Volume Title: Fiscal Policy after the Financial Crisis

Volume Author/Editor: Alberto Alesina and Francesco Giavazzi, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-01844-X, 978-0-226-01844-7 (cloth)

Volume URL: http://www.nber.org/books/ales11-1

Conference Date: December 12-13, 2011

Publication Date: June 2013

Chapter Title: Government Spending and Private Activity

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Chapter URL: http://www.nber.org/chapters/c12632

Chapter pages in book: (p. 19 - 55)

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Government Spending and Private Activity

Valerie A. Ramey

1.1 Introduction

The potential stimulus effects of fiscal policy have once again become an active area of academic research. Before the Great Recession, the few researchers who estimated the effects of government spending did so in order to understand which macroeconomic models were the best approximation to the economy. Rather than analyzing differences in estimated multipliers, most of the literature debated whether the movements of key variables, such as real wages and consumption, were more consistent with Keynesian or neoclassical views of fiscal policy (e.g., Rotemberg and Woodford 1992; Ramey and Shapiro 1998; Blanchard and Perotti 2002; Burnside, Eichenbaum, and Fisher 2004; and Perotti 2008). Starting with the stimulus debate, however, the focus shifted to empirical estimates of multipliers. In Ramey (2011b), I surveyed the growing recent literature that estimates government spending multipliers in aggregate national data as well as in state panel data. Reviewing that literature, I found that the range of estimates of the GDP multiplier is often as wide within studies as it is across studies. I concluded that the multiplier for a deficit-financed temporary increase in

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This is a revised version of a paper presented at the NBER conference, "Fiscal Policy after the Financial Crisis," in Milan in December 2011. I am grateful to Roger Farmer, Garey Ramey, and David Romer for discussions that led to the questions analyzed in this chapter and to Alberto Alesina, Francesco Giavazzi, Roberto Perotti, and participants in the conference for their comments. I thank Jonas Fisher for sharing the Fisher-Peters defense excess returns variable. For acknowledgments, sources of research support, and disclosure of the author's material financial relationships, if any, please see http://www.nber.org/chapters/c12632.ack.

government spending probably lies somewhere between 0.8 and 1.5, but could be as low as 0.5 or as high as 2.

Two of the key questions for deciding whether policymakers should use government spending for short-run stabilization policy are: (1) Can an increase in government spending stimulate the economy in a way that raises private spending? and (2) Can an increase in government spending raise employment and lower unemployment? With respect to the first question, if an increase in government spending raises GDP without raising private sector spending, then private welfare does not necessarily rise. With respect to the second question, most economists and policymakers would agree that job creation is at least as important a goal as stimulating output. In theory, one can use Okun's law to translate GDP multipliers to unemployment multipliers. However, because of variations in the parameters of this "law" over time, the advent of jobless recoveries, and the frictions involved in creating and filling jobs, the translation of output multipliers to employment or unemployment multipliers is not straightforward. Thus, it makes sense to devote as much attention to the employment effects of government spending as to the output effects.

This chapter empirically studies the effect of government spending on private spending, unemployment, and employment. I define private spending to be GDP less government spending. I show that whether one uses structural vector autoregressions (SVARs) or expectational vector autoregressions (EVARs), whether the sample includes World War II and Korea or excludes them, an increase in government spending never leads to a significant rise in private spending. In fact, in most cases it leads to a significant fall. These results imply that the government spending multiplier is more likely below one rather than above one.

These estimates are based on samples in which part of the increase in government spending is financed by an increase in tax rates, so the multipliers are not necessarily the ones applicable to current debates on deficit-financed stimulus packages. I thus explore two different ways to adjust for the increase in taxes in order to determine a deficit-financed government spending multiplier. One method uses the VARs to create counterfactuals and the other uses more structural instrumental variables estimates. Surprisingly, both methods suggest that the behavior of marginal tax rates does not have a significant effect on the size of the spending multiplier.

In the final part of the chapter I investigate the effects of government spending on unemployment and employment. I begin by conducting a case study of labor markets during the World War II period. I then use the VAR methods on various samples and find that an increase in government spending lowers unemployment. However, I find the surprising result that in the great majority of time periods and specifications, all of the increase in employment after a positive shock to government spending is due to an increase in government employment, not private employment. There is only one exception. These results suggest that the employment effects of government spending work through the direct hiring of workers, not stimulating the private sector to hire more workers.

1.2 Background

1.2.1 Output Multipliers

There has been a dramatic increase in research on the output multiplier in the last few years. The aggregate studies that estimate the multiplier fit in two general categories. The first are the studies that use long spans of annual data and regress the growth rate of GDP on current and one lag of defense spending, or government spending instrumented by defense spending (e.g., Hall 2009; Barro and Redlick 2011). These studies tend to find multipliers that are less than one. The second type are the VARs estimated on quarterly data, such as those used by Ramey and Shapiro (1998), Blanchard and Perotti (2002), Mountford and Uhlig (2009), Fisher and Peters (2010), Auerbach and Gorodnichenko (2012), and Ramey (2011c). Some of these papers calculate the multipliers based on comparing the peak of the government spending response to the peak of the GDP response. Others compare the area under the two impulse response functions. As I discuss in my forum piece for the *Journal of Economic Literature* (Ramey 2011b), the range of multiplier estimates are often as wide within studies as across studies. An interesting, but unnoticed, pattern arises from this literature. In particular, the Blanchard-Perotti style SVARs yield smaller multipliers than the expectational VARs (EVARS), such as the ones used in my work. This result is intriguing because the SVARs tend to find rises in consumption whereas the EVARs tend to find falls in consumption in response to an increase in government spending. Overall, most output multiplier estimates from the aggregate literature tend to lie between 0.5 and 1.5.

There are also numerous papers that use cross-sections or panels of states to estimate the effects of an increase in government spending in a state on that state's income. These papers typically find multipliers of about 1.5. However, translating these state-level multipliers to aggregate multipliers is tricky, as discussed in Ramey (2011b).

While the explicit instrumental variables frameworks with few dynamics provide statistical confidence bands around the implied multipliers, the VAR-based literature does not. Typically, the VAR literature provides separate impulse responses of government spending, GDP, and the spending subcomponents, and then calculates an implied multiplier by either comparing the peak response of GDP to the peak response of government spending, or comparing the integral under the two impulse response functions. As I will show later, a simple permutation of the VAR makes it easy to provide confidence intervals of the multiplier relative to unity.

1.2.2 Labor Market Effects of Government Spending

A few of the older papers and a growing number of recent papers have studied government spending effects on labor markets. Most of the studies that exploit cross-state or locality variation focus on employment as much as income. For example, Davis, Loungani, and Mahidhara (1997) and Hooker and Knetter (1997) were among the first to study the effects of defense spending shocks on employment in a panel of states. Nakamura and Steinsson (2011) study similar effects in updated data. Fishback and Kachanovskaya (2010) analyze the effects of various New Deal programs during the 1930s on states and localities. Chodorow-Reich et al. (2012) and Wilson (2012) estimate the effects of the recent American Recovery and Reinvestment Act (ARRA) on employment using cross-state variation. As summarized by Ramey (2011b), on average these and related studies produce estimates that imply that each $35,000 of government spending produces one extra job. However, some of these studies, such as by Wilson (2012), find that the jobs disappear quickly.

At the aggregate level, the recent paper by Monacelli, Perotti, and Trigari (2010) analyzes the effects of government spending shocks on a number of labor market variables. In particular, they use a standard structural VAR to investigate the effects of government spending shocks on unemployment, vacancies, job-finding rates, and separation rates in the post-1954 period. Their point estimates suggest that positive shocks to government spending lower the unemployment rate and the separation rate, and increase vacancies and the job finding rate. However, their estimates are imprecise, so most of their points estimates are not statistically different from zero at standard significance levels. On the other hand, Brückner and Pappa (2010) study the effects of fiscal expansions on unemployment in a sample of Organization for Economic Cooperation and Development (OECD) countries using quarterly data. Whether they use a standard SVAR, sign restrictions, or the Ramey-Shapiro military dates, they find that a fiscal expansion often *increases* the unemployment rate. In most cases, these increases are statistically significant at the 5 percent confidence level.

In sum, the studies using state or local panel data find more robust positive effects of government spending on employment than the aggregate studies. As discussed earlier, translating state-level multipliers to the aggregate is not straightforward.

1.2.3 The Distinction between Government Purchases and Government Value Added

To understand why there is not a one-to-one correspondence between output multipliers and private employment multipliers, it is useful to consider the distinction between government spending on private goods versus government output. In the National Income and Product Accounts (NIPA), government purchases (G) includes both government *purchases of goods* from the private sector, such as aircraft carriers, and government *value added,* which is comprised of compensation of government employees, such as payments to military and civilian personnel, and consumption of government capital. Rotemberg and Woodford (1992) made this distinction in their empirical work by examining shocks to total defense spending after conditioning on lags of the number of military personnel. Wynne (1992) was the first to point out the theoretical distinction between government spending on purchases of goods versus compensation of government employees. He used comparative statistics in a neoclassical model to demonstrate the different effects. Finn (1998) explored the issue in a fully dynamic neoclassical model. She showed that increases in G resulting from an increase in government employment and increases in G resulting from an increase in purchases of goods from the private sector have opposite effects on private sector output, employment, and investment. Other authors exploring this distinction include Cavallo (2005), Pappa (2009), and Gomes (2010).

Figure 1.1 shows the two ways of dividing the output of the economy. The top panel shows the usual way of dividing goods and services according to which entity purchased the goods. Variable G is the usual NIPA category of "Government Purchases of Goods and Services." The rest of the output is purchased by the private sector, either as consumption, investment, or net exports. The middle panel divides the economy according to who produces the goods and services. Production by the government occurs when it directly hires workers and buys capital stock. The value added is counted as production by this sector. Examples include education services, police services, military personnel services, and other general government activities.[1] All other production is done by the private sector. The third panel superimposes these two ways of dividing the economy. As the panel illustrates, government purchases (G) consist of the value added of government (Y^{Gov}), which the government itself produces and essentially "sells" to itself, and government purchases of goods and services from the private sector (G^{Priv}). During the typical military buildup, the government hires more military personnel, resulting in more government production, and buys tanks from the private sector. Thus, both components of G rise.

To see why different types of government spending can have different effects, consider the following key equations from an augmented neoclassical model. Consider first the production function for private value added:

$$Y^{\text{Priv}} = F(N^{\text{Priv}}, K^{\text{Priv}}), \tag{1}$$

1. The output of government enterprises, such as post offices of the US Postal Service, are included in the private business sector.

