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3

The Household Effects of Government Spending

Francesco Giavazzi and Michael McMahon

3.1 Introduction

This chapter provides new evidence on the effects of fiscal policy by studying (using household-level data) how households respond to a shift in government spending. Evidence based on micro data is interesting for three reasons. First, individual households' data allow us to identify how different groups (defined, for example, by their age, income, occupation, and the state of the labor market where they live) respond to the same shift in fiscal policy. For instance, Ercolani and Pavoni (2012), using Italian micro data, find that the response to shifts in government spending differs depending on the age of the head of household and on where the family lives (Northern or Southern Italy). Thus, if studies using aggregate data find that consumption does not respond to a shift in public spending, it could simply be the result of averaging across households who all respond significantly but with offsetting signs. Moreover, knowing how different groups respond to a shift in fiscal policy allows such shifts to be better designed and targeted to groups or

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areas where they might be more effective. Second, if households' responses to fiscal shocks differ depending on their characteristics, multipliers would change over time depending on the composition—for instance, by age, occupation, or geographical distribution—of the population, or by the state of the labor market as pointed out in Auerbach and Gorodnichenko (2011). Finally, to the extent that responses to fiscal shocks differ across households, aggregation bias might impair analyses that use aggregate data (such as the consumption time series from the national accounts) to study households' response to fiscal shocks. The problems raised by the aggregation bias in consumer behavior are well-known, at least since Gorman's (1953) seminal contribution.[1]

We use data from the Panel Study of Income Dynamics (PSID) of US households. Theory suggests that households could respond to a shift in fiscal policy in two ways: by changing their consumption and/or by changing their labor supply. We use the information on hours worked contained in the PSID to estimate the response labor supply to fiscal shocks. To build household consumption, which is not collected in the PSID, we use the methodology proposed by Blundell, Pistaferri, and Preston (2008a, 2008b) that combines Consumer Expenditure Survey (CEX) and PSID data. The combined data set is a panel of up to nearly 3,000 US households covering the period from 1967 to 1992.

There are lively disagreements over the effects of fiscal policy on consumption, on labor supply, and, through changes in labor supply, on real wages, the third variable we analyze. They center on theory—the very different predictions of alternative models—and on the way the empirical evidence is analyzed. Starting from theory, the sharpest difference arises between the predictions of the textbook Keynesian model and of models based upon representative agents who base their choices on optimal intertemporal decisions. The first, as is well-known, predicts that a positive spending shock raises consumption and the real wage, while the model has no predictions for hours worked. Intertemporal models give the opposite result: the negative wealth effect associated with an increase in government spending lowers consumption and (if consumption and leisure are complements) raises hours worked; this in turn lowers the real wage. The sharp difference between these results is attenuated in optimizations models that allow for nominal rigidities, or introduce consumers subject to credit constraints: the latter is one case in which the response of consumption to a spending shock can be positive despite a negative wealth effect.

On the empirical front the main issue is how the shifts in fiscal policy

1. Among many others, Constantinides (1982), Atkeson and Ogaki (1996), and Maliar and Maliar (2003) make the point that household heterogeneity collapses into parameters of the representative agent model, modifying its stochastic properties—a result extended by Lopez (2010) to the case of incomplete markets.

are identified, whether through vector autoregression (VAR) techniques or the "narrative" approach. This chapter does not take a stand on this issue but follows a third path: like Nakamura and Steinsson (2011), the shifts in government spending we analyze are variations in military contracts across states. This allows us to control for time-specific aggregate effects (such as the stance of monetary policy—common across US states—that accompanies a shift in fiscal policy) and instead measure the fiscal shock as the state-specific variation in military contracts driven by aggregate changes in US military spending. Along with Nakamura and Steinsson (2011), this is, as far as we know, the only other attempt at estimating the effects of government spending controlling for time fixed effects; that is, holding constant everything that varied over time and focusing on comparing different states in the same year.

When the effects of government spending shocks are studied, identifying such shocks within a VAR, one typically finds that a positive spending shock raises consumption, hours worked, and real wages (see, e.g., Blanchard and Perotti 2002; Mountford and Uhlig 2009; Perotti 2008; Galí, López-Salido, and Vallés 2007). In contrast, analyses that use narrative spending shocks (typically shifts in defense spending) find that while government spending raises hours, it lowers consumption and the real wage (e.g., Ramey and Shapiro 1998; Edelberg, Eichenbaum, and Fisher 1999; and Burnside, Eichenbaum, and Fisher 2004). The difference between these two sets of results could be due to the fact that narrative shocks, as mentioned before, are mostly shocks to military spending, while shocks identified within a VAR refer to overall government spending. A comparison of the effects of military and nonmilitary spending shocks, both identified with a VAR, is reported in Blanchard and Perotti (2002): they find similar multipliers in both cases, suggesting that the difference seems to be related to the way shocks are identified. Event studies such as Giavazzi and Pagano's (1990) analysis of fiscal consolidations in two European countries and Cullen and Fishback's (2006) analysis of World War II spending on local retail sales in the United States generally show a negative effect of government spending on private consumption. Hall's (1986) analysis using annual data back to 1920 and also identifying government spending shocks through shifts in military spending, finds a slightly negative effect of government purchases on consumption.

The main advantage of our identification strategy—namely, as already mentioned, that it allows us to use time fixed effects and thus control for time-specific aggregate effects such as the stance of monetary policy—comes at the cost of limiting the interpretation of our results. If households expect that the Federal government will satisfy its intertemporal budget constraint by raising taxes on all US households, independently of where they live and other characteristics, the negative wealth effect associated with the increase in spending will be the same for all households and therefore it will be absorbed in the time fixed effect. This means that while we are able to estimate the *direct* effect of spending shocks on consumption, hours worked, and real wages, we may not be capturing the *indirect* effect arising from the reduction in wealth associated with the expectation of higher taxes in the future. As we shall discuss, this problem would be compounded if the negative wealth effect associated with higher government spending were to differ across households—for instance, if higher income households were expected to pay a larger fraction of the future taxes than lower-income households.

In a textbook Keynesian framework there are no wealth effects: thus, within such a framework, what we estimate is indeed the multiplier of shifts in government spending. But if wealth effects are important, what we estimate is the multiplier net of the wealth effect that is captured in the fixed effect. In the extreme case in which government spending is pure waste, the effect we estimate (shutting down the wealth channel) should be exactly zero. Thus the finding of a positive response of consumption to these spending shocks is uninformative on the size of the multiplier because the wealth effect could turn that positive response into a negative one. But the finding—which we do estimate for some groups—of a negative response of consumption indicