse response estimated using the four-quarter-ahead forecast errors are statistically insignificant at all horizons considered. The

[21] We provide the confidence bands to the IRFs in Figures 2.12 and 2.13.

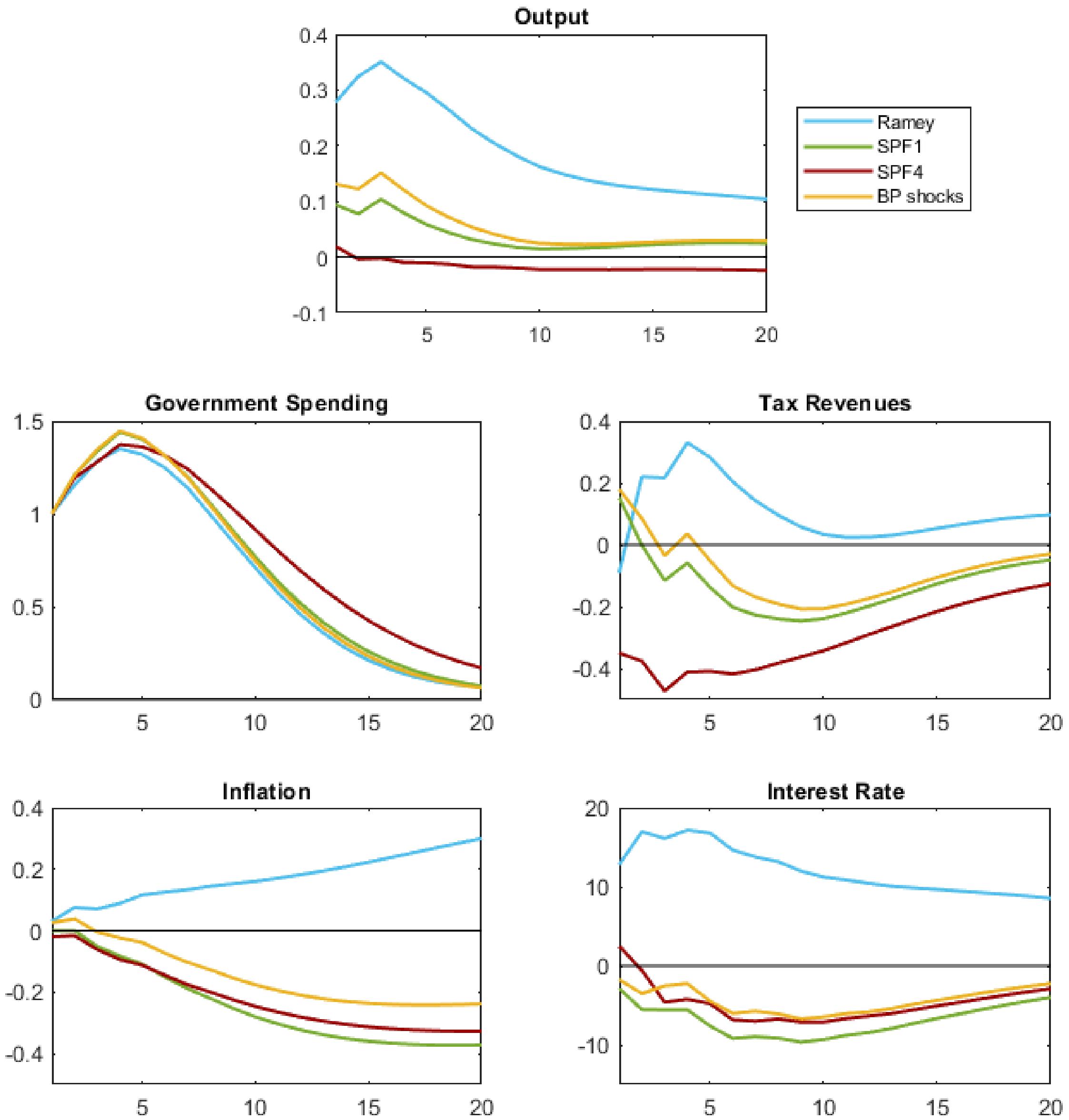


Figure 2.5: Impulse responses to a one percent increase in government spending shock under identification scheme I. A five-variable model with taxes, government spending, GDP, consumer price inflation and 3-month T-bill rate. Ramey: Ramey (2011) defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks.

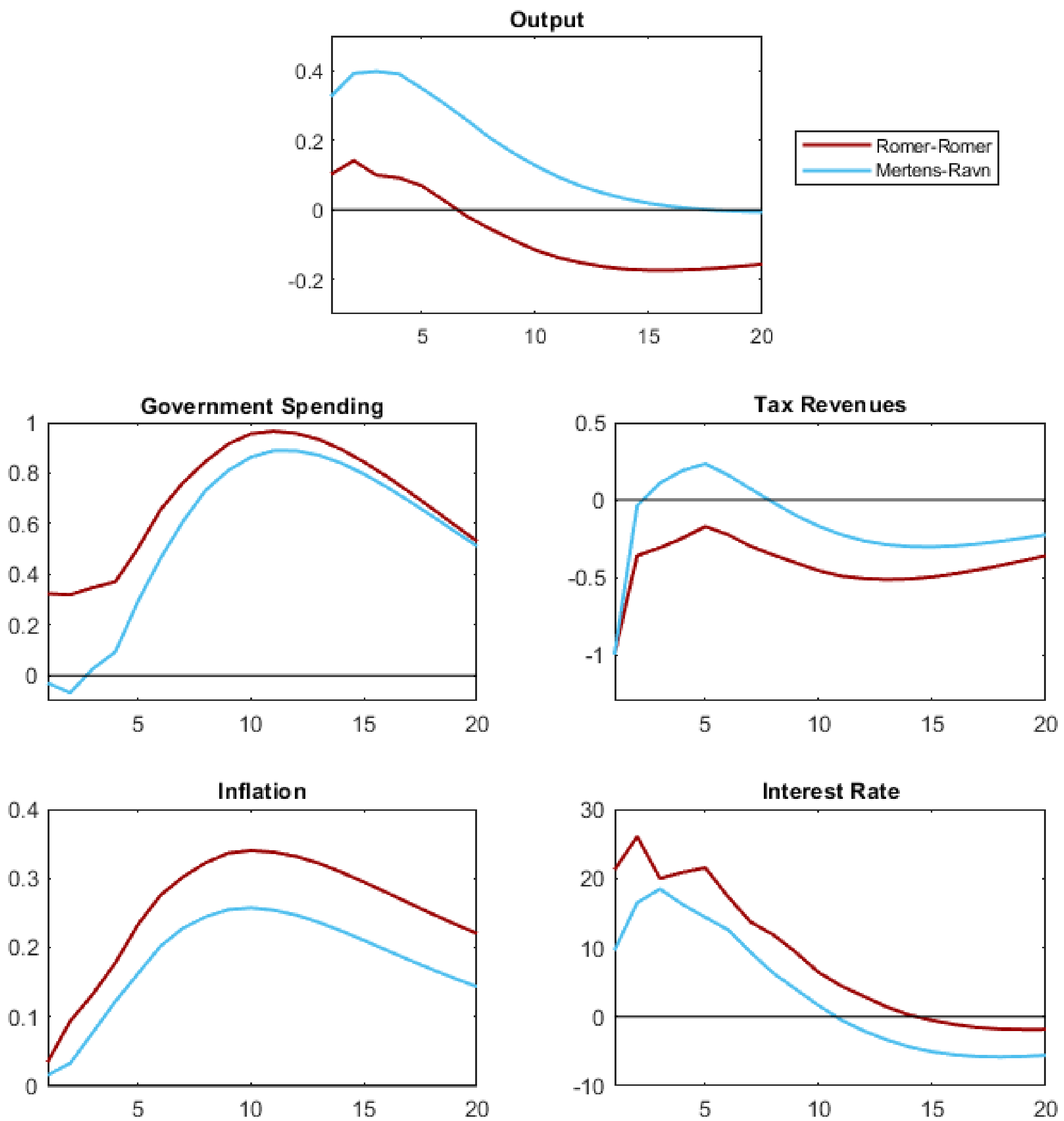


Figure 2.6: Impulse responses to a one percent decrease in tax shock under identification scheme I. A five-variable model with taxes, government spending, GDP, consumer price inflation and 3-month T-bill rate. RR: Romer and Romer (2010) exogenous tax shock, MR: Mertens and Ravn (2014b) unanticipated narrative tax shock.

last two columns of Table 2.8 document the estimates of tax multipliers. The results reveal qualitatively similar findings to our baseline model.

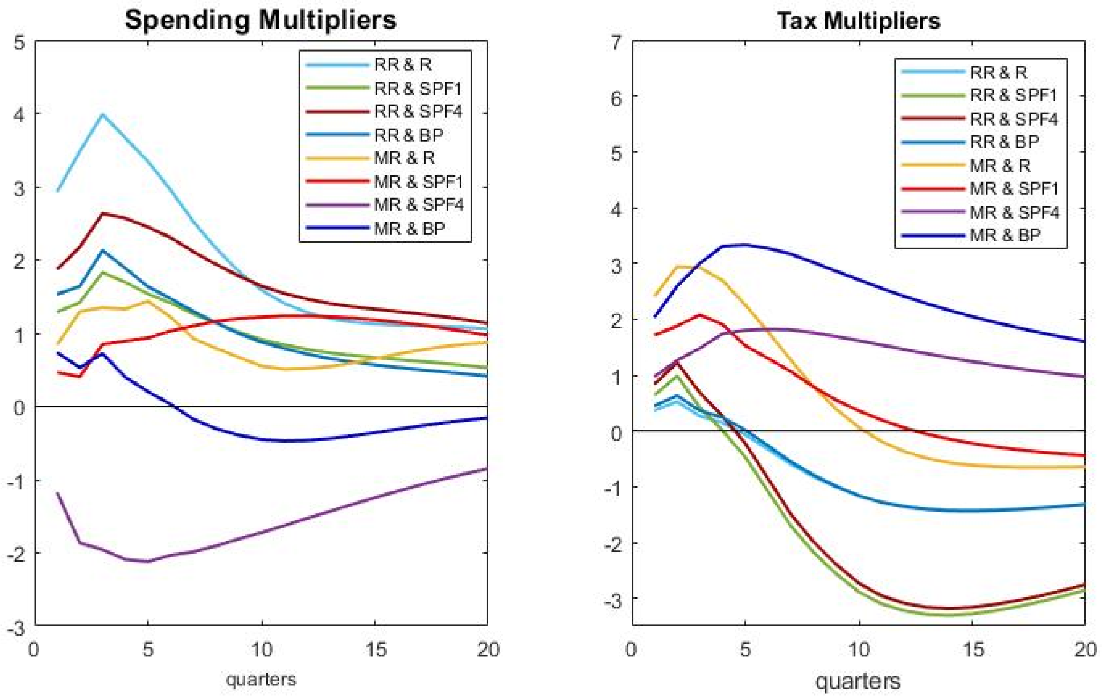


Figure 2.7: Responses of output to a one dollar increase in government spending and one dollar cut in taxes under identification scheme II. A five-variable model with taxes, government spending, output, consumer price inflation and 3-month T-bill rate. Ramey: Ramey (2011) defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks, RR: Romer and Romer (2010) exogenous tax shock, MR: Mertens and Ravn (2014b) unanticipated narrative tax shock. The response is expressed as a multiplier.

To test the difference among the fiscal multipliers for the five-variate model, we perform a similar exercise to the baseline model and report the results in Table 2.14 in the Appendices. The results detect little qualitative difference in comparison with the fiscal-only model, which indicates that the fiscal multiplier estimates are robust when taking into account the role of monetary policy.

Identification scheme II. Figure 2.7 illustrates the output multipliers in the extended fiscal-monetary policy model. The left panel of Figure 2.7 shows that the results of spending multiplier hold when including the monetary policy variables in all combinations. The right panel of Figure 2.7 indicates little robustness to the inclusion of the additional variables for tax multipliers. We also find that the inclusion of monetary policy variables

Table 2.8: Fiscal multipliers: Identification I using a fiscal-monetary model.

		Spending Multipliers			Tax Multipliers	
Horizon	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RR}	m_t^{MR}
1	2.74	0.93	0.19	1.30	0.57	1.82
	[2.01, 3,29]	[0.86, 1.10]	[-0.49, 0.33]	[1.22, 1.47]	[0.38, 1.06]	[1.38, 2.36]
4	3.17	0.78	-0.09	1.19	0.51	2.17
	[1.80, 3.86]	[0.23, 1.27]	[-1.32, 0.23]	[0.59, 1.68]	[0.13, 1.11]	[1.35, 2.66]
8	2.01	0.23	-0.18	0.4	-0.30	1.15
	[0.55, 2.77]	[-0.49, 1.01]	[-1.53, 0.40]	[-0.31, 1.17]	[-0.87, 0.13]	[0.17, 1.26]
12	1.37	0.16	-0.22	0.23	-0.85	0.38
	[-0.14, 2.35]	[-0.66, 1.19]	[-1.60, 0.58]	[-0.56, 1.27]	[-1.42, -0.28]	[-0.58, 0.50]
16	1.16	0.24	-0.21	0.28	-0.97	0.05
	[-0.37, 2.25]	[-0.62, 1.31]	[-1.54, 0.68]	[-0.54, 1.36]	[-1.37, -0.20]	[-0.75, 0.32]
20	1.03	0.25	-0.24	0.29	-0.87	-0.03
	[-0.41, 2.05]	[-0.55, 1.20]	[-1.43, 0.66]	[-0.48, 1.25]	[1.25, -1.07]	[-0.65, 0.32]

Notes: The table reports multipliers under identification scheme I for a five-variable VAR with taxes, government spending, GDP, inflation and 3-month T-bill rate. Log-values of the GIRFs of output rescaled by the sample mean of output over public spending (both taken in levels) to convert percent changes into dollars. The bands are 68 percent confidence bands computed using 10,000 moving block bootstrap replications.

induces contraction in the impact of tax shock on output. In addition, the response of real GDP turns negative after five quarters when using the exogenous tax shock by Romer and Romer (2010) together with a government spending instrument to identify the fiscal shocks simultaneously.

Identification scheme III. We provide evidence on the strength of multiple instruments for government spending shocks using the extended model in Table 2.15 in the Appendices. The results reveal similar findings. First, the instruments are jointly sufficiently strong based on the F-statistics. Second, the Sargan test suggests that the validity of the overidentifying restrictions is not rejected for all combinations.

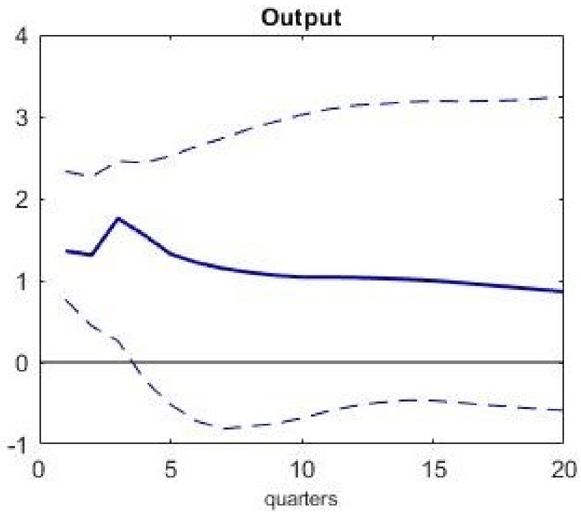


Figure 2.8: Responses of output to a one dollar increase in government spending under identification III. A five-variable model using government spending, net taxes, GDP, consumer price inflation and 3-month T-bill rate. The external instruments are Ramey (2011)'s defense spending news, one-quarter-ahead and four-quarter-ahead SPF forecast errors and Blanchard and Perotti (2002) news as proxies for government spending shocks. The dashed lines show the 68 percent confidence bands based on 10,000 moving block bootstrap replications. The response is expressed as a multiplier.

Figure 2.8 illustrates the estimated multipliers for the fiscal-monetary policy model in this scenario. Not surprisingly, the results are very similar when controlling for the role of monetary policy up to ten quarters following the government spending shock. As far as the confidence band is concerned, the inclusion of monetary variables does not change the qualitative features of the results. The spending multipliers remain statistically significant four quarters after the shock.

2.6 Conclusion

This chapter provides an examination of the identification of fiscal shocks using a structural VAR model with external instruments, a method developed by Mertens and Ravn (2013) and Stock and Watson (2012). We present an extensive comparative study on several alternative identification strategies within this proxy SVAR framework including (1) identification with one instrument and one fiscal shock; (2) identification with two instruments and two fiscal shocks and (3) identification with multiple instruments and one fiscal shock. Using data on the U.S. federal government spending and tax revenue over the period 1950Q1-2019Q4, we find evidence of differences between the responses computed using various instruments to identify government spending shock. Likewise, we show that the tax multipliers vary substantially when we employ different proxies by taking into account the anticipation effects, thus highlighting the importance of anticipations in quantifying the impact of fiscal policy shocks.

2.7 Appendices

2.7.1 Identification

We provide details on identification of the structural shocks of interest using the external instruments $\mathbf{m}_t$ as in Mertens and Ravn (2013). The covariance restriction (2.2) can be expressed as

$$\boldsymbol{\Sigma}_{uu'} = \mathbf{B}\mathbf{B}' = \begin{pmatrix} \boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12} & \boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{22} \\ \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12} & \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} \end{pmatrix} = \begin{pmatrix} \boldsymbol{\Sigma}_{11} & \boldsymbol{\Sigma}_{12} \\ \boldsymbol{\Sigma}_{21} & \boldsymbol{\Sigma}_{22} \end{pmatrix} \tag{2.7}$$

The expressions in (2.7) imply

$$\begin{aligned} \boldsymbol{\Sigma}_{21} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}_{11} &= \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}(\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}) \\ &= (\boldsymbol{\beta}_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})\boldsymbol{\beta}'_{12} \end{aligned}$$

Define

$$\begin{aligned} \mathbf{Z} &= (\boldsymbol{\beta}_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})(\boldsymbol{\beta}_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})' \\ &= \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &= \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} \\ &\quad - \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' + \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &= \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{21} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{11}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &\quad - \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &= \boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}(\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{21} + \boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{22}) - (\boldsymbol{\beta}_{21}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{12})(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &\quad + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}(\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11})(\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11} + \boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12})(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' \\ &= \boldsymbol{\Sigma}_{22} - \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}'_{21} - \boldsymbol{\Sigma}_{21}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})' + \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}_{11}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})'. \end{aligned}$$

The estimations of $\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12},\ \boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11},\ \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22}$ and $\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}$ are given by

$$
\begin{aligned}
\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12} &= \boldsymbol{\beta}_{12}[(\boldsymbol{\beta}_{22}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})^{-1}(\boldsymbol{\beta}_{22}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})]'[(\boldsymbol{\beta}_{22}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})(\boldsymbol{\beta}_{22}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{12})^{-1}]\boldsymbol{\beta}'_{12}\\
&= (\boldsymbol{\Sigma}_{21}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}_{11})'Z^{-1}(\boldsymbol{\Sigma}_{21}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}_{11})\\
\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11} &= \boldsymbol{\Sigma}_{11}-\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}\\
\boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} &= \boldsymbol{\Sigma}_{22}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})'\\
\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1} &= (\boldsymbol{\beta}_{12}\boldsymbol{\beta}'_{12}(\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})'+(\boldsymbol{\Sigma}_{21}-\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\Sigma}_{11})')(\boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22})^{-1}
\end{aligned}
$$

Then it is possible to obtain the identifications of impulse response functions to $\boldsymbol{\varepsilon}_{1t}$ by estimating $\boldsymbol{\beta}_{11}S_1^{-1}, \boldsymbol{\beta}_{21}S_1^{-1}$ and S_1S_1'

$$
\begin{aligned}
(A1) \qquad \boldsymbol{\beta}_{11}S_1^{-1} &= \mathbf{I}+\boldsymbol{\eta}(I-\boldsymbol{\zeta}\boldsymbol{\eta})^{-1}\boldsymbol{\zeta}\\
&= \mathbf{I}+\boldsymbol{\eta}\boldsymbol{\beta}_{21}S_1^{-1}\\
&= \mathbf{I}+\boldsymbol{\eta}(\mathbf{I}-\boldsymbol{\zeta}\boldsymbol{\eta})^{-1}S_2S_2^{-1}(\mathbf{I}-\boldsymbol{\zeta}\boldsymbol{\eta})\boldsymbol{\beta}_{21}S_1^{-1}\\
&= \mathbf{I}+\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}S_1^{-1}\\
&= \mathbf{I}+\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}S_1^{-1}\\
&= (\mathbf{I}-\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})^{-1}\\
(A2) \qquad \boldsymbol{\beta}_{21}S_1^{-1} &= \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}(\mathbf{I}-\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})^{-1}\\
(A3) \qquad S_1S_1' &= (\mathbf{I}-\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})\boldsymbol{\beta}'_{11}\boldsymbol{\beta}_{11}(\mathbf{I}-\boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1})',
\end{aligned}
$$

2.7.2 Alternative identification

This section provide an alternative analytical derivation of the parameters of interest that correspond to the structural shocks, $\boldsymbol{\beta}_1 = [\boldsymbol{\beta}'_{11},\, \boldsymbol{\beta}'_{21}]'$, using proxies m_t. $\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}$ can be estimated from the two-stage least squares as shown in the text. We denote $\boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}$ by Φ. Estimates of $\boldsymbol{\Sigma}_{11}$, $\boldsymbol{\Sigma}_{21}$ and $\boldsymbol{\Sigma}_{22}$ are also available from the reduced-form VAR. So the objective is to estimate $\boldsymbol{\beta}_{11}$ and $\boldsymbol{\beta}_{21}$ separately. Define

$$\mathbf{P} = \boldsymbol{\beta}_{11}\boldsymbol{\beta}'_{11} \quad ; \quad \mathbf{Q} = \boldsymbol{\beta}_{22}\boldsymbol{\beta}'_{22} \quad ; \quad \boldsymbol{\Pi} = \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}$$

The expressions in (2.6) and (2.7) imply

$$\boldsymbol{\Sigma}_{11} = \boldsymbol{\beta}_{11}\boldsymbol{\beta}_{11}' + \boldsymbol{\beta}_{12}\boldsymbol{\beta}_{22}^{-1}\boldsymbol{\beta}_{22}\boldsymbol{\beta}_{22}'(\boldsymbol{\beta}_{22}')^{-1}\boldsymbol{\beta}_{12}' = \mathbf{P} + \boldsymbol{\Pi}\,\mathbf{Q}\,\boldsymbol{\Pi}' \tag{2.8}$$

$$\boldsymbol{\Sigma}_{21} = \boldsymbol{\beta}_{21}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}\boldsymbol{\beta}_{11}' + \boldsymbol{\beta}_{22}\boldsymbol{\beta}_{22}'(\boldsymbol{\beta}_{22}')^{-1}\boldsymbol{\beta}_{12} = \Phi\,\mathbf{P} + \mathbf{Q}\,\boldsymbol{\Pi}' \tag{2.9}$$

$$\boldsymbol{\Sigma}_{22} = \boldsymbol{\beta}_{11}\boldsymbol{\beta}_{11}^{-1}\boldsymbol{\beta}_{11}\boldsymbol{\beta}_{11}'(\boldsymbol{\beta}_{11}')^{-1}\boldsymbol{\beta}_{21}' + \boldsymbol{\beta}_{22}\boldsymbol{\beta}_{22}' = \Phi\,\mathbf{P}\,\Phi' + \mathbf{Q} \tag{2.10}$$

These equations yields the following solutions:

$$\boldsymbol{\Pi} = (\boldsymbol{\Sigma}_{21}' - \boldsymbol{\Sigma}_{11}\Phi')(\boldsymbol{\Sigma}_{22} - \boldsymbol{\Sigma}_{21}\Phi')^{-1} \tag{2.11}$$

From (2.10),

$$\mathbf{Q} = \boldsymbol{\Sigma}_{22} - \Phi\,\mathbf{P}\,\Phi' \tag{2.12}$$

From (2.8) and (2.12),

$$\begin{aligned}
\boldsymbol{\Sigma}_{11} &= \mathbf{P} + \boldsymbol{\Pi}\,Q\,\boldsymbol{\Pi}' \\
&= \mathbf{P} + \boldsymbol{\Pi}(\boldsymbol{\Sigma}_{22} - \Phi\,P\,\Phi')\,\boldsymbol{\Pi}' \\
&= \mathbf{P} + \boldsymbol{\Pi}\,\boldsymbol{\Sigma}_{22}\,\boldsymbol{\Pi}' - \boldsymbol{\Pi}\,\Phi\,\mathbf{P}\,\Phi'\,\boldsymbol{\Pi}'
\end{aligned}$$

thus

$$\begin{aligned}
\mathrm{vec}[\boldsymbol{\Sigma}_{11} - \boldsymbol{\Pi}\,\boldsymbol{\Sigma}_{22}\,\boldsymbol{\Pi}'] &= \mathrm{vec}(\mathbf{P}) - [\boldsymbol{\Pi}\,\Phi \otimes \boldsymbol{\Pi}\,\Phi]\,\mathrm{vec}(\mathbf{P}) \\
&= [\mathbf{I}_{k^2} - (\boldsymbol{\Pi}\,\Phi \otimes \boldsymbol{\Pi}\,\Phi)]\,\mathrm{vec}(\mathbf{P}) \\
\Rightarrow \quad \mathrm{vec}(\mathbf{P}) &= [\mathbf{I}_{k^2} - (\boldsymbol{\Pi}\,\Phi \otimes \boldsymbol{\Pi}\,\Phi)]^{-1}\,\mathrm{vec}[\boldsymbol{\Sigma}_{11} - \boldsymbol{\Pi}\,\boldsymbol{\Sigma}_{22}\,\boldsymbol{\Pi}']
\end{aligned}$$

These solutions yield identification of impulse response functions to $\boldsymbol{\varepsilon}_{1,t}$. Similarly to the derivations proposed by Mertens and Ravn (2013), additional restrictions are required when there are more than one structural shock of interest.

2.7.3 Residual-based wild bootstrap

In this section, we present the algorithms for the widely used residual-based wild bootstrap proposed by Gonçalves and Kilian (2004). The bootstrap algorithm is as follows:

1. Generate bootstrap VAR residuals $\mathbf{u}_t^*$ using $\mathbf{u}_t^* = \hat{\mathbf{u}}_t \boldsymbol{\eta}_t$, and bootstrap proxies $\mathbf{m}_t^*$ using $\mathbf{u}_t^* = \mathbf{m}_t \boldsymbol{\eta}_t$, where $\boldsymbol{\eta}_t$ follows a Rademacher distribution with $E(\boldsymbol{\eta}_t) = 0, E(\boldsymbol{\eta}_t^2) = 1$ and $E(\boldsymbol{\eta}_t^4) < \infty$.[22]

2. Set the initial condition $(\mathbf{X}_{-p+1}^*, \ldots, \mathbf{X}_0^*) = (\mathbf{X}_{-p+1}, \ldots, \mathbf{X}_0)$.[23]

3. Generate pseudo-data $(\mathbf{X}_1^*, \ldots, \mathbf{X}_T^*)$ recursively using condition from previous step and estimated coefficient matrices $\widehat{\mathbf{A}}_j, j = 1, \ldots, p$ where

$$\mathbf{X}_t^* = \widehat{\mathbf{A}}_1 \mathbf{X}_{t-1}^* + \cdots + \widehat{\mathbf{A}}_p \mathbf{X}_{t-p}^* + \mathbf{u}_t^*.$$

4. Estimate the reduced-form model by least squares from the bootstrap sample and produce $\widehat{\mathbf{A}}_1^*, \ldots, \widehat{\mathbf{A}}_p^*$ and $\widehat{\mathbf{u}_t^*} = \mathbf{X}_t^* - \widehat{\mathbf{A}}_1^* \mathbf{X}_{t-1}^* - \cdots - \widehat{\mathbf{A}}_p^* \mathbf{X}_{t-p}^*$.

5. Use the pseudo-proxies generated from the previous step to identify the model and obtain the bootstrap statistics of interest.

2.7.4 Residual-based moving block bootstrap

This section illustrates the residual-based moving block bootstrap proposed by Jentsch and Lunsford (2019). The procedure is instigated by choosing a block length ℓ and let $N = [T/\ell]$ be the number of blocks such that $N\ell \geq T$. Next, collect the $n \times \ell$ blocks $\mathcal{U}_i = (\hat{\mathbf{u}}_i, \ldots, \hat{\mathbf{u}}_{i+\ell-1})$ and $k \times \ell$ blocks $\mathcal{M}_i = (\mathbf{m}_i, \ldots, \mathbf{m}_{i+\ell-1})$ for $i = 1, \ldots, T - \ell + 1$. Then, the algorithm is as follows:

1. Let $i_1, \ldots, i_N$ be i.i.d. random variables uniformly distributed on the set $\{1, \ldots, T - \ell + 1\}$. Collect the blocks $(\mathcal{U}_{i_1}, \ldots, \mathcal{U}_{i_N})$ and $(\mathcal{M}_{i_1}, \ldots, \mathcal{M}_{i_N})$ and discard the last $N\ell - T$ elements to generate the sets of residuals $\tilde{\mathbf{u}}_t^* = (\tilde{\mathbf{u}}_1^*, \ldots, \tilde{\mathbf{u}}_T^*)$ and proxies $\widetilde{\mathbf{m}}_t^* = (\widetilde{\mathbf{m}}_1^*, \ldots, \widetilde{\mathbf{m}}_T^*)$.

[22] As noted by Jentsch and Lunsford (2019), an alternative approach is to draw $\boldsymbol{\eta}_t$ from a standard normal distribution.

[23] See Mertens and Ravn (2013).

2. Center $(\widetilde{\mathbf{u}}_1^*, \dots, \widetilde{\mathbf{u}}_T^*)$ according to

$$\mathbf{u}_{j\ell+s}^* = \widetilde{\mathbf{u}}_{j\ell+s}^* - \frac{1}{T-\ell+1}\sum_{r=1}^{T-\ell}\widehat{\mathbf{u}}_{s+r-1}$$

for $s = 1, \dots, \ell$ and $j = 0, 1, \dots, N-1$ in order to produce bootstrap residuals $\mathbf{u}_t^* = (\mathbf{u}_1^*, \dots, \mathbf{u}_T^*)$.

3. Center $\mathbf{m}_t^* = (\widetilde{\mathbf{m}}_1^*, \dots, \widetilde{\mathbf{m}}_T^*)$ similarly to the VAR errors in the previous step in order to produce $(\mathbf{m}_1^*, \dots, \mathbf{m}_T^*)$.[24]

4. Set the initial condition $(\mathbf{X}_{-p+1}^*, \dots, \mathbf{X}_0^*) = (\mathbf{X}_{-p+1}, \dots, \mathbf{X}_0)$.

5. Generate pseudo-data $(\mathbf{X}_1^*, \dots, \mathbf{X}_T^*)$ recursively using condition from previous step and estimated coefficient matrices $\widehat{\mathbf{A}}_j, j = 1, \dots, p$ where

$$\mathbf{X}_t^* = \widehat{\mathbf{A}_1}\mathbf{X}_{t-1}^* + \cdots + \widehat{\mathbf{A}_p}\mathbf{X}_{t-p}^* + \mathbf{u}_t^*.$$

6. Estimate the reduced-form model by least squares from the bootstrap sample and produce $\widehat{\mathbf{A}_1^*}, \dots, \widehat{\mathbf{A}_p^*}$ and $\widehat{\mathbf{u}_t^*} = \mathbf{X}_t^* - \widehat{\mathbf{A}_1^*}\mathbf{X}_{t-1}^* - \cdots - \widehat{\mathbf{A}_p^*}\mathbf{X}_{t-p}^*$.

7. Use the pseudo-proxies generated from the previous step, $\mathbf{m}_t^*$, to identify the model and obtain the bootstrap statistics of interest.

2.7.5 List of instruments

[24] Jentsch and Lunsford (2019) only apply the centering to the non-zero observations in the proxies due to censoring in the Mertens and Ravn (2013) proxy variables. The censored values remain at zero.

Table 2.9: Instruments for government spending shocks

Name	**Sample Period**	**Construction Method**
Ramey Defense News (Ramey, 2011)	1950Q1-2015Q3	Expected discounted value of government spending changes due to foreign political events constructed from periodicals such as Business Week
Ramey 1-Quarter Forecast Errors (Ramey, 2011)	1981Q4-2019Q4	Forecast errors based on the difference between actual one-quarter real spending growth and the forecasted growth of the same period by professional forecasters
Ramey 4-Quarter Forecast Errors (Ramey, 2011)	1982Q3-2019Q4	Forecast errors based on the difference between actual four-quarter real spending growth and the forecasted growth of the same period by professional forecasters
Blanchard - Perotti spending shocks Blanchard and Perotti (2002)	1950Q1-2019Q4	Residuals from the regression of real potential government spending on lags of potential GDP and government spending
Fisher-Peters (Fisher and Peters, 2010)	1950Q1-2008Q4	Excess stock returns to military contractors
Revisions of Expectations (Gambetti, 2012)	1950Q1-2007Q4	Sum of forecast revisions collected from the Survey of Professional Forecasters
Ben Zeev-Pappa (Ben Zeev and Pappa, 2017)	1950Q1-2007Q4	Defense spending news identified as a shock that (i) is orthogonal to current defense spending; and (ii) best explains future movements in defense spending over a five-year horizon

Table 2.10: Instruments for tax shocks

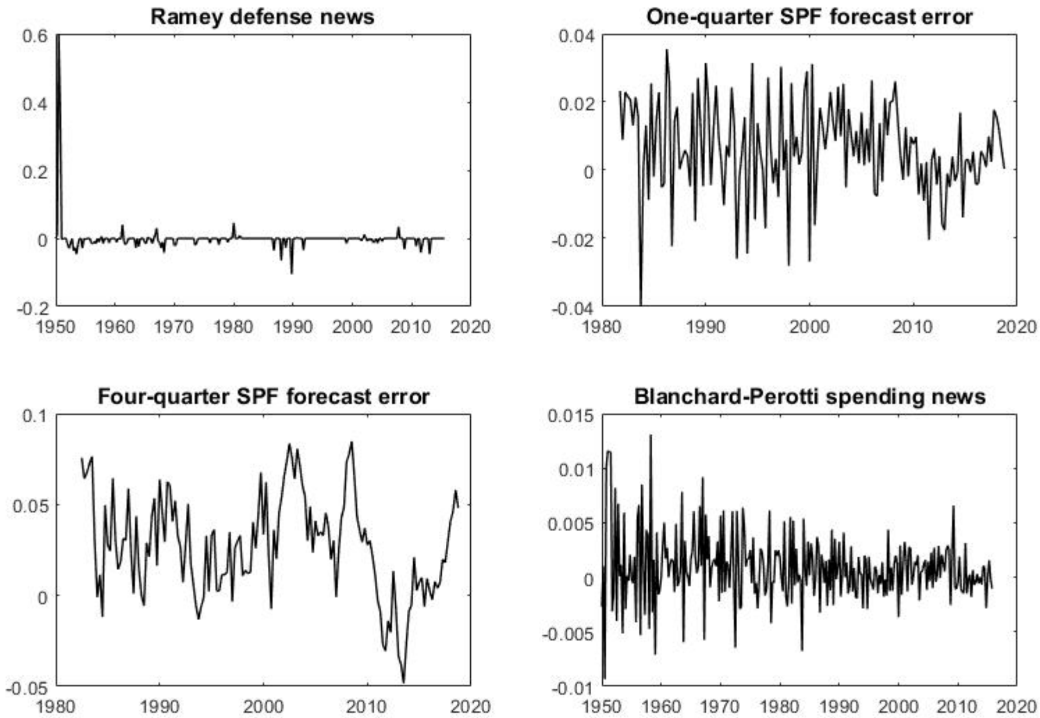
Name	Sample Period	Construction Method
Romer-Romer (Romer and Romer, 2010)	1950Q1-2006Q4	Exogenous tax changes relative to GDP
Anticipation Adjusted Romer-Romer	1950Q1-2006Q4	Residuals from regression of anticipated Romer-Romer tax narrative on lags of Leeper et al. (2013)'s implicit tax rate
Mertens-Ravn (Mertens and Ravn, 2012)	1950Q1-2007Q4	Exogenous tax changes based on that were implemented less than 90 days after becoming law
Anticipation Adjusted Mertens-Ravn	1950Q1-2007Q4	Residuals from regression of Mertens-Ravn tax narrative on lags of Leeper et al. (2013)'s implicit tax rate
Personal Financial Expectation	1960Q1-2019Q4	Expected change in one's personal financial situation in a year, extracted from the University of Michigan's Survey of Consumers
Implicit tax rate (Leeper et al., 2013)	1954Q1-2005Q4	Tax rate implied by the municipal bond spreads

Table 2.11: Instruments for non-fiscal shocks

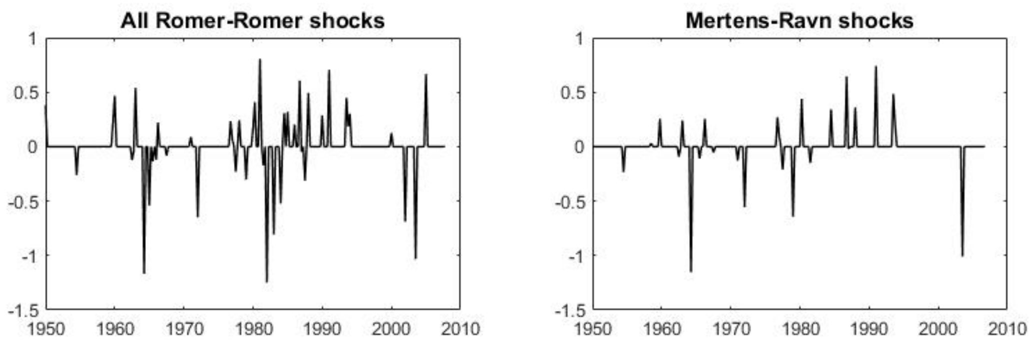
Name	Sample Period	Calculation Method
Technology shocks (Fernald, 2012)	195Q1-2006Q4	Total factor productivity adjusted for changes in factor utilization
Oil Shocks (Hamilton, 2003)	1950Q1-2006Q4	Based on a nonlinear transformation of the nominal price of crude oil
Monetary Policy Shocks (Romer and Romer, 2004)	1950Q1-2006Q4	Deviations of federal fund rate from its target, conditional on Greenbook forecasts

Figure 2.9: Instruments for fiscal shocks

(A): Instruments for government spending shocks



(B): Instruments for tax shocks



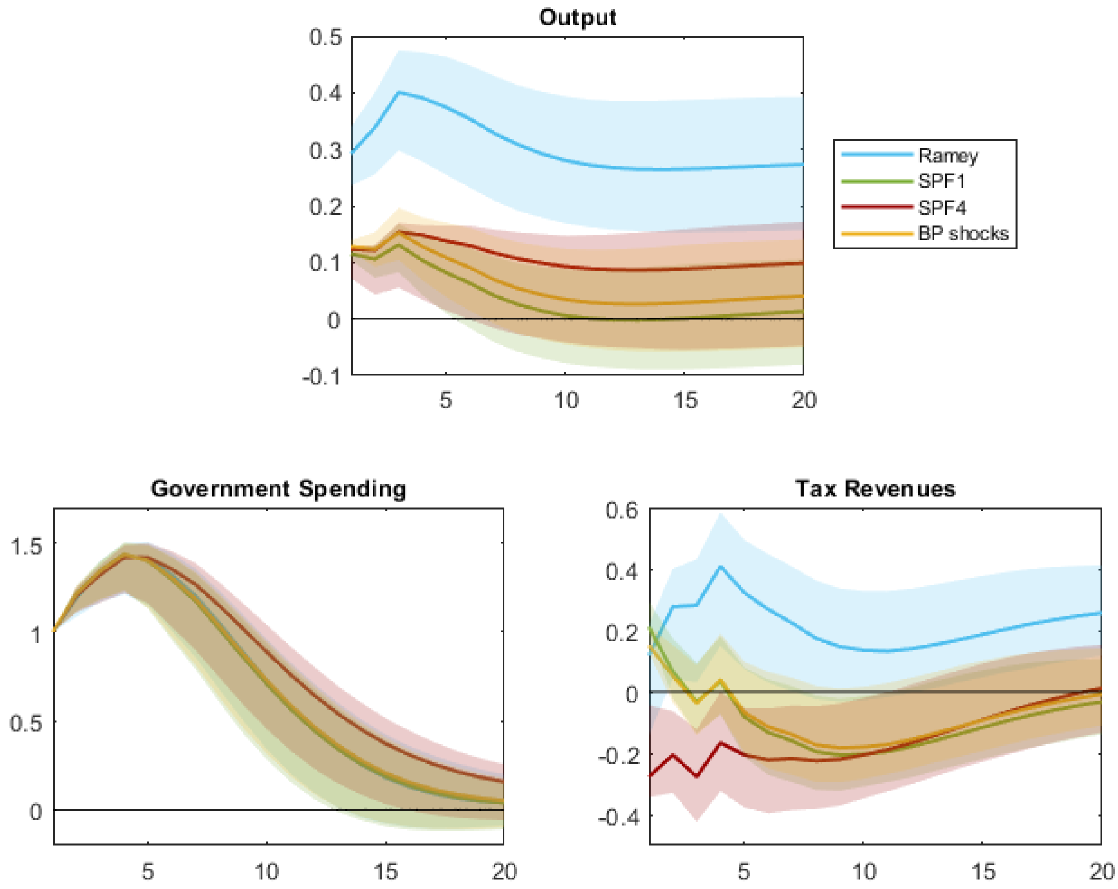


Figure 2.10: Impulse responses to a one percent increase in government spending shock under identification scheme I. Model with three variables including taxes, government spending and GDP. Ramey: Ramey (2011)'s defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks. The shaded areas display the 95 percent confidence band using a residual-based wild bootstrap.

2.7.6 Tests and additional results

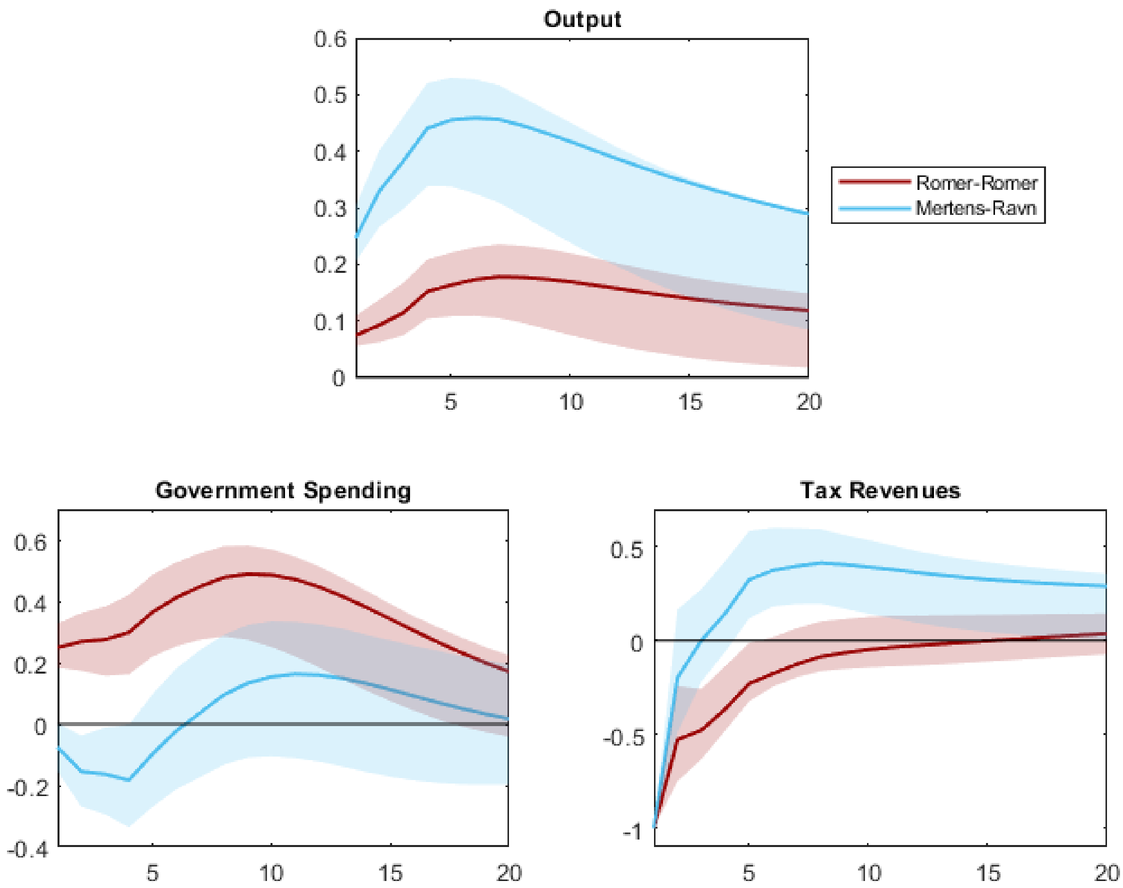


Figure 2.11: Impulse responses to a one percent decrease in tax shock under identification scheme I. Model with three variables including taxes, government spending and GDP. Romer-Romer: Romer and Romer (2010) exogenous tax shock, Merten-Ravn: Mertens and Ravn (2014b) unanticipated narrative tax shock. The shaded areas display the 95 percent confidence band using a residual-based wild bootstrap.

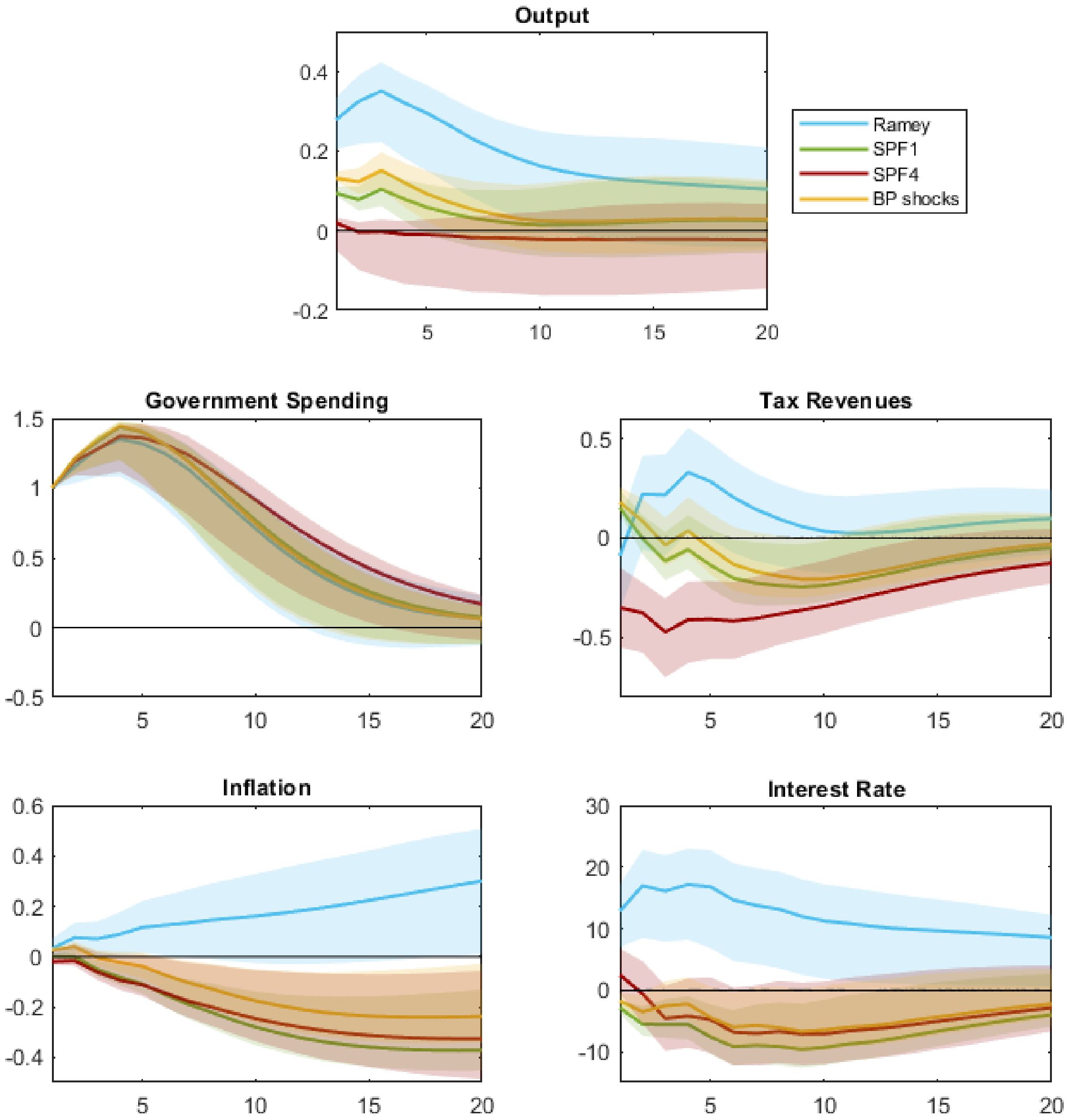


Figure 2.12: Impulse responses to a one percent increase in government spending shock under identification scheme I. A five-variable model with taxes, government spending, GDP, consumer price inflation and 3-month T-bill rate. Ramey: Ramey (2011) defense spending news, SPF1: 1-quarter-ahead federal spending forecast error, SPF4: 4-quarter-ahead federal spending forecast error, BP: Blanchard and Perotti (2002) spending shocks. The shaded areas display the 95 percent confidence band using a residual-based wild bootstrap.

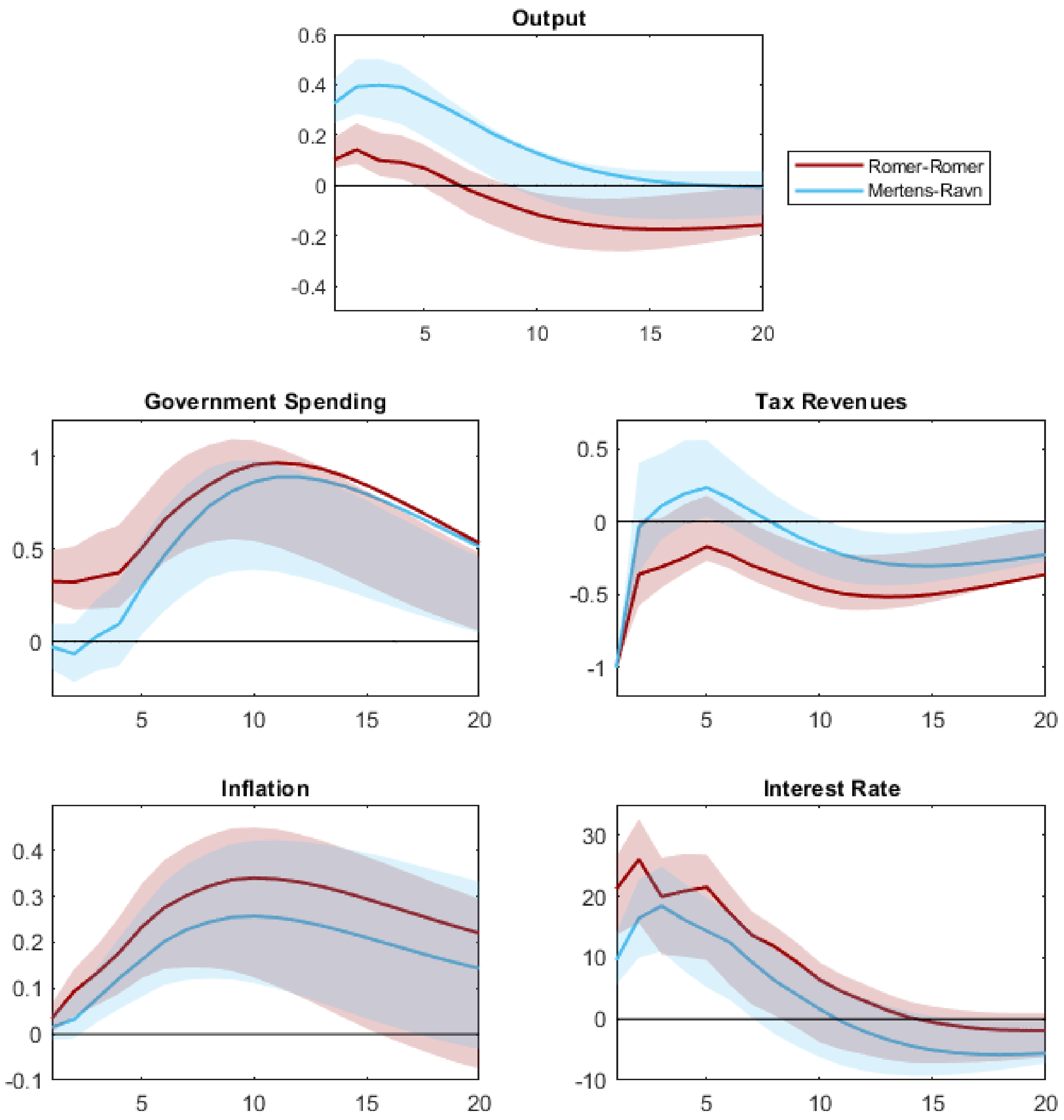


Figure 2.13: Impulse responses to a one percent decrease in tax shock under identification scheme I. A five-variable model with taxes, government spending, GDP, consumer price inflation and 3-month T-bill rate. RR: Romer and Romer (2010) exogenous tax shock, MR: Mertens and Ravn (2014b) unanticipated narrative tax shock. The shaded areas display the 95 percent confidence band using a residual-based wild bootstrap.

Table 2.12: Granger causality

	3-variable VAR	**5-variable VAR**
Instruments for spending shocks		
Ramey Defense News	0.723	0.992
1-Quarter SPF Forecast Errors	0.317	0.316
4-Quarter SPF Forecast Errors	0.112	0.108
Blanchard-Perotti Shocks	0.534	0.865
Instruments for tax shocks		
Romer-Romer Tax Shocks	0.774	0.453
Mertens-Ravn Shocks	0.813	0.613

Notes: The table reports the p-values from tests of the null hypothesis that the lagged macro variables do not Granger cause the instruments. The three-variable VAR includes government spending, taxes and GDP and the five-variable VAR includes government spending, taxes, GDP, consumer price inflation and 3-month T-bill rate. We use four lags in the regressions and first differences for the variables, which eliminate the non-stationarity.

Table 2.13: Correlations among estimated structural shocks and instruments using a fiscal-monetary model.

A. Correlations among estimated structural shocks						
	ε_t^R	ε_t^{SPF1}	ε_t^{SPF4}	ε_t^{BP}	ε_t^{RR}	ε_t^{MR}
Estimated spending shocks						
ε_t^R	1.000					
ε_t^{SPF1}	0.760	1.000				
ε_t^{SPF4}	0.783	0.939	1.000			
ε_t^{BP}	0.774	0.995	0.915	1.000		
Estimated tax shocks						
ε_t^{RR}	-0.701	-0.179	-0.371	-0.182	1.000	
ε_t^{MR}	-0.559	0.008	-0.067	-0.014	0.881	1.000

B. Correlations between estimated structural shocks and instruments						
	m_t^R	m_t^{SPF1}	m_t^{SPF4}	m_t^{BP}	m_t^{RR}	m_t^{MR}
Estimated spending shocks						
ε_t^R	–	–	–		-0.098	-0.074
ε_t^{SPF1}	–	–	–		-0.088	0.028
ε_t^{SPF4}	–	–	–		-0.104	-0.024
ε_t^{BP}	–	–	–		-0.183	-0.035
Estimated tax shocks						
ε_t^{RR}	-0.167	-0.058	-0.097	-0.058	–	–
ε_t^{MR}	-0.122	0.002	-0.001	-0.004	–	–

Notes: The table reports the cross-correlations among instruments and estimated structural shocks over their data availability. The estimated structural shocks are produced as the predicted values from regressions of the instruments on reduced-form residuals obtained from a five-variable VAR with taxes, government spending, GDP, consumer price inflation and 3-month T-bill rate. The instruments are of the following order:

- Government spending shocks: Ramey (2011) spending news (m_t^R), one-quarter-ahead SPF error (m_t^{SPF1}), four-quarter-ahead SPF error (m_t^{SPF4}) and Blanchard and Perotti (2002) spending shocks (m_t^{BP}).
- Tax shocks: tax shocks by Romer and Romer (2010) (m_t^{RR}) and unanticipated tax narrative by Mertens and Ravn (2012) (m_t^{MR}).

Table 2.14: Difference in multipliers: Identification I using a fiscal-monetary model.

	1 qrt	4 qrts	8 qrts	12 qrts	16 qrts	20 qrts
Spending Multipliers						
$m_t^R - m_t^{SPF1}$	0.037	0.063	0.035	0.367	0.179	0.943
$m_t^R - m_t^{SPF4}$	0.047	0.028	0.025	0.035	0.284	0.283
$m_t^R - m_t^{BP}$	0.001	0.022	0.490	0.233	0.583	0.395
$m_t^{SFP1} - m_t^{SPF4}$	0.877	0.878	0.852	0.872	0.722	0.602
$m_t^{SFP1} - m_t^{BP}$	0.513	0.232	0.132	0.274	0.353	0.473
$m_t^{SFP4} - m_t^{BP}$	0.264	0.240	0.073	0.046	0.036	0.046
Tax Multipliers						
$m_t^{MR} - m_t^{RR}$	0.027	0.051	0.023	0.024	0.061	0.084

Notes: The table reports p-values associated from testing the significance of the differences in multipliers computed using available proxies one at a time. Model with five variables including taxes, government spending, output, consumer price inflation and 3-month T-bill rate. The results are obtained from 10,000 moving block bootstrap replications.

Table 2.15: Tests on the strength of multiple instruments for government spending shocks: Identification III using a fiscal-monetary model.

	u_t^{τ}	$u_t^{\tau *}$	u_t^{g}	u_t^{y}	u_t^{π}	u_t^{i}
Instruments: Ramey news, 1-quarter SPF forecast error, 4-quarter SPF forecast error, Blanchard-Perotti shocks						
F-statistic	3.063	4.446	104.068	3.300	1.536	0.796
p-value	(0.023)	(0.002)	(0.000)	(0.013)	(0.196)	(0.529)
Sargan p-value	0.295	0.295	-	0.276	0.100	0.324
Instruments: Ramey news, 1-quarter SPF shock, Blanchard-Perotti shocks						
F-statistic	0.076	3.665	126.183	3.300	1.346	2.198
p-value	(0.972)	(0.007)	(0.000)	(0.013)	(0.262)	(0.092)
Sargan p-value	0.347	0.301	-	0.317	0.766	0.488
Instruments: Ramey news, 4-quarter SPF forecast error, Blanchard-Perotti shocks						
F-statistic	3.341	5.404	57.205	2.210	0.784	0.329
p-value	(0.021)	(0.000)	(0.000)	(0.090)	(0.804)	(0.092)
Sargan p-value	0.860	0.523	-	0.356	0.532	0.522
Instruments: 1-quarter SPF forecast error, 4-quarter SPF forecast error, Blanchard-Perotti shocks						
F-statistic	4.292	5.492	140.404	2.210	1.598	0.855
p-value	(0.001)	(0.000)	(0.000)	(0.090)	(0.193)	(0.092)
Sargan p-value	0.721	0.795	-	0.849	0.601	0.791
Instruments: Ramey news, 1-quarter SPF forecast error						
F-statistic	0.113	4.910	95.202	2.305	0.876	1.935
p-value	(0.891)	(0.002)	(0.000)	(0.103)	(0.419)	(0.148)
Sargan p-value	0.532	0.542	-	0.569	0.437	0.827
Instruments: 1-quarter SPF forecast error, Blanchard-Perotti shocks						
F-statistic	0.086	4.504	190.733	1.943	1.327	2.984
p-value	(0.891)	(0.005)	(0.000)	(0.147)	(0.268)	(0.054)
Sargan p-value	0.418	0.421	-	0.696	0.332	0.698

Notes: Results are reported for a five-variable VAR model which contains government spending, net taxes and GDP, consumer price inflation and 3-month T-bill rate. The results in column $u_t^{\tau *}$ are obtained from regressions of u_t^{τ} on each instrument, adjusted for impact of surprise movements in output on taxes. For each panel of the table, the five-variable VAR residuals are regressed on a different combination of instruments of government spending shocks. The F-statistics and the corresponding p-values refer to the overall significance tests and the Sargan p-values are for the tests of over-identifying restrictions.

Chapter 3

Understanding the fiscal price puzzle: Evidence from a nonlinear VAR approach

3.1 Introduction

In the wake of the global financial crisis (GFC) in 2007-2008, interest rates in many countries dropped towards the zero lower bound (ZLB) thus limiting the options to stimulate the economy through reductions in the policy rate. Since then, in addition to the introduction of alternative monetary policy tools, such as quantitative easing (QE) and forward guidance, discretionary fiscal stimulus packages have started to serve as additional policy tools in advanced countries and interest has grown anew towards studying the impacts of fiscal policy on output and other key macroeconomic variables. The majority of studies have focused on the impact of government spending shocks on aggregate output, consumption and other measures of real activity, while the literature on the response of inflation to fiscal spending shocks remains relatively limited. In this chapter, we aim to re-examine the implications of fiscal policy for macroeconomic variables, focusing on the causal relationship between government spending and inflation.

As suggested by a standard theoretical framework, be it Real Business Cycle or the textbook New Keynesian model, a positive government spending shock is likely to raise the price level via its effect on increased aggregate demand. Nonetheless, from the literature dedicated to this topic, there is no consensus on the results. While Fatás and Mihov (2001), Canova and Pappa (2007) and Nakamura and Steinsson (2014) find that the effects of government spending on prices are mixed, although usually small and not always significant, Ben Zeev and Pappa (2017), Caldara and Kamps (2017) and Ferrara et al. (2021) show that a fiscal spending shock is inflationary. In contrast, Mountford and Uhlig (2009), Dupor and Li (2015), Ricco et al. (2016), Jordà (2005) and D'Alessandro et al. (2019) provide empirical evidence that a government spending shock decreases prices, the so-called "fiscal price puzzle", which does not conform with conventional theories analyzing the economic effects of fiscal policies. We recognize that this finding might be due to the nonlinearity in the interaction between monetary and fiscal policy. Thus, in attempts to delve into the discrepancy between theoretical predictions and empirical evidence, we look at the impact of fiscal spending on inflation under different states of the monetary policy rate. A nonlinear analysis of this finding would be important because it allows us to gain further insight into how the effects of government spending shocks differ at or away from the ZLB.

It is important to note that the ZLB regime only accounts for a small proportion of the sample, which includes 28 observations for the period 2008Q4-2015Q4 and amounts to about 13 percent of all observations in our baseline sample.[1] This constitutes a challenge when comparing this relationship with normal times. To tackle this challenge, we analyze the role of inflation in the transmission of fiscal spending shocks to real activity by exploiting an interacted VAR (IVAR) model using quarterly US data. This model builds on the IVAR model employed by Towbin and Weber (2013) and Caggiano et al. (2017), which augments a standard VAR model by an interaction term between government spending and interest rate variables. This enables us to quantify the varied effects of a government spending shock on all endogenous variables and to capture the possible presence of nonlinearity in the transmission mechanism. The limited number of observations available for the ZLB period makes this method more suitable than alternative regime-switching models as it uses the whole sample of observations. The proposed model leaves us sufficient degrees of freedom to gauge the economy's response conditional on the stance of the monetary policy. Importantly, since the interaction term is constructed using two endogenous variables, the state of the system is designed to evolve endogenously over

[1] Federal Reserve set its target federal funds rate to the 0-25 basis points range in December 2008.

time after a fiscal policy shock hits. This feature enhances the original model by Towbin and Weber (2013), where the interaction terms enter the model as exogenous control variables, hence makes the model fully nonlinear and allows us to comprehensively explore both the endogenous evolution of interest rate (our conditioning variable) which indicates the switch of the economy from a state to another and its feedback on the dynamics of the system.

An additional consideration is the role of uncertainty in governing the dynamics of economic activity and the effectiveness of fiscal policy. This has received a great deal of attention in the literature, see, for example, the seminal paper by Bloom (2009) for a discussion of how the effectiveness of government policies is hindered by increased uncertainty among private agents. To the degree that agents' uncertainty affects aggregate demand independently of economic fundamentals, we also include a measure of the uncertainty index in our model and assess the relevance of this channel.

Overall, the baseline model specification includes measures of real government spending, GDP, private consumption, taxes, prices, a proxy for uncertainty index, the Wu and Xia (2016) shadow rate and Fernald (2012) total factor productivity. First, we identify the structural shock to government spending using an exogenous variable to address the concerns related to possible anticipation of fiscal spending policy or "fiscal foresight" effects. For this purpose, we incorporate the forecast errors of the growth rate of government spending, similar to Auerbach and Gorodnichenko (2012) in the vector of endogenous variables to extract the unanticipated shock. This series captures the anticipated component of government spending and appears to be a relevant instrument in our sample. Second, we quantify the response of the economy to the fiscal spending shock by following Koop et al. (1996) and computing the Generalized IRFs (GIRFs) to incorporate the state-dependent impact.[2] The GIRFs are calculated for two different regimes of the US economic data, i.e., "Normal times" and "ZLB", where the ZLB period is defined as above and we label the remaining periods as normal times.[3]

[2]In linear model, impulse responses are invariant to initial condition, proportional to size of the shock and symmetric to positive and negative shocks. However, in nonlinear model, responses can vary depending on history, size and sign of the shock.

[3]The labeling remains consistent throughout the chapter, including the robustness checks.

Our main results can be summarized as follows. First, we find robust evidence that prices decline significantly and persistently following an increase in government spending in both states of the economy. This observation is supportive of the prevalent finding of the price puzzle among empirical studies on the effects of fiscal policy on inflation (Mountford and Uhlig, 2009, Dupor and Li, 2015, D'Alessandro et al., 2019) and (Jørgensen and Ravn, 2022). Specifically, we find a less deflationary response in the presence of the ZLB, which seems conceivable from a theoretical point of view since we expect a stronger inflation response when active monetary policy is not constrained by short-term interest rates at their lower bound. Although their response is not significantly different across the two states, it is still interesting to see that the price puzzle is present regardless of the monetary stance.

Second, we find that, during periods of active monetary policy, output reacts positively to a fiscal spending surprise on impact. The positive effect dies down after about three years following the shock. However, this pattern changes when the effective rate is at the zero bound. We observe a quick drop in the levels of private consumption and output in response to a government spending shock. They remain negative for most of the horizons before recovering slowly to pre-shock levels. A potential explanation is that the impact of fiscal stimuli is weakened when ZLB is binding due to uncertainty about the state of the economy and future economic policies. Consequently, private consumption declines, resulting in a drop in output. Our speculation is verified by the estimated response of the uncertainty level. We find a rapid rise in uncertainty levels in response to a jump in government spending during the ZLB and the timing of heightened uncertainty matches well with the drop in real activity variables.

Third, estimates obtained from our quarterly IVAR show that the cumulative multipliers under both regimes are high on impact when the fiscal spending shock hits the economy and go down to less than one after one year. We find that the multiplier in the normal state is more persistent and remains positive in the long run while the multiplier in the ZLB state indicates a quick drop and turns negative after two years following the shock. However, we find no statistical difference between the multipliers, thereby supporting Ramey and Zubairy (2018)'s main finding that the size of government spending multiplier is not magnified when the short rate is in the zero region in comparison with normal times.

Several robustness checks of the baseline model are conducted. First, we consider the classic Cholesky identification scheme as in Blanchard and Perotti (2002), i.e., we identify the shock to fiscal spending with timing restrictions using the government spending variable. We find qualitatively similar results when neglecting fiscal anticipation effects. Next, we vary our specification to investigate if our findings are driven by the business cycle. We also examine whether our results are sensitive to the choice of various proxies for monetary policy and uncertainty index since they appear to be the key channels for our results. Finally, we estimate the model when the composition of government expenditure is taken into account. Our main findings are supported by these further exercises.

The rest of the chapter is organized as follows. Section 3.2 discusses the relation of this chapter to the literature. Section 3.3 describes our baseline specification, data sample and revisits the evidence from a linear VAR model. Section 3.4 provides evidence on nonlinearity and illustrates the empirical model, inference method and how we compute generalized impulse responses. Section 3.5 presents our main findings concerning the impact of government spending shocks and the magnitude of government spending multipliers during ZLB episodes and normal times. Section 3.6 provides evidence on robustness checks. Section 3.7 concludes and Section 3.8 contains the appendices.

3.2 Literature review

This chapter relates to different strands of the macroeconomic literature on fiscal policy. First, it is closely related to the literature examining the empirical impact of fiscal policy on inflation. There has been a lively discussion on this topic, yet the results remained varied. Fatás and Mihov (2001), Canova and Pappa (2007) and Nakamura and Steinsson (2014) suggest that the effects of government spending on prices are mixed. However, the responses appear insignificant in magnitude and from a statistical point of view. Other papers including Ben Zeev and Pappa (2017), Caldara and Kamps (2017) and more recently Ferrara et al. (2021) provide evidence of an inflationary behaviour when a fiscal spending shock occurs. On the other hand, a larger strand of the literature on this topic argues that a government spending shock decreases prices, the so-called "price puzzle" (Mountford and Uhlig, 2009, Dupor and Li, 2015, Ricco et al., 2016, Jørgensen and Ravn, 2022, D'Alessandro et al., 2019). In particular, Jørgensen and Ravn (2022) investigate the interaction between fiscal policy and productivity by using a model with added variable technology utilization resembling Bianchi et al. (2019). They provide a potential explanation for the negative movement in prices by showing that in their model, firms adopt new technologies that raise productivity following an increase in government spending and aggregate demand. Higher productivity results in a drop in prices and inflation due to a supply-side boost that exceeds the growth in demand. Similarly, D'Alessandro et al. (2019) employ a Bayesian VAR model which includes a fiscal variable and a proxy for total factor productivity (TFP), and find that inflation responds negatively to a positive fiscal innovation.

Next, in light of examining the variation in response of real activity indicators and inflation to a fiscal spending shock across different states of the economy, this study naturally touches upon the rapidly growing literature on state-dependent fiscal policy effects. In this aspect, two main strands of this literature have emerged. The first one focuses on providing evidence of whether the impact of government spending differs based on the amount of slack in the economy, i.e., in recessions versus expansions, see among others, Auerbach and Gorodnichenko (2012), Fazzari et al. (2015), Caggiano et al. (2015). Results are mixed. The other strand constructs an assessment of the state-dependent fiscal spending effects via the monetary policy channel, i.e., when short-term interest rates are near and away from the ZLB, which is the focus of our investigation.

Most research on this topic revolves around whether fiscal multipliers are significantly higher when active monetary policy tools are constrained, results again are mixed. A large number of studies find that government fiscal multipliers are generally below one in normal times, yet rise significantly above one at the ZLB (Christiano et al., 2011, Woodford, 2011) and (Miyamoto et al., 2018). On the other hand, a number of scholars show that the impact of government spending shocks on the real activity does not vary significantly with the monetary policy stance (Mertens and Ravn, 2014a, Ramey and Zubairy, 2018). Despite the lack of consensus on the fiscal policy implication of the ZLB, the conventional idea is that neglecting the incidence of ZLB episodes when addressing the macroeconomic policy can result in biases in response of economic activity variables. This is owing to the central bank's inability to use conventional monetary instruments to tackle economic shocks, hence generating elevated volatility in these variables during the ZLB period.

Our work is also related to the literature examining the relationship between fiscal and monetary policy. See, for instance, Rossi and Zubairy (2011), Canova and Pappa (2007). Rossi and Zubairy (2011) point out the importance of considering the interactions between monetary and fiscal policy when studying the impact of monetary and fiscal policy innovation on macroeconomic activity. Similarly, Mountford and Uhlig (2009) stress how neglecting the impact of monetary policy can severely affect the identification of fiscal policy shocks. From a theoretical point of view, Davig and Leeper (2011) show that accommodating a fiscal sector is crucial for the analysis of optimal monetary policy and vice versa. Numerous studies look at the effectiveness of government spending in different states of the business cycle and claim that the impact of changes in government spending is substantially determined by movement in monetary policy. Indeed, the response of real activity following a fiscal spending shock depends on the monetary policy position and inflationary effects triggered by the fiscal stimulus. It is argued that, when monetary policy is constrained by the lower bound, the interaction between monetary and fiscal policy matters in the sense that the nominal interest rate stays constant in response to fiscal spending surprises, causing deflationary effects that lead to large output drops. Mertens and Ravn (2014a) state that deflationary responses arise in an expectations-driven liquidity trap, wherein loss of confidence and soaring uncertainty surrounding private agents - households and firms - weaken the stimulating impacts of fiscal policy.

From a methodological viewpoint, IVARs have been used recently for analyzing asymmetrical effects of macroeconomic shocks. Towbin and Weber (2013) examine how output and investment's reaction to external shocks vary with external debt, import structure and exchange rate regimes. Sá et al. (2014) study how the impact of capital inflows changes with the structure of the mortgage market and the level of securitization in different countries. Caggiano et al. (2017) use the IVAR model to investigate the response of real activity to uncertainty shock with and without the presence of a binding zero lower bound of the policy rate. Similarly, Pellegrino (2021) uses an IVAR model to assess the impact of monetary policy shocks under times of high and low uncertainty. When applied to the estimation of the effects of fiscal policy shocks under different regimes, a close paper to our analysis is Di Serio et al. (2020) that apply a factor-augmented IVAR model to the US using Bayesian methods to investigate whether the government spending multiplier is larger when the ZLB is binding. Their model builds on the IVAR model developed by Towbin and Weber (2013) and Sá et al. (2014) with factors from a large informational dataset to tackle the limited information problem. They use the sign restriction strategy to identify an unexpected government spending shock and provide empirical evidence that the government spending multiplier at the ZLB is larger than in normal times. Compared to their study, we differ in the following key aspects. First, they use a fixed conditioning variable in computing impulse responses, whereas our model employs a fully nonlinear IVAR model by endogenizing the interaction term. Second, we focus on a different research question, i.e., the prices response to a government spending shock in normal times vs. when the interest rate hits the ZLB.

3.3 Fiscal policy and the prices: Revisiting the evidence from a linear VAR model

In this section, we revisit the evidence from existing papers that employ a linear structural VAR model for the US economy to study the impact of a government spending shock on macroeconomic variables. We estimate the following VAR model on U.S data:

$$\mathbf{X}_t = \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 t + \sum_{j=1}^{p} \mathbf{A}_j \mathbf{X}_{t-j} + \mathbf{u}_t \tag{3.1}$$

where $\mathbf{X}_t$ is an $n \times 1$ vector of endogenous variables; $\boldsymbol{\alpha}_0$ is an $n \times 1$ vector of deterministic terms; $\boldsymbol{\alpha}_1$ is an $n \times 1$ vector of slope coefficients for the linear time trend; $\mathbf{A}_j, j = 1, \ldots, p$, are $n \times n$ coefficient matrices, p is the number of lags in the model and $\mathbf{u}_t$ is an $n \times 1$ vector of VAR innovations. We select the number of lags as determined by Akaike's Information Criterion (AIC). This criterion suggests an optimal lag length of $p = 4$. We include the linear time trend as in Blanchard and Perotti (2002).[4]

[4]Blanchard and Perotti (2002) allow for linear and quadratic time trends in their model. Nonetheless, our results remain qualitatively similar when including the quadratic time trend.

Data. In addition to the fiscal policy, interest rate and prices, the vector of endogenous variables also consists of measures of real activity and factor productivity. One of the concerns about VAR models is that they can be easily over-parameterized if the number of variables and lags included in the specification is large. To maintain a sufficiently large sample size, we use the following endogenous variables:

$$\mathbf{X}_t = [FE_t \quad G_t \quad Y_t \quad C_t \quad T_t \quad P_t \quad R_t \quad VIX_t \quad A_t]'.$$

where the variables denote the implied forecast errors of the survey-based forecasts of the growth rate of government spending (FE_t), log of real government expenditure and investment per capita (G_t), log of real GDP per capita (Y_t), log of real private consumption per capita (C_t), log of real net tax revenues per capita (tax receipts less current transfers, interest payments and subsidies) (T_t), log of the personal consumption expenditure (PCE) price index (P_t), the "shadow rate" (R_t), a measure of uncertainty (VIX_t) and log of the utilisation-adjusted total factor productivity (TFP) (A_t). The forecast errors (FE_t), the interest rate (R_t) and the uncertainty (VIX_t) are measured in levels.[5] We use the US macroeconomic dataset for a sample starting from 1966Q4 in accordance with the availability of the forecast errors. We end the sample period in 2019Q4 to include the ZLB episodes. Although there are more recent data available, we choose to avoid observations related to the COVID-19 period since they can bias the VAR responses (Lenza and Primiceri, 2022).

The measure of tax revenues is included because empirical findings indicate much interaction between fiscal spending and taxes (Burnside et al., 2004, Yang, 2005). Moreover, Ramey and Zubairy (2018) argue that the inclusion of taxes as a control variable is crucial in interpreting the impact of government spending shocks. Next, in the baseline analysis, we use a measure of implied stock market volatility (VIX) by Bloom (2009) as the uncertainty indicator. We update the series up to 2019Q4. The VIX index has been favored as a proxy for the uncertainty index in the literature since it is a forward-looking variable and captures market expectations. In addition to the baseline uncertainty indicator, we also employ the financial, macroeconomic and real uncertainty indexes proposed by Ludvigson et al. (2021) to assess the robustness of our main results. Regarding the measure

[5]Details on data construction and sources are given in Appendices.

of the monetary policy rate, we use the shadow rate from Wu and Xia (2016) instead of the conventional proxy of monetary policy tools, such as the federal funds rate or the 3-month Treasury bill rate. Wu and Xia (2016) use a Gaussian Affine Term Structure Model (GATSM) to construct an effective rate, which can capture the effects of unconventional monetary policy measures. In particular, the shadow rate turned negative in early 2009 after the Federal Open Market Committee (FOMC) set a target federal funds rate range of 0 to 0.25 percent and the effective lower bound became binding. Therefore, it might be a more appropriate indicator of monetary policy after December 2008.

Figure 3.1 reports responses to a government spending shock using the identification method based on forecast errors on the 1966Q4-2019Q4 sample. Following a positive innovation in government spending, output and consumption increase substantially. In contrast, the price level shows a persistent decrease in response to a fiscal spending shock throughout the horizons. In addition, this declining pattern remains significant at all horizons. The interest rate displays almost no reaction on impact and declines by around 20 basis points at the trough. Therefore, the price puzzle is evident using a linear model.

3.4 Effects of a government spending shock on prices through the lens of an IVAR model

3.4.1 Linearity test and nonlinear model specification

The conventional wisdom is that the linear VARs are standard in many empirical macroeconomic studies. However, a large part of the literature has shown that the response of macroeconomic variables to a fiscal policy shock displays asymmetric dynamics across different states of the economy, see Auerbach and Gorodnichenko (2012), Caggiano et al. (2015), Fazzari et al. (2015) among others. Therefore, nonlinearity in a macroeconomic framework appears to be important for understanding the impact of fiscal policy shocks on economic activities.

We start the development of our nonlinear model by performing a nonlinearity test at the multivariate level on the linear VAR model against an interacted-VAR (IVAR), i.e., our model of interest. This model augments an otherwise standard linear VAR with an

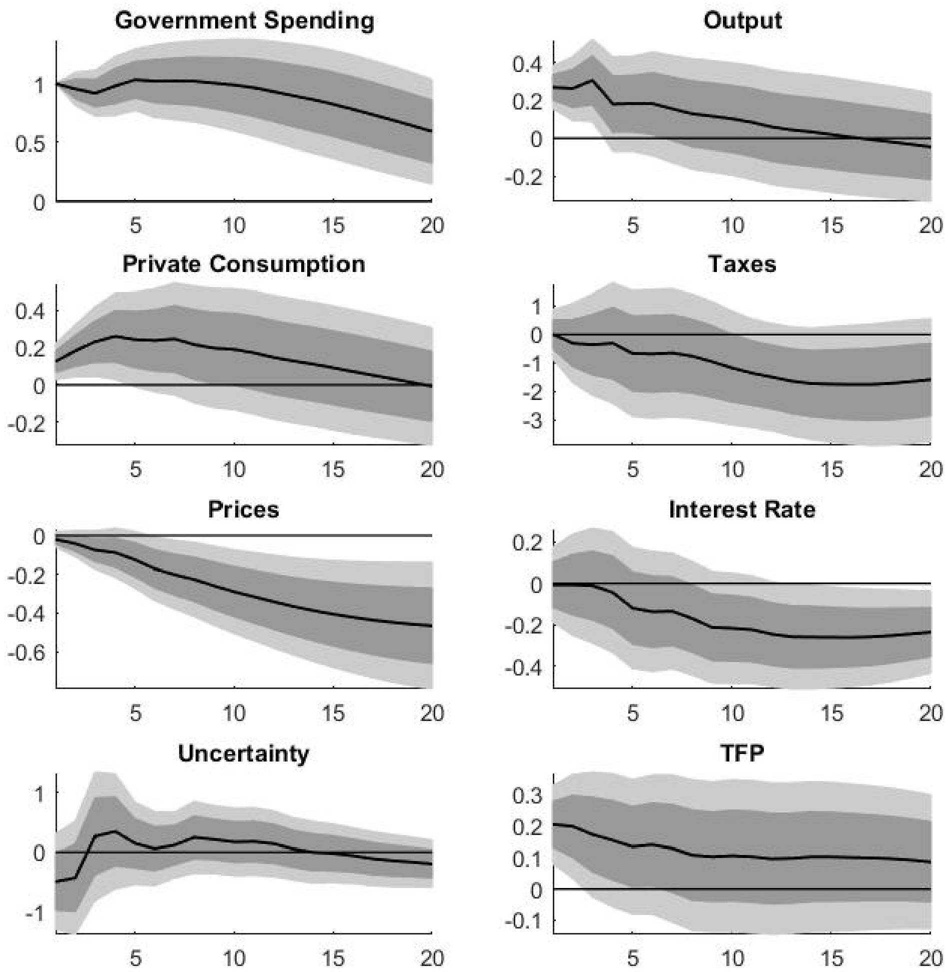


Figure 3.1: Impulse responses to a one percent increase in government spending shock using the identification scheme based on forecast errors in a standard linear VAR model. Sample period: 1966Q4-2019Q4. Grey areas: 68 percent and 90 percent confidence bands.

interaction term consisting of two endogenous variables: the variable by which we identify the fiscal spending shock, and the proxy for the monetary policy stance, the interest rate. The latter variable acts as a conditioning variable that indicates the switch between states. The model is given as follows:

$$\mathbf{X}_t = \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 t + \sum_{j=1}^{p} \mathbf{A}_j \mathbf{X}_{t-j} + \left[\sum_{j=1}^{p} \mathbf{c}_j FE_{t-j} \times R_{t-j} \right] + \mathbf{u}_t \tag{3.2}$$

The interaction term in brackets comprises of forecast errors FE_t and interest rate R_t.

This can influence the dynamic relationship between the endogenous variables through $n \times 1$ vectors of coefficients, $\mathbf{c}_j, j = 1, \ldots, p$.

In order to provide statistical motivation for our nonlinear framework, we conduct a likelihood ratio (LR) test for the null hypothesis of linearity, i.e., the exclusion of the interaction terms, versus the alternative of an IVAR model since our model nests the linear VAR. Using the baseline specification, when the shadow rate is a proxy for the monetary policy stance, the LR test suggests a test statistic of 94.83, with an associated p-value less than 0.01.[6] Therefore, the use of a nonlinear IVAR framework is justified.

Given that the model is linear in parameters and does not include any latent variable, the estimation using OLS is efficient. Similar to the linear VAR model, we select the number of lags as determined by AIC. This criterion again suggests an optimal lag length of $p = 4$, irrespective of whether we consider the sample with the ZLB episodes or not. This is reasonable since the data is quarterly and four lags might be sufficient to capture the dynamic relationship of the endogenous variables.[7]

The IVAR model displays a number of advantages. First, our research question involves working with two different regimes and the results can be unstable due to the limited number of observations available for the ZLB period if the model obtains estimates under each regime separately. This IVAR model utilises all information available for the full sample to estimate the impulse response functions. Accounting for the state-dependency in this way avoids jeopardising degrees of freedom and at the same time enables us to capture the abrupt changes in monetary policy.[8]

Second, the IVAR model directly captures the nonlinearity of interest, as reflected by the interaction between government spending and interest rate. In particular, while the estimation of thresholds or the calibration of smoothness parameter/transition functions

[6]We obtain similar conclusions when using other interest rate variables.

[7]A robustness check using alternative lag length of $p = 3$ or $p = 5$ shows qualitatively similar results.

[8]To tackle the lack of sufficient data in analysing the impact of government spending shocks at the ZLB, some papers rely on a rich historical dataset that enables estimation based on more than 100 episodes during which the ZLB was binding (Ramey and Zubairy, 2018). They employ historical US data from 1890Q1-2015Q4 to estimate the government spending multiplier in normal times, at the ZLB, and across states of economic slack. While this approach considerably enlarges the degrees of freedom, it also injects more uncertainty into inference due to the inclusion of several wartime periods. Choi et al. (2022) exploit a high-frequency (daily) dataset on US defense spending proposed by Auerbach and Gorodnichenko (2016) and the online price index constructed by Cavallo and Rigobon (2016) to identify exogenous government spending shock. They find that prices react negatively to fiscal spending surprises at the ZLB.

can be of interest in certain cases, the IVAR model does not require this step as in the TVAR or STVAR models. Rather, it allows for the identification of a smaller set of parameters, thereby avoiding the estimation of a highly parameterised model. This model also allows us to examine whether the possible asymmetric responses of real activity to a fiscal spending shock in different states of the economy are due to the interaction between government spending and monetary policy stance or due to other sources of nonlinearity.

We acknowledge, however, that the rejection of the linear model against the IVAR alternative, by no means implies that the IVAR model is "true". This test has power against many forms of nonlinearity. The specific IVAR model that we choose is an "ad hoc" choice for our specific purpose. We perform some analyses in Section 3.6 to ensure that the nonlinear dynamics generated by the interaction terms are not driven by the business cycle or other structural changes in the economy.

3.4.2 Identification of the government spending shock

The empirical literature has mostly identified a structural government spending shock from a vector of reduced form residuals imposing short-run restrictions following the seminal study by Blanchard and Perotti (2002). In particular, government spending is ordered first in the vector of endogenous variables, i.e., the government spending does not react to any other variables within the same period, and the exogenous structural government spending shock is identified via recursive Cholesky factorization of the reduced-form residual variance-covariance matrix. An implicit assumption when using VARs is that we obtain and work with fundamental shocks, i.e., shocks that can be recovered from past and present observation variables.

However, as discussed by Ramey (2011) and Leeper et al. (2013) among others, the Cholesky identification strategy fails to overcome the fiscal foresight problem related to anticipation effects by economic agents. This happens when they rely on additional information in decision-making that causes misalignment of information sets. Thus, we consider an identification scheme that controls for fiscal foresight to account for this. For this purpose, we follow Auerbach and Gorodnichenko (2012) and identify an unanticipated government spending shock as an innovation to the forecast errors of the growth rate of government spending. The forecast errors are defined as the differences between the forecasts made at the previous quarter $t-1$ for the contemporaneous quarter t and the actual government spending. Intuitively, this measure of forecast errors represents the "news" that becomes available to private agents about fiscal spending policy in near future, hence a reliable proxy for the anticipation effects.

We order the forecast errors (FE_t) first in the system and then disentangle the structural shocks, $\boldsymbol{\varepsilon}_t$, from the reduced-form shocks, $\mathbf{u}_t$ in equation (3.2). This is achieved by imposing a lower triangular structure on a contemporaneous matrix B such that $\mathbf{u}_t = \mathbf{B}^{-1}\boldsymbol{\varepsilon}_t$. This implies that forecast errors do not respond on impact to any other variables but are allowed to affect other variables within the same period. Similar to Auerbach and Gorodnichenko (2012), real government spending (G_t) is ordered immediately after FE_t. The ordering of other variables follows the literature. In fact, since we only consider the shock to government spending, as long as the government spending variables are ordered first, alternative orderings would yield similar results.

3.4.3 Generalized impulse response functions

Similar to recent studies using nonlinear VAR models, we compute the state-dependent impact of government spending shocks via generalized impulse response functions (GIRFs) suggested by Koop et al. (1996). This method acknowledges the fact that the system's evolution can vary endogenously after a shock in the nonlinear model. Specifically, the conditioning variable, interest rate and consequently, the interaction term is determined by the future values of the interest rate and government spending. Hence, computing GIRFs allows us to fully capture the interaction between the endogenous variables and the state of the system. Another reason is that in a nonlinear system, the impulse response depends on the size, sign and timing of the shock. Therefore, GIRFs reflect the fully nonlinear empirical responses in accordance with the initial condition when the shock occurs. Technically, the generalized impulse response at horizon h of the vector $\mathbf{X}_t$ to a shock of size δ computed conditional on history $\boldsymbol{\omega}_{t-1} = \mathbf{X}_{t-1}, \ldots, \mathbf{X}_{t-p}$, is given by the following difference of conditional means:

$$GIRF_{\mathbf{X}}\left(h, \delta, \boldsymbol{\omega}_{t-1}\right) = E\left[\mathbf{X}_{t+h} | \delta, \boldsymbol{\omega}_{t-1}\right] - E\left[\mathbf{X}_{t+h} | \boldsymbol{\omega}_{t-1}\right], \quad h = 0, 1, \ldots, H$$

In computing GIRFs, we follow Kilian and Vigfusson (2011) and work with orthogonalized residuals to identify government spending shocks. We obtain our state-dependent GIRFs by averaging the history-dependent GIRFs per each horizon, over a particular subset of initial conditions. These GIRFs are referred to as the average response of the macroeconomic activity to a shock in a given regime.[9]

3.5 The state-dependent effects of government spending shocks and fiscal multipliers

This section reports the impact of a government spending shock according to the stance of monetary policy. We start with the estimation of the averaged effects in the ZLB periods and normal times (which refer to the period 2008Q4-2015Q4 and otherwise, respectively as defined in Section 3.1), and then turn to the quantification of the fiscal multipliers. The spending shock is normalized to one for a better read.

3.5.1 GIRFs

Figure 3.2 reports the point estimates for the state-conditional GIRFs of the same variables on the same sample. First, the price level reacts negatively to a fiscal spending shock and this drop is persistent and statistically significant regardless of the state of the economy. This finding is in line with a large set of empirical works on the impact of government spending on price level Jørgensen and Ravn (2022), D'Alessandro et al. (2019). Further, the results display some variation in the response of the macroeconomic variables that is triggered by the interaction between government spending and interest rate in response to a fiscal spending shock between two states. We find that the short-term interest rate exhibits a more pronounced negative effect in response to fiscal shock under the constraint of the ZLB.

Consumption increases on impact in both states and declines after around one year after the spending shock hits at the ZLB period. The suggested fall in consumption is in line with Ramey (2011) that links this to the crowding-out effect due to higher tax to be

[9]Details on the GIRFs procedure are presented in Section 3.8.2 in the Appendices.

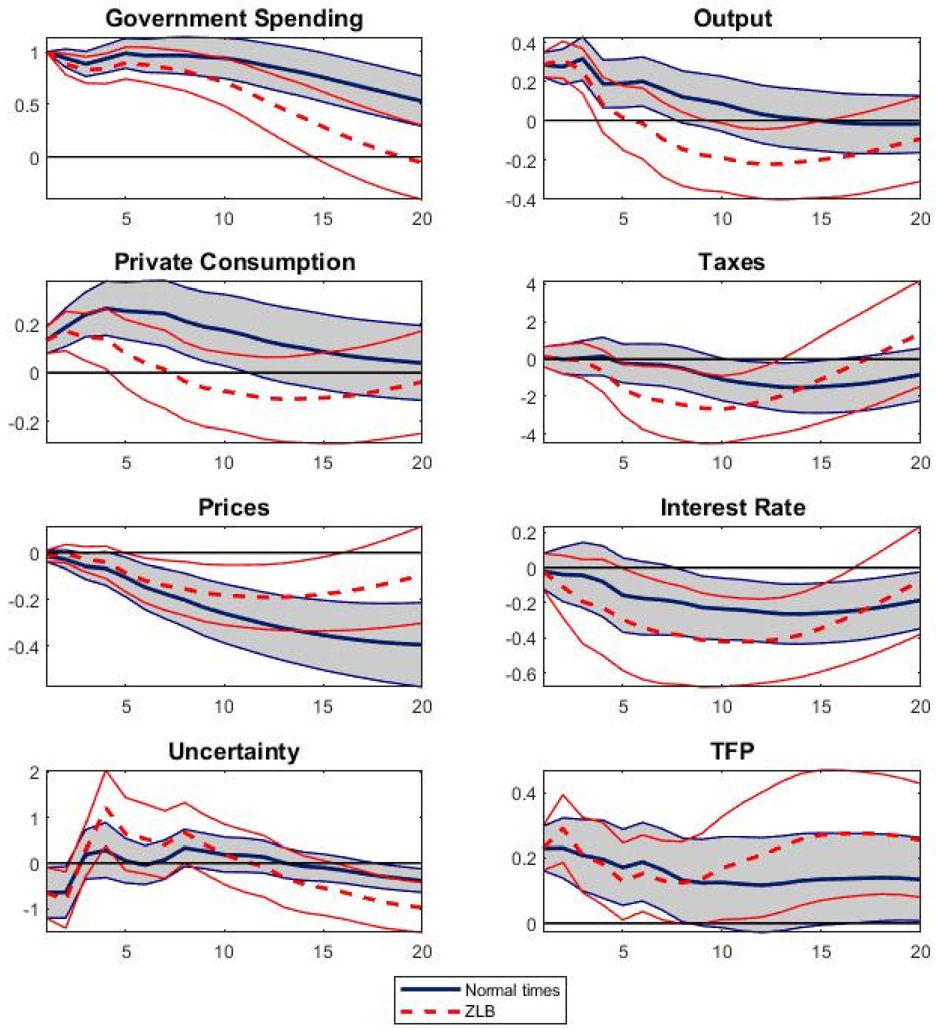


Figure 3.2: Generalized impulse responses to a one percent increase in government spending shock: ZLB vs. Normal times. Estimates obtained using the identification scheme based on forecast errors. Sample period: 1966Q4-2019Q4. Dashed-red lines: ZLB periods. Solid-blue lines: Normal times. Solid-red lines and grey areas: 68 percent confidence bands.

paid to compensate for the increase in fiscal spending. Similar to the finding of Jørgensen and Ravn (2022), TFP rises on impact, suggesting an upward shift in supply, which is due to the adoption of new technologies and higher productivity, exceeds the increase in aggregate demand following a positive government spending shock, thus results in a drop in prices.

In terms of the other variables in the model, real GDP increases on impact irrespective of the monetary policy stance. However, the response of output to a spending shock quickly becomes insignificant and turns negative after around five quarters when the shock occurs in the presence of the ZLB. The reaction of GDP mirrors well the endogenous behaviour of private consumption. From a theoretical perspective, one might anticipate an increase in aggregate output due to the decrease in *ex-post* real interest rate, which is implied by the responses of nominal interest rate and inflation at the ZLB. We speculate that this result can be reconciled with some recent empirical studies, which propose that during times of high uncertainty when nominal interest rates hit the ZLB and agents perceive gloomy prospects for the economy, risk-averse households tend to save more, which in turn lowers consumption, output and inflation and causes the deflationary effect of fiscal stimulus (Fernández-Villaverde et al., 2015). Indeed, uncertainty rises significantly in response to a positive government spending innovation during the ZLB. In addition, the spike of uncertainty is consistent with the drop in real activity variables, thus supports our speculation.

Figure 3.2 also plots the associated 68 percent confidence intervals for both regimes. We can see that the confidence bands of these responses overlap substantially, implying that reaction of economic activity to a fiscal spending shock is not necessarily different when the economy is facing a binding zero lower bound on the nominal interest rate, as compared to normal times. This finding coincides with some relatively recent papers put forth by Owyang et al. (2013), Ramey and Zubairy (2018) where they employ a different identification strategy, i.e., using defense spending news shock developed by Ramey (2011), and estimation method, i.e., local projections (Jordà, 2005). We assess this result by computing cumulative spending multipliers.

3.5.2 Cumulative multipliers

In this section, we compute the cumulative multipliers and the associated 68 percent confidence intervals. The cumulative multiplier is defined as the integral of the response of output divided by the integral of the response of government spending, i.e., $\sum_{h=1}^{H} Y_h / \sum_{h=1}^{H} G_h$, where 1 is the period in which the shock hits and H refers to a chosen horizon. Since government spending and output enter the VAR in logs, the responses are re-scaled by the sample average of output to government spending to convert them

Table 3.1: Estimates of government spending multipliers using the identification based on forecast errors: Normal times vs. ZLB.

Quarters/State	ZLB	Normal
1	1.34	1.30
	[0.76, 1.91]	[0.73, 1.87]
4	1.04	1.21
	[0.01, 2.07]	[0.39, 2.26]
8	0.31	0.94
	[-1.47, 1.74]	[0.16, 2.38]
12	-0.19	0.74
	[-2.39, 2.01]	[0.14, 2.39]
16	-0.38	0.57
	[-4.36, 3.59]	[0.01, 2.31]
20	-0.58	0.43
	[-9.57, 8.41]	[-0.11, 2.13]

Notes: The table reports cumulative spending multipliers conditional on our baseline nonlinear VAR analysis using the identification based on forecast errors considering various horizons. Log-values of the GIRFs of output rescaled by the sample mean of output over public spending (both taken in levels) to convert percent changes into dollars.

into dollar terms. Woodford (2011) argues that this conversion method is able to account for the persistence of fiscal shocks.[10]

The fiscal spending multipliers for each regime and along with 68 percent confidence intervals are reported in Table 3.1, where we set $H = 20$ to focus on the impact of spending shock up to five years. As indicated in Table 3.1, we see little difference in the multiplier estimates for the ZLB and normal states at shorter horizons. The multipliers are larger than one on impact for both regimes. Interestingly, the point estimates of the multiplier when ZLB is binding are lower in the medium and long run and turn negative after about two years (from 1.34 on impact to 0.31 after eight quarters). Noticeably, these effects become statistically insignificant after two years. In contrast, the multipliers under normal times are persistent and remain statistically significant after around five

[10] Recently, a strand of the literature has highlighted reasons against the use of this *ex-post* conversion factor. Ramey and Zubairy (2018) point out that the output-to-spending ratio ranges between 2 and 24 with a mean close to 8 in the historical dataset spanning from 1889Q1-2015Q4. Hence, using the sample average over this range for such a ratio might bias the multiplier estimates up in the sample. In our sample, however, the value of this ratio varies from 4.13 to 5.74 with a mean of 4.93 in the full sample, from 4.13 to 5.74 with a mean of 4.88 during normal times and from 4.67 to 5.64 with a mean of 5.09 at ZLB periods. Therefore, the *ex-post* conversion from percent changes into dollar terms does not appear to be a potential problem for our estimation. The results remain qualitatively similar when we use the average of output-over-spending ratio in the full sample to compute state-dependent spending multipliers.

years. Nevertheless, the 68 percent confidence intervals suggest that the multipliers are not statistically different between the two regimes.

In sum, our findings do not align with the new Keynesian DSGE models that predict government spending shocks are more expansionary at the ZLB via the inflation channel, which translate into a larger government spending multiplier during the ZLB than normal times (Christiano et al., 2011, Woodford, 2011, Coenen et al., 2012). Instead, they are in favor of the finding of Ramey and Zubairy (2018) which suggests that the stimulating effect of fiscal spending shock on output is not magnified when the economy is constrained by the ZLB.

3.6 Robustness checks

3.6.1 The role of fiscal foresight

In the baseline model specification, we identify the exogenous structural fiscal shock using the forecast errors of the growth rate of government spending to account for fiscal anticipation. However, one might raise the question if using the forecast errors is necessary in our context. Although it is widely acknowledged in the literature that neglecting the anticipation effects can severely affect impulse responses produced by VARs modeling, Mertens and Ravn (2014b) show that the role of automatic stabilizers is negligible in the US. They argue that it is plausible to assume that government spending responds to economic conditions with a lag. Therefore, as a first robustness check, we consider the classic Cholesky identification scheme by Blanchard and Perotti (2002). In particular, we drop the forecast errors (FE_t) from our model and identify the government spending shock with timing restrictions. The vector of endogenous variables is as follows:

$$\mathbf{X}_t = [G_t \quad Y_t \quad C_t \quad T_t \quad P_t \quad R_t \quad VIX_t \quad A_t]'.$$

where the government spending is ordered first, that is, we assume that government spending does not respond contemporaneously to any other variables due to implementation and legislation lags. The ordering of other variables remains the same. The following estimation is performed using the sample spanning 1962Q3-2019Q4 subject to data availability.

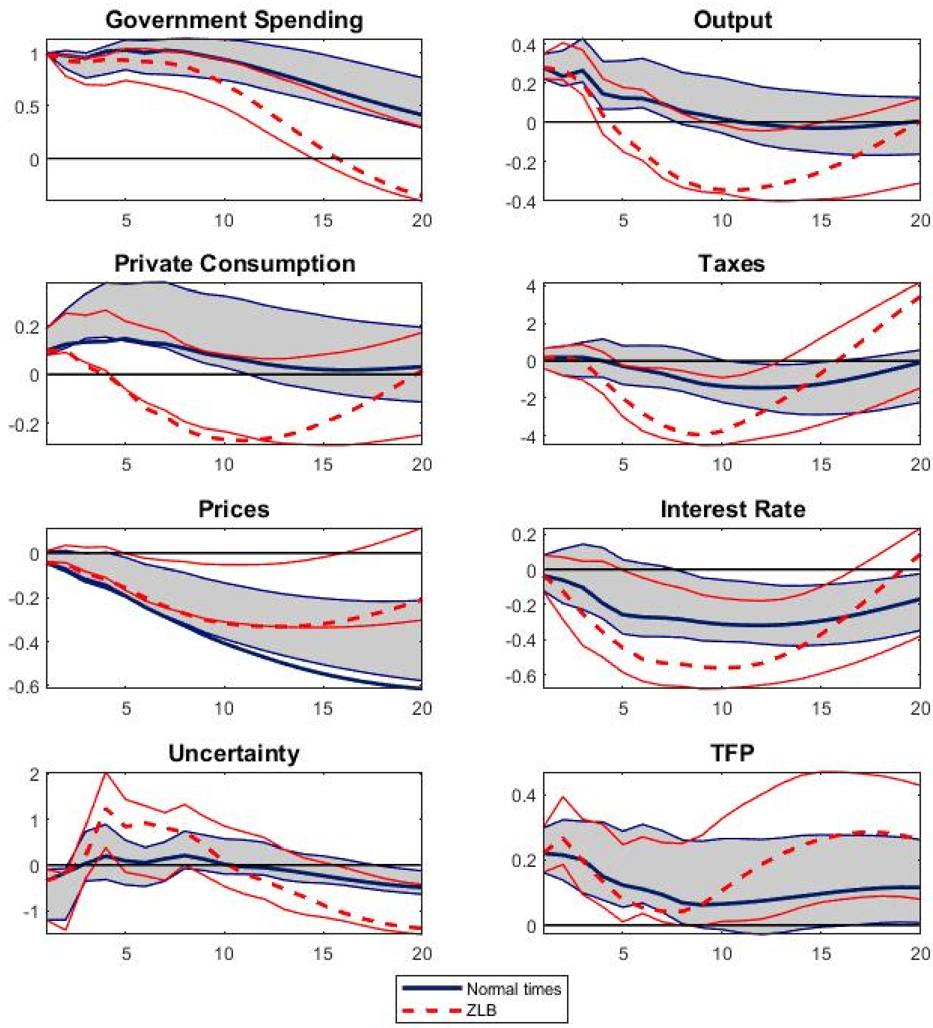


Figure 3.3: Generalized impulse responses to a one percent increase in government spending shock: ZLB vs. Normal times. Estimates obtained when dropping the forecast errors from the baseline model. Sample period: 1962Q3-2019Q4. Dashed-red lines: ZLB regime. Solid-blue lines: Normal times. Solid-red lines and grey areas: 68 percent confidence bands. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and normal times states of the baseline model.

Figure 3.3 shows the GIRFs obtained with this specification. A first observation is that dropping the forecast errors produces qualitatively similar results to our baseline model. There is no evidence of an increase in the price level. Instead, prices decline by around 0.4 at the trough. We find that responses of output, private consumption and prices are lower when the interest rate is near the ZLB compared to our baseline results. Interestingly, the responses of private consumption and prices lie outside the plotted 68 percent confidence bands associated with the baseline model at some horizons. This result suggests that the role of fiscal anticipation is not negligible in our analysis.

Table 3.2: Estimates of government spending multipliers using the Cholesky identification: Normal times vs. ZLB.

Quarters/State	ZLB	Normal
1	1.31	1.26
	[0.97, 1.91]	[0.93, 1.83]
4	0.81	0.99
	[0.29, 1.88]	[0.74, 2.12]
8	-0.09	0.74
	[-0.94, 1.56]	[0.51, 2.14]
12	-0.70	0.64
	[-1.88, 1.59]	[0.34, 2.08]
16	-0.96	0.71
	[-2.91, 2.25]	[0.17, 2.01]
20	-0.76	0.70
	[-3.75, 3.05]	[0.05, 1.98]

Notes: The table reports cumulative spending multipliers using the Cholesky identification scheme considering various horizons. Log-values of the GIRFs of output rescaled by the sample mean of output over public spending (both taken in levels) to convert percent changes into dollars.

Turning to fiscal spending multipliers, Table 3.2 reveals that the point estimates behave similarly to the results obtained with the baseline specification. Overall, these results suggest our findings of deflationary response to government spending shock and government spending multiplier not being larger when ZLB is binding are supported regardless of controlling for fiscal foresight.

3.6.2 The role of the business cycle

In the previous empirical analysis, we show that output drops in the medium and long run in response to a fiscal spending shock and the spending multiplier is not larger when the interest rate hits the ZLB. Nevertheless, without considering the interest rate channel, various studies find that the stimulating effect of fiscal innovation operates via channels including the business cycle. In particular, they provide evidence of a larger reaction of output to a fiscal shock during recessionary compared to non-recessionary periods (Auerbach and Gorodnichenko, 2012, Caggiano et al., 2015). Indeed, in the time of crisis, households and firms are prone to credit constraints due to rising risk premium and borrowing costs, implying a relatively high marginal propensity to consume.

It seems reasonable that the low-interest rates and slack states often overlap as central banks tend to reduce the policy rate as a response to periods of economic downturn. Given this, one might argue that the results arise potentially due to the recessionary effect rather than the ZLB phase. In the following, we provide evidence that the response of economic variables following a fiscal expansion in the ZLB does not attribute to the effects of fiscal spending in recessions.

We confront this potential identification issue by estimating the GIRFs conditional on the recessionary phase over the sample 1966Q4-2019Q4 using the baseline model specification. If the variation in the effects of fiscal spending is not driven by the stance of monetary policy but rather the business cycle, we would expect qualitatively similar results regardless of whether the GIRFs are computed conditional on the ZLB or recessionary periods. On the other hand, if the impulse responses produced with this model are different, then the nonlinear effects across monetary regimes should not be due to the business cycle channel. Similar to Caggiano et al. (2015), we identify the US recessions by the NBER business cycle dates.[11] Given that the purpose of this exercise is to disassociate the effects of the ZLB on macroeconomic activity from the effects of economic slack, the discussion in this section focuses on the responses in the recession state.

Figure 3.4 reports the estimation results and 68 percent confidence interval obtained from this exercise. The result indicates that the response of macroeconomic variables conditional on recessionary episodes is considerably similar to the one obtained from the baseline model conditional on normal times. We find a less persistent response of output and private consumption to a positive government spending shock, with output displaying a slight negative impact around two to three years after the shock occurs. Noticeably, the responses of all endogenous variables lie within the 68 percent confidence bands estimated for normal times in the baseline IVAR. This finding suggests that the state-dependent fiscal spending effects presented with our baseline model with regard to the ZLB episodes are not driven by the nonlinear effects across different states of the business cycle.

[11]The NBER defines a recessions as “a period between a peak and a trough in the business cycle where there is a significant decline in economic activity that is spread across the economy and that lasts more than a few months”, see The NBERâĂŹs Business Cycle Dating Procedure.

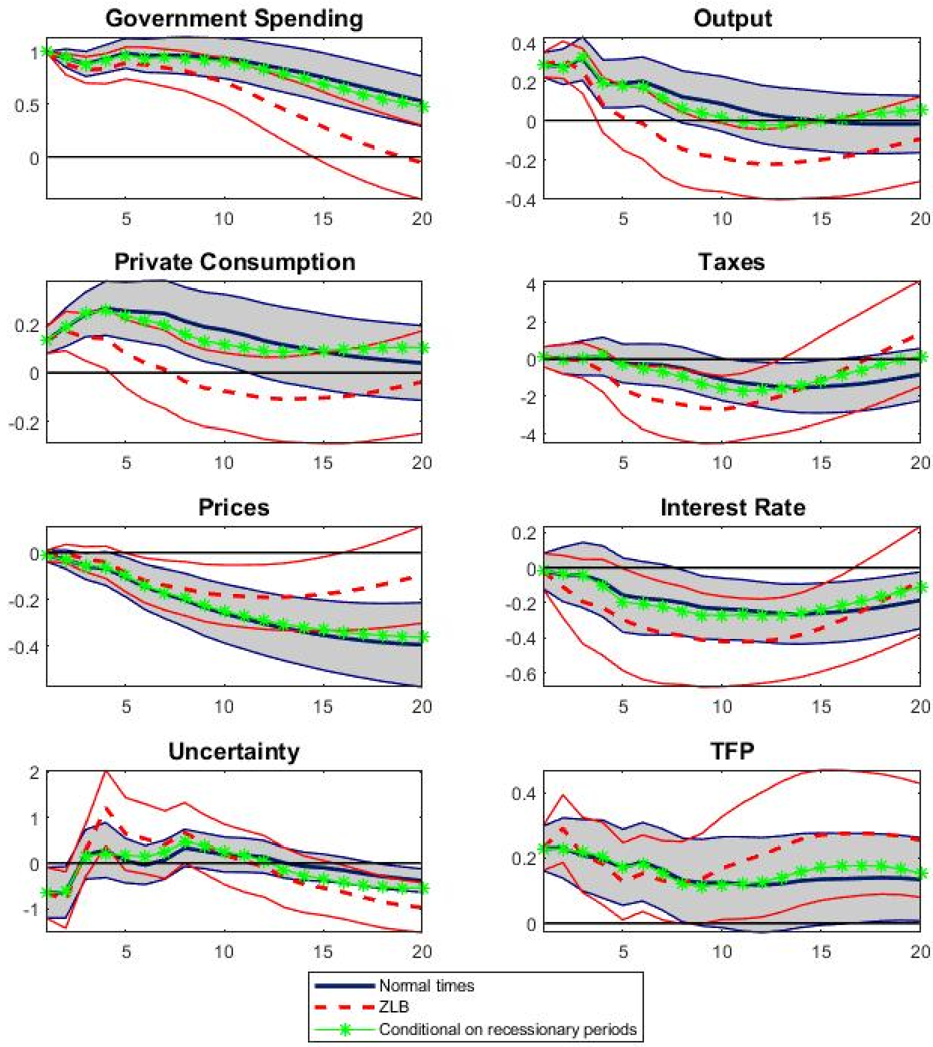


Figure 3.4: Generalized impulse responses to a one percent increase in government spending shock using the identification based on forecast errors. Sample period: 1966Q4-2019Q4. Dashed-red lines: Baseline GIRFs in the ZLB regime. Solid-blue lines: Baseline GIRFs in the Normal times. Starred-green lines refer to the GIRFs obtained by conditioning on recessionary periods defined by the NBER recession indicator. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and Normal times states of the baseline model.

One might argue that the reason why the GIRFs of the variables of interest conditional on the recessionary periods resemble the baseline GIRFs conditional on the ZLB is due to the fact that the nonlinearity is still generated via the monetary policy rate channel. Moreover, given that our sample only features a small subset of ZLB observations, the recession periods defined by the NBER recession indicator are likely to contain a greater number of normal times observations, hence the similarity in the responses. To address this concern, we estimate an alternative version of the IVAR model, featuring an interaction term between government spending and a measure of economic slack. To this end, we use the quarterly growth rate of GDP, calculated as the first difference of natural logarithm of real GDP, as a proxy for the state of the business cycle. We then estimate the following model to investigate the role played by the business cycle channel:

$$\mathbf{X}_t = \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 t + \sum_{j=1}^{p} \mathbf{A}_j \mathbf{X}_{t-j} + \left[\sum_{j=1}^{p} \mathbf{c}_j FE_{t-j} \times \Delta Y_{t-j} \right] + \mathbf{u}_t \tag{3.3}$$

where ΔY_{t-j} is the quarterly GDP growth rate. Our purpose is to look at the role of the business cycle stance. Thus we run this model over the same sample period 1966Q4-2019Q4 and compute the GIRFs conditional on the same ZLB period as in the baseline estimation. The logic of this check is similar to the previous one. If the nonlinear effects of government spending policy across monetary regimes are simply reflections of nonlinear effects across the business cycle, the alternative framework using the interaction terms featuring the GDP growth rate should return the GIRFs which mirror the baseline results, especially during the ZLB periods.

The use of GDP growth rate as an indicator of recessionary periods is common in the literature. For instance, Auerbach and Gorodnichenko (2012) and Bachmann and Sims (2012) employ a STVAR model and use a seven-quarter moving average of the quarterly growth rate of GDP as the transition variable. Caggiano et al. (2015) also use the moving average series but of different order for the same purpose. The major advantage of using this variable as a proxy for the stance of the business cycle is that we can incorporate the dynamic feedback from policy shocks to the state of the regime.[12]

[12]Note that there are various potential proxies for the stance of business cycle. For instance, Stock and Watson (1989) utilize a coincident index of the business cycle, now called the Chicago Fed National Activity Index. Chauvet and Piger (2008) consider a smoothed recession probabilities for the US obtained from a Dynamic Factor Markov-Switching model. Ramey and Zubairy (2018), on the other hand, use unemployment rate to define the state of the business cycle. In particular, they add the unemployment into the vector of endogenous variables and use 6.5 as the threshold to define recessions (expansions) as periods where the unemployment rate is above (below) 6.5 percent. A variety of studies also consider alternative indicators of slack periods, such as output gaps or capacity utilization (Fazzari et al., 2015, Morley et al., 2021), however, due to data availability and the fact that it is generally accepted as a business cycle indicator, we use the GDP growth rate for the sake of this exercise. Nevertheless, our

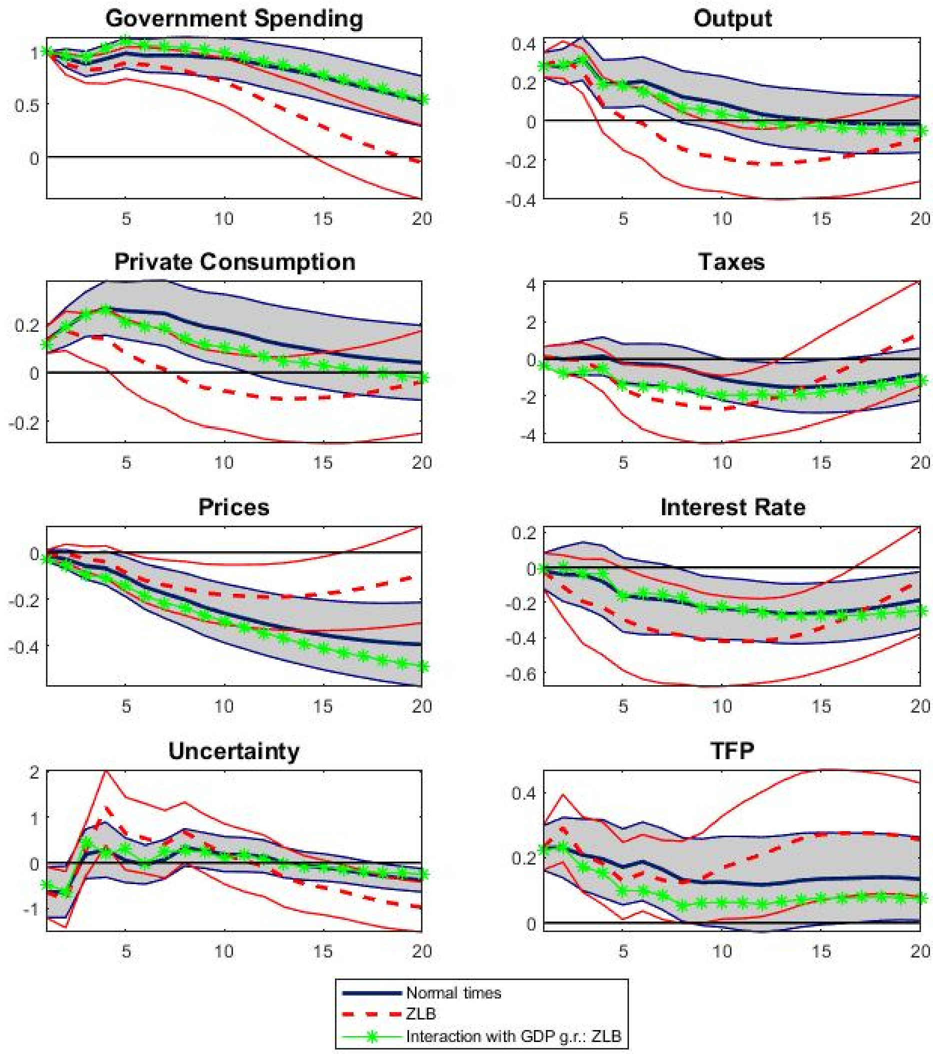


Figure 3.5: Generalized impulse responses to a one percent increase in government spending shock using the identification based on forecast errors. Sample period: 1966Q4-2019Q4. Dashed-red lines: Baseline GIRFs in the ZLB regime. Solid-blue lines: Baseline GIRFs in the Normal times. Starred-green lines refer to the GIRFs obtained by the model featuring interaction terms between government spending and real GDP growth rate conditional on the ZLB states. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and Normal times states of the baseline model.

Figure 3.5 reports the estimation results and 68 percent confidence interval obtained from this exercise. The results show that if we form the interaction terms between government spending and GDP growth rate to reflect the nonlinear impacts of fiscal spending shocks, the response of macroeconomic variables conditional on ZLB episodes appears different from the baseline ZLB response. In particular, output and private consumption react positively to a government spending shock on impact, followed by a temporary overshoot, suggesting fiscal intervention's effectiveness in stimulating GDP in times of economic distress. This is in line with Auerbach and Gorodnichenko (2012). We find that the estimated responses exhibit the same pattern as those produced with the base model in normal times. Noticeably, these responses lie within the 68 percent confidence bands estimated for normal times in the baseline IVAR. This finding once again confirms that the baseline results with regard to the ZLB episodes are not determined by the effects of business cycle conditions, thus underpins the significance of integrating the ZLB into the discussion of macroeconomic performance and policy analysis.

3.6.3 Alternative measures of monetary policy and uncertainty

We examine a series of alternative specifications of our baseline IVAR model with forecast error identification to check the robustness of our findings.

Federal funds rate. The baseline model uses the shadow rate constructed by Wu and Xia (2016) as a proxy for the stance of monetary policy. This variable has been widely used in the literature since it accommodates the effect of unconventional monetary policy intervention during the ZLB episodes. Specifically, the shadow rates are negative when the actual short-term rates remain at the zero bound. To account for the possibility that our results might have been biased by the choice of policy rate variable, we re-estimate the model using conventional monetary policy tools, i.e. the federal funds rate.

1-year Treasury bill rate. We also check if the Federal Reserve could have been able to affect the long-term rates via unconventional monetary policy including asset purchases and forward guidance when the conventional policy rate was stuck at its effective lower bound. Swanson and Williams (2014) discuss the effects of FOMC announcements on long-term yields and show that private agents have expected the constraint to be present for about one or two years into the future. To account for this expectation, we re-estimate our IVAR model featuring the 1-year Treasury bill rate.[13]

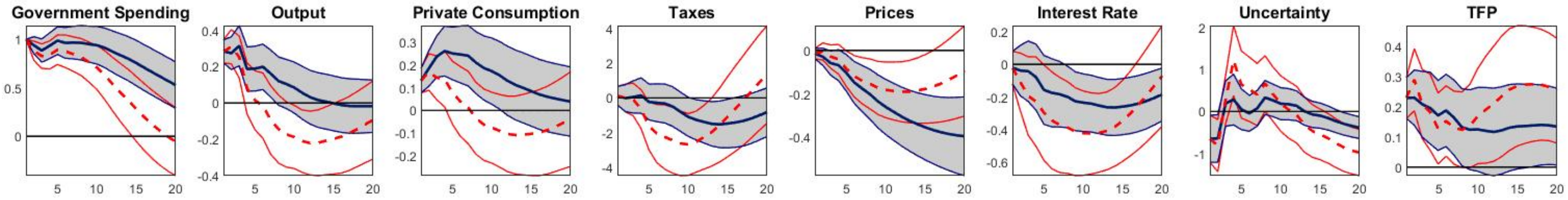
baseline results are robust to using alternative indicators of business cycle. We report the results obtained using capacity utilization in Figure 3.11.

[13]We select the 1-year series for longer sample period. Nevertheless, the results obtained with the 2-year bond yield rates are very similar.

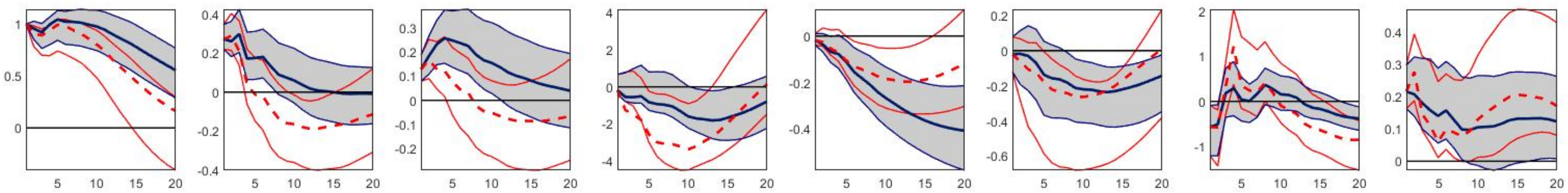
Inflation rate. Our main research question involves the price puzzle, hence it is of interest to investigate this issue using rates of inflation. Some authors tend to utilize the inflation measures rather than price or log price levels to eliminate the nonstationary trend and noisy price-level shocks. We then run an exercise in which we replace the log price level with the inflation rates based on the CPI (seasonally adjusted) and re-run the model.

The results of these exercises are documented in Figure 3.6. The second row plots the GIRFs of the endogenous variables to a government spending shock with and without the ZLB with the federal funds rate. The third row illustrates the responses to government spending surprises at and away from the ZLB using the 1-year Treasury bill rate. The last row of Figure 3.6 reports the responses when replacing the price levels with the inflation rate. Our baseline finding is robust to alternative proxies of the policy rate as very little difference can be detected with respect to the responses obtained in our baseline framework. The responses of real activity indicators are statistically in line with those obtained with the baseline model at 68 percent in both states of the economy. Likewise, while a surprise fiscal spending expansion raises inflation in the long run after about three years, the response is insignificant due to the confidence bands.[14] Thus, the main finding that government spending shocks produce disinflationary responses is robust to alternative specifications.

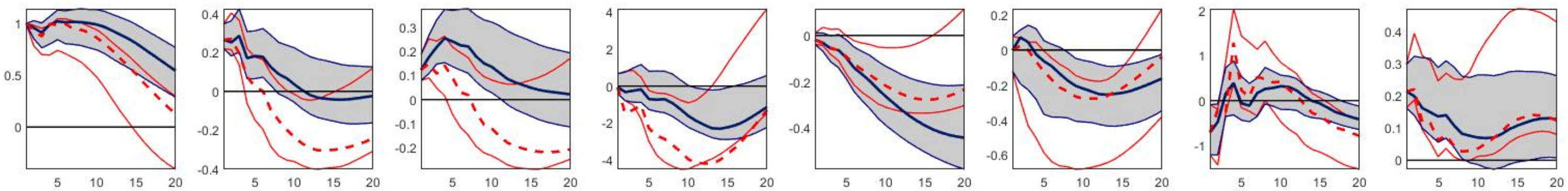
[14]The ZLB response is insignificant at 90 percent level.



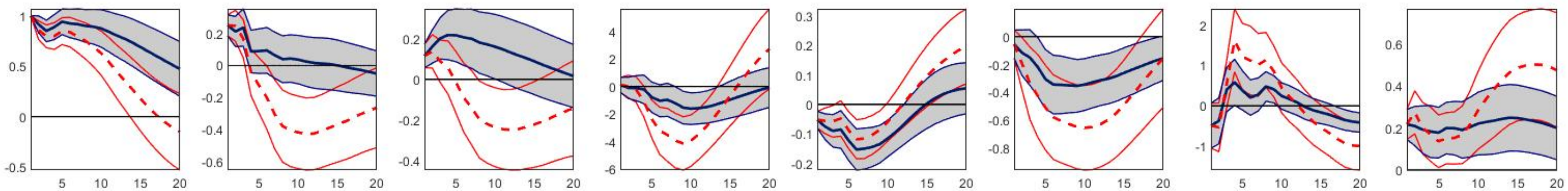
(a) Baseline



(b) Federal funds rate



(c) 1-year Treasury bill rate

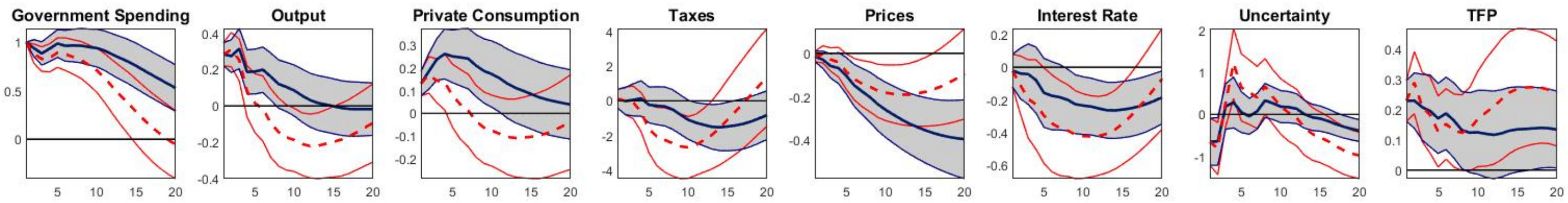


(d) Inflation

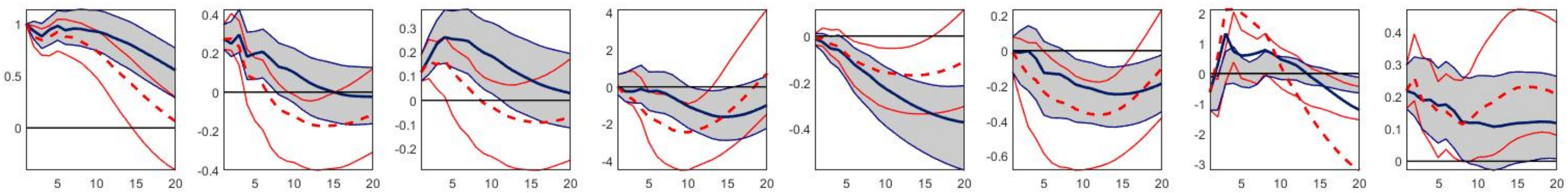
Figure 3.6: Generalized impulse responses to a one percent increase in government spending shock using the identification based on forecast errors: ZLB vs. Normal times for alternative measures of monetary policy. Sample period: 1966Q4-2019Q4. Dashed-red lines: GIRFs in the ZLB regime. Solid-blue lines: GIRFs in the Normal times: (a) Baseline analysis; (b) Federal funds rate; (c) 1-year Treasury bill rate (d) Inflation rate. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and Normal times states of the baseline model.

The second set of checks look at the uncertainty channel. The baseline result indicates that uncertainty appears to be an important channel for the transmission of government spending shock to macroeconomic variables under different states of monetary policy. In the baseline estimation, we follow Bloom (2009) and use the VIX as an uncertainty indicator. However, it is not surprising that there has been an active discussion in the literature on how to gauge the uncertainty due to its abstract nature. Generally, uncertainty is defined as the conditional volatility of unforeseeable disturbance arising from the perspective of economic participants. A number of studies have suggested various measures of uncertainty through different channels. Recently, Ludvigson et al. (2021) propose some new proxies of uncertainty, which are constructed via a factor approach to h-step-ahead forecast errors related to a large panel of US macroeconomic time series. They argue that it is critical to consider a measure of uncertainty that captures the unpredictability of the economy as a whole, rather than the mere volatility of some specific economic variables. To control for the potential predictability of our uncertainty indicator, we experiment with the financial, macroeconomic and real uncertainty index developed by Ludvigson et al. (2021).[15] Figure 3.7 supports the existence of the price puzzle and the heightened uncertainty when the ZLB is binding for the Ludvigson et al. (2021) measures of uncertainty. Similarly, the GIRFs of all real activity variables are included within the 68 percent confidence bands associated with different regimes of monetary policy computed by our baseline IVAR at all horizons. This result suggests that our finding is robust to alternative proxies of monetary policy and uncertainty index.

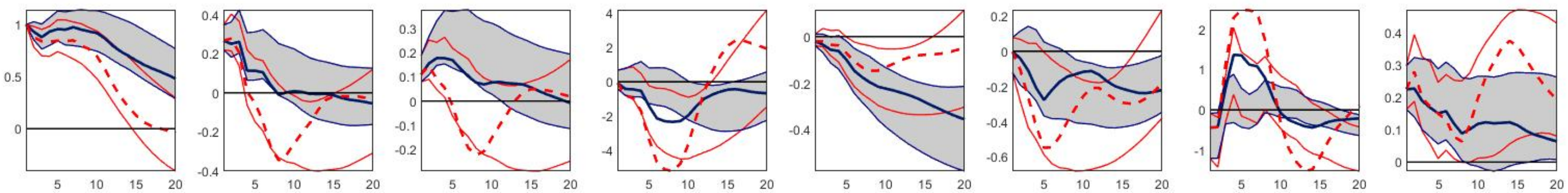
[15]Bloom et al. (2018) proposed an alternative proxy of uncertainty as the interquartile range (IQR) of sales growth for a sample of Compustat firms. This variable has been preferred by some researchers due to the fact it reflects the firm-specific shocks whereas the majority of uncertainty indicators capture aggregate economic behaviour. However, we do not consider this variable for our robustness checks due to the fact that it is only available up to 2010, which leaves too few observations for the ZLB period.



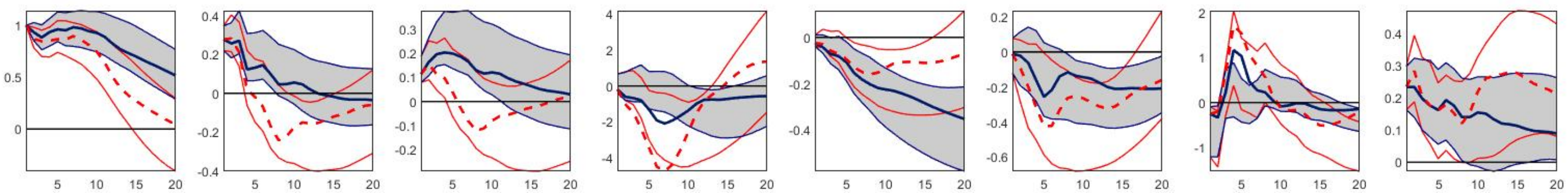
(a) Baseline



(b) Financial uncertainty



(c) Macroeconomic uncertainty



(d) Real uncertainty

Figure 3.7: Generalized impulse responses to a one percent increase in government spending shock using the identification based on forecast errors: ZLB vs. Normal times for alternative measures of uncertainty index. Sample period: 1966Q4-2019Q4. Dashed-red lines: GIRFs in the ZLB regime. Solid-blue lines: GIRFs in the Normal times: (a) Baseline analysis; (b) Financial Uncertainty Index; (c) Macroeconomic Uncertainty Index (d) Real Uncertainty Index. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and Normal times states of the baseline model.

3.6.4 Components of government spending

Recently, a rising body of the literature focuses on examining the behaviour of economic activity in response to innovations of different components of government purchases. Does the deflationary response when the economy is at the ZLB relate to the composition of government spending shocks? Indeed, it is conceivable to look at the types of government spending when investigating the relationship between fiscal shock and its potentially state-dependent impact on macroeconomic activity. The composition of fiscal policy, i.e., public consumption expenditures and gross investment, can affect the overall impact of fiscal stimulus since they embody different types (demand and supply) of shocks, thus triggering different reactions of inflation when monetary policy is constrained by the ZLB. Boehm (2020) provides evidence that a government consumption shock leads to a more expansionary response compared to a same-size government investment shock, resulting in a smaller government investment multiplier. In this section, we decompose the total government purchases and consider the shocks to public consumption and gross investment to obtain a deeper view of the interaction between fiscal spending and monetary policy rate.

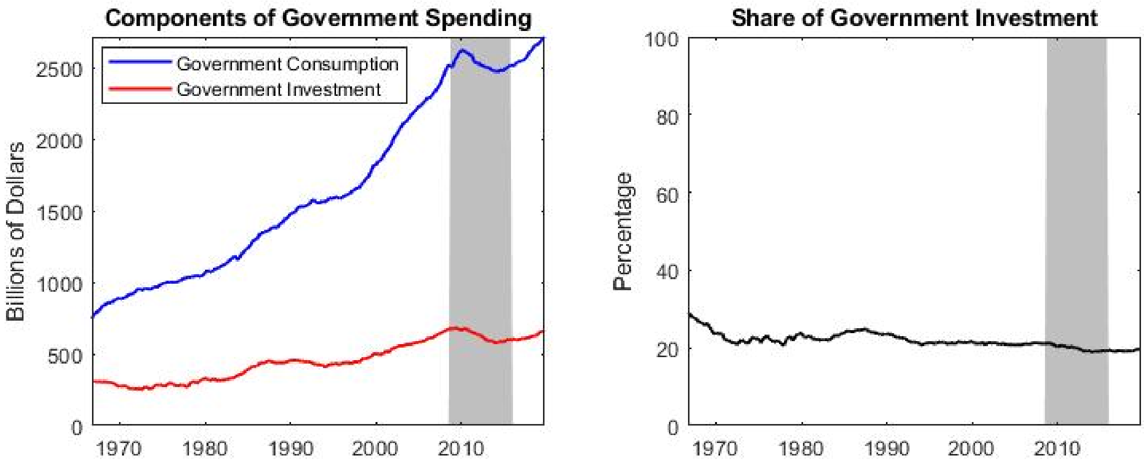


Figure 3.8: Composition of government spending. The left column displays the real government consumption expenditures and gross investment. The right column shows the share of investment in total government purchases. Sample period: 1966Q4-2019Q4. The shaded area denotes the ZLB period.

The left column of Figure 3.8 illustrates the components of government spending policy for our sample (1966Q4-2019Q4). It can be seen that total government purchases are dominated by government consumption expenditure across the entire sample. Additionally, we observe that there is a drastic increase in both government consumption and investment at the beginning of the ZLB episodes, followed by a quick drop and rebound near the end of this phase. Next, we look at the share of components in total government expenditure. The right column of Figure 3.8 reveals that the proportion of investment is rather stable at approximately 20 percent during the ZLB period, implying that there is no substantial systematic change in the composition of government spending from normal times to ZLB. Therefore, the deflationary response during the ZLB episodes is not driven by the composition changes in total fiscal spending.

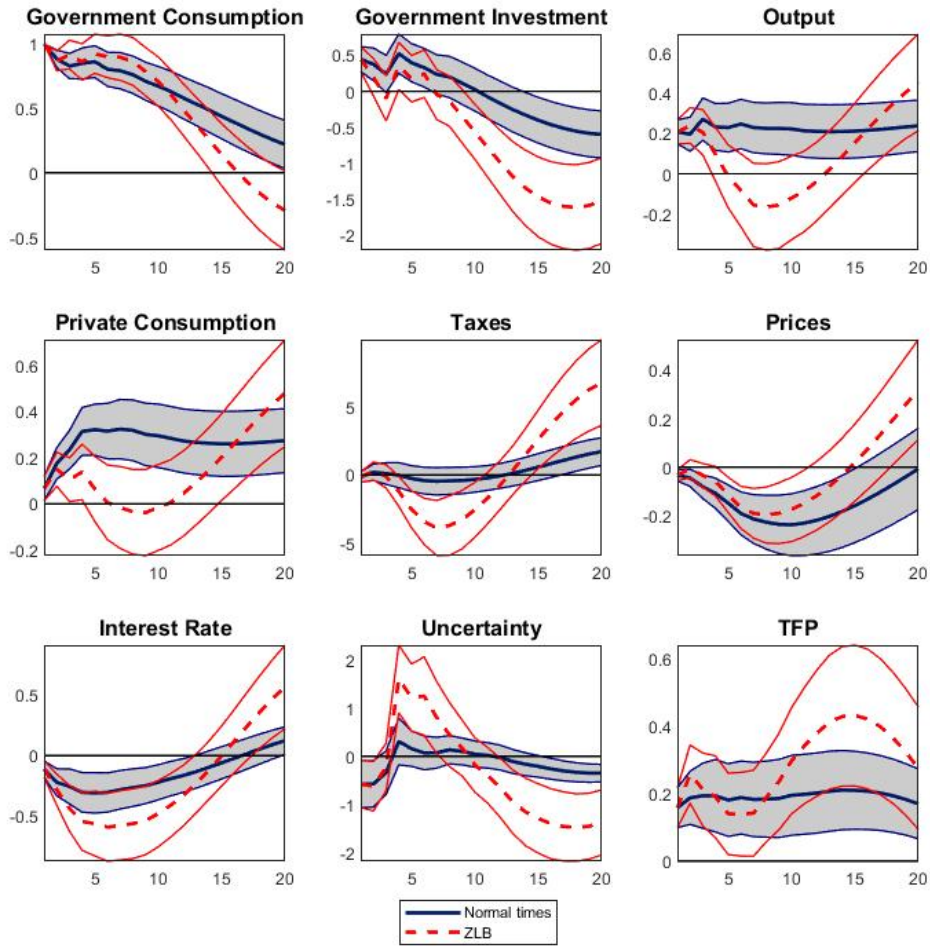


Figure 3.9: Generalized impulse responses to a one percent increase in government consumption shock using the identification based on forecast errors: ZLB vs. Normal times. Sample period: 1966Q4-2019Q4. Dashed-red lines: ZLB regime. Solid-blue lines: Normal times. Solid-red lines and grey areas: 68 percent confidence bands.

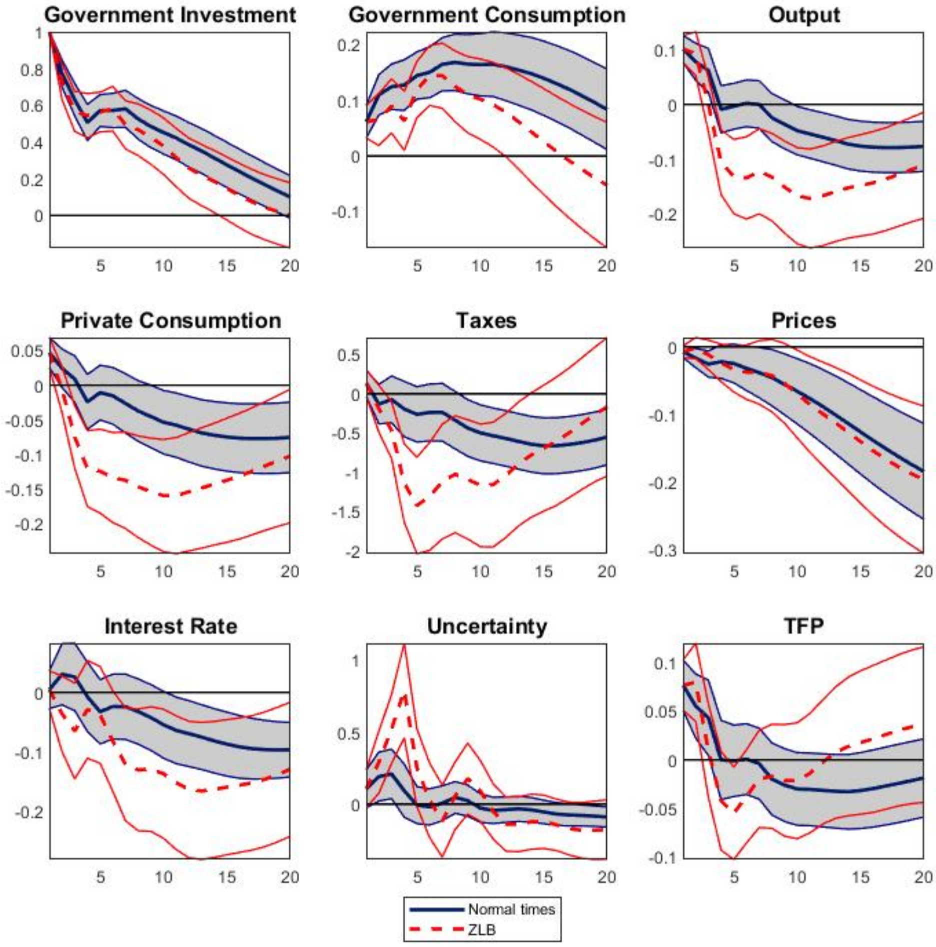


Figure 3.10: Generalized impulse responses to a one percent increase in government investment shock using the identification based on forecast errors: ZLB vs. Normal times. Sample period: 1966Q4-2019Q4. Dashed-red lines: ZLB regime. Solid-blue lines: Normal times. Solid-red lines and grey areas: 68 percent confidence bands.

In order to examine the role of spending composition, we explore the effects of government consumption and government investment separately. The model specification for each instance is as follows:

$$\mathbf{X}_t = \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1 t + \sum_{j=1}^{p} \mathbf{A}_j \mathbf{X}_{t-j} + \left[\sum_{j=1}^{p} \mathbf{c}_j FE^i_{t-j} \times R_{t-j} \right] + \mathbf{u}_t \tag{3.4}$$

where i represents the type of government spending, i.e., consumption or investment.

Corresponding GIRFs are depicted in Figures 3.9 and 3.10, respectively. In light of our main research question, the results show no sizable difference compared to the responses obtained with our baseline model featuring the interaction between total government spending and interest rate, i.e., prices decline regardless of the types of government spending. This finding, therefore, provides support for our baseline result.

We observe some quantitative differences in responses of the endogenous variables, for instance, the negative effect of private consumption, output, prices and interest rate after a government investment shock during the ZLB state are more persistent and does not diminish in the medium to long run as in the government consumption case. Furthermore, regarding the response of TFP, one would have expected to find an elevated response of TFP to a government investment shock. However, Figure 3.10 shows that although a positive investment shock raises TFP on impact, the effect dies down after three quarters and becomes insignificant in the following periods. In contrast, a positive government consumption shock keeps productivity above its trend at all horizons in both regimes. This finding, in conjunction with the observation that prices decline more dramatically in response to an investment shock, make it difficult to reconcile with the notion that a supply shock enhances productivity, which leads to the deflationary response of price level. Nevertheless, it does not rule out the potential role of the supply channel in understanding the propagation mechanism of fiscal policy shocks and we leave this for future research.

We compute the fiscal spending multipliers corresponding to the government consumption and investment shocks. The estimates are reported in Table 3.3. It is not surprising to find that the government consumption multipliers are remarkably larger than the investment counterparts on impact. The investment multipliers turn negative quickly, two years and three years following an government investment innovation at the ZLB periods and normal times, respectively.

3.7 Conclusion

We study the macroeconomic effects of government spending shocks at the ZLB and in normal times using an IVAR model to take into account the endogenous movement of interest rate following a government spending innovation. Starting from a linear VAR

Table 3.3: Estimates of government consumption and investment multipliers: Normal times vs. ZLB

	Gov. Consumption		Gov. Investment	
Quarters/State	ZLB	Normal	ZLB	Normal
1	1.36 [1.33 , 1.39]	1.36 [1.33 , 1.38]	0.43 [0.40 , 0.46]	0.41 [0.39 , 0.45]
4	1.02 [0.93 , 1.11]	1.55 [1.49 , 1.61]	0.07 [-0.02 , 0.36]	0.30 [0.23 , 0.36]
8	0.22 [-0.01, 0.44]	1.86 [1.76 , 1.87]	-0.54 [-0.77 , 0.32]	0.09 [-0.02 , 0.19]
12	0.04 [-0.46, 0.55]	2.23 [2.07 , 2.38]	-0.97 [-1.47, 0.06]	-0.06 [-0.22 , 0.09]
16	0.82 [-0.05, 1.69]	2.59 [2.39 , 2.79]	-1.32 [-2.18, -0.04]	-0.19 [-0.39 , 0.01]
20	2.28 [0.84 , 3.65]	3.05 [2.81 , 3.30]	-1.63 [-2.31, -0.11]	-0.31 [-0.55 , -0.06]

Notes: The table reports cumulative multipliers for components of total government spending. Log-values of the GIRFs of output rescaled by the sample mean of output over public spending (both taken in levels) to convert percent changes into dollars.

model specification, we show that when moving to a nonlinear setting, the puzzling effects of government spending shocks on inflation are supported regardless of whether the economy is at or away from the ZLB. Our finding contradicts the predictions of the textbook new Keynesian models in the presence of the ZLB, yet agrees with and serves as complementary to a large body of the empirical literature on this topic (Jørgensen and Ravn, 2022, D'Alessandro et al., 2019). This suggests that the impacts of fiscal stimuli might not translate into the traditional inflation channel in practice. Instead, the negative behaviour of inflation is induced by increased domestic productivity, hence generates a supply-side boost that offsets the increase in demand.

In addition, we find that the response of GDP and considered measures of real activity appears to vary with the monetary policy stance. A shock in government spending induces positive output and private consumption impacts in normal times, while it turns out to be contractionary when ZLB is binding. This finding is rationalized via the uncertainty channel as documented in our result. In particular, it is possible that a jump in fiscal spending fails to stimulate the economy as expected due to the pessimistic state of economic expectations which affects agents' sentiments and results in the deflationary effect of government spending at the ZLB. However, the difference between states is not always significant.

We also provide evidence that government spending multipliers are not larger in low-interest rate state than in high-interest rate state. Similar results are obtained when we neglect the role of fiscal anticipation. While the point estimates of cumulative spending multipliers are positive and persistent throughout the horizons in normal times, they become negative after around three years at the binding ZLB. However, the confidence bands are so wide that the effects are statistically insignificant. Indeed, one conclusion that can be drawn from the growing literature on the subject is that the size of fiscal multipliers when nominal interest rates are stuck at the ZLB is extremely uncertain. Further research into alternative model specifications and quantifying the economic response of fiscal policy at the ZLB will be fruitful.

3.8 Appendices

3.8.1 Data

The data used in the baseline specification of the IVAR model (except for the Wu and Xia (2016) shadow rate, Bloom (2009) volatility index and total factor productivity (TFP)) is taken from Federal Reserve Economic Data (FRED). We describe the data along with series name in FRED in brackets:

FE_t: Forecast errors of the government spending growth rate. Following Auerbach and Gorodnichenko (2012), forecast errors are computed as the difference between forecasts of the growth rate of government spending and the actual, first-release data for the growth rate of government spending from the Bureau of Economic Analysis. The forecasts are obtained from the Greenbook data of the Federal Reserve Board (1966Q4-1981Q2) combined with the Survey of Professional Forecasters (SPF, 1982Q3-2019Q4). We take the Greenbook data from Auerbach and Gorodnichenko (2012) and data from the SPF in overlapping observations.

G_t: Government consumption expenditure and gross investment (GCEC1, billions of dollars, seasonally adjusted, chained 2012 dollars.

Y_t: Real GDP (GDPC1, billions of dollars, seasonally adjusted, chained 2012 dollars).

C_t: Real personal consumption expenditures (PCECC96, billions of dollars seasonally adjusted, chained 2012 dollars).

T_t: Government current tax receipts (W054RC1Q027SBEA, billions of dollars, seasonally adjusted) - Government current transfer receipts (A084RC1Q027SBEA, billions of dollars, seasonally adjusted) - Government interest payments (A180RC1Q027SBEA, billions of dollars, seasonally adjusted) - Government subsidies (GDISUBS, billions of dollars, seasonally adjusted). The nominal terms are converted to real terms using the GDP deflator. The series turns negative at some points, hence a constant is added before taking logs.

P_t: Personal consumption expenditures price index (PCECTPI, seasonally adjusted, 2012=100).

R_t: Wu and Xia (2016) shadow rate, extracted from Federal Reserve Bank of Atlanta.

VIX_t (Stock market volatility index): The volatility index is constructed by Bloom (2009) by splicing the Chicago Board Options Exchange VXO index for the period after 1986 with the quarterly standard deviation of the daily S&P500 for the period before that. The monthly series is taken from Nick Bloom's website:
(https://people.stanford.edu/nbloom/sites/default/files/r.zip) and is available up to 2012Q4. We update Bloom's series up to 2019Q4 by using the VXO series taken from FRED (VXOCLS, daily, not seasonally adjusted). Quarterly data are obtained by averaging monthly and daily data.

A_t: Total factor productivity series constructed by the Federal Reserve Bank of San Francisco based on the methodology of Fernald (2012). The series is taken from:
https://www.frbsf.org/economic-research/indicators-data/total-factor-productivity-tfp/.

The first four series are converted to per capita terms using the Total population: all ages including armed forces overseas estimates, which is also collected from the FRED database (POP), available from 1952 onward (we take quarterly averages of monthly observations). We take logs of all variable except the interest rate, R_t.

In addition, we use the following series for the additional checks:

Real wage: Nonfarm business sector: real compensation per hour (COMPRNFB, seasonally adjusted, 2012=100).

1-year Treasury bill rate: Nominal interest rate on 1-year Treasury bills (TB1YR).

Alternative measures of uncertainty: Financial, macroeconomic and real uncertainty indexes constructed by Ludvigson et al. (2021). The variables are taken from Sydney Ludvigson's website (https://www.sydneyludvigson.com/macro-and-financial-uncertainty-indexes). All of the indicators used refer to a forecasting horizon equal to 1 quarter.

Government consumption expenditures: (A955RC1Q027SBEA, billions of dollars, seasonally adjusted)..

Government gross investment: (A782RC1Q027SBEA, billions of dollars, seasonally adjusted).

FE_t^C: Forecast errors of the government consumption growth rate. The forecast errors are computed as the difference between forecasts of the growth rate of government consumption and the actual, first-release data for the growth rate of government consumption from the Bureau of Economic Analysis.

FE_t^I: Forecast errors of the government investment growth rate. The forecast errors are computed as the difference between forecasts of the growth rate of government investment and the actual, first-release data for the growth rate of government investment from the Bureau of Economic Analysis.

Except for the last two series, which are extracted from the OECD Economic Outlook: Statistics and Projections database, the other series are taken from the FRED database. Government consumption expenditures and government gross investment are deflated into real values using the GDP implicit price deflator (GDPDEF, 2012=100) and converted to per capita terms as above. All variables are measured in levels except for government consumption expenditures and government gross investment, which are measured in log levels.

3.8.2 Generalized impulse response functions

This section documents the algorithm employed to compute the GIRFs and their confidence intervals. The algorithm follows Koop et al. (1996) with the modification of considering an orthogonal structural shock as in Kilian and Vigfusson (2011).

The GIRFs of the endogenous variables, $\mathbf{X}_t$, at h periods ahead, for a starting condition $\boldsymbol{\omega}_{t-1} = \mathbf{X}_{t-1}, \ldots, \mathbf{X}_{t-p}$, where p is the number of VAR lags, and a structural shock of size δ_t at time t is given as

$$GIRF_{\mathbf{X}}\left(h,\delta,\boldsymbol{\omega}_{t-1}\right)=E\left[\mathbf{X}_{t+h}|\delta,\boldsymbol{\omega}_{t-1}\right]-E\left[\mathbf{X}_{t+h}|\boldsymbol{\omega}_{t-1}\right],\quad h=0,\,1,\ldots,H$$

where $E[.]$ is the expectation operator. The procedure to estimate the state-conditional GIRFs is as follows:

1. Pick an initial condition $\boldsymbol{\omega}_{t-1}=\mathbf{X}_{t-1},\ldots,\mathbf{X}_{t-p}$. i.e., the historical values for the lagged endogenous variables at a particular date. This set includes the values for the interaction terms, $\left[\sum_{j=1}^{p}\mathbf{c}_j G_{t-j}\times R_{t-j}\right]$, since both interaction variables are considered as endogenous.

2. We draw randomly with repetition a sequence of n-dimensional residuals $\mathbf{u}_{t+h}^{s}$, $h=0,1,\ldots,H=19$ from the empirical distribution $d(0,\hat{\boldsymbol{\Omega}})$, where $\hat{\boldsymbol{\Omega}}$ is the estimated residual variance-covariance matrix, s denotes particular sequence of residuals used. In order to preserve the contemporaneous structural relationships among variables, residuals are assumed to be jointly distributed, so that we draw all n residuals together for period t.

3. Conditional on $\boldsymbol{\omega}_{t-1}$, on the estimated model equations in Section 2, we simulate the evolution of the vector of endogenous variables over H periods to obtain the path $[\mathbf{X}_{t+h}|\boldsymbol{\omega}_{t-1}]^s$. s denotes the dependence of the path on the particular sequence of residuals used.

4. Conditional on $\boldsymbol{\omega}_{t-1}$, on the estimated model equations in Section 2, we simulate the evolution of the vector of endogenous variables over H periods to obtain the path $[\mathbf{X}_{t+h}|\delta,\boldsymbol{\omega}_{t-1}]^s$ when a shock δ is imposed to $\mathbf{u}_t^s$. In particular, we obtain the $\mathbf{B}$ matrix, where $\boldsymbol{\Omega}=\mathbf{B}\mathbf{B}'$ using Cholesky decomposition and $\mathbf{B}$ is a lower-triangular matrix. The structural innovations are then recovered as $\boldsymbol{\varepsilon}_t^s=\mathbf{B}^{-1}\mathbf{u}_t^s$. We then add quantity $\delta>0$ to the scalar element of $\boldsymbol{\varepsilon}_t^s$ that refers to government spending, i.e., $\varepsilon_{G,t}^s$. We then move again to the reduced-form residuals such that $\mathbf{u}_t^{s,\delta}=\mathbf{B}\boldsymbol{\varepsilon}_t^{s,\delta}$ and use $\mathbf{u}_t^{s,\delta}$ to obtain $[\mathbf{X}_{t+h}|\delta,\boldsymbol{\omega}_{t-1}]^s$.

5. We compute the difference between paths for each simulated variable at each simulated horizon $[\mathbf{X}_{t+h}|\delta,\boldsymbol{\omega}_{t-1}]^s-[\mathbf{X}_{t+h}|\boldsymbol{\omega}_{t-1}]^s$, $h=[0,1,\ldots,H]$.

6. We repeat steps 2-5 a number of times ($S = 500$). We then store the horizon-wise average realization across s repetitions by computing a consistent estimate of the GIRFs for each given starting quarter of our sample, i.e., $\widehat{GIRF}_{\mathbf{X}}\left(h, \delta, \boldsymbol{\omega}_{t-1}\right) = \widehat{E}\left[\mathbf{X}_{t+h}|\delta, \boldsymbol{\omega}_{t-1}\right] - \widehat{E}\left[\mathbf{X}_{t+h}|\boldsymbol{\omega}_{t-1}\right] \quad h = [0, 1, \ldots, 19]$. If a given initial condition $\boldsymbol{\omega}_{t-1}$ leads to an explosive response for most of the S sequences of residuals, which means that the response of the shocked variable diverges instead of reverting to zero, then such initial condition is discarded and we repeat the s^{th} draw. Note that this stability condition is imposed for bootstrapped simulated responses, not the point-estimated responses (i.e., our GIRFs estimated using actual data), so that to back up the stability of the estimated IVAR.

7. These history-dependent GIRFs are then averaged over a particular subset of initial conditions of interest to produce the state-dependent GIRFs. For this purpose, an initial condition $\boldsymbol{\omega}_{t-1} = \mathbf{X}_{t-1}, \ldots, \mathbf{X}_{t-p}$ is classified as belonging to the ZLB state if t lies in the range of T_{ZLB}, where T_{ZLB} refers to the period spanning $2008Q4-2015Q4$, otherwise, $\boldsymbol{\omega}_{t-1}$ belongs to normal times state.

8. Confidence bands around the point estimates obtained in step 7 are computed using bootstrap. In particular, we simulate $R = 1000$ bootstrapped datasets of size equivalent to the actual sample and for each of them the interaction terms and constructed coherently with the simulated series. Then, for each dataset, we re-estimate the IVAR model and repeat steps 1-7. In implementing the procedure this time, the starting conditions and variance-covariance matrix used in the computation depend on the particular dataset r used, i.e., $\boldsymbol{\omega}_{t-1}^{r}$ and $\hat{\boldsymbol{\Omega}}^{r}$. Of the resulting distribution of state-conditional GIRFs, we consider the point estimates of the impulse responses $\pm$ 1.64 (0.98) times the bootstrapped standard errors to construct the 90 percent (68 percent) confidence bands. As regards the implementation of step 7, due to the randomness of the realization of the residuals, we classify as ZLB observations those corresponding to the lowest 13 percent realizations of the interest rate in each given simulated sample, 13 percent being the share of the ZLB periods out of the overall number of observations in the actual sample we employ in our empirical analysis.[16]

[16]If dealing with an alternative sample, this reference is adjusted accordingly.

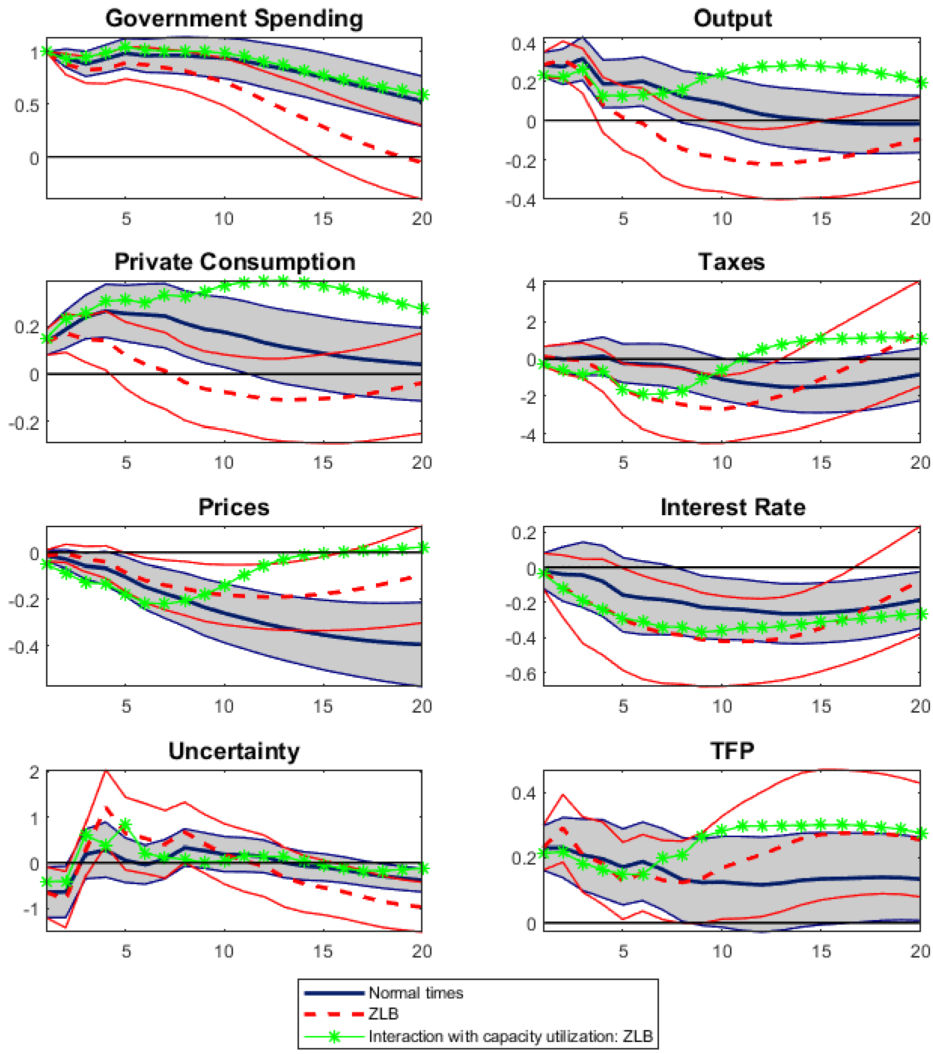


Figure 3.11: Generalized impulse responses to a one percent increase in government spending shock using the identification based on forecast errors. Sample period: 1966Q4-2019Q4. Dashed-red lines: Baseline GIRFs in the ZLB regime. Solid-blue lines: Baseline GIRFs in the Normal times. Starred-green lines refer to the GIRFs obtained by the model featuring interaction terms between government spending and capacity utilization on the ZLB states. Solid-red lines and grey areas: 68 percent confidence bands for the GIRFs conditional on ZLB and Normal times states of the baseline model.

Chapter 4

Government spending shocks and unconventional monetary policy

4.1 Introduction

The latest global financial crisis (GFC), which started after the subprime mortgage crisis in 2007 and subsequently intensified with the collapse of Lehman Brothers in September 2008, is considered one of the worst global economic crises in modern history. In the face of the severe challenge, many central banks immediately embarked on aggressive expansionary monetary policy measures to support the aggregate demand by reducing their policy rates. For instance, the Federal Reserve cut down the federal funds rate to less than 0.25 percent in December 2008. Similarly, the European Central Bank adjusted its deposit rate to 0.25 percent in May 2009.

As policy rates approached and eventually got stuck at their effective lower bounds, central banks started to employ unconventional monetary policy (UMP) to stimulate the economy and mitigate the risks to financial stability. The so-called "unconventional monetary policy" refers to those policies that attempt to influence long-term interest rates through central bank communication, i.e., "forward guidance", and expanding their balance sheets via asset purchases. In the U.S., the Federal Reserve engaged in large-scale asset purchase (LSAP) campaign, also referred to as quantitative easing (QE), to provide additional monetary support when the federal funds rate was kept close to zero from late 2008 to 2015. Particularly, the central bank purchased a substantial amount of private or government bonds using cash. This policy essentially aims to increase the monetary base and consequentially encourages lending and accelerates the economy by providing liquidity. The QE policy modifies the composition of the central bank's balance sheet, thus it is also called the central bank balance sheet policy. As of the pre-COVID-19 period, the Federal Reserve announced and implemented three QE programs and a maturity extension program that purchased over $4 trillion in medium and long-term securities - Treasury and mortgage-backed securities (MBS). Indeed, over the past decade, a number of studies have provided evidence that LSAPs were effective in easing financial conditions and stimulating the economy (D'Alessandro et al., 2019, Bauer and Neely, 2014, Swanson, 2021).

Simultaneously, in addition to the introduction of alternative monetary policy tools when the policy rates were trapped near their effective lower bound, discretionary fiscal stimulus packages have also been considered to stabilize the business cycles by policy-makers. In early 2009, the U.S. government implemented an unprecedented package, the American Recovery and Reinvestment Act (ARRA), which came in the form of government spending increases and payroll tax changes. Similarly, the European Union proceeded with the European Economic Recovery Plan (EERP) to tackle the financial crisis and boost aggregate demand. While assessing the effectiveness of fiscal policy under normal circumstances is already challenging, this becomes even more so with the unusual combination of unconventional monetary policy and fiscal policy during the ZLB periods. From a theoretical perspective, the central bank's purchases of long-term government bonds would flatten the yield curve at longer maturity and reduce the fiscal burden on the government, which might lead to an expansionary effect in government spending, as the government can use the extra liquidity to stimulate real economic growth.

An additional concern is related to the frequency of observations which are typically only available on a quarterly or yearly basis. These low frequencies could potentially induce an information loss. For example, responses of output, which vary by a large margin due to surprises in fiscal spending in the same quarter, might have been averaged out once aggregated. Moreover, limited observations when the economy is at the ZLB reduce the statistical power and distort the magnitude of effect sizes. This chapter attempts to overcome this challenge by using higher-frequency (monthly) data on fiscal spending, extracted from the monthly statements released by the Bureau of the Fiscal Service. To the best of our knowledge, no other work has proposed a study on whether the impact of government spending shock differs according to the joint presence of the ZLB and unconventional monetary policy - in particular, QE programs - using monthly-frequency data.[1]

In this chapter, we provide empirical evidence that aims to answer this question and make contributions to the existing strand of the literature focused on the variation in the responses of real activity to fiscal spending shocks according to the stance of monetary policy. We first analyze the state-dependent impact of fiscal spending shocks involving the effective lower bound. In the next step, we study the state dependence during the ZLB when QE is present versus when it is not. We use monthly data for fiscal and macroeconomic variables rather than data in quarterly or lower frequencies as in the majority of the literature. Our results show that some of the widely cited findings no longer remain when exploiting a monthly frequency. Ultimately, the findings in this chapter suggest that, in the context of assessing the impact of the fiscal shock on macroeconomic variables, the choice of frequency is not negligible.

Following Auerbach and Gorodnichenko (2016) and Ramey and Zubairy (2018), we use the local projections (LP) approach developed by Jordà (2005) to conduct our analysis. The LP approach offers several advantages over existing methodologies. First, this method does not suffer from the curse of dimensionality as in the VAR model, neither does it require a specific ordering as in a Cholesky decomposition. Thus, the LP estimates are more robust to model misspecification than the VAR impulse response functions. Furthermore, one can easily modify the local projection method to estimate a model that is contingent on the state of the economy. Finally, the LP framework allows for a direct

[1] There have been various attempts to address the lack of sufficient data to investigate the effect of spending shock when interest rates are near zero. A number of papers rely on a rich historical dataset that enables estimation based on more than 100 episodes during which the ZLB was binding (Ramey and Zubairy, 2018). They employ historical U.S. data from 1890Q1-2015Q4 to estimate the government spending multiplier in normal times, at the ZLB, and across states of economic slack. While this approach considerably enlarges the degrees of freedom, it also injects more uncertainty into inference due to the inclusion of several wartime periods. Auerbach and Gorodnichenko (2016) and Choi et al. (2022) exploit a high-frequency (daily) dataset on U.S. defense spending to identify exogenous government spending shock. Using daily variation enhances the plausibility of the identifying assumptions and enables one to focus on a particular period of interest. High-frequency analysis, however, makes it difficult to measure the response of macroeconomic variables consisting of GDP and consumption due to data unavailability and longer response lags of these variables to a policy shock. For this reason, they mainly focus on fast-moving variables such as asset or commodity prices, which are available at a daily frequency.

estimation of the responses at each h-step ahead by regressing the variable of interest on the identified shocks and control variables. As an alternative approach, we conduct similar exercises using the smooth local projections (SLP) as in Barnichon and Brownlees (2019). The SLP is computed based on B-spline smoothing that shrinks the impulse responses toward a polynomial. Nevertheless, the results produced by these procedures are qualitatively similar.

Our key finding is that government spending shocks illustrate asymmetric effects on real activity across different states of the economy. Specifically, output and private consumption display a positive and persistent response to a surprise increase in government spending in normal times. This finding is consistent with the standard economic theories on positive aggregate demand effects in response to an expansionary government spending shock. In contrast, when the effective lower bound takes place, the economy is unresponsive to the fiscal stimulus. Although our finding is difficult to reconcile with the strand of the literature in favour of stronger real effects of fiscal stimulus during economic slacks (Auerbach and Gorodnichenko, 2012), we speculate that other than the difference in model specification, this might be also due to the additional assumptions they use when conducting their study. In addition, this result can be associated with intensified uncertainty about economic prospects during heightened financial distress, which induces contraction in real activity. Furthermore, we find that prices increase following a fiscal expansion in the linear model.[2] When allowing for state-dependent responses, we find no evidence of an inflationary response to a spending shock at the ZLB. Exercises conducted with two separate regimes involving QE during the ZLB periods show that there is little evidence that the presence of QE programs gives rise to distinctive reactions of real activity to a government spending shock. However, the observation that uncertainty reacts differently and meaningfully to a fiscal expansion when QE becomes effective, again, indicates the potential role of uncertainty level in our study. Therefore, the results lend support to employing uncertainty as a potential channel to understand the propagation mechanism of fiscal spending shocks to the real economy.

This empirical study directly relates to the strand of the literature investigating the real effect of fiscal spending shocks and their effects conditional on the stance of monetary

[2] Interestingly, this is different from the results of Chapter 3. We discuss this later in the chapter.

policy. The closest papers to ours are Ramey and Zubairy (2018) and Choi et al. (2022). Ramey and Zubairy (2018) employ the LP approach to analyze the state-dependent impact of government spending shocks and compute fiscal multipliers in normal times and at the ZLB. They find no statistical evidence of larger fiscal multipliers in the ZLB state for the U.S. Despite using the same methodology, there are substantial distinctions between our exercises and theirs. First, they conduct an analysis using unanticipated fiscal spending shocks identified by news about changes in military spending while we investigate the role of an exogenous fiscal spending shock using the actual outlays. Second, they use a historical quarterly dataset starting from the 1890s to enlarge the sample size. Differently, we utilize the data at a higher frequency, i.e., monthly basis, to tease out, if any, the effects that might have been neglected when averaged out. Similarly, Choi et al. (2022) use the local projections method to investigate the impact of government spending shocks on inflation using daily data for the U.S. Our study differs from theirs due to a number of different modeling choices including the proxy for fiscal spending and variables of interest among others. Our findings are similar to theirs when it comes to the behaviour of prices in normal times vs. the ZLB period. Specifically, we observe an inflationary impact of government spending during normal times, while government spending shocks tend to be more deflationary when the ZLB is binding.

The remainder of this chapter is organized as follows. Section 4.2 describes our baseline specification, the identification scheme and the data used in our analysis. Section 4.3 presents our main findings concerning the impact of government spending shocks on the real economy using the linear and state-dependent models. Section 4.4 provides evidence on robustness checks. Section 4.5 concludes and Section 4.6 contains the appendices.

4.2 Empirical methodology

4.2.1 Local projections and identification of government spending shocks

In this section, we describe the main empirical approach used in the baseline analysis. We estimate the impact of government spending shocks using Jordà (2005)'s local projections method. This method simply requires estimation of a series of regressions for each horizon

h for each variable of interest. The linear model is given as follows:

$$x_{t+h} = \alpha_h + \beta_h shock_t + \psi_h(L) z_{t-1} + \varepsilon_{t+h} \quad \text{for } h = 0, 1, 2, \ldots, H \tag{4.1}$$

where x is the variable of interest, $shock_t$ is the identified shock, i.e., the government spending shock, $\psi_h(L)$ is a polynomial in the lag operator, and z is the vector of control variables. The vector of control variables contains GDP, government outlays, personal consumption expenditures (PCE), prices, a measure of monetary policy rate and a measure of uncertainty index. We take logarithms of all variables except for the monetary policy rate and the uncertainty index. In addition, GDP, government outlays and PCE are measured in logarithms of real per capita terms.[3] As determined by the AIC and BIC, we select the lag length of three throughout. The coefficient β_h measures the response of x at time $t + h$ to the shock at time t for each $h = 0, 1, 2, \ldots, H$ and $h = 0$ is the period when the shock occurs. Therefore, the impulse responses are computed as a sequence of the β_h in each single regression. As the error terms in (4.1) are serially correlated by construction, we apply the Newey-West correction for the standard errors (Newey and West, 1987, Ramey and Zubairy, 2018).

Using the LP method allows one to identify the shock of interest either externally, such as the narrative shocks Ramey and Zubairy (2018), or internally within the system. In this study, we adopt the latter approach by using the Blanchard and Perotti (2002) shocks. This shock is identified simply by assuming that the government spending or government outlays variable does not react contemporaneously to surprises in any other variables within the same month. In other words, the shock is given by current government spending, i.e., the variable in the regression for the $shock_t$ is g_t, since the set of controls z_t also contains lags of government spending and other variables of interest. Thus, the LP framework corresponds to the standard VAR method in identifying a government spending shock as in Blanchard and Perotti (2002), where government spending is ordered first in the vector of endogenous variables in the Cholesky decomposition.[4]

Note that the use of the Cholesky identification scheme is prone to the fiscal foresight problem related to anticipation effects by economic agents. Due to the implementation and

[3] The data section contains full details of our dataset.

[4] The Blanchard and Perotti (2002) shock has been largely adopted in much of the literature (Bachmann and Sims, 2012, Rossi and Zubairy, 2011).

legislation lags in the political process, changes in government spending might be anticipated long before they appear in the NIPA accounts. Consequently, the shock estimated with the usual timing restriction might not able to capture the exogenous component of government spending, hence providing misleading results. Various attempts have been made to tackle this issue. Ramey (2011) employs a narrative method to construct a defense spending series as changes in the expected present discounted value of government spending and uses this military news variable to identify the fiscal shocks. Auerbach and Gorodnichenko (2012) identify an unanticipated government spending shock as an innovation to the forecast errors of the growth rate of government spending. The forecast errors are defined as the differences between the forecasts and the actual government spending. Intuitively, this measure of forecast errors represents the "news" that becomes available to private agents about fiscal spending policy in near future. As pointed out by Ramey and Zubairy (2018), the main variation of the Ramey (2011) military news occurs during the WWII and the Korean War. Therefore, the military news variable after the Korean War has very low explanatory power and might not be informative as an instrument. In addition, data on military build-ups as well as the government spending forecasts are not available at the monthly frequency.

Auerbach and Gorodnichenko (2016) use daily data on U.S. defense spending and conduct an empirical exercise focused on asset prices available at a daily frequency. In particular, they construct two daily series, i.e., the actual defense outlays obtained from the daily statements of the U.S. Treasury and the announcement of future defense spending contracts based on the daily reports of the Department of Defense (DoD). Although both series are available at a higher frequency, we do not use these variables in our analysis for the following reasons. First, both series display a low correlation of less than 0.2 to the total government spending when aggregated to quarterly values. Second, as Auerbach and Gorodnichenko (2012) point out, the DoD reports a mix of existing and new contracts, thus it could also suffer from the anticipated effects. To overcome this issue, they only use announcements of new contracts. Nevertheless, since the series is constructed directly from the daily postings and requires many judgment calls, it is highly volatile. We speculate some of this volatility may be due to the inevitable measure error in the construction process.

For these reasons, we employ the Blanchard and Perotti (2002) identification method. We acknowledge the potential limitations of this approach but note that the estimated effects of fiscal spending stimulus on macroeconomic variables are consistent across empirical specifications. We show in the last column of Figure 4.11 in the Appendices that the results obtained using the Cholesky decomposition and the forecast errors as in Auerbach and Gorodnichenko (2012) to identify government spending shock are qualitatively similar at a quarterly frequency.

We estimate the models using monthly data over the period 1980:10 to 2019:12. We choose the starting point to coincide with the availability of the outlays series taken from the MTS. The data includes the recent crisis, during which the quantitative easing programs were implemented with a binding ZLB on the short-term nominal interest rates, which is necessary to our investigation of how the impact of fiscal spending varies by the state of the economy. We end the sample period in 2019:12 to exclude the extreme observations during COVID-19 due to the huge data variation in the months following the pandemic outbreak. This can bias the estimation of parameters and impulse responses during the ZLB period.

This method is particularly useful in the context of our study since it can be easily adapted to accommodate state-dependence. In addition, it allows for more flexible impulse response estimation and avoids imposing strict assumptions on the dynamics of the data. Not surprisingly, numerous studies in the literature have exploited the local projections methodology to evaluate the nonlinear impacts of fiscal policy on economic activity, see, for example, Auerbach and Gorodnichenko (2016) and Ramey and Zubairy (2018). Following Ramey and Zubairy (2018), we extend the local projections approach to allow the responses to vary by the state of the economy by estimating the following model:

$$\begin{aligned} x_{t+h} &= I_{t-1} \times [\alpha_{A,h} + \beta_{A,h} g_t + \psi_{A,h}(L) z_{t-1}] \\ &\quad + (1 - I_{t-1}) \times [\alpha_{B,h} + \beta_{B,h} g_t + \psi_{B,h}(L) z_{t-1}] + \varepsilon_{t+h}, \end{aligned} \tag{4.2}$$

where I_{t-1} is a dummy variable that indicates the state of the economy when the shock occurs.

Furthermore, we can easily extend the model to accommodate more sophisticated regimes. For example, in the case where one is interested in studying the impact of a government spending shock at the ZLB when QE is in place versus when it is not, the three regimes correspond to normal times, ZLB with QE and ZLB without QE. We then estimate specifications of the following type:

$$\begin{aligned} x_{t+h} &= I_{t-1}^{ZLB} \times I_{t-1}^{QE} \times [\alpha_{A,h} + \beta_{A,h} g_t + \psi_{A,h}(L) z_{t-1}] \\ &+ I_{t-1}^{ZLB} \times \left(1 - I_{t-1}^{QE}\right) \times [\alpha_{B,h} + \beta_{B,h} g_t + \psi_{B,h}(L) z_{t-1}] \\ &+ \left(1 - I_{t-1}^{ZLB}\right) \times [\alpha_{C,h} + \beta_{C,h} g_t + \psi_{C,h}(L) z_{t-1}] + \varepsilon_{t+h}, \end{aligned} \tag{4.3}$$

where I_{t-1}^{ZLB} is a dummy variable that equals one when the economy is at the ZLB and zero otherwise and I_{t-1}^{QE} is a dummy variable that equals one when the QE is in place and zero otherwise. The impulse responses of variable x_t to the government spending shock g_t in the ZLB combined with active QE, for instance, are given by $\{\beta_{A,h}\}$. As in Ramey and Zubairy (2018), all of the coefficients of the model are allowed to vary according to the states of the economy to obtain the state-dependent impulse response function.

4.2.2 Smooth local projections

It is arguable that the local projections approach is more flexible and can be easily adapted to a state-dependent setting. However, despite the advantages of this methodology, the impulse response estimates from LP are highly volatile. To tackle the excessive variability in the estimates computed by this method, one might use polynomials, splines or other smoothing toolkits. In this section, we test the sensitivity of our results using the smooth local projections (SLP) framework as proposed by Barnichon and Brownlees (2019). This method imposes the restriction that the estimated impulse responses are a smooth function of the forecast horizon based on the B-spline smoothing as per Eilers and Marx (1996) and generalized ridge estimation. The approximate value of β_h is obtained using linear B-spline basis function expansion in the forecast horizon h, so that x_{t+h} is approximated as:

$$x_{t+h} = \sum_{k=1}^{K} a_k B_k(h) + \sum_{k=1}^{K} b_k B_k(h) g_t + \sum_{k=1}^{K} c_k(L) B_k(h) z_{t-1} + \varepsilon_{t+h} \tag{4.4}$$

where B_k for $k = 1, \ldots, K$ is a set of B-spline basis functions and $\alpha_h = \sum_{k=1}^{K} a_k B_k(h)$, $\beta_h = \sum_{k=1}^{K} b_k B_k(h)$ and $\psi_h(L) = \sum_{k=1}^{K} c_k(L) B_k(h)$ in which α_k, β_k and φ_k are vectors of coefficients in (4.2); and a_k, b_k and c_k for $k = 1, \ldots, K$ are sets of scalar parameters. We estimate the SLP using the same set of controls in the baseline model for consistency and comparability. Similarly, the SLP can be extended to allow for state-dependent impulse responses. Thus, we perform the SLP exercise for all models considered in the previous section, including the linear and state-dependent models.

4.2.3 Data

Our objective is to analyze whether the impact and the transmission mechanism of fiscal spending shocks on real activity differ when the ZLB and quantitative easing (QE) are in place. To this end, the vector of endogenous variables includes measures of fiscal policy stance, real activity, prices and uncertainty index. Government spending data is crucial to evaluate our research questions, however, they are only available at a quarterly frequency. This low frequency does limit the scope of our investigation given the relatively short sample in our analysis.[5] To exploit the information at a higher frequency, we use an alternative measure of government expenditures which is extracted from the outlays item included in the Monthly Treasury Statement (MTS) provided by the Bureau of Fiscal Services, U.S. Department of the Treasury. The outlays variable is converted into real terms using the PCE price index and seasonally adjusted by the X-13 algorithm.

The use of federal outlays as a measure of fiscal spending is not completely unfamiliar in the existing literature. A number of works have utilized this variable and a variety of items in the MTS to investigate the spectrum of fiscal policy relevant to the times. Cohen and Follette (2003) use data on defense spending on a daily and monthly basis from the Daily Treasury Statement (DTS) and MTS to produce forecasts of defense spending. Landefeld et al. (2008) provide an overview of how the National Income and Product Accounts (NIPA) employ the outlays data for the GDP final expenditures approach. In a more recent study, Cortes et al. (2022) use the MTS to construct a monthly dataset of expenditures and analyze the impact of military shock on stock volatility in the U.S.

[5] The Federal Reserve pursued the unconventional policies between January 2009 and October 2015, when its traditional monetary policy tool, the federal funds rate, was near the zero bound.

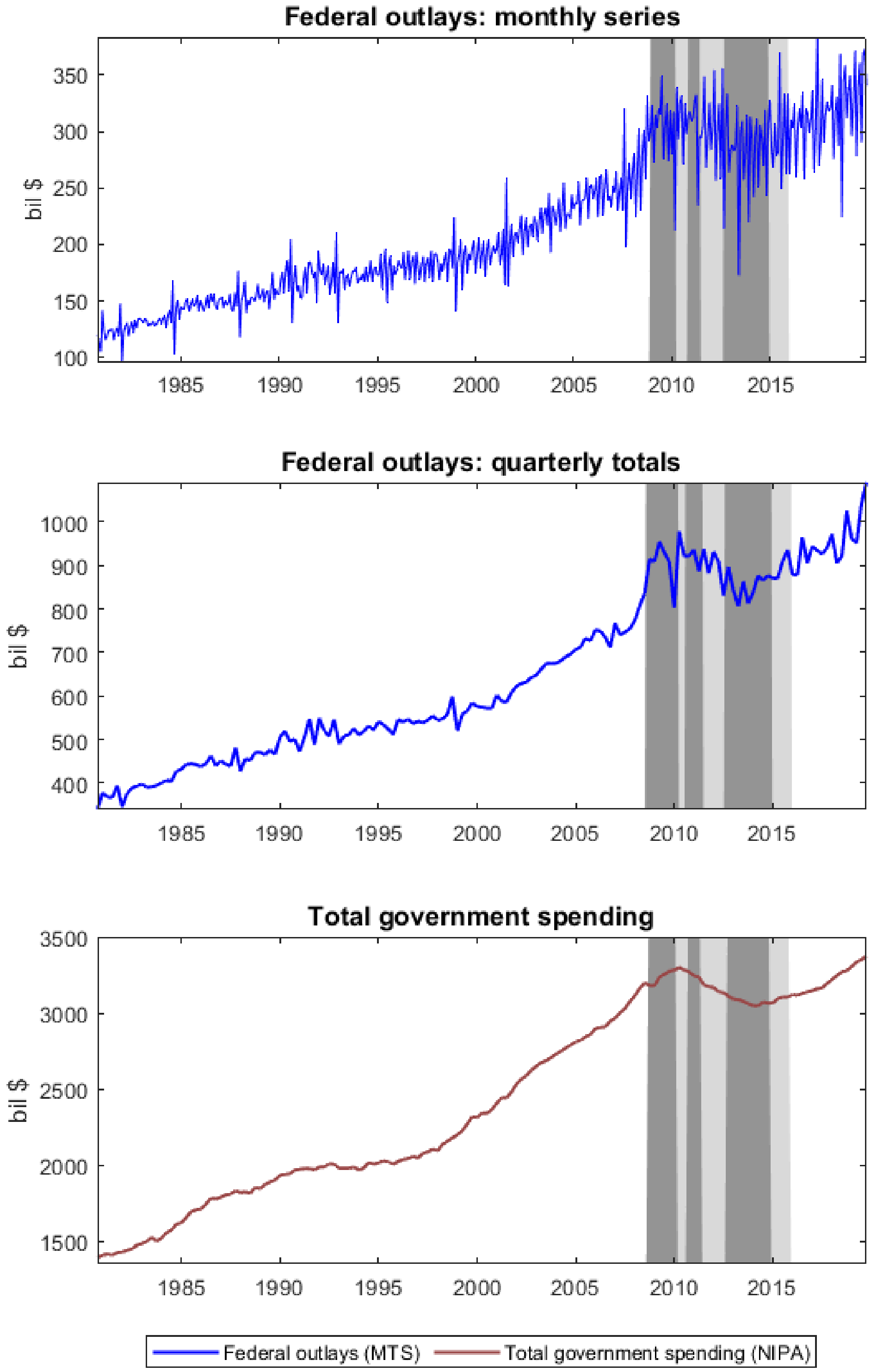


Figure 4.1: Federal outlays and government spending (NIPA). The federal outlays series is taken from the Monthly Treasury Statement (MTS) of the Bureau of the Fiscal Service, fiscal years 1981-2022. Monthly and quarterly series are seasonally adjusted using the X-13 algorithm. Shaded areas represent the ZLB period, in which the dark grey areas indicate the ZLB periods combined with QE and the light grey areas indicate the ZLB periods without QE.

To assess the quality of the outlays series, we aggregate them to quarterly data and compare the resulting series with the corresponding data from the Bureau of Economic Analysis (BEA), NIPA. The NIPA counterpart is quarterly government consumption expenditures and gross investment, which is reported in the BEA's NIPA Table 3.9.5. We expect a certain degree of discrepancy between the series due to the volume and timing of purchases. Besides, the NIPA series has been revised periodically while the outlays series has not. Figure 4.1 illustrates the outlays series in monthly and quarterly frequencies and the quarterly NIPA series. Although the quarterly totals of the outlays appear to be more volatile than the conventional government spending variable, the overall patterns of the two series resemble each other closely. The key observation regarding the Great Recession is that the dramatic increase in fiscal expenditures during this period are reflected in both series. This reinforces the rationale that using the federal outlays should ideally capture the major variation in government spending during the Great Recession. Apparently, this is even more relevant when our focus is on the specific period of ZLB. In fact, the correlation between the outlays series and its NIPA counterpart is 0.95 in our sample. Thus, it is conceivable to employ the outlays variable as a monthly proxy for the quarterly measure of total government spending.[6]

Our baseline measures of real activity include real GDP. Since the GDP series is only available at a quarterly frequency, we follow Stock and Watson (2010) and interpolate real GDP and GDP deflator to a monthly frequency. This method employs a Kalman filter to distribute the quarterly GDP across months using a monthly dataset that includes measures of economic activity and prices. Another measure of real activity used in the baseline analysis is available at a monthly frequency and standard in the literature, i.e., the personal consumption expenditures (PCE). The government outlays, GDP and personal consumption expenditures variables are in logs and measured in real per-capita terms. To control for the role of systematic monetary policy, our baseline model also features a measure of price level and a measure of the monetary policy stance. In particular, we use the logarithms of the personal consumption expenditures (PCE) price index in the

[6]As a further check, we compute the impulse responses to a fiscal spending shock using a linear local projections and quarterly data for the baseline model specification. In particular, we compare the estimates obtained using the conventional government spending variable with those obtained using the aggregate quarterly total of federal outlays. The results (reported in Figure 4.11 in the Appendices) show a remarkably similar behaviour in the impulse responses, i.e., justify our choice of using the outlays series as a measure of fiscal spending in the empirical analysis.

analysis.[7] Regarding the monetary policy rate, we employ the shadow rate constructed by Wu and Xia (2016).[8] They compute a multi-factor shadow rate term structure model representing an implied illustration of the evolution of the U.S. term structure when the ZLB binds. They show that the effective rate might have been lower than the actual federal rate, i.e., it may be negative to reflect the policy accommodation during this period. The shadow rate is identical to the nominal rate otherwise. Thus, using the shadow rate as a proxy for the policy rate could reflect the actual behaviour of the effective rate under both normal conditions and unconventional monetary policy interventions.

An additional consideration is the role of uncertainty in governing the dynamics of economic activity and the effectiveness of fiscal policy. Indeed, the expansionary fiscal measures taken by the government during the ZLB are likely to cause an increase in expected future government debt. Consequently, this intensifies private agents' uncertainty about fiscal sustainability and pessimism about the prospects of economic recovery, which hinders the effectiveness of government policies. For this reason, we use a measure of economic policy uncertainty developed by Baker et al. (2016). This measure is constructed based on searches of newspaper articles in Access World News Newsbank that contain at least one term from each of the following three sets: "uncertainty" or "uncertain"; "economic" or "economy"; "legislation" or "deficit" or "regulation" or "congress" or "federal reserve" or "white house". We show that our results are robust to the employment of an alternative measure of financial uncertainty, VIX, which is a measure of implied stock market volatility proposed by Bloom (2009).

4.3 Results

In this section, we document the results obtained with the standard local projections and smooth local projections obtained with the model specifications described in the previous section. Specifically, we start with a linear setting and then extend the model to investigate the state-dependent impacts of government spending shocks.

[7] The PCE price index (PCECTPI) is available on the Federal Reserve Bank of St. Louis's website.

[8] The shadow rate is extracted from the Federal Reserve Bank of Atlanta.

4.3.1 Linear model

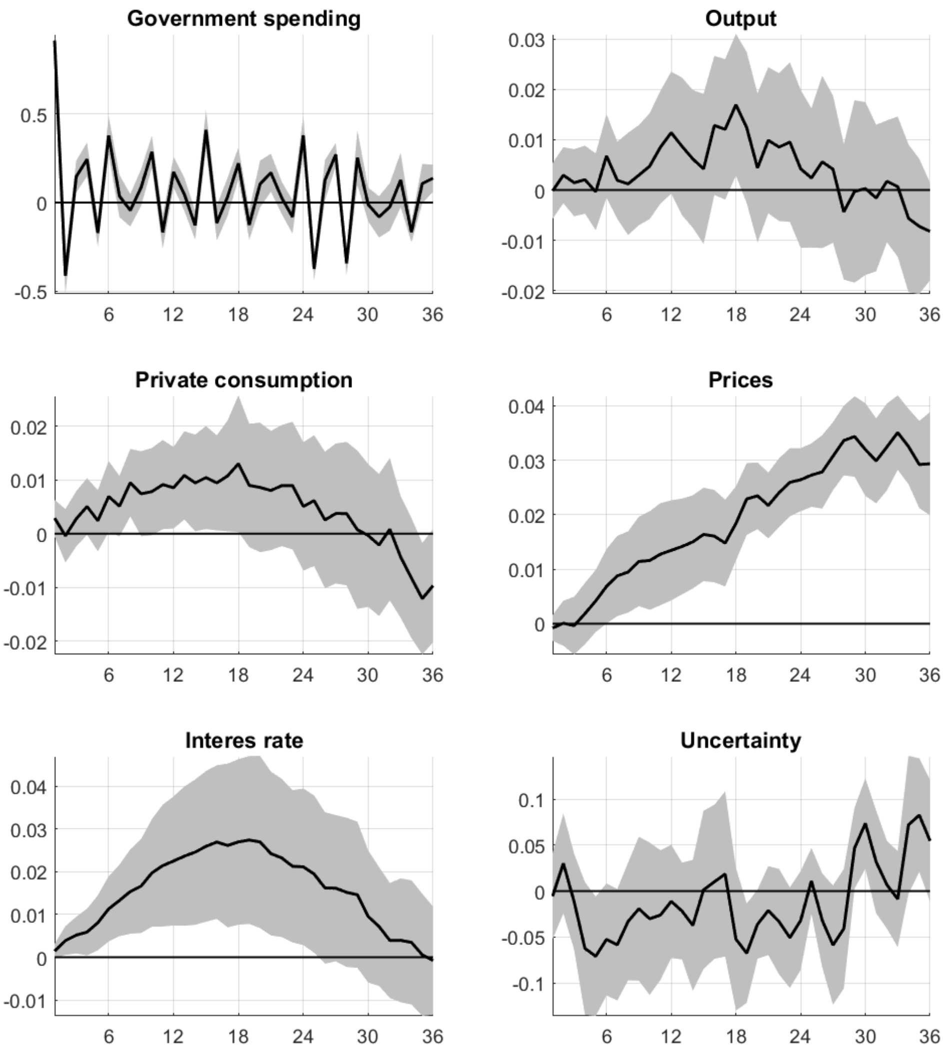


Figure 4.2: Impulse responses to a one percent unanticipated increase in government outlays. The solid black lines denote the estimated responses using the linear local projections method. The shaded area represents the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

As a starting point, we estimate the linear projections model in (4.1) using the baseline specification. Figure 4.2 shows the impulse responses to a shock in the government outlays series, where we show the impact of spending shock for up to thirty-six months or three years. As the top right panel illustrates, a surprise fiscal expansion induces an increase in output on impact and peaks at 0.02 percent around eighteen months or one and a half

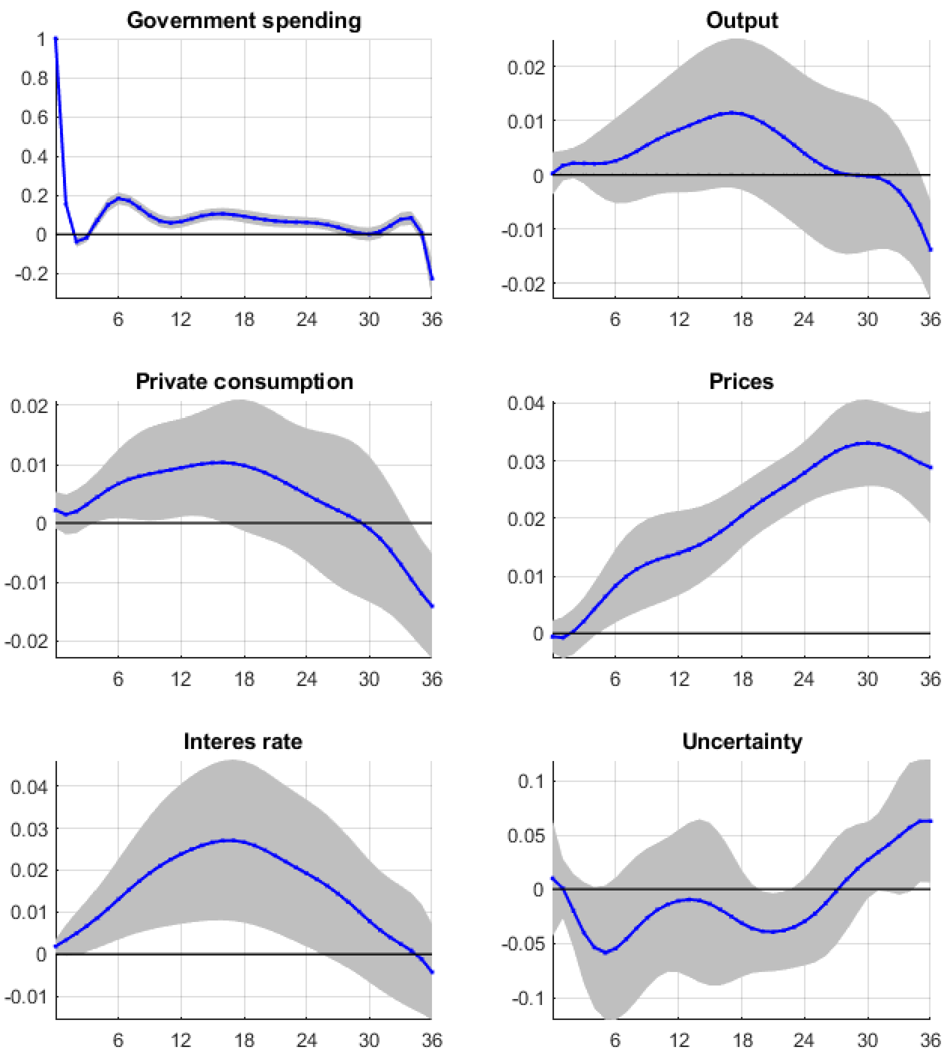


Figure 4.3: Impulse responses to a one percent unanticipated increase in government outlays. The blue lines with dots denote the estimated responses using the linear smooth local projections method. The shaded area represents the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

years later before reverting the trend. Private consumption follows the same pattern, but the responses turn negative at a faster rate around two years after the shock. However, the confidence bands are so large that the estimates are not significantly different from zero. Following a government spending shock, the uncertainty index drops after three months, and remains negative for about two and a half years followed by a quick recovery, although the response is not significant from a statistical viewpoint.

Interestingly, we find that the increase in prices are persistent and statistically significant following a fiscal expansion, thus showing contrasting evidence to the so-called "fiscal price puzzle" discussed in a large set of empirical works (Dupor and Li, 2015, Jørgensen and Ravn, 2022, D'Alessandro et al., 2019).[9] This finding aligns with the standard theoretical predictions that government spending shocks are inflationary and offers a potential solution to the existent price puzzle. It is important to notice that, in comparison with the exercise conducted using data at a quarterly frequency that we report in the middle column of Figure 4.11 in the Appendices, the response of prices is dramatically different. Note that we use the same definitions of the variables and empirical methodology, with the only difference being that the monthly data is aggregated to quarterly data.[10] The contrast between the impact of an expansionary government spending shock on price levels using the data at a monthly and quarterly is remarkable.[11] Hence, searching for the root of this "puzzle" would be fruitful and we leave this issue for future study.

Figure 4.3 illustrates the corresponding results obtained by the smoothing LP procedure. The erratic nature of LPs is no longer seen in the SLP counterparts under smoothing. Nevertheless, they indicate similar findings to our baseline results. An exogenous positive

[9]The price puzzle refers to the disinflationary (or deflationary) response of prices to government spending shocks.

[10]In Chapter 3, we provide further linear and nonlinear evidence on the puzzling effects of government spending shocks on price level, i.e., the price puzzle remains and this finding is robust to a variety of model specifications. The analysis in Chapter 3 employs quarterly data and conducted using an interacted-VAR (IVAR) model.

[11]We get the same switch in the sign of the impulse response of prices when estimating a simple VAR model with outlays as an instrument for government expenditure and prices using monthly and quarterly frequencies. Another check concerns the lag length of prices. While the baseline specification uses three lags as suggested by the information criteria, the conventional lag lengths for monthly and quarterly series are usually set at twelve and four, respectively. In the same vein, we use four lags in the exercise conducted using aggregated quarterly data. To examine the possibility that a lag length of three is not sufficient to remove the residual autocorrelation in prices, we re-compute the impulse responses by increasing the number of lags of prices in each regression to twelve. We observe very little qualitative difference in the response of prices. The results obtained from these checks suggest that time aggregation may be the underlying reason for the "price puzzle" in models that are estimated using quarterly data.

government spending shock raises output and private consumption on impact, although the estimates for output are not significant. Prices rises persistently up to thirty months before reverts to equilibrium and interest rates show a hump-shaped increase to a fiscal expansion. Uncertainty declines following a fiscal expansion at most horizons, although the response is insignificant.

4.3.2 Model dynamics at the ZLB

Over the last decade, considerable attention has been dedicated to investigating the effects of government spending shocks when the nominal interest rate is bound at the zero region. Two main strands of this literature have emerged. The first strand finds that the impacts of government spending shocks on the real activity do not vary significantly with the monetary policy stance (Mertens and Ravn, 2014a, Ramey and Zubairy, 2018). The second strand holds that the effect of government spending shocks differs when active monetary policy tools are constrained. Indeed, a large body of the literature shows that government fiscal multipliers are generally below one when the ZLB is not in place, yet rise substantially above one at the ZLB (Christiano et al., 2011, Miyamoto et al., 2018). Therefore, it is important to provide further empirical evidence on this issue.

For this purpose, we estimate a state-dependent local projections model in (4.2) using the same set of variables containing logs of real per capita government outlays, GDP, private consumption expenditure, log prices and uncertainty index. We define the ZLB to be 2009:1-2015:10 and show the impulse responses along with the 90 percent confidence bands in Figures 4.4 and 4.5 obtained with LP and SLP, respectively. The figures demonstrate that there is little difference between the standard and smoothed responses. In normal times, output and private consumption begin to rise and then peak at around eighteen months after a surprise fiscal expansion. Interest rate rises in the short-run (up to twenty months), whereas prices show an insignificant drop on impact followed by a quick recovery before returning to the pre-shock level. Uncertainty declines in response to a one percent increase in government outlays and remains negative for about thirty months, although the negative impact becomes insignificant after roughly twenty months when the shock kicks in.

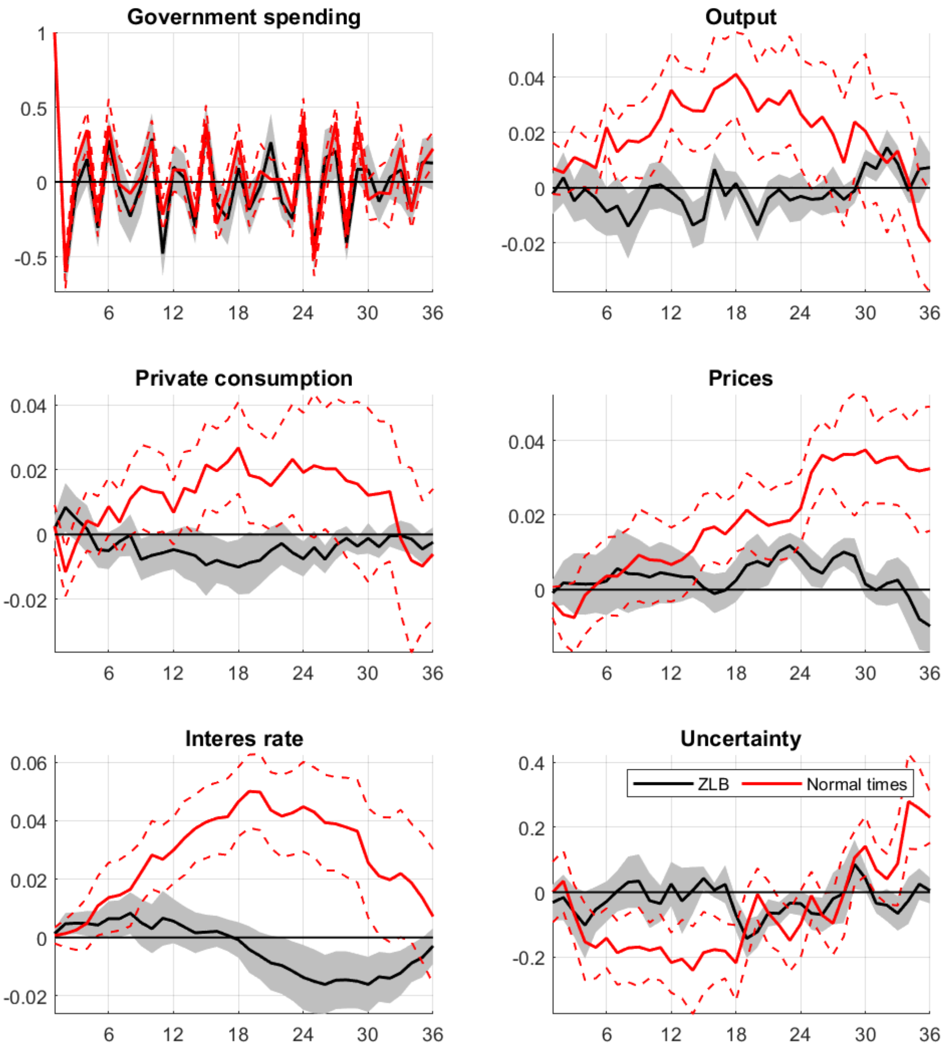


Figure 4.4: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB vs. Normal times using a local projections method. The solid black lines illustrate the impulse response at the ZLB and the solid red lines denote the impulse responses during normal times. The shaded area and the dashed lines are 90 percent confidence intervals. The estimation sample is 1980:10-2019:12.

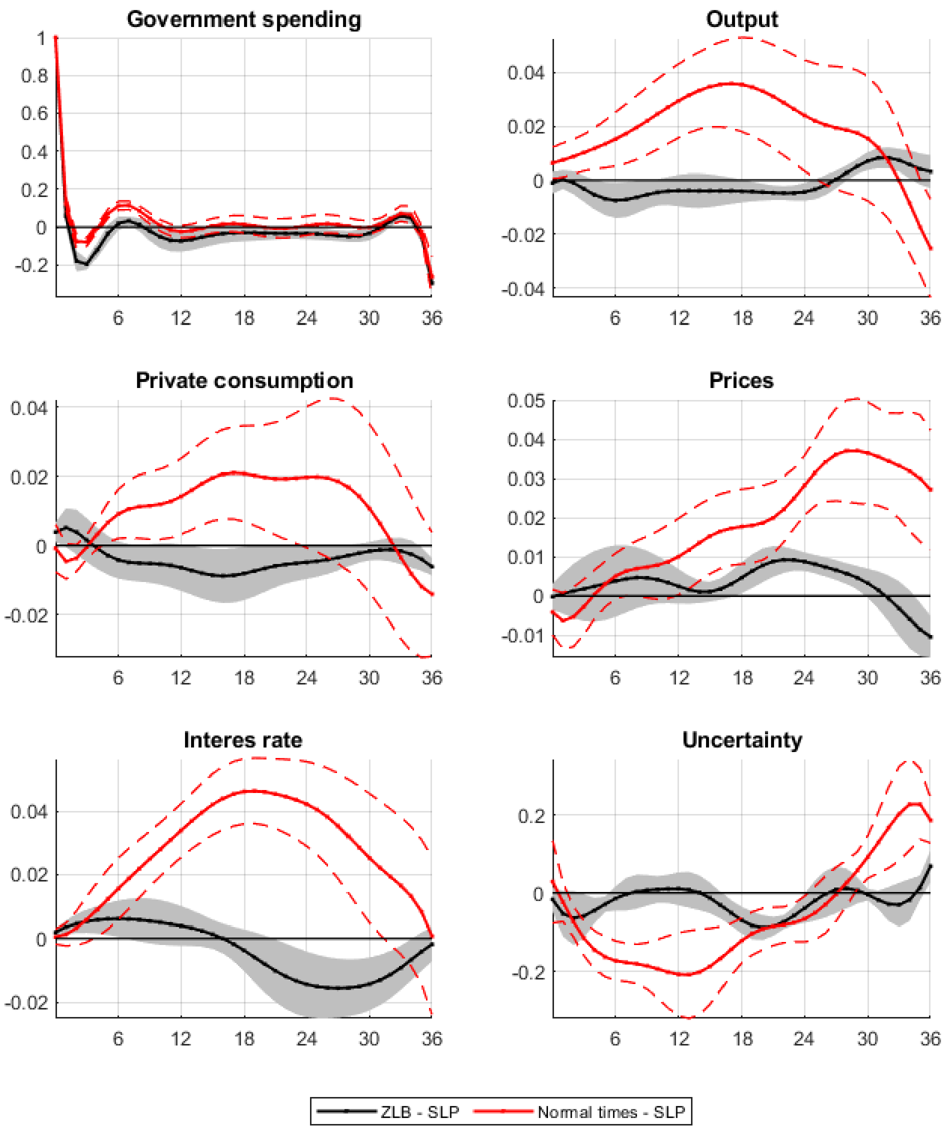


Figure 4.5: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB vs. Normal times using the smooth local projections method. The black lines with dots illustrate the impulse response at the ZLB and the red lines with dots denote the impulse responses during normal times. The shaded area and the dashed lines are 90 percent confidence intervals. The estimation sample is 1980:10-2019:12.

On the other hand, we find very different responses at the ZLB regime using the state-dependent local projections model, in which a surprise increase in government spending fails to produce any boost in confidence and any inflationary effects. In particular, real GDP shows a minimal response to an innovation to fiscal outlays. Personal consumption slightly increases on impact and quickly drops after around four months. The response of prices follows the same dynamic pattern. Specifically, prices show little reaction to a surprise fiscal expansion for about eighteen months, reaching their peak around two years and returning to the equilibrium level. In addition, we observe a temporary dip in the response of uncertainty index following a fiscal spending shock around one year in normal times. It is worth noting that the patterns of real activity and uncertainty appear to inversely resemble each other, where the response of uncertainty precedes that of real activity by a few months. Therefore, it is plausible to suspect a causation from uncertainty to real activity, though additional evidence is needed.

4.3.3 Model dynamics at the ZLB by QE programs

We now explore whether the effect of fiscal spending differs when QE policy is in effect during the ZLB period. In addition to the ZLB, the QE policy was a distinctive feature of the Great Recession. Gambacorta et al. (2014) provide evidence that the behaviors of output and consumer prices can be different under the influence of the QE measures. For this reason, we study the impact of QE on the responses of macroeconomic variables to a fiscal spending shock in the ZLB state.

We estimate the three-regime LP model in (4.3) for the periods split into ZLB with QE, ZLB without QE and normal times since QE is only activated during the ZLB periods. Figure 4.6 shows the impulse responses. The results suggest that for all variables considered in the baseline specification, the responses during the ZLB when QE is in effect are less volatile than when it is not, although the differences across the two states are not significant. We find that when controlling for the effect of QE, a surprise fiscal expansion leads to a temporary rise in private consumption and the estimates are statistically significant. The muted response of output to a surprise increase in government spending in both states is consistent with the behaviour observed in the model with two regimes. We find little evidence of an increase in prices whether QE is present up to eighteen months at the binding ZLB with QE and at all horizons in the case of ZLB without QE. Similar to the estimates obtained at the ZLB in the two-regime case, the lack of inflationary responses regardless of controlling for QE suggests that fiscal spending shocks might not be expansionary at the ZLB periods. This finding, even though inconsistent with the theoretical prediction that government spending shocks are more expansionary when interest rates stay in the zero region than during normal times, is aligned with a large number of empirical works in the literature using quarterly data (Jørgensen and Ravn, 2022, D'Alessandro et al., 2019).

Turning to the responses of uncertainty, we find that during the ZLB periods irrespective of the presence of QE, uncertainty falls on impact after a surprise increase in fiscal spending. However, in contrast to the drop in the first six months when QE takes effect, a similar expansionary spending shock raises the level of uncertainty by the same magnitude in absence of QE. These short-run effects are significant at 90 percent confidence level. While uncertainty exhibits asymmetric response across different QE states following a surprise increase in government spending, further investigation should be conducted prior to assuming a causal relationship from the uncertainty channel to economic activity when QE policy takes place.

Figure 4.7 presents the results produced by the SLP approach for the three-regime model specification. As expected, the observations obtained with the LP method are preserved. When dissecting further to investigate the impact of quantitative easing on the transmission of fiscal spending shock, we find little evidence of significant changes in real activity and price levels in response to a positive fiscal spending shock under the ZLB regardless of the implementation of quantitative measures.

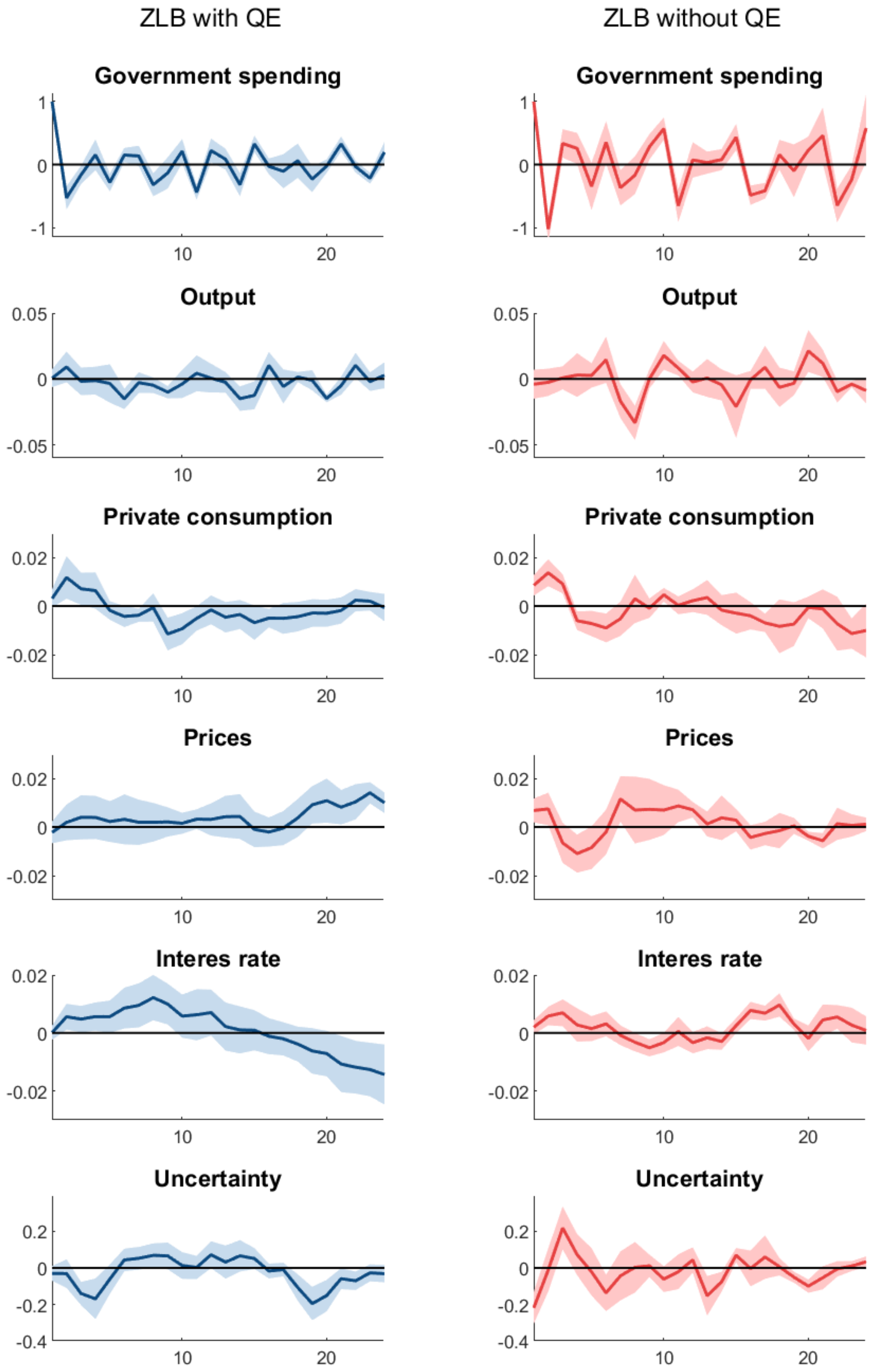


Figure 4.6: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB with QE vs. ZLB without QE using the local projections method. The solid blue lines denote the estimated responses at the ZLB with active QE. The solid red lines denote the estimated responses at the ZLB when QE is not in place. The shaded area and dashed lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

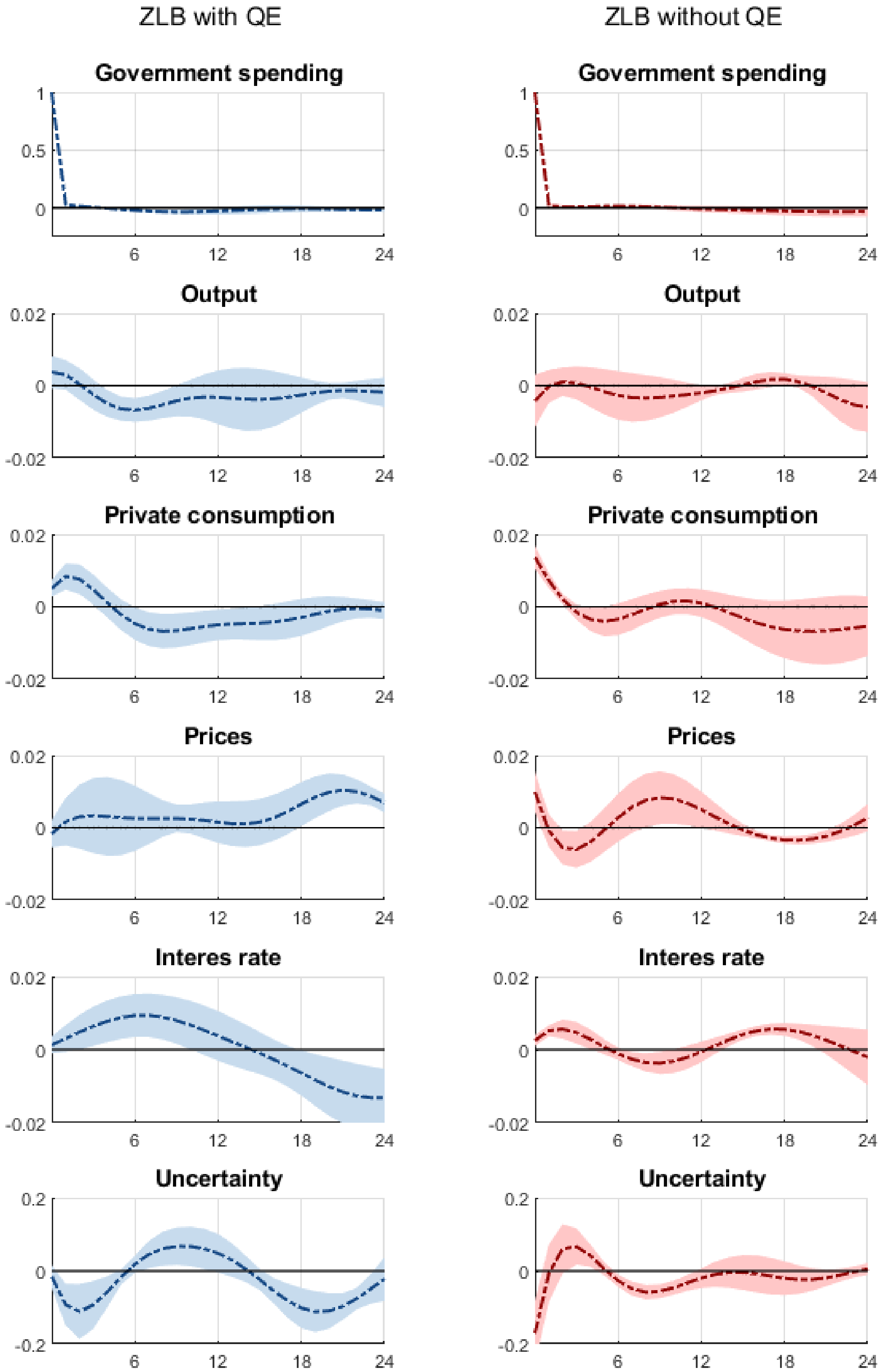


Figure 4.7: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB with QE vs. ZLB without QE using the smooth local projections method. The dashed blue lines denote the estimated responses at the ZLB with active QE. The dashed red lines denote the estimated responses at the ZLB when QE is not in place. The shaded area and dashed lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

4.4 Robustness checks

4.4.1 Moving average analysis for government spending

Our baseline model employs the government outlays series as a proxy for total government expenditure at a monthly frequency. However, despite the high correlation between the quarterly aggregated government outlays and the quarterly government spending, there might be variations in their monthly patterns. To address this issue, we consider the three-month moving average of government outlays to validate the results obtained from the original series and perform the exercises for all three model specifications outlined in (4.1), (4.2) and (4.3). Figure 4.12, 4.13 and 4.14 illustrate the results. While the responses of government outlays to their own shocks appear less erratic, the responses of other variables are robust to using the smoothed variable. This supports the use of the original series and the main findings of our study.

4.4.2 Alternative measure of output

The baseline analysis adopt an interpolated GDP series to capture real activity at a monthly frequency. It is well-known that interpolated series are estimated based on certain assumptions and techniques, thus might be susceptible to measurement errors. We are aware that a number of studies have utilized the industrial production index as a proxy of output at a monthly frequency. However, we opt to use the interpolated GDP series as an alternative measure of output due to the fact that it provides a more accurate representation of overall economic growth, whereas industrial production only accounts for physical output of goods in the industrial sectors.

Nevertheless, we check the solidity of our results to using the interpolated GDP series to capture real activity at a monthly frequency. Figure 4.15, 4.16 and 4.17 present our checks using this variable. The results remain qualitatively similar. This is expected since the correlation between the interpolated GDP series that we use in this exercise and the industrial production is as large as 0.95.

4.4.3 Omitted variable bias

The exercises performed so far concern the effect of fiscal spending shocks when we control for the short-term interest rates. While our analysis is conducted using the shadow rate in lieu of the conventional federal funds rate to take into account the unconventional monetary policy interventions during the ZLB episodes, it does not feature any factor that might have been affected directly by these unconventional measures. To alleviate concern regarding omitted variable bias, we estimate the following models by modifying the baseline specification.

The first check looks at the relationship between government spending and long-term interest rates. Economic theories suggest that an increase in government spending tightens credit markets. Alternatively, a growing body of empirical evidence shows that government spending can cause a decline in long-term interest rates (Murphy and Walsh, 2022, Miranda-Pinto et al., 2023). These studies point to a gap in our understanding of the long-term interest response to fiscal stimulus. Weale and Wieladek (2016) provide evidence that the long-term bond yields in the U.S. respond to announcements of unconventional monetary policy when the policy rate was stuck at its lower bound. Swanson and Williams (2014) scrutinize the response of interest rates at various maturities to macroeconomic shocks. They find that in the presence of the ZLB, the medium to long-term interest rates were responsive to surprises in macroeconomic variables. Given our focus on the ZLB period, it is particularly important to control for the unconventional monetary policy via the effects of long-term rates. For this purpose, we augment the baseline specification with the 10-year government bond yield and re-estimate the responses.

The second experiment explicitly examines the QE policy, also referred to as the balance sheet policy. Following Gambacorta et al. (2014), we use the central bank assets as a proxy of the quantitative policy instrument and re-estimate the model by adding this variable into the baseline specification. Given that the Federal Reserve did not explicitly implement the purchases of assets before the GFC, changes in the size of the central bank's balance sheet are arguably sufficient to capture the Fed's exposure to such policy.[12]

[12]We thank Eric Leeper for providing the data on the Federal Reserve's balance sheet for the period of 1915-2011. The extended series (2011 onwards) is available on the Federal Reserve's website. As an alternative for total assets, one could use liabilities-based measures such as the monetary base. However, Gambacorta et al. (2014) provide a rationale behind the choice of central bank assets as a more accurate

The results are reported in Figures 4.8-4.10. The second column in Figure 4.8 plots the state-dependent impulse responses to a positive government spending shock produced with the model augmented with the 10-year government bond yield and the corresponding 90 percent confidence bands. We observe no sizable distinction with respect to the baseline results irrespective of the monetary policy stance. In addition, we find that the long-term rates fall on impact in response to an expansionary government spending innovation in both states and no evidence of an increase in the long rates detected at all horizons, particularly at the ZLB. This finding, thus, is consistent with the large body of the existent empirical literature. Miranda-Pinto et al. (2023) associate this finding with the level of consumer debts and argue that private agents tend to allocate their additional income generated by fiscal stimulus to debt obligations, especially during economic slacks and high uncertainty periods. Consequently, this causes downward pressures on the long-term rates.

The third column in Figure 4.8 reports the responses from the model with the central bank's total assets. Similar to the previous case, we find no evidence of a substantial difference between the baseline framework and its extended counterpart. The response of total assets to the government spending shock is minimal and statistically insignificant under both regimes. Overall, two noticeable results emerge from these exercises. First, the baseline finding that the reactions of economic activity are less pronounced when the ZLB binds is supported. Second, there is no evidence of an increase in the long-term interest rates in response to a government spending shock.

gauge of unconventional monetary policy in the presence of the ZLB. Notice, however, the results are robust to using the monetary base as the proxy for the QE program. In fact, total assets and the monetary base track each other closely during our sample period, including the ZLB periods, with a correlation coefficient of 0.98.

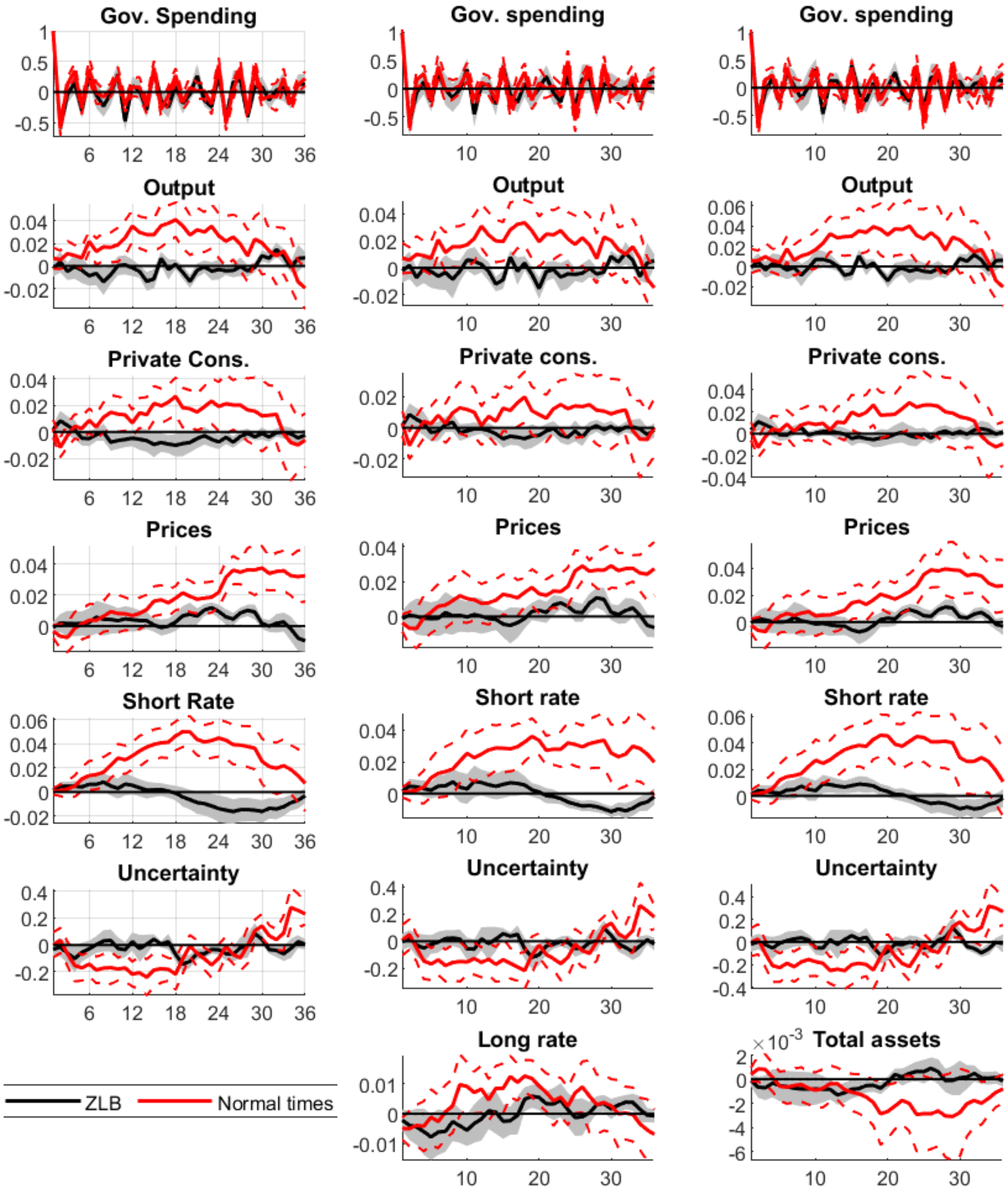


Figure 4.8: State-dependent impulse responses to a one percent increase in government outlays shock using local projections. The solid-black lines illustrate the impulse response at the ZLB and the red lines with circles denote the impulse responses during normal times. The shaded area and the dashed lines are 90 percent confidence intervals. The first column (a) shows the responses to a one percent increase in government spending using the baseline model. The second column (b) shows the responses to a one percent change in government spending when the 10-year Treasury yield is added to the model. The third column (c) shows the responses to a one percent increase in government spending when the central bank's total assets is added to the model. Sample: 1980:Q1-2019:Q4.

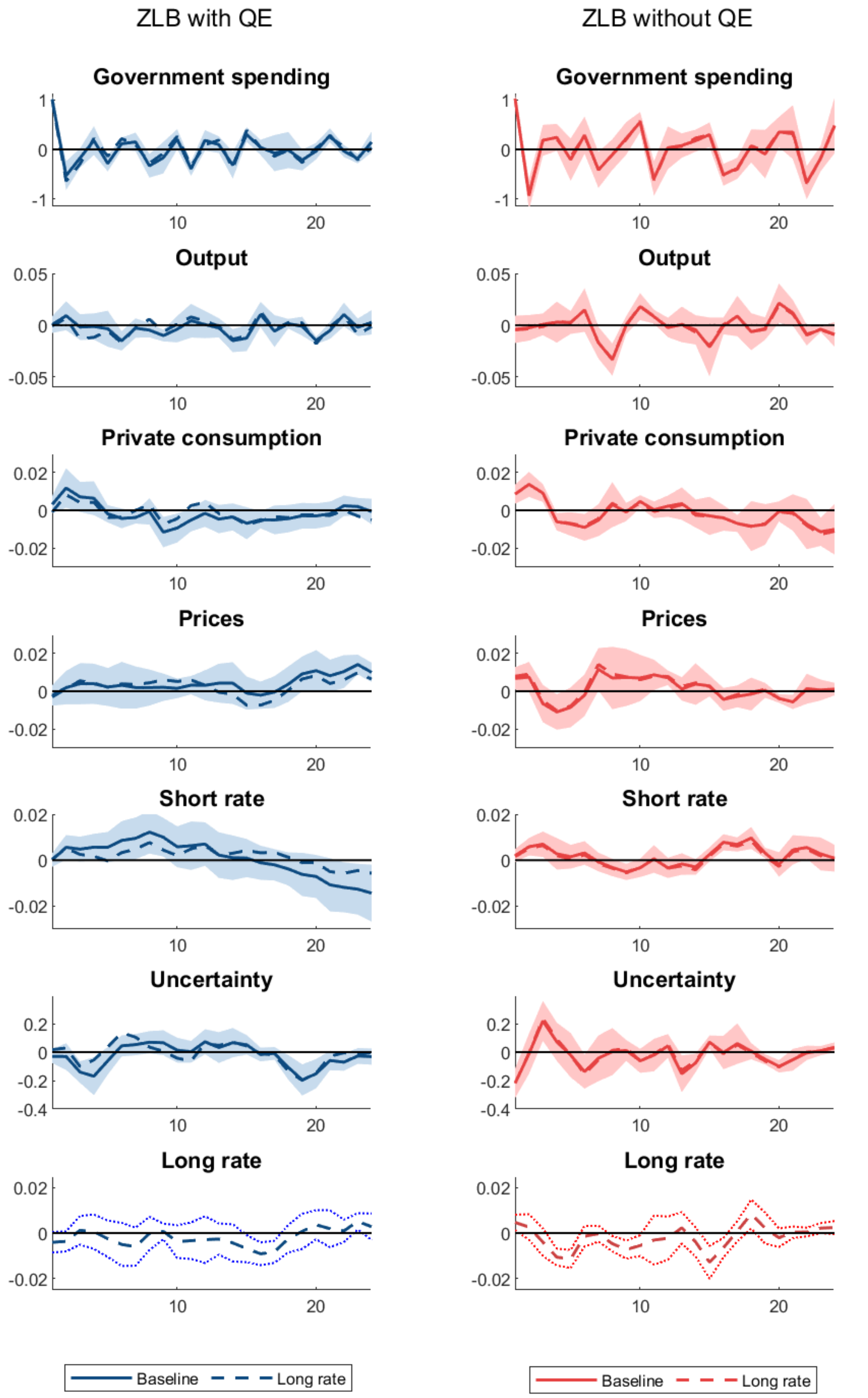


Figure 4.9: State-dependent impulse responses to a one percent increase in government outlays shock using local projections: ZLB with QE vs. ZLB without QE. The solid blue lines denote the estimated responses at the ZLB with active QE. The solid red lines denote the estimated responses at the ZLB when QE is not in place. The dashed lines are responses when the 10-year government bond yield is added to the baseline model. The shaded area and dotted lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

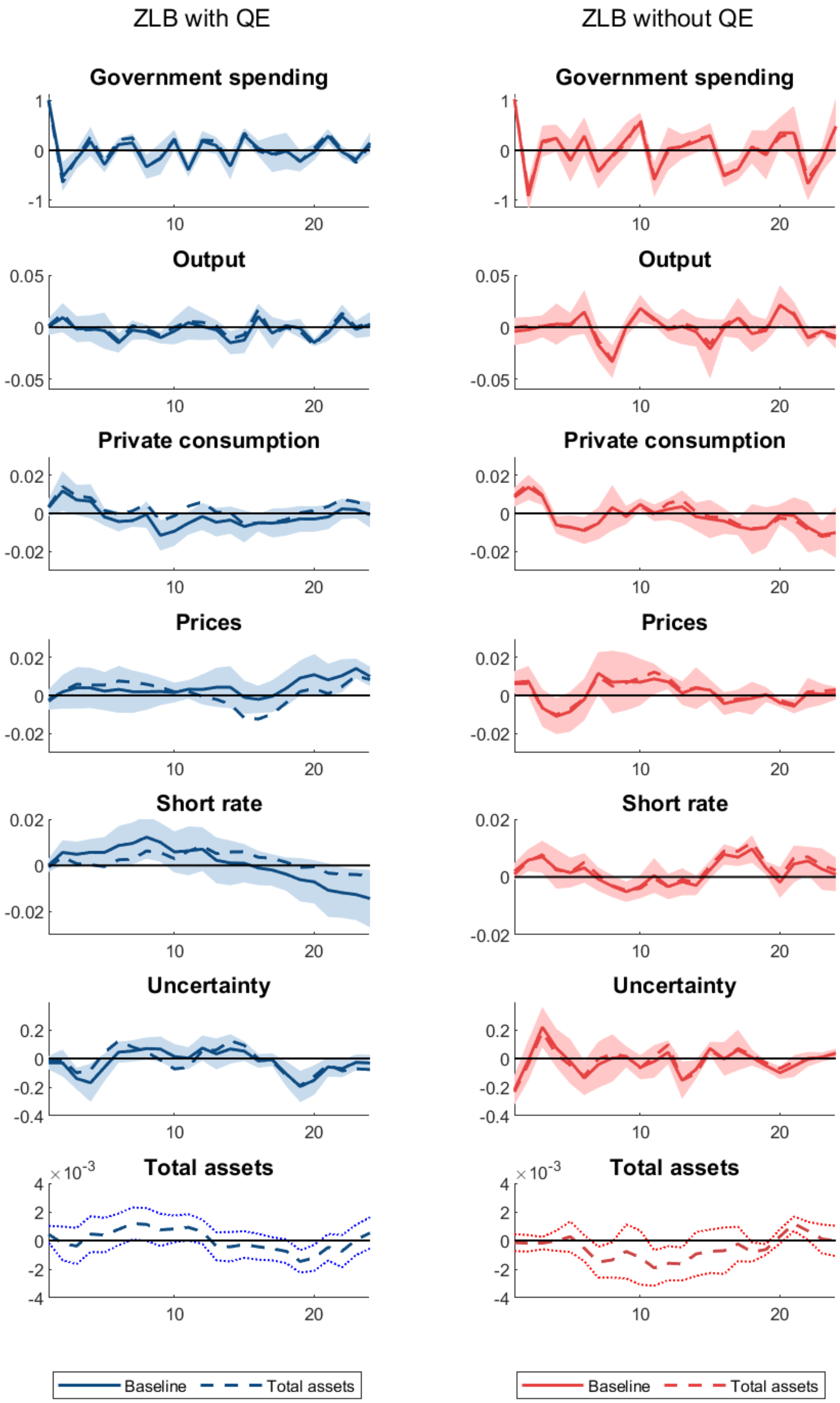


Figure 4.10: State-dependent impulse responses to a one percent increase in government outlays shock using local projections: ZLB with QE vs. ZLB without QE. The solid blue lines denote the estimated responses at the ZLB with active QE. The solid red lines denote the estimated responses at the ZLB when QE is not in place. The dashed lines are responses when the central bank's total assets is added to the baseline model. The shaded area and dotted lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

Figures 4.9-4.10 present the additional checks when we specifically look at the impact of quantitative aggregate on the dynamic effects of a government spending shock to the economy. As in the previous scenario with two regimes, the baseline observations reported above are not altered qualitatively by these modifications. These figures provide support for the finding of an insignificant response of real activity to a positive spending shock and that the impact of government spending shocks under the ZLB episodes is not distinctive, regardless of the presence of QE policy.