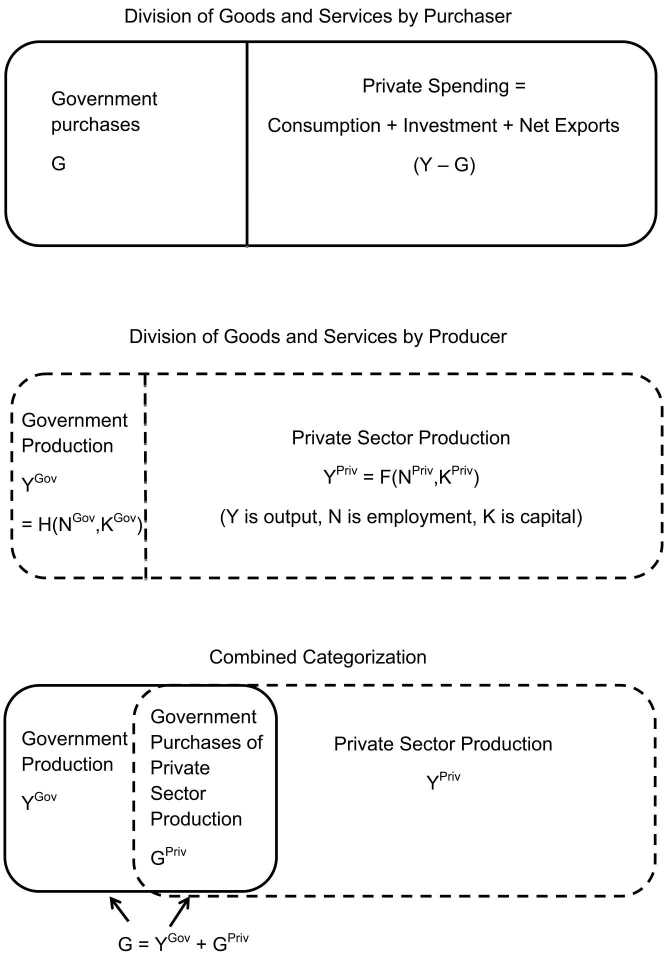

Fig. 1.1 Government spending versus government output distinction
Notes: The region sizes are not to scale.

where Y^{Priv} is private value added, N^{Priv} is private employment, and K^{Priv} is the private capital stock. The number of workers available for private employment is determined by the labor resource constraint:

$$(2) \qquad N^{\text{Priv}} = \bar{T} - N^{\text{Gov}} - L,$$

where $\bar{T}$ is the time endowment, N^{Gov} is government employment, and L is leisure. Thus, one way that the government draws resources from the private sector is through the labor resource constraint. Another way that the government draws resources from the private sector is through its purchases of private goods. In this case, the affected resource constraint is the one for private output, given by:

$$Y^{\text{Priv}} = C + I + NX + G^{\text{Priv}}, \tag{3}$$

where G^{Priv} is government purchases from the private sector. Total G from the NIPA is:

$$G = G^{\text{Priv}} + Y^{\text{Gov}}, \tag{4}$$

where Y^{Gov} is government value added, created by combining government employment with government capital as follows:

$$Y^{\text{Gov}} = H(N^{\text{Gov}}, K^{\text{Gov}}). \tag{5}$$

Under reasonable assumptions about labor markets and production functions, the relative price of private and government output is one, so total GDP is given by:

$$Y = Y^{\text{Priv}} + Y^{\text{Gov}}. \tag{6}$$

In the context of this type of model, an increase in government spending raises total employment. However, the extent to which government spending raises private employment depends on whether the increase in G is due more to an increase in purchases of private sector output or more to an increase in government output and employment. We would expect private sector employment to rise in the first case but to fall in the second case. Thus, a rise in overall employment does not necessarily imply a rise in private sector employment, so it is important to distinguish private versus government employment in the data.

1.3 The Effects on Private Spending

In most studies using aggregate data and VARs, government spending multipliers are usually calculated by comparing the peak of the output response to the peak of the government spending response or by comparing the integral under the impulse response functions up to a certain horizon. Usually, no standard errors are provided, but given the wide standard error bands on the output and government spending components, the standard error bands on the multipliers are assumed to be large. Studies of the subcomponents of private spending, such as nondurable consumption or nonresidential fixed investment, often give mixed results with wide error bands.

As I will now show, a simple permutation of the variables in a standard VAR can lead to more precise estimates for the relevant policy question: on average does an increase in government spending raise private spending? To answer this question, I will use a standard set of VAR variables employed by many in the literature with one modification: I will use private spending ($Y-G$) rather than total GDP. Since previous VAR studies have shown that the peak of government spending and the peak of total GDP are roughly coincident in the impulse response functions, I do not distort the results by considering only the contemporaneous multiplier.

1.3.1 Econometric Framework

To study the effects of government spending shocks on private spending, I will estimate the following VAR system:

$$X_t = A(L)X_{t-1} + U_t, \tag{7}$$

where X_t is a vector of variables that includes the log of real per capita government spending on goods and services (G), the log of real per capita private spending ($Y-G$), the Barro-Redlick average marginal tax rate, and the interest rate on three-month Treasury bills, as well as key variables for identification that I will discuss shortly. The interest rate and tax variables are used as controls for monetary and tax policy, and $A(L)$ is a polynomial in the lag operator. As is standard, I include four lags of all variables, as well as a quadratic time trend.

I consider several of the main identification schemes used in the literature. These are as follows:

1. *Ramey News EVAR:* Concerned that most changes in government spending are anticipated, Ramey and Shapiro (1998) used a dummy variable for military events that led to significant rises in defense spending as the exogenous shock. In more recent work in Ramey (2011c), I extended this idea and used sources such as *Business Week* to construct a series of changes in the expected present discounted value of government spending caused by military events. I divided this series by the previous quarter's GDP to create a "news" series. This series augments the list of variables in the X matrix in the previous VAR and the shock is identified as the shock to this series, using a standard Choleski decomposition with the news series ordered first. Perotti (2011) has termed VARs that incorporate news "Expectational VARs" or "EVARs."

2. *Blanchard-Perotti SVAR:* Blanchard and Perotti (2002) identify the shock to government spending with a standard Choleski decomposition with total government spending ordered first. No news series are included in the VAR.

3. *Perotti SVAR:* Perotti (2011) claims that the structural VAR (SVAR) equivalent to my news EVAR is one that replaces the news series with defense spending or federal spending. Shocks are then identified as shocks to this variable ordered first. (Total government spending is also included in the VAR.) As my reply argues (Ramey 2011a), there is little difference between the impulse response functions generated by this scheme and the original

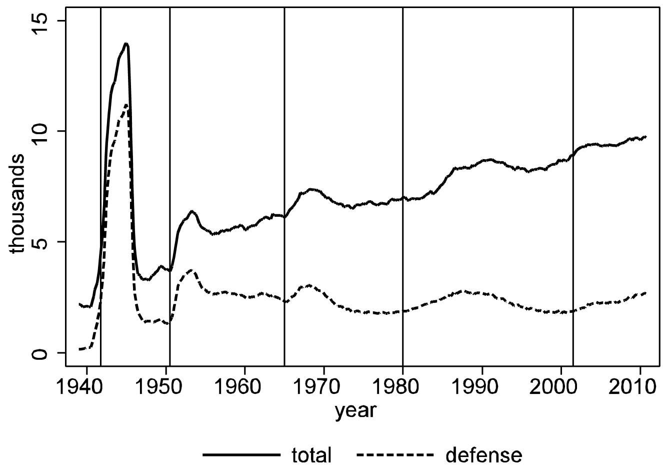

Fig. 1.2 Real government spending per capita, 1939:Q1–2010:Q4
Notes: Data from the NIPA and the Census. Amounts are stated in 2010 dollars.

BP scheme. For the sake of argument, however, I will also show the results from this scheme, where I augment the system with defense spending. Since the results are so similar to the Blanchard-Perotti SVAR, these results are shown in the data appendix.

4. *Fisher-Peters EVAR:* Fisher and Peters (2010) develop an alternative measure of anticipated increases in government spending based on stock returns. They use the cumulative excess returns on stocks of defense contractors relative to the rest of the stock market as an indicator of anticipated increases in defense spending. This series is available for 1958 to 2008. Thus, this specification is the same as the first one, but with the Fisher-Peters news variable replacing the Ramey news variable.

While some, such as Blanchard and Perotti (2002), have argued that one should omit World War II and the Korean War from the sample, Hall (2009), Ramey (2011c), and Barro and Redlick (2011) argue that there is not enough variation in government spending after 1954 to identify the effects of government spending. Consider figure 1.2, which updates the figure shown in numerous other papers. It is clear that the movements in government spending during World War II and the Korean War are orders of magnitude greater than any other movements. The notion that there is much less information in the post-1954 period is also supported by statistical analysis. As I demonstrate in Ramey (2011c), the first-stage F-statistic for my news series is well above the Staiger and Stock (1997) safety threshold of 10 for samples that include either World War II or Korea. However, the F-statistic is very low for samples that exclude both periods. Fisher and

Peters' (2010) excess returns measure has a first-stage *F*-statistic of 5.5 for defense spending, but only 2.3 for total government spending. Both are below the Staiger-Stock safety threshold. An additional concern about the Fisher-Peters measure of news is raised in Ramey (2011c). Because exports of military goods constitute part of the profits of defense companies, the Fisher-Peters' variable might be capturing news about exports as well as news about future US government spending.

Given the debate on this issue, I estimate the first three specifications on three samples each: 1939:1 to 2008:4, 1947:1 to 2008:4, and 1954:1 to 2008:4. The Fisher-Peters EVAR is estimated from 1958:1 to 2008:4. In all cases, the shock is normalized so that the peak of government spending is 1 percent of GDP. The response of private spending is converted to percentage points of total GDP. The standard error bands are 95 percent bands based on bootstrap standard errors.

1.3.2 VAR Results

Figure 1.3 shows results from the EVAR using my news variable. In the first two samples, government spending rises significantly and peaks at around six quarters. The delayed response of actual government spending to the news variable is consistent with my hypothesis that government spending changes are anticipated at least several quarters before they happen. In the 1939 to 2008 sample, private spending rises slightly on impact, but then falls significantly below zero, troughing at around 0.5 percent of GDP. In the 1947 to 2008 sample, private spending rises significantly on impact, to about 0.5 percent of GDP, but then falls below zero within a few quarters. These results are consistent with the effects of anticipations discussed in the theoretical section of Ramey (2009b). As that paper showed, in a simple neoclassical model, news about future increases in government spending lead output to rise immediately, even though government spending does not rise for several quarters. Thus, the theory predicts that private output should jump on impact and then fall. In addition, as discussed as well in Ramey (2011c), the Korean War is influential in the post–World War II sample. As observed in the consumer durable expenditure data and discussed in the press at that time, the start of the Korean War led to panic buying of durable goods in the United States because many feared rationing (such as during World War II) was imminent. This is another likely source of the positive impact effect. For the post–Korean War period, the low first-stage *F*-statistic of my news variables means that any results for that sample are questionable. Nevertheless, they are shown for completeness. The standard error bands are much larger for this sample. Private spending falls, but the estimates, though large, are not statistically different from zero.

Figure 1.4 shows the responses based on the Blanchard-Perotti SVAR. In contrast to the EVAR, this specification implies that government spending jumps up immediately in all three samples. Private spending declines

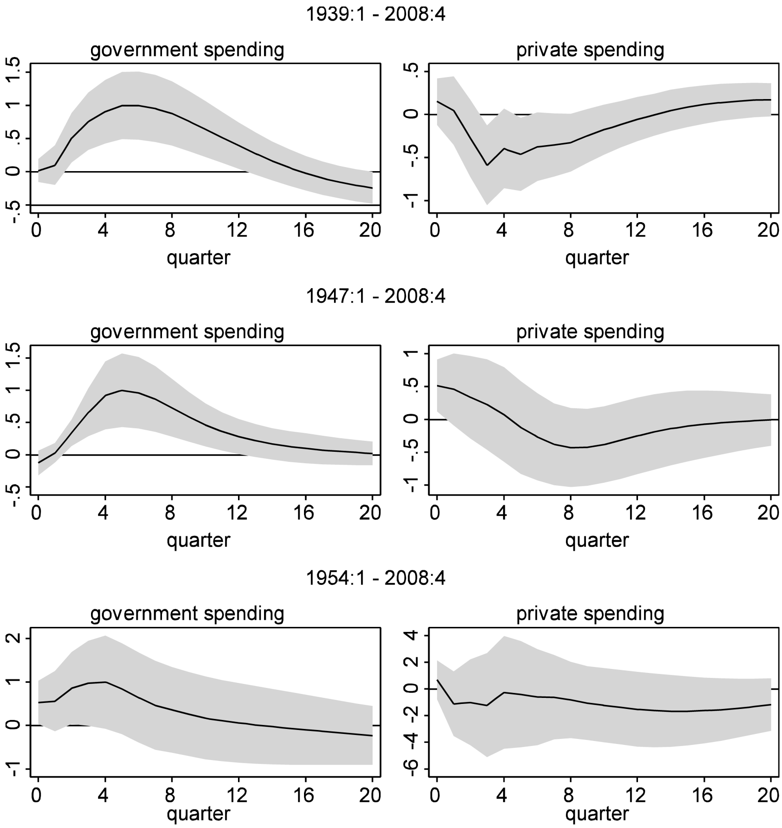

Fig. 1.3 Private spending responses: Ramey news EVAR

Notes: The government spending shock is identified as the shock to the news variable, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Private GDP is denoted as a percent of total GDP. The standard error bands are 95 percent bands based on bootstrap standard errors.

significantly in response to a rise in government spending in the first two samples. The declines are sizeable, suggesting multipliers well below one. In the post–Korean War sample, private spending falls slightly below zero, but is not statistically significant. Appendix figure 1A.1 shows that the results of the augmented SVAR advocated by Perotti (2011) are essentially the same.

Figure 1.5 shows the responses based on the Fisher-Peters type of SVAR, where government spending shocks are identified as shocks to the excess stock returns for defense contractors. In contrast to the three previous specifications in which government spending peaks around quarter six and returns to normal between twelve and fourteen quarters, this shock leads to a more sustained increase in government spending. Government spending barely falls from its peak, even after twenty quarters. Private

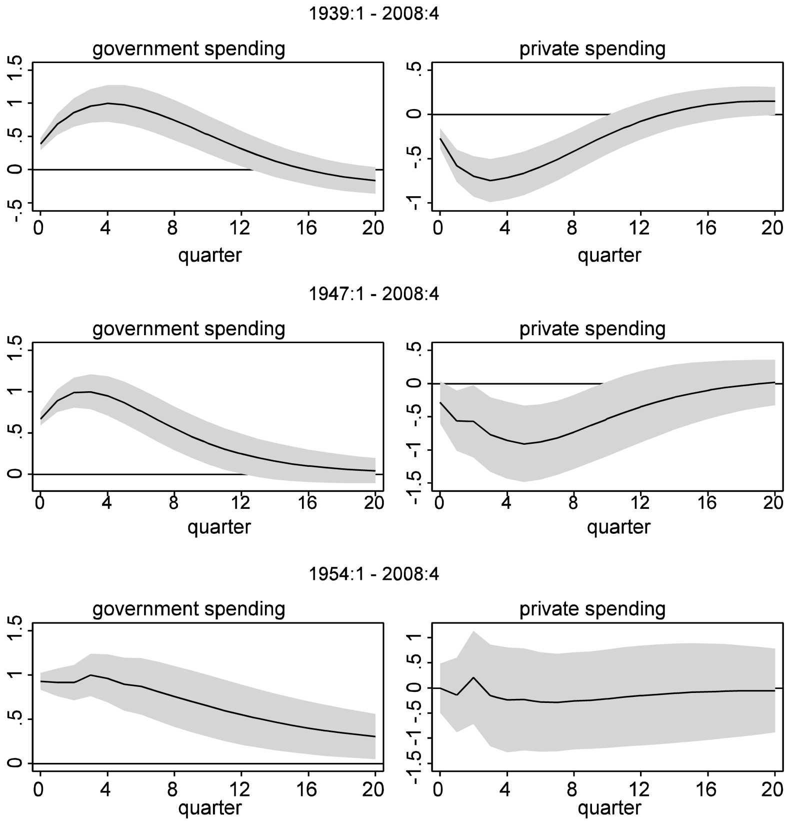

Fig. 1.4 Private spending responses: Blanchard-Perotti SVAR

Notes: The government spending shock is identified as the shock to total government spending, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Private GDP is denoted as a percent of total GDP. The standard error bands are 95 percent bands based on bootstrap standard errors.

spending oscillates around zero, but it only becomes statistically different from zero when it becomes negative at longer horizons.

Thus, the SVAR specifications give essentially the same answer to the question posed as do the EVAR specifications: a rise in government spending does not appear to stimulate private spending. In fact, in many samples and specifications, it reduces private spending.

An interesting point to note is that the VAR results imply a time-varying multiplier that shrinks as government spending hits its peak. This result is consistent with Gordon and Krenn's (2010) finding of a higher multiplier in samples ending in mid-1941, when the increase in government spending was more modest than in samples ending later.

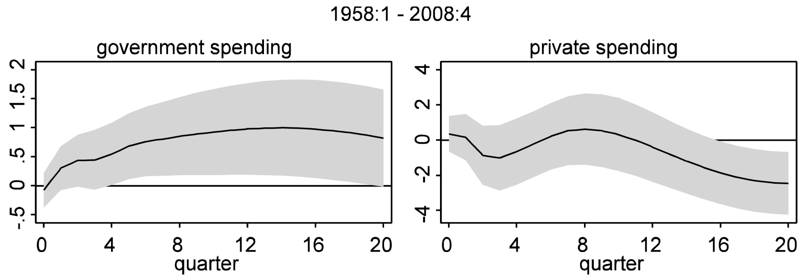

Fig. 1.5 Private spending responses: Fisher-Peters news EVAR

Notes: The government spending shock is identified as the shock to excess stock returns of top defense contractors, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Private GDP is denoted as a percent of total GDP. The standard error bands are 95 percent bands based on bootstrap standard errors.

1.3.3 The Effects of Taxes and Implications for Multipliers

These results imply that for the types of changes in government spending identified by the various schemes, the total GDP multiplier lies below unity. In every case, there is evidence that government spending crowds out private spending. On average, though, these increases in government spending were financed partly by a rise in distortionary taxes. Figure 1.6 shows the impulse responses of the Barro and Redlick (2011) average marginal tax rate for the various samples for both the Ramey News EVAR and Blanchard-Perotti SVAR specifications. In five of the six cases, the tax rate rises significantly. It rises much more in the Ramey News EVAR.

Romer and Romer (2010) construct a measure of exogenous tax shocks using a narrative approach that summarizes tax legislation. They show that the reduced-form effect of a tax shock equal to 1 percent GDP leads to a multiyear decline in GDP equal to 2.5 to 3 percent of GDP by the end of the third year. These estimates suggest that the multiplier could potentially be greater for a deficit-financed increase in government spending than for one in which taxes rise.

To gauge how much the rise in taxes dampens the spending multiplier, I conduct two different kinds of experiments. The first one uses the estimated VARs to conduct counterfactual analysis and the second uses instrumental variables estimation of a more structural model. In the first method, using the estimated VARs, I compare the actual estimated impulse response to one in which I assume counterfactually that the tax rate did not change. That is, I set all of the coefficients in the tax rate equation to zero. I then compute the alternative impulse response based on a dynamic simulation using the actual estimated coefficients from the other equations and the zero coefficients from the tax rate equation.

Figure 1.7 shows the results for government spending and private output.

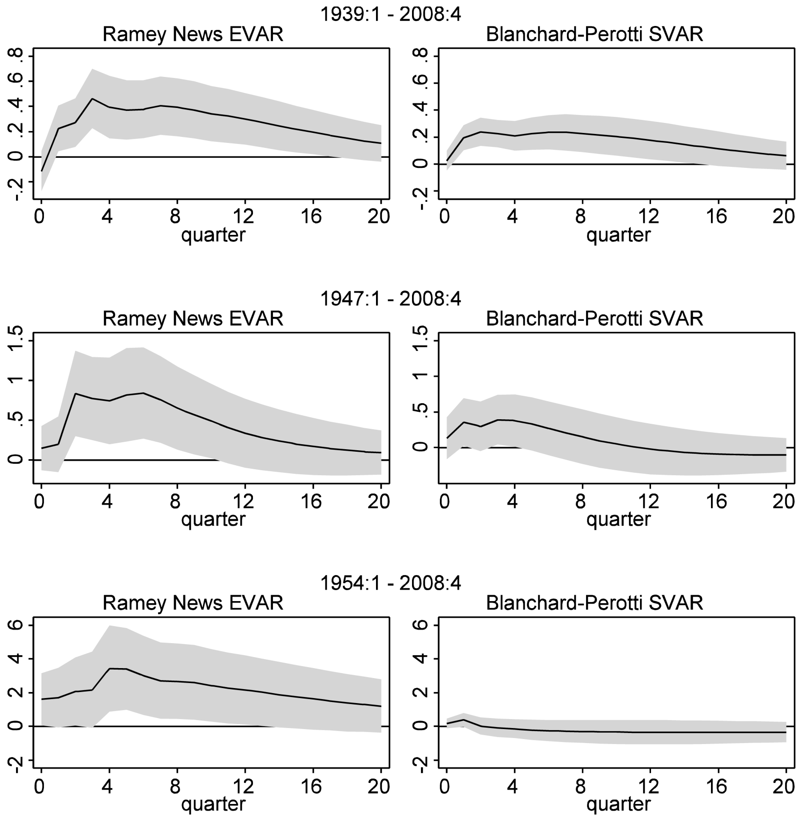

Fig. 1.6 Barro-Redlick tax rate responses

Notes: The shock is normalized so that government spending peaks at 1 percent of GDP. The response of taxes is shown in percentage points. The standard error bands are 95 percent bands based on bootstrap standard errors.