4.5 Conclusion

This chapter contributes to the empirical literature focused on examining the varied effects of government spending on macroeconomic activity according to the state of the economy when the unconventional monetary policy is in place. We use a measure of government outlays extracted from the monthly treasury statements to identify the fiscal spending shock. This variable is available at a monthly frequency and closely tracks the conventional measure of total government spending. The higher frequency greatly enhances our ability to quantify the response of macro aggregates. Using state-dependent local projections frameworks, we find that government spending shocks exhibit asymmetric effects on the economy across different monetary policy stances. In the absence of the ZLB effects, government spending shocks induce a persistent and significant increase in real activity for up to two years. On the contrary, macroeconomic variables display minimal reactions in response to a positive government spending innovation when the ZLB is binding.

We extend the non-linear local projections to accommodate more regimes. In particular, we investigate the impact of quantitative easing programs on the transmission of government spending shock on real activity by looking separately at the ZLB episodes when quantitative easing is in place versus when it is not. Our results suggest that during the periods in which quantitative easing policy is not present, the response of economic activity turns out to be bumpier than the one obtained when the policy actually takes effect. The difference between the two states is not statistically significant at most horizons, though, except for the uncertainty level in the short run. This provides mild support for the potential role played by the uncertainty index in fiscal policy-related analysis.

To the extent that monthly-frequency data used in our analysis offers enriching information about the government expenditures, and potentially a more reliable identification of fiscal spending shock, our finding contributes to resolving the fiscal price puzzle discussed by a growing body in the empirical literature (Jørgensen and Ravn, 2022, D'Alessandro et al., 2019) in normal times. However, in the presence of active ZLB, there is no evidence of an inflationary effect of government spending shocks in the short-to-medium term. The stark difference observed in the response of prices when using data at a monthly versus quarterly frequency deserves further attention and opens up a promising avenue for future research. In any case, this chapter provides evidence that one should not expect fiscal policy to be effective during the periods when interest rates are at the ZLB, regardless of the stance of non-conventional monetary policy.

4.6 Appendices

Figures/

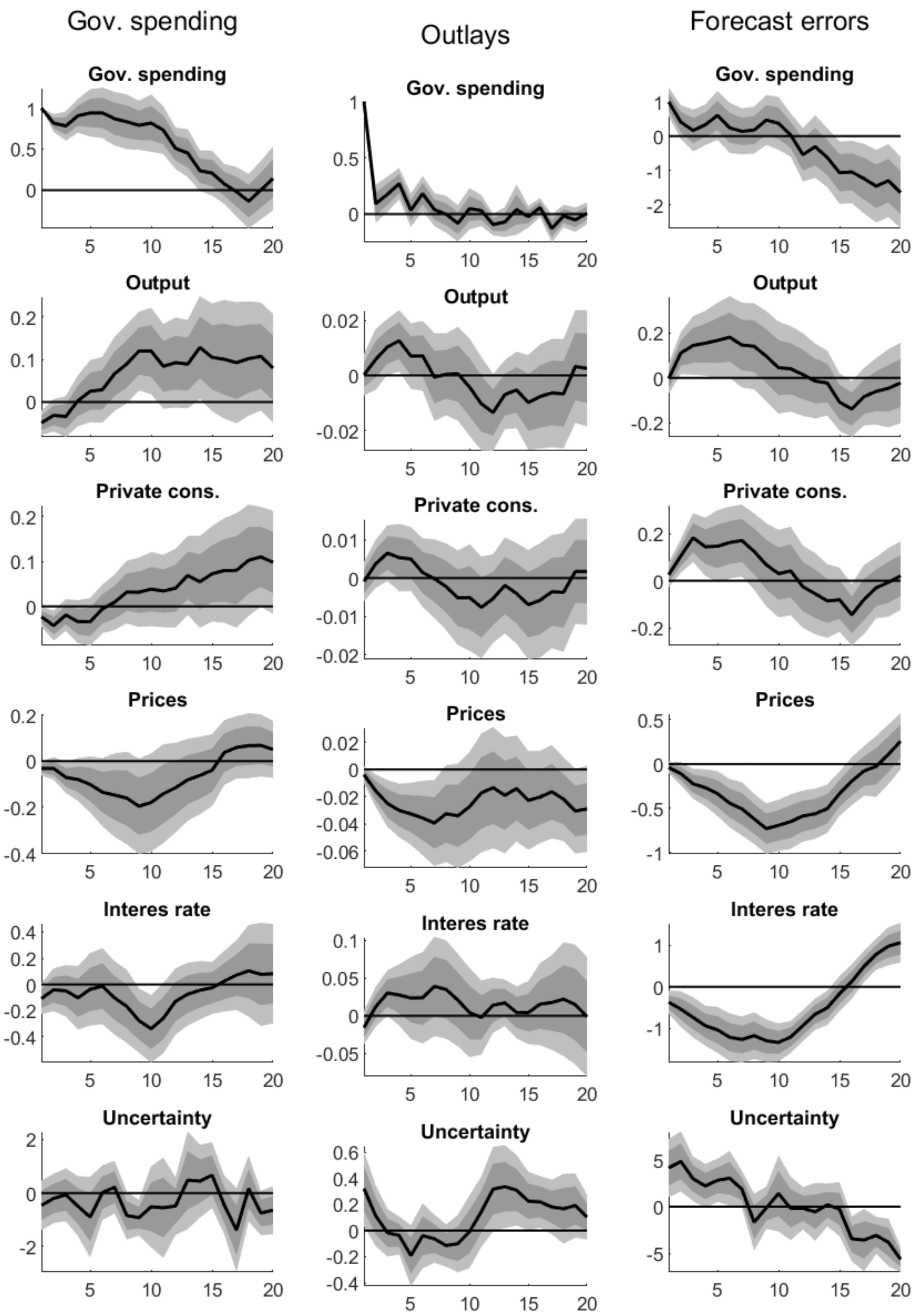


Figure 4.11: Impulse responses to a one percent unanticipated increase in government spending using linear local projections at the quarterly frequency. The first column shows the responses to a shock in total government spending using Cholesky decomposition. The second column shows the responses to a one percent unanticipated increase in government outlays extracted from the MTS. The third column reports the responses to a one percent increase in government spending identified using the Auerbach and Gorodnichenko (2012)'s forecast errors. We normalize the shock size to one in each case for comparability. Sample: 1980:Q4-2019:Q4. Grey areas: 68 percent and 90 percent confidence bands.

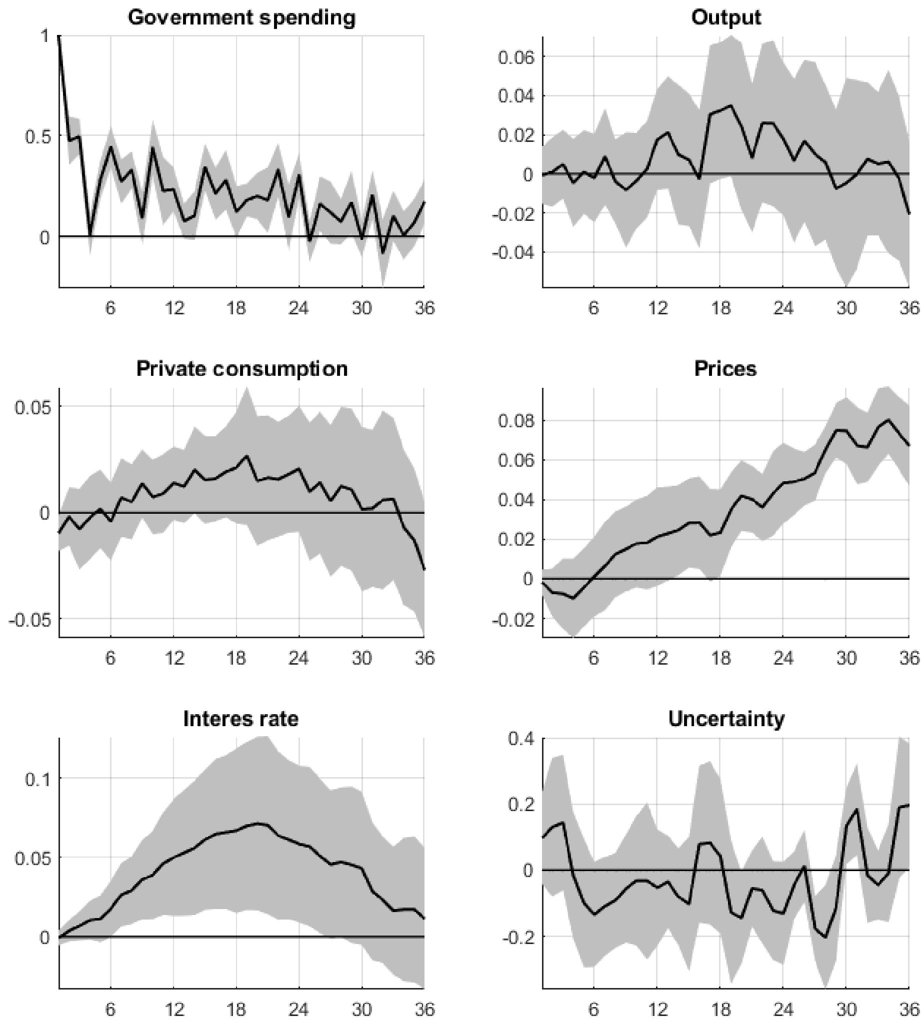


Figure 4.12: Impulse responses to a one percent unanticipated increase in the 3-month moving average of government outlays. The solid black lines denote the estimated responses using the linear local projections method. The shaded area represents the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

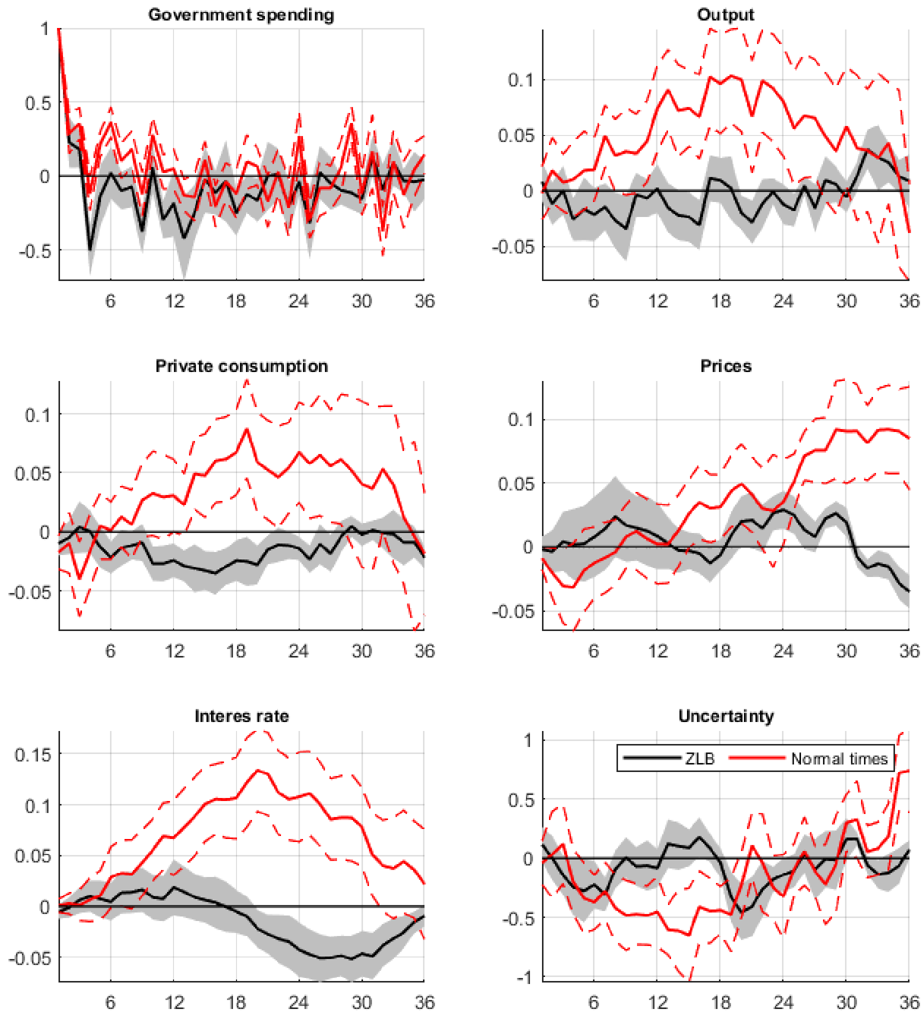


Figure 4.13: State-dependent impulse responses to a one percent unanticipated increase in the 3-month moving average of government outlays: ZLB vs. Normal times using a local projections method. The solid black lines illustrate the impulse response at the ZLB and the solid red lines denote the impulse responses during normal times. The shaded area and the dashed lines are 90 percent confidence intervals. The estimation sample is 1980:10-2019:12.

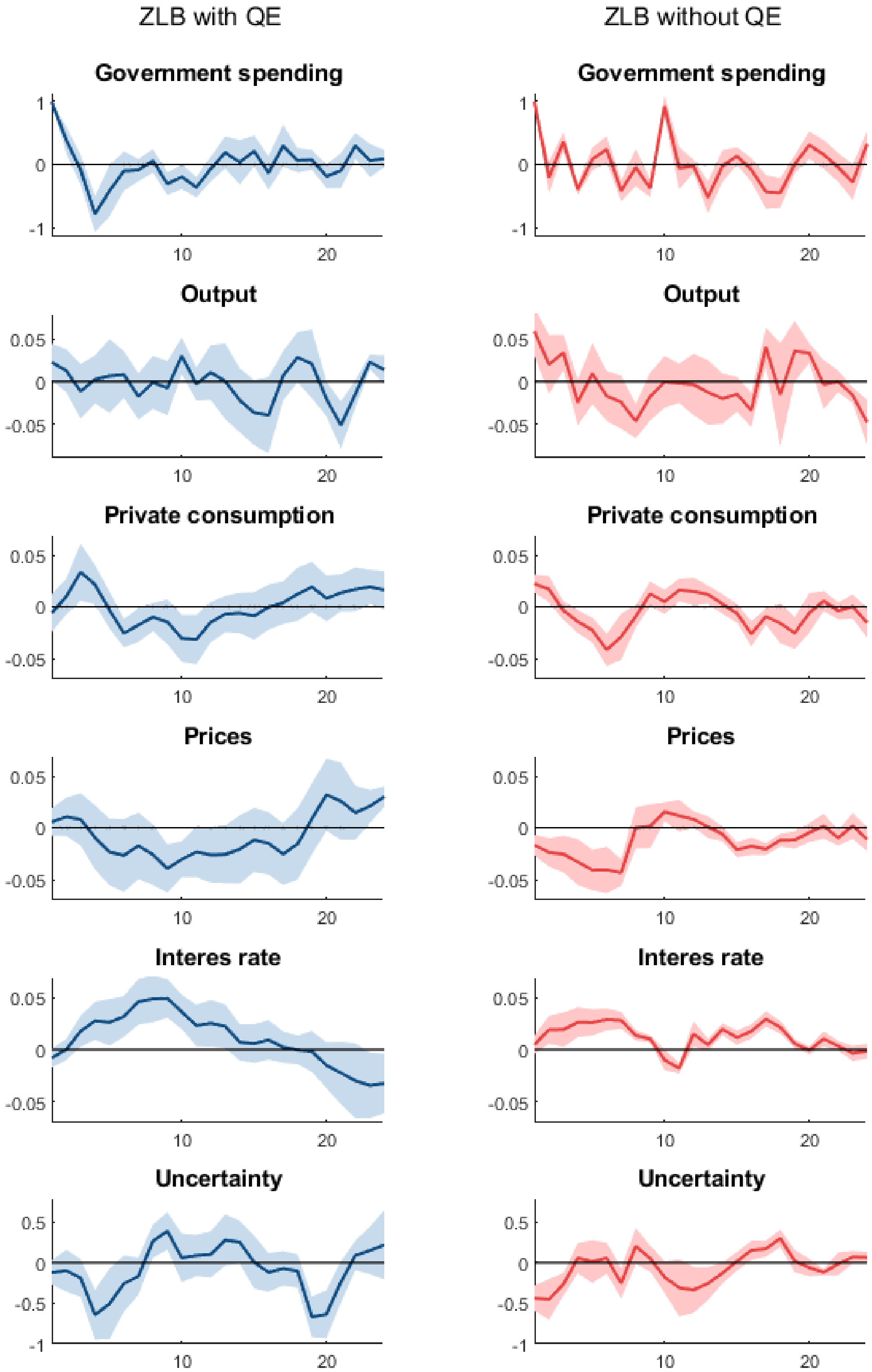


Figure 4.14: State-dependent impulse responses to a one percent unanticipated increase in the 3-month moving average of government outlays: ZLB with QE vs. ZLB without QE using the local projections method. The solid blue lines denote the estimated responses at the ZLB with active QE. The solid red lines denote the estimated responses at the ZLB when QE is not in place. The shaded area and dashed lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

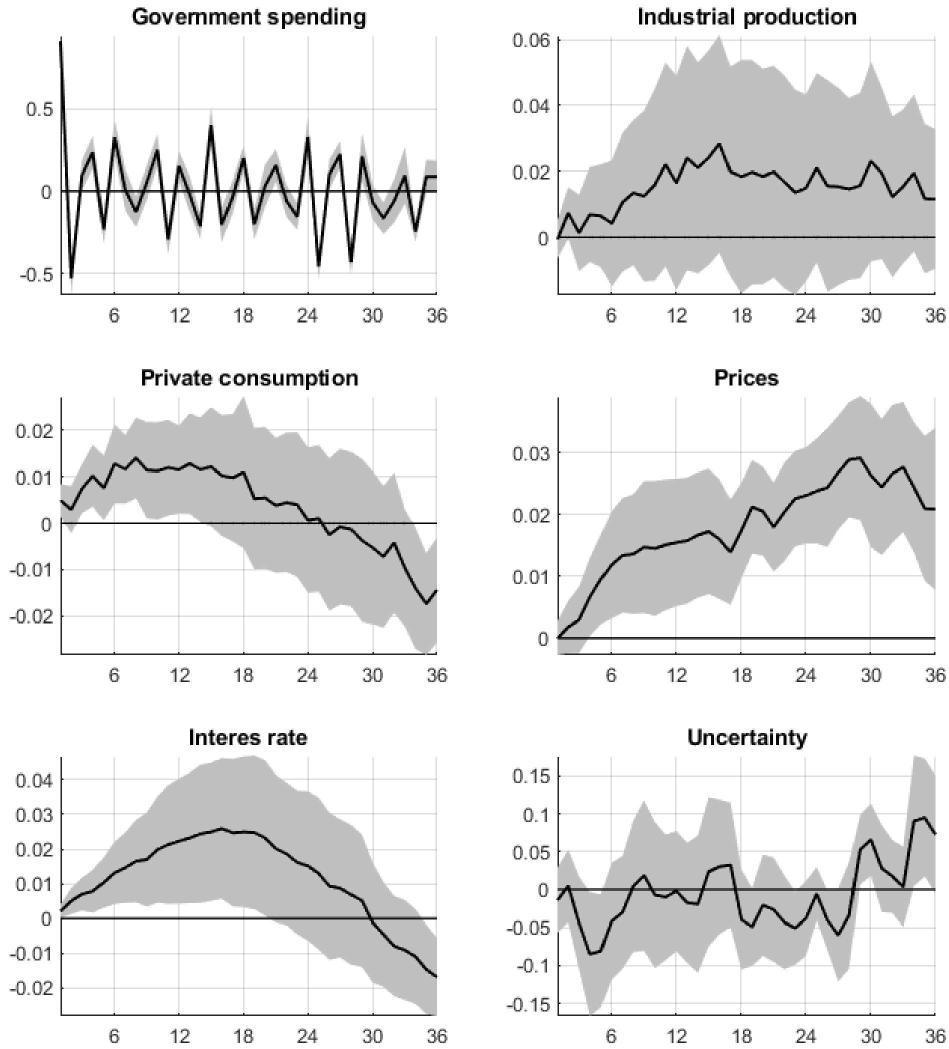


Figure 4.15: Impulse responses to a one percent unanticipated increase in government outlays with industrial production. The solid black lines denote the estimated responses using the linear local projections method. The shaded area represents the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

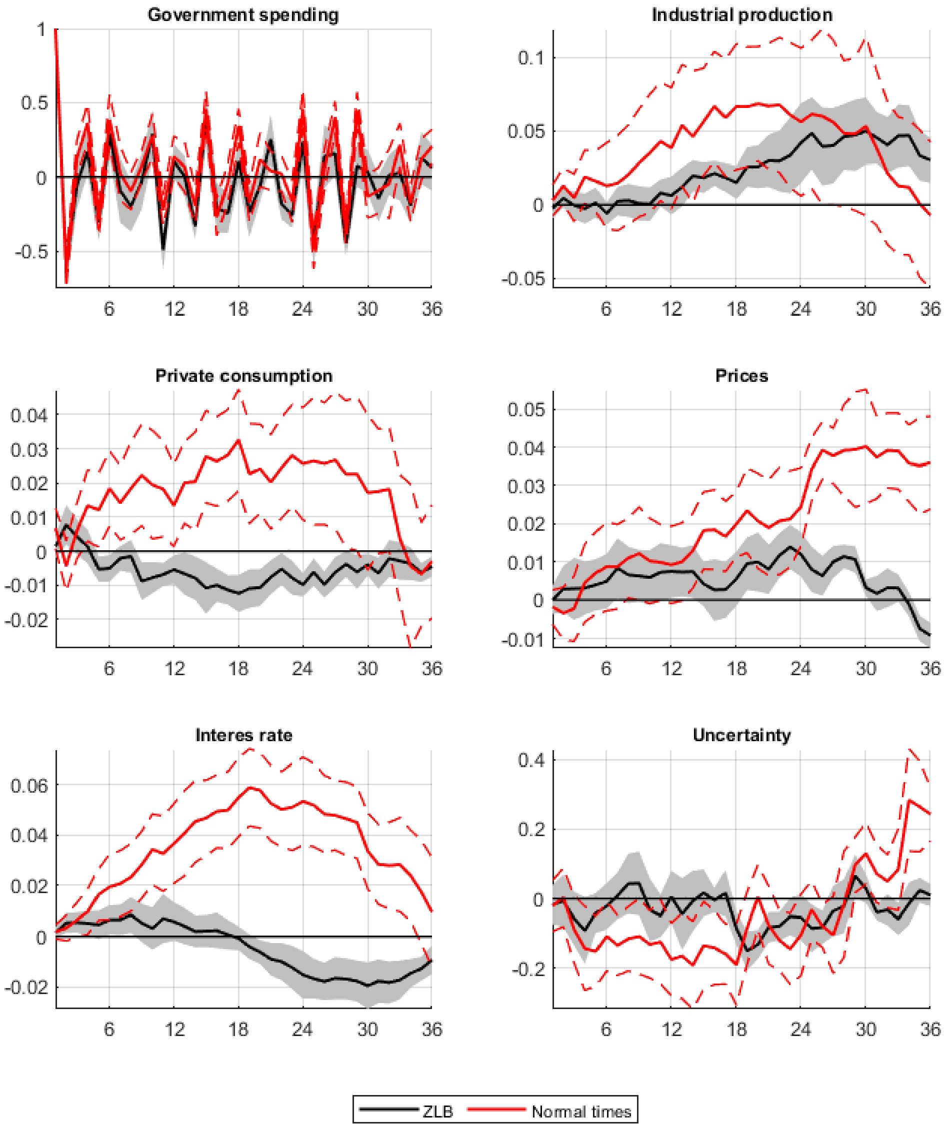


Figure 4.16: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB vs. Normal times using a local projections method with industrial production. The solid black lines illustrate the impulse response at the ZLB and the solid red lines denote the impulse responses during normal times. The shaded area and the dashed lines are 90 percent confidence intervals. The estimation sample is 1980:10-2019:12.

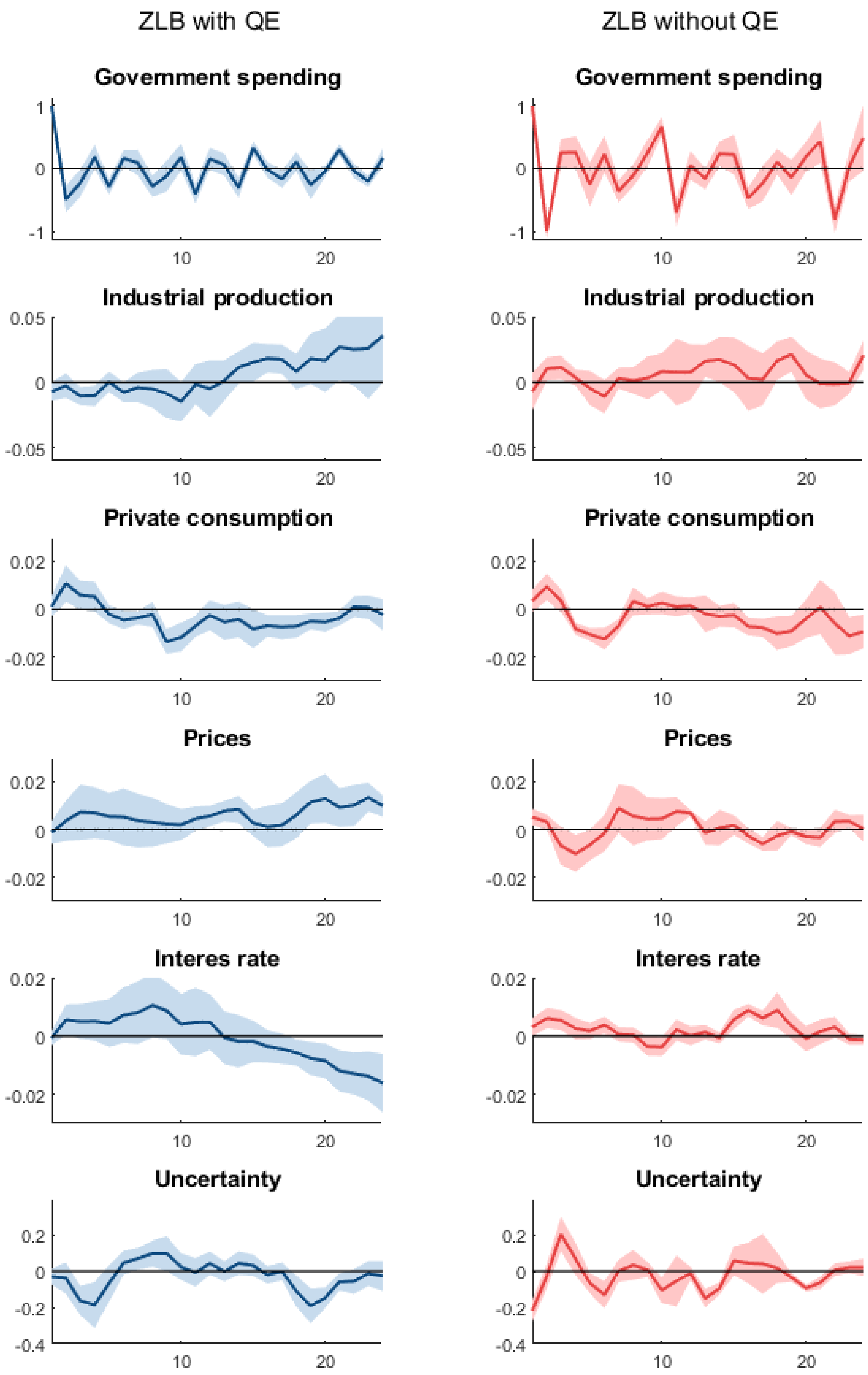


Figure 4.17: State-dependent impulse responses to a one percent unanticipated increase in government outlays: ZLB with QE vs. ZLB without QE using the local projections method with industrial production. The solid blue lines denote the estimated responses at the ZLB with active QE. The solid red lines denote the estimated responses at the ZLB when QE is not in place. The shaded area and dashed lines represent the 90 percent confidence bands. The estimation sample is 1980:10-2019:12.

Chapter 5

Conclusion and future directions

Following the recent global financial crisis in 2007-2008 and the reduction of interest rates to near zero region, the effectiveness of monetary policy in boosting economic activity becomes more limited, thus interest grew anew towards studying the impacts of fiscal policy. In addition to the introduction of unconventional monetary policy tools when the short-term interest rate was trapped near the effective lower bound, a wide spectrum of fiscal packages was implemented to stabilize the economy. While there is a rich literature on this topic, a consensus regarding the sign and qualitative magnitude of the effects of fiscal shocks on macroeconomic aggregates is still far-fetched.

In light of this discrepancy, this thesis presents essays with a common theme of fiscal policy shocks identification, and explores to what extent the fiscal policy impacts depend on the mechanism of monetary-fiscal policy interactions in normal times and at the binding zero lower bound. This empirical research is conducted in the context of the U.S. macroeconomy.

In Chapter 2, we conduct an examination of the identification of fiscal shocks using a proxy SVAR framework developed by Mertens and Ravn (2013) and Stock and Watson (2012). We present a comparative analysis of three identification schemes including (1) identification with one instrument and one fiscal shock; (2) identification with two instruments and two fiscal shocks and (3) identification with multiple instruments and one fiscal shock. We detect little difference between the responses obtained using a single instrument and those computed via joint use of instruments to identify government spending shock. The estimated impact spending multipliers range from 1.2 to 1.8 across different identification strategies. This finding is in line with Caldara and Kamps (2017). On the contrary, the tax multiplier estimates are sensitive to our choice of model specification and identification approach.

This chapter has reviewed a variety of potential instruments for fiscal policy shocks in light of the U.S. economy. We see several different but related lessons for future work. As Owyang et al. (2013) show, conclusions concerning the size of the fiscal multiplier for one country (e.g., the U.S.) are not necessarily applicable and representative to other countries. Likewise, the implications for fiscal policy shocks differ between national and local levels. While state and local-level data on the instruments and macroeconomic variables relevant to this kind of analysis in the U.S. is haphazard and not always readily available, studies of the euro area or OECD countries might be worth pursuing.

In Chapter 3, we analyze the propagation of fiscal spending policy at the ZLB and in normal times using an interacted-VAR model. The main focus of this study is to examine the effects of government spending shocks on prices and we provide empirical evidence in support of a negative price response, the so-called "fiscal price puzzle" discussed intensively in the empirical literature (Jørgensen and Ravn, 2022, D'Alessandro et al., 2019). We show that when moving to a nonlinear setting, the price puzzle remains irrespective of the presence of the ZLB. This finding indicates that the impact of fiscal expansion might not induce the typical inflation effects. Instead, an increase in fiscal spending shock boosts productivity, leading to a negative impact on inflation.

Additionally, we find that a surprise government spending shock produces contractionary impacts on output and private consumption when the interest rate is kept near zero. We associate this finding with the heightened uncertainty index and highlight the role of uncertainty channel when studying the transmission of fiscal shocks at the ZLB. However, the difference is not always statistically significant. We show that the estimated fiscal spending multipliers are not larger when interest rate is near zero compared with normal times. While the cumulative spending multipliers are positive and persistent in normal times, they become negative after three years when the ZLB is in place, although the confidence bands are so wide that the estimates are statistically insignificant.

Moving forward, it would be interesting to investigate the behaviour of prices in different sub-samples, in particular, whether the issue of price puzzle persists during the Great Inflation and the Great Moderation periods in relation to the fiscal-monetary mix. There is existing evidence suggesting that a passive monetary and active fiscal (PMAF) regime prevailed during the pre-Volcker period, in which the fiscal theory of the price level (FTPL) was in play. Alternatively, the Great Moderation period is considered to be consistent with an active monetary and passive fiscal (AMPF) (Sims, 2011, Davig and Leeper, 2011). Therefore, a study on the price response to unanticipated and anticipated government spending shocks under these regimes and to what extent the regime in place matters can be a fruitful avenue for future research.

Another potential direction for future research is to specifically look at the price puzzle issue within the business sector. The result obtained with our baseline model lends support to Jørgensen and Ravn (2022)'s argument that an exogenous government spending shock boosts productivity as a result of firms adopting new technology and resources, which induces positive supply shocks that trump the typical positive demand effects. This suggests that it might be critical to take into account the supply-side effects to obtain a sharper understanding of the transmission of fiscal policy. However, when we consider responses of TFP and prices to different components of government spending, we observe a less pronounced reaction of TFP, yet substantial deflationary pressures on prices in response to a government investment shock compared with a government consumption shock. This casts doubts on the rationale for the price puzzle and hence requires further exploration. To the best of our knowledge, there is a dearth of empirical evidence in this regard. Therefore, an investigation on the impact of fiscal shocks - in particular - fiscal spending and its component shocks on firms and their pricing strategies using firm-level data remains an open topic for future research.

In Chapter 4, we aim to shed more light on studying the responses of economic variables at higher frequency (monthly) to understand the impacts and propagation mechanism of government spending shocks. An existing challenge in the literature is the identification of fiscal spending shocks when most of the macroeconomic variables relevant for this kind of analysis are available only at a quarterly or even annual frequency. To address this challenge, we propose using an alternative measure of outlays that is available at a higher frequency and closely tracks the actual government expenditures to identify the fiscal spending shock.

Using the local projections methodology developed by Jordà (2005), we find that an expansionary government spending innovation exerts considerably stronger effects on real activity when the nominal interest rate is not constrained by the effective lower bound. The finding of deflationary response at the ZLB, however, is inconsistent with the existing theoretical models on the aggregate demand effects of the fiscal stimulus during periods of financial distress. We also find that prices increase in response to a government spending shock in normal times. This observation differs sharply from the results reported in Chapter 3 as well as previous studies that find deflationary response of inflation following a government spending shock (Jørgensen and Ravn, 2022, D'Alessandro et al., 2019). We argue that this discrepancy might arise due to the frequency of the data used in the analysis. In particular, using higher-frequency (monthly) data rather than quarterly data enables us to tease out the responses of economic variables that might have been averaged out once aggregated. In any case, this finding deserves further attention and we leave this issue for future research.

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