In both the Ramey News EVAR and the Blanchard-Perotti SVAR, the response of neither government spending nor private output changes much. The fact that the paths change little implies that the coefficients on tax rates in the equations for the other variables are not economically different from zero.

Because VARs are essentially reduced-form relationships, it is difficult to make structural interpretations. Thus, my second method uses instrumental variables to estimate the separate effects of government spending and taxes on private output. I specify the following baseline quarterly model, which is similar in structure to the one used by Barro and Redlick (2011) on their long sample of annual data:

$$\frac{\Delta S_t^{\text{Priv}}}{Y_{t-1}} = \beta_0 + \beta_1 \frac{\Delta G_t}{Y_{t-1}} + \beta_2 \Delta_4 \text{tax}_t + \beta_3 \text{News}_t + \varepsilon_t, \tag{8}$$

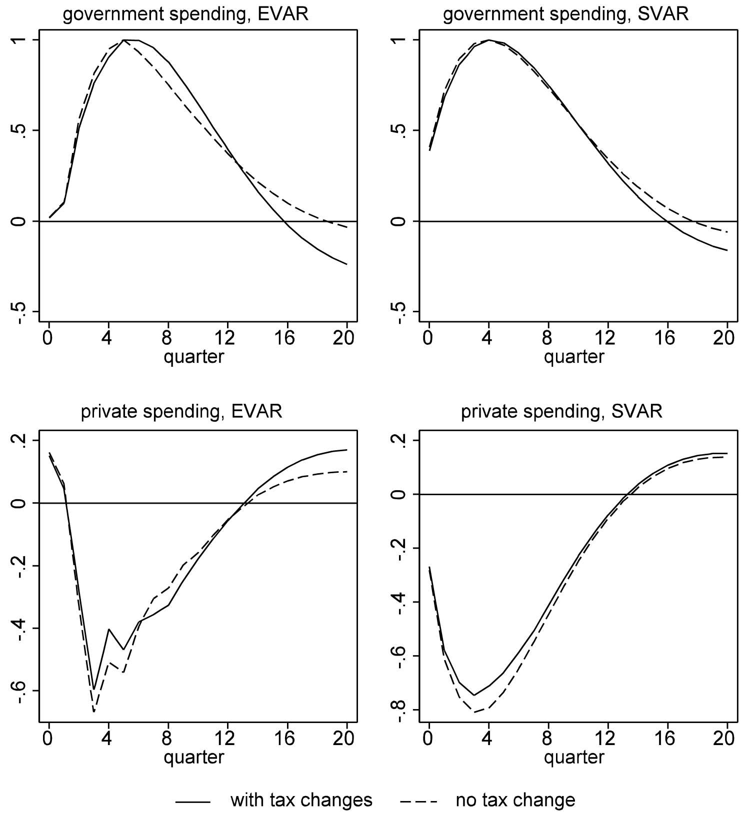

Fig. 1.7 Estimating the effects of taxes on the government spending multiplier, 1939:Q1–2008:Q4

Notes: In the EVAR the government spending shock is identified as the shock to the news variable, ordered first. In the SVAR, the government spending shock is identified as the shock to total government spending, ordered first. The line labeled "with tax changes" is the estimated effect of a government spending shock, allowing taxes to change as estimated. The line labeled "no tax change" is the computed counterfactual response in which taxes are not allowed to change.

where S^{Priv} is real private spending ($Y - G$), Y is real GDP, G is real government spending (deflated by the GDP deflator), *tax* is a measure of the tax rate, *News* is from Ramey (2011c) and is equal to the change in the expected present discounted value of government purchases caused by military events, and ε is an error term. The four-quarter difference of tax rates is used because the Barro-Redlick variable only changes once per year. It is potentially important to include the current value of the news variable as a control. According to the argument made in my earlier work, private agents respond to news about future government spending before the spending even occurs. My earlier work emphasized the negative wealth effect, but other possible factors include building up capital in anticipation of future government spending because of investment adjustment costs and buying consumer goods because of fears of future rationing.

Because both government spending and taxes are potentially affected by the state of the economy, which also impacts private spending, we would expect these fiscal variables and tax rates to be correlated with the error term ε. Thus, estimation calls for instrumental variables. A natural instrument for the tax variable is the Romer and Romer (2010) narrative series on exogenous tax changes. This variable calculates the annualized change in tax liabilities due to legislation based on either deficit concerns or long-run growth promotion. Thus, the identification assumption I am making is that the tax legislation changes affect the economy only through changes in tax rates. Because the Romer-Romer tax instruments are available only from 1945 to 2007, the estimation must exclude the World War II sample. For government spending, I use *lags* of my news variable as an instrument. Because the current value of news is an included variable, my identification assumption is that while current news can independently affect private spending, lagged values of news affect the economy only through current changes in government spending. This assumption might be questionable if there are additional lags in the effects. Thus, I will assess the robustness of the results to adding lags of spending growth, government spending, and taxes to the specification. Using the period 1948 to 2007, I explored various lags of the two instruments up to twelve lags. I use four lags of each instrument since this number of lags maximized the Cragg and Donald (1993) statistic.

Table 1.1 shows the estimates from the model presented in equation (8). The top panel shows the results when the tax rate is measured with the Barro-Redlick average marginal tax rate and the bottom panel shows the results when the tax rate is measured as the ratio of current tax receipts to GDP. The first column shows the results when tax rate changes are excluded from the equation. The estimated effect of a change in government spending on private output is –0.7, with a standard error of 0.26. This estimate implies a multiplier on total GDP of only 0.3. In contrast, news about future government spending increases current private spending. An increase in the expected present discounted value of future government spending of one dollar raises current private spending by about five cents. The effect of this variable is estimated precisely. The high Cragg and Donald (1993) statistics imply that we can reject the null hypothesis of weak instruments at any relative bias level.

The second column shows the results of the baseline model when tax rates are included. For both tax rate specifications, the coefficient on government spending implies that a one dollar increase in government spending lowers private spending by 55 cents. The news variable continues to be positive and significant, while the tax variable is negative, but is not statistically different

Table 1.1 **Instrumental variables regressions for private spending**

Specification	No tax	Tax-1	Tax-2	Tax-3
		Dependent variable: $\Delta S_t^{\text{Private}}/Y_t$		
		Barro-Redlick average marginal tax rate		
$\Delta G_t/Y_{t-1}$	–0.705***	–0.539*	–0.506*	–1.057*
	(0.259)	(0.284)	(0.268)	(0.548)
$\Delta G_{t-1}/Y_{t-2}$				0.632
				(0.535)
News_t	0.056***	0.059***	0.038**	0.042***
	(0.016)	(0.017)	(0.016)	(0.017)
$\Delta_4\tau_t$		–0.097	–0.057	0.215
		(0.116)	(0.109)	(0.435)
$\Delta_4\tau_{t-1}$				–0.293
				(0.388)
$\Delta S_{t-1}^{\text{Private}}/Y_{t-2}$			0.319***	0.346***
			(0.060)	(0.069)
Cragg-Donald Wald F-statistic	51.80	8.13	8.16	0.98
		Average tax rate		
$\Delta G_t/Y_{t-1}$		–0.548**	–0.483*	–0.770
		(0.278)	(0.263)	(0.540)
$\Delta G_{t-1}/Y_{t-2}$				0.294
				(0.521)
News_t		0.067***	0.043**	0.045**
		(0.020)	(0.018)	(0.018)
$\Delta_4\tau_t$		–0.194	–0.153	–0.271
		(0.196)	(0.183)	(0.417)
$\Delta_4\tau_{t-1}$				0.129
				(0.393)
$\Delta S_{t-1}^{\text{Private}}/Y_{t-2}$			0.353***	0.380***
			(0.073)	(0.082)
Cragg-Donald Wald F-statistic		7.25	8.08	2.14

Notes: All regressions contain 240 quarterly observations, estimated from 1948:Q1 to 2007:Q4. Variable S^{Private} denotes real private spending, Y denotes real GDP, G denotes real government spending (deflated by the GDP deflator), and τ denotes the tax rate. The average tax rate is calculated as the ratio of current tax receipts divided by GDP. Variable Δ_4 denotes the four-quarter difference. Current values of government spending are instrumented with four lagged values of Ramey news and current values of tax rate changes are instrumented with the current value and four lags of the Romer-Romer exogenous tax shock. When lags are included, an extra lag of the instruments is used. Standard errors are reported in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

from zero. The Cragg and Donald (1993) statistics, ranging from seven to eight, imply that we can reject the null hypothesis of weak instruments for a 15 percent maximal relative bias, according to the Stock and Yogo (2005) critical values for instrument relevance. Thus, controlling for taxes reduces the magnitude of the negative effect of government spending on private spending by 0.15, from –0.7 to –0.55. Given the size of the standard errors, however, the change is probably not statistically significant.

I also investigated the effect of omitting the current value of the news variable as a regressor and instead including it as an instrument for government spending (results not shown in the table). When the Barro-Redlick tax rate is used, the coefficient on government spending is estimated to be –0.64, with a standard error of 0.29. Thus, the negative effect of government spending on private spending becomes even more negative if the news variable is omitted. The coefficient on the tax variable becomes slightly positive but not different from zero.

The third column of table 1.1 explores the effect of controlling for the lagged growth of private spending. This variable is statistically significant, but it lowers the magnitude of the government spending coefficient only slightly, to about 0.5. Finally, the last column also adds lags of government spending and taxes. This results in imprecise estimates for a number of the coefficients and leads to unacceptably low Cragg-Donald statistics. Not shown in the table are the results of other explorations, which either replace the four-quarter difference of tax rates with the one-quarter difference of tax rates or replace the one-quarter difference of government spending with the four-quarter difference, or substitute the change in real tax receipts relative to lagged GDP for the Barro-Redlick tax rate. The results do not change in any meaningful way in these alternative specifications, except to become more imprecise and/or have inferior first-stage statistics.

My survey of the estimates in the literature in Ramey (2011b) concluded that the multiplier for a deficit-financed short-run increase in government spending was probably between 0.8 and 1.5, but that reasonable people could argue for multipliers as low as 0.5 or as high as 2. The main reason that I placed the lower end of the probable range at 0.8 was my belief, based on the Romer and Romer (2010) reduced-form results, that the tax effects on GDP were large. The results of this section suggest otherwise. The counterfactuals constructed from the VAR estimates imply that accounting for current tax rates has no impact on the estimated government spending multiplier. The instrumental variables estimates imply that controlling for changes in tax rates raises the multiplier slightly, by about 0.15 to 0.2. The instrumental variables estimates imply a government spending multiplier on total GDP of about 0.5. This is very close to the estimate obtained using annual data by Barro and Redlick (2011).

1.4 The Effects of Government Spending on Unemployment and Employment

As we saw in the last section, no matter which identification scheme or which sample period was used, an increase in government spending did not lead to an increase in private sector spending. In most cases, it led to a significant decrease. Even in the face of this result, however, policymakers might still want to use stimulus packages to reduce unemployment. There is substantial microeconomic evidence that long spells of unemployment lead to persistent losses of human capital. Thus, even if government spending cannot stimulate private spending, it might still have positive effects by raising employment.

This section studies several aspects of the labor market. It begins with a case study of the labor market during World War II because the dramatic changes of this era highlight some useful points. In the second subsection, I use the VARs from earlier to study the effects of government spending shocks on unemployment. In the third subsection, I study their effects on private versus government employment.

1.4.1 A Case Study of the Labor Market during World War II

World War II is especially interesting from a labor market point of view because the economy went from an unemployment rate of 12 percent (18 percent if one includes emergency workers as unemployed) at the start of 1939 to an unemployment rate of less than 1 percent by 1944. Thus, it is useful to conduct a case study before moving on to the formal statistical analysis. To review the history briefly, Germany invaded Poland on September 1, 1939, and Britain and France declared war on September 3, 1939. Initially, the US stock market increased, as exports to the belligerents increased business profits. The stock market started to decline in spring 1940, as Germany took Norway in April 1940, invaded the low countries in May 1940, and took Paris in June 1940. As discussed in Ramey (2009a), the United States began gearing up for war well before Pearl Harbor, which occurred on December 7, 1941. Figure 1.8 shows Gordon and Krenn's (2010) monthly interpolated data on real government spending (in 1937 dollars) from 1938 to 1945. Between January 1938 and September 1940 when the draft was enacted, real government spending rose by 18 percent (measured in log differences), or by 2.6 percent of initial January 1938 GDP. Between September 1940 and December 1941, real government spending increased 89 percent (in log differences), or by 20 percent of initial September 1940 GDP. It increased another 113 percent (in log differences), or 56 percent of December 1941 GDP, between December 1941 and its peak in May 1945.

Consider the labor market at the start of World War II. Based on new labor market data that I have compiled for this period, we can track the flows of individuals between various labor market states. I use September 1940 as the starting point, since that is when the draft was instituted and because government spending started rising in the fourth quarter of 1940. Figure 1.9 shows the behavior of various employment components around this time and table 1.2 presents the net changes. The first feature to note from the figure is that, despite the fact that government spending rose by only 2.6 percent of initial GDP, total employment rose by 8 percent from 1938 to

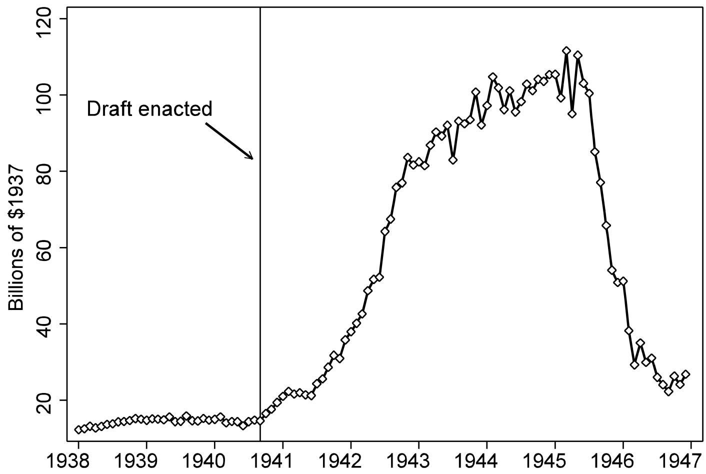

Fig. 1.8 Real government spending during World War II
Notes: Data from gordon.krenn-2010-nber.

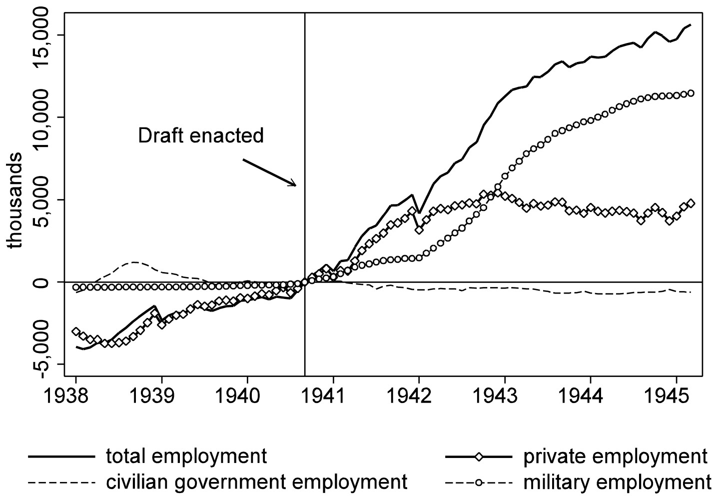

Fig. 1.9 Decomposition of employment changes during World War II
Notes: Each employment component has been normalized to zero in September 1940. Numbers are in thousands. Employment data based on data compiled by V. Ramey. See data appendix for details.

September 1940, and that most of the increase in total employment was due to an increase in private employment. As the table shows, from September 1940 to the peak in March 1945, total employment rose by 15.6 million, a 27 percent increase in log differences. Most of the rise was due to the rise in military employment, however. Government civilian employment (including

Table 1.2 Labor market changes during World War II: September 1940 to March 1945

Component	Change (in millions)
Total employment	15.6
Military employment	11.5
Population ages 14+	5.4
Labor force	11.1
Unemployment	–4.5
Emergency workers	–2.5

Source: Data compiled by the author. See data appendix for details.

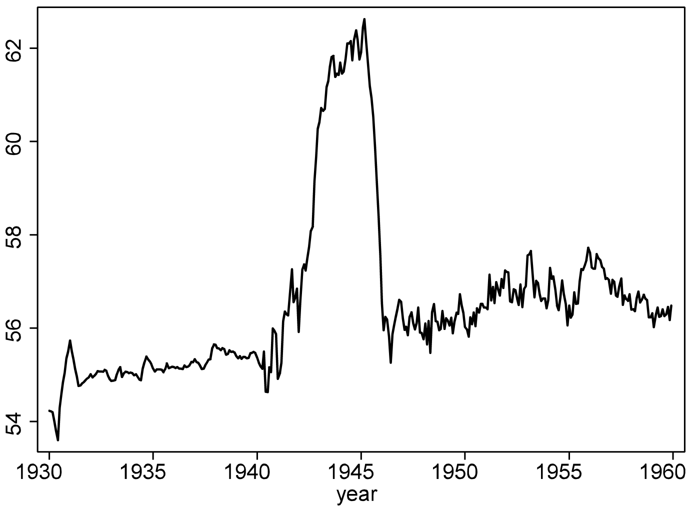

Fig. 1.10 Labor force participation rate

Notes: Labor force participation rate of population ages fourteen and above. Based on data compiled by V. Ramey. See data appendix for details.

New Deal emergency workers) declined slightly during this period. As the figure shows, private employment rose robustly from 1938 through most of 1941, but then leveled off.

Over this same time period, the population ages fourteen and above rose by 5.4 million, but the labor force rose by 11.1 million. Figure 1.10 shows the dramatic increase in the labor force participation rate. Decennial data from before 1930 suggests a typical labor force participation rate of around 56 percent. It was a little lower during the 1930s because of the Great Depression. As the graph shows, during World War II, the participation rate was 6 percentage points higher than it was before or in the decade after. Thus, 70 percent of the increase in employment during World War II is accounted for by the rise in the labor force, with a large part of that increase due to an increase in the participation rate. It is likely that an important part of that rise was due to the effects of the draft and patriotism. The number in the military rose by 11.5 million during World War II. The rise was only 2.2 million during the Korean War.

Over this same period from 1940 to 1945, the number unemployed fell by 4.5 million. Thus, the remaining 30 percent of the increase in employment was due to flows from unemployment to employment.

These numbers omit one other important flow of workers. My unemployment numbers do not include emergency workers, who were workers employed by the various New Deal government programs. Like Darby (1976) and Weir (1992), I included those workers in the "employed" category rather than unemployed. The number of individuals employed as emergency workers decreased from 2.5 million in September 1940 to zero by mid-1943. Thus, these workers represented an additional 2.5 million workers available for other sectors.

While total employment rose by 27 percent, real GDP rose by 58 percent (in log points), meaning that labor productivity rose by 31 percent. Thus, during the five-year period from 1940 to 1945, labor productivity rose at an average annualized rate of 7 percent. This rate of growth is substantially greater than the growth of productivity in the decade before or the decade after. For example, from 1947 to 1960, labor productivity growth was about 3.3 percent. In their study of the behavior of the economy during World War II, McGrattan and Ohanian (2010) find that the neoclassical model can explain the data only if one also assumes large positive productivity shocks.

Since the GDP multiplier is intimately linked to the effect of government spending on employment and productivity, the combination of the unprecedented rise in the labor force participation rate and the exceptionally high rate of productivity growth most likely raised the GDP multiplier during World War II relative to more normal times. Some researchers, such as Hall (2009), Gordon and Krenn (2010), and Perotti (2011), have argued that the multiplier estimated from samples that include World War II may be lower because of price controls and rationing. While there is no denying that price controls and rationing distorted allocations, their argument only makes sense if the price controls and rationing depressed employment and productivity *more* than the other factors, such as conscription and patriotism, raised it. The extraordinary increases in labor force participation rates, employment, and productivity that I have just documented suggest that this argument is implausible.

1.4.2 The Effects of Government Spending on Unemployment

Given the previous case study, it is interesting to use more formal analysis to determine the effect of government spending on unemployment. To do this, I estimate the following modification of the VARs described in equation (7) in the earlier section. First, I revert to using total GDP rather than just

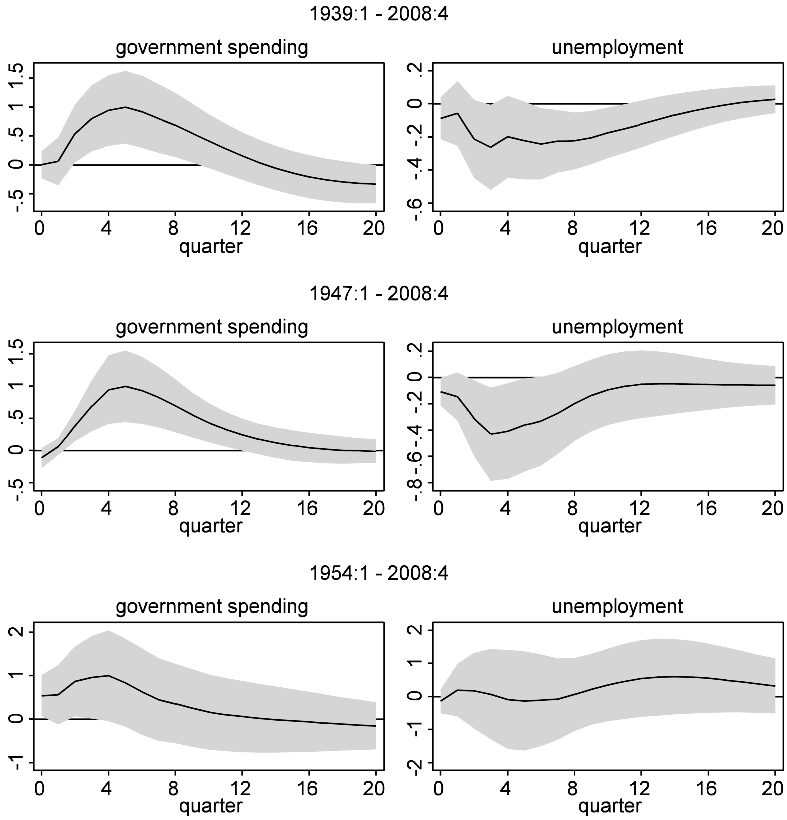

Fig. 1.11 Unemployment responses: Ramey news EVAR

Notes: The government spending shock is identified as the shock to the news variable, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Unemployment is denoted as a percent of the civilian labor force. The standard error bands are 95 percent bands based on bootstrap standard errors.

private GDP, as is common in the literature. Second, I add the log of per capita unemployment to the VARs. For the impulse response functions, I rescale unemployment so that it has the same scale as the civilian unemployment rate, based on a long-run average unemployment rate of 5.5 percent.

Figures 1.11 through 1.13 show the impulse response functions. For the various identification schemes and samples, the point estimates suggest that an increase in government spending leads to a fall in unemployment. The fall is always statistically significant in the period from 1939 to 2008 and is sometimes significant in the other periods for a number of the specifications. Most estimates imply that an increase in government spending that peaks at 1 percentage point of GDP lowers the unemployment rate by between 0.2 and 0.5 percentage points. The exception is the EVAR that uses the Fisher-

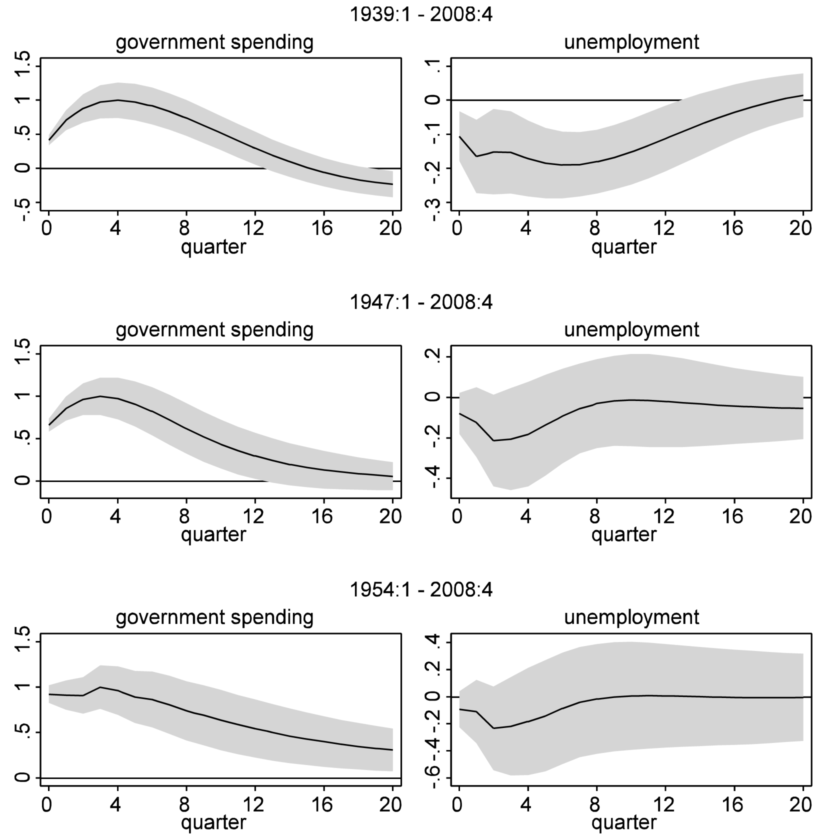

Fig. 1.12 Unemployment responses: Blanchard-Perotti SVAR

Notes: The government spending shock is identified as the shock to total government spending, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Unemployment is denoted as a percent of the civilian labor force. The standard error bands are 95 percent bands based on bootstrap standard errors.

Peters stock market variable. In this case, the unemployment rate falls by a full percentage point.

As noted previously, the Fisher-Peters experiment appears to involve a much more sustained increase in government spending. However, even comparing the ratio of the integral of unemployment to the integral of government spending over the five-year period, the Fisher-Peters specification implies a much larger effect on unemployment. In contrast, the Blanchard-Perotti SVAR implies the smallest effect.

1.4.3 The Effects of Government Spending on Employment

The bulk of evidence just presented suggests that a rise in government spending tends to lower the unemployment rate. Given the earlier discussion

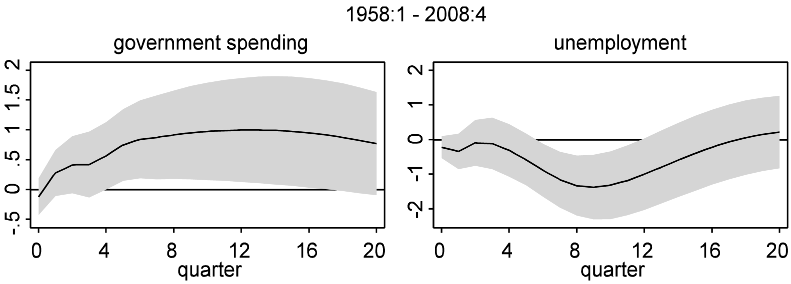

Fig. 1.13 Unemployment responses: Fisher-Peters news EVAR

Notes: The government spending shock is identified as the shock to excess stock returns of top defense contractors, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Unemployment is denoted as a percent of the civilian labor force. The standard error bands are 95 percent bands based on bootstrap standard errors.

about how much of government spending is actually compensation of government employees, it is useful to decompose the employment effects into rises in government employment versus rises in private employment.

To study this issue, I estimate the following modification of the VARs presented in the last section. In each of the VARs, I omit the unemployment variable and instead include both the log of per capita government employment and the log of per capita private employment. Government employment includes civilian government workers and armed forces employment, as well as emergency worker employment during the late 1930s and early 1940s. The four identification schemes are the same ones discussed before. In all cases, the employment numbers are converted so that they are a percent of total employment.

Figure 1.14 shows the results from the specification with my defense news variable. In the full sample from 1939:1 to 2008:4, a rise in government spending equal to 1 percent of GDP leads to a rise in government employment of close to 0.5 percent of total employment. Private employment rises by about 0.2 percent of total employment, but is never significantly different from zero at the 5 percent level. The story for the 1947:1 to 2008:4 sample is the same. For the post–Korean War sample, the estimates are even less precise (note the change in scale). It appears that private employment initially dips, then rises, but there is much uncertainty about that path.

Figure 1.15 shows the responses based on the Blanchard-Perotti SVAR. Private employment falls in the first two sample periods for this specification. In the third sample period it rises, but the standard errors bands are very wide.[2]

Figure 1.16 shows the responses based on the Fisher-Peters type of SVAR. In contrast to the previous cases, this identification and sample suggests

2. The Perotti SVAR gives very similar results. The graphs are shown in the data appendix.

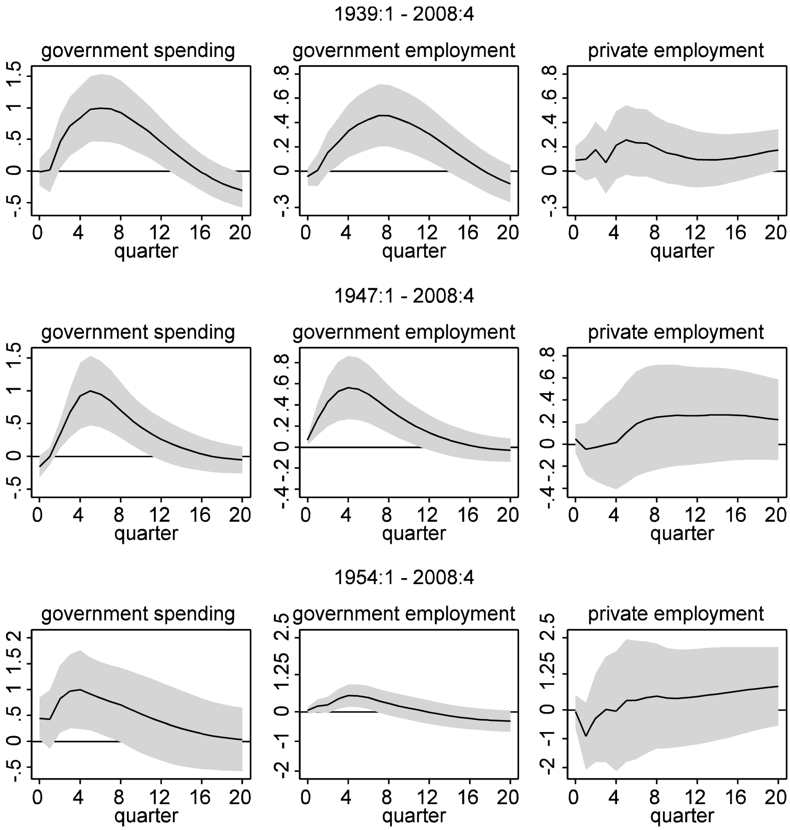

Fig. 1.14 Employment responses: Ramey news EVAR

Notes: The government spending shock is identified as the shock to the news variable, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

that increases in government spending raise both government employment and private employment. Although government spending rises steeply throughout the first six quarters after the shock, private employment does not begin to rise until after the fourth quarter. It peaks during the third year at about 1.5 percent of total employment. Since this identification scheme seems to pick up more persistent movements in government spending, it might be the case that only sustained increases in government spending raise private sector employment. More research on this issue is required.

To summarize, the EVAR using my defense news variable and the Blanchard-Perotti SVARs suggest that for the most part, increases in government spending raise government employment but not private

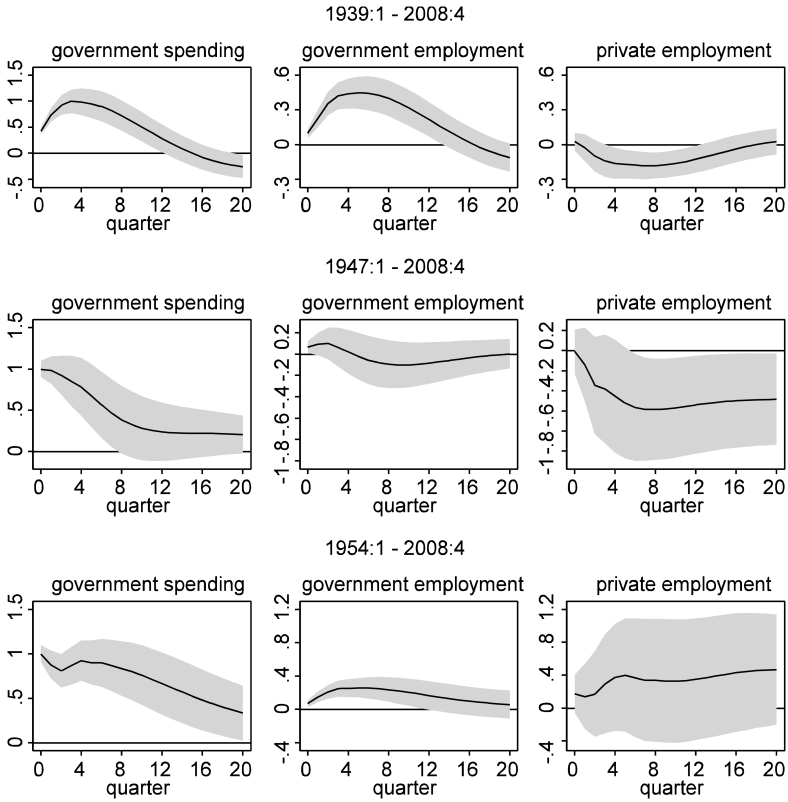

Fig. 1.15 Employment responses: Blanchard-Perotti SVAR

Notes: The government spending shock is identified as the shock to total government spending, ordered first. The shock is normalized so that government spending peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

employment. In contrast, the Fisher-Peters identification scheme suggests that government spending shocks that lead to sustained rises in government spending also raise private employment significantly, even more so than government employment. One should be mindful of the caveat discussed earlier: since exports of military goods have been an important part of profits of defense companies during some time periods, the Fisher-Peters' variable might be capturing news about exports as well as news about future US government spending. Because an increase in export demand would be expected to increase private sector employment, some of the increase might be due to this factor.

A question of interest is whether the employment effects depend on if it is government purchases of private goods or government value added that

Fig. 1.16 Employment responses: Fisher-Peters news EVAR

Notes: The government spending shock is identified as the shock to excess stock returns of top defense contractors, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

increases. As shown earlier in the chapter, both components of government spending typically increase at the same time. Because separate shocks to each cannot be identified with my one news instrument, I explore the effects of separate shocks using only the Blanchard-Perotti SVAR. In particular, I estimate a system with the (log per capita) values of real government purchases of private goods, real government value added, real GDP, government employment, private employment, the three-month Treasury bill rate, and the Barro-Redlick average marginal tax rate. The shocks are normalized so that in each case the shock is equal to 1 percent of GDP. The employment responses shown are rescaled to indicate the percentage of total employment.

Figure 1.17 shows the responses to the two types of shocks for the period 1939 to 2008 and figure 1.18 shows the responses for the period 1947 to 2008. Consider first the period from 1939 to 2008. It is clear that shocks to government purchases of private goods also raise government value added and vice versa. Thus, the data do not allow us to disentangle the separate effects. In response to both shocks, government employment rises, whereas private employment falls. The exception is the shock to government value added, which leads private employment to fall in the short-run, but then rise in the long-run, even after the government spending variable has returned to normal. In the post–World War II period, the results differ in a few ways. First, a shock to government purchases does not appear to raise government value added. Second, a shock to government value added has a positive effect on private employment, but the effect is never close to being statistically significant. Thus, separating the shocks into the two components does not paint a different picture from that presented earlier.

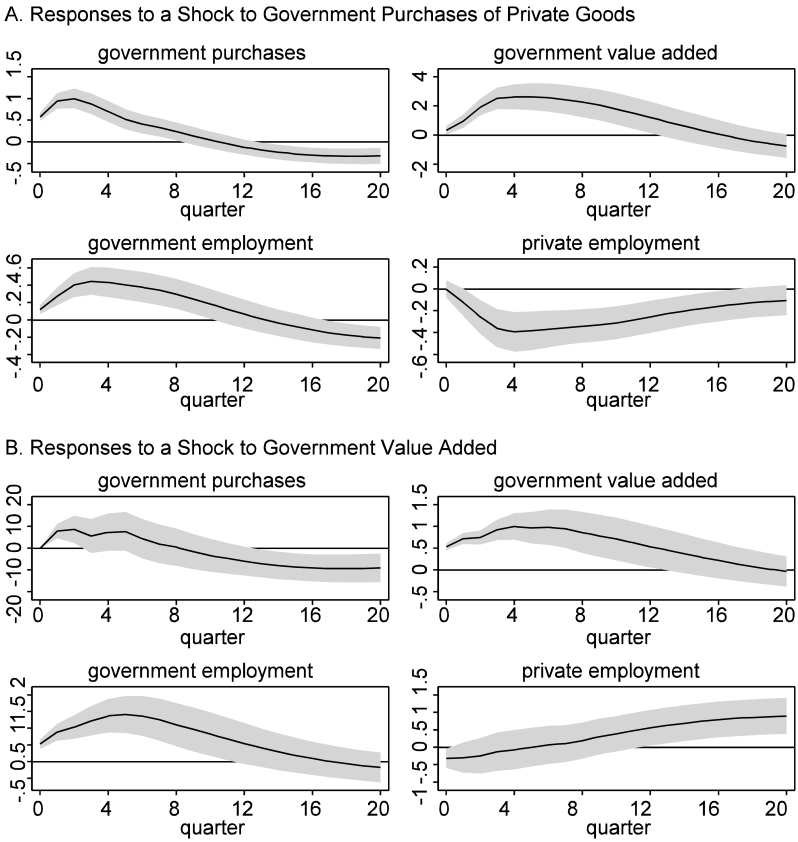

Fig. 1.17 Employment responses to shocks to components of government spending: 1939:1–2008:4

Notes: The SVAR uses the Blanchard-Perotti methodology. The government purchases shock is identified as the shock to real government purchases, ordered first. The government value added shock is identified as the shock to real government value added, ordered second. The shocks are normalized so that the government spending component peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

1.5 Conclusion

This chapter has investigated the effects of government spending on private spending, unemployment, and employment. For the most part, it appears that a rise in government spending does not stimulate private spending; most estimates suggest that it significantly lowers private spending. These results imply that the government spending multiplier is below unity. Adjusting the implied multiplier for increases in tax rates has only a small effect. The results imply a multiplier on total GDP of around 0.5.

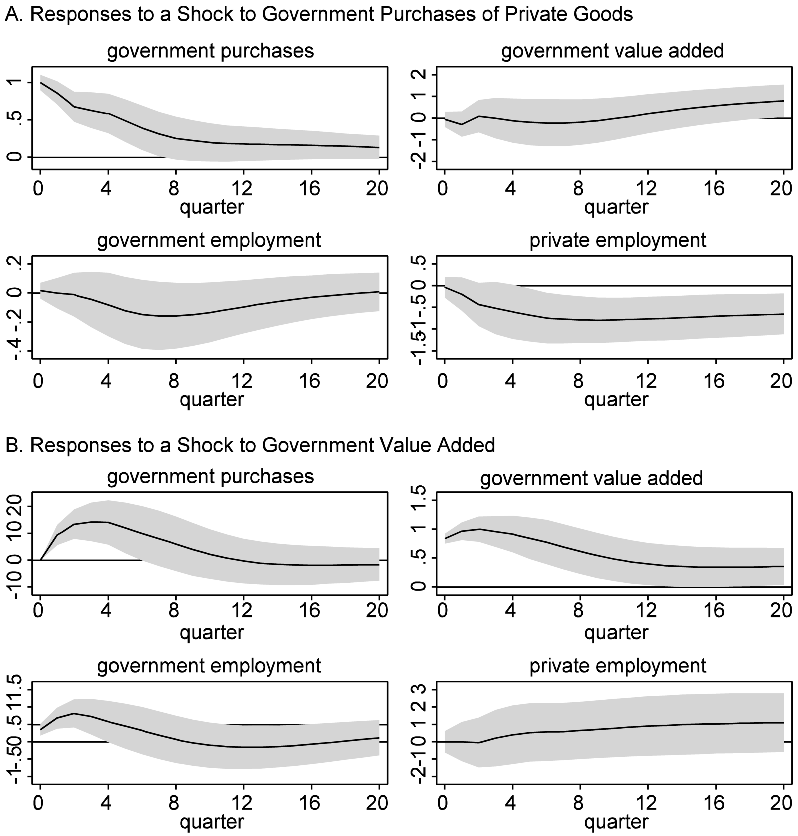

Fig. 1.18 Employment responses to shocks to components of government spending: 1947:1–2008:4

Notes: The SVAR uses the Blanchard-Perotti methodology. The government purchases shock is identified as the shock to real government purchases, ordered first. The government value added shock is identified as the shock to real government value added, ordered second. The shocks are normalized so that the government spending component peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

Increases in government spending do reduce unemployment. For all but one specification, however, it appears that all of the employment increase is from an increase in government employment, not private employment. The only exception is in the specification using the Fisher-Peters measure of defense news for the 1958 to 2008 period. This specification implies that a sustained increase in government spending has a robust positive effect on private employment. On balance, though, the results suggest that direct hiring of workers by the government may be more effective than relying on multiplier effects of government purchases.

Data Appendix

Quarterly GDP Data

The quarterly GDP data from 1939 to 1946 are the same that were constructed in my earlier work Ramey (2011c). The data from 1947 to the present are from www.bea.gov. The only difference from the earlier work is that I deflated total government and defense spending by the GDP deflator, rather than specific deflators, so that the multiplier is easier to interpret. Private spending is defined as nominal GDP less nominal government spending, and the result is deflated by the GDP deflator. All variables are converted to a per capita basis by dividing by total population, including the armed forces overseas.

Tax Data

The Barro-Redlick average marginal tax rate is from Barro and Redlick (2011). Annual values are repeated for each quarter in the year. The average tax rate is calculated by dividing current tax receipts from table 3.1, line 2, by nominal GDP.

Instruments

The defense news variable is discussed in Ramey (2011c). The excess returns on defense stocks are described in Fisher and Peters (2010). The data series were kindly provided by Jonas Fisher. The Romer-Romer exgenous tax series is the variable labeled *EXOGENRRATIO* in the Romer and Romer (2010) online data file.

Employment and Unemployment Data

The various employment and unemployment components are from monthly data and are converted to quarterly. The Conference Board data are from the 1941 to 1942 and 1945 to 1946 editions of *The Economic Almanac* published by the Conference Board (see Conference Board 1941 and Conference Board 1945.)

Civilian Employment Data. The data from 1930 through 1940 are based on employment data from the Conference Board. I seasonally adjusted these data using the default X-12 features of Eviews. I then used the twelve-month moving average of the ratio of the annual average of Weir's (1992) civilian employment series to the annual average of these series to make the monthly series match Weir's (1992) data. From 1941 to 1947, I used the monthly series published in the 1947 and 1949 *Supplement to the Survey of Current Business.* Again, I adjusted them so that the annual averages matched Weir (1992). Data from 1948 to the present are from the Bureau of Labor Statistics (BLS) Current Population Survey.

Civilian Government Employment. The series from 1930 to 1938 are interpolated from the annual series from the BLS' establishment survey. The monthly data from 1939 to the present were from the establishment survey and were downloaded from www.bls.gov.

Armed Forces Employment. The series from 1930 to 1937 was interpolated from the annual series from the 1942 *Supplement to the Survey of Current Business.* From 1938 to 1941, the series was reported monthly in the 1942 *Supplement to the Survey of Current Business.* From 1942 to 1947, the numbers from the 1947 and 1949 *Supplement to the Survey of Current Business* were spliced (using the difference in 1948:1) to the unpublished BLS quarterly employment numbers (provided by Shawn Sprague), that are available from 1948 to the present. (The 1947 and 1949 *Supplement* numbers that were spliced to match the newer BLS numbers matched the older 1942 *Supplement* numbers very closely at the overlap.)

Emergency Workers. Monthly data are from the Conference Board.

Unemployment. Monthly data from 1930 to February 1940 are from the Conference Board, and are available from the NBER Macrohistory Database. These data were rescaled to match Weir's (1992) annual series (with emergency workers added, to be consistent). Data from March 1940 through 1946 were from the Bureau of the Census. Both the Conference Board and Census data were seasonally adjusted with Eviews. (For the case of the Conference Board unemployment data, the results using the default multiplicative seasonal factors looked odd, so I used additive factors instead.)

Population. Total population was the same as used in my earlier work. The population aged 14 and older was interpolated from annual data.

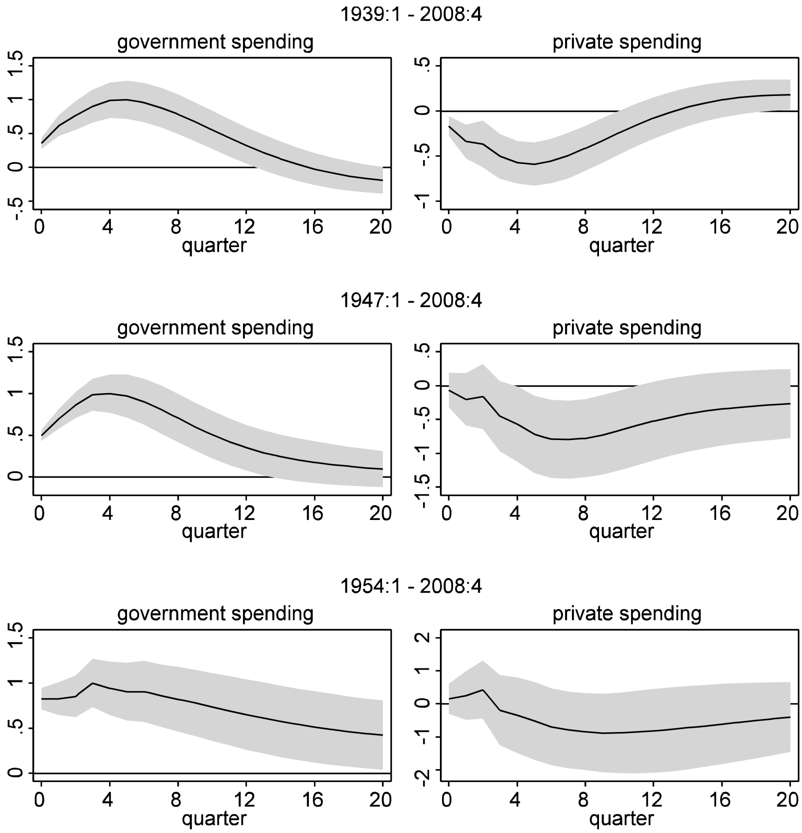

Fig. 1A.1 Private spending responses: Perotti SVAR

Notes: The government spending shock is identified as the shock to defense spending, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Private GDP is denoted as a percent of total GDP. The standard error bands are 95 percent bands based on bootstrap standard errors.

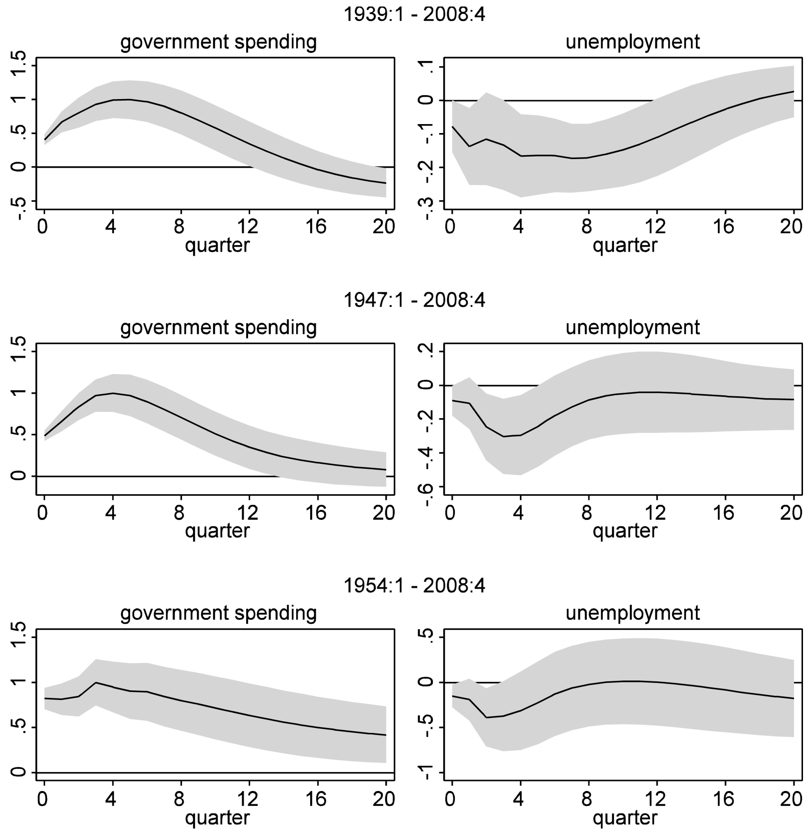

Fig. 1A.2 Unemployment responses: Perotti SVAR

Notes: The government spending shock is identified as the shock to defense spending, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Unemployment is denoted as a percent of the civilian labor force. The standard error bands are 95 percent bands based on bootstrap standard errors.

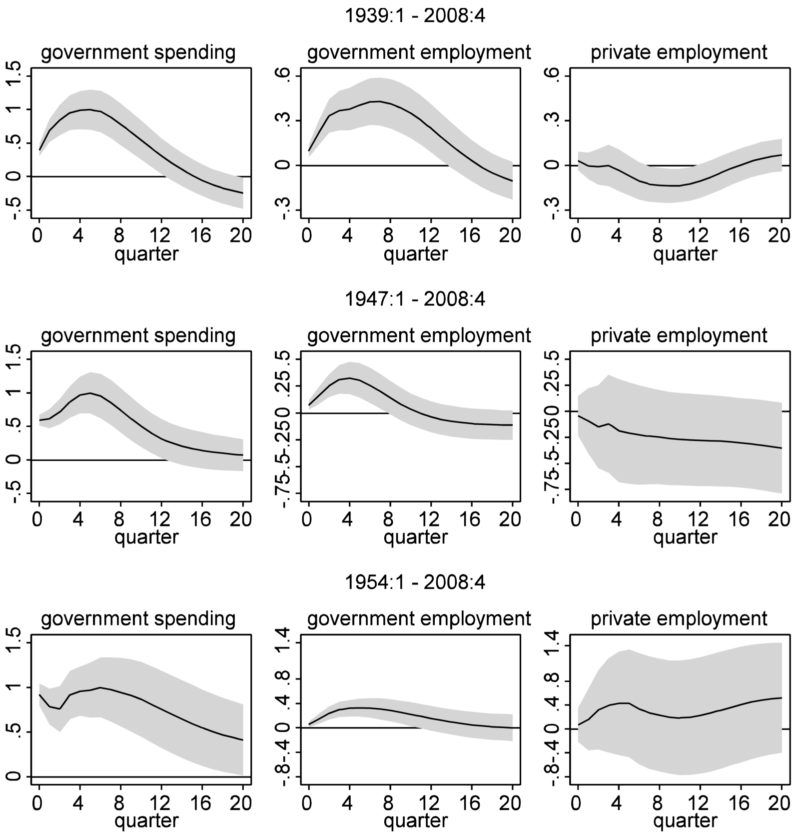

Fig. 1A.3 Employment responses: Perotti SVAR

Notes: The government spending shock is identified as the shock to defense spending, ordered first. The shock is normalized so that total government spending peaks at 1 percent of GDP. Each employment response is rescaled to represent a percentage of total employment. The standard error bands are 95 percent bands based on bootstrap standard errors.

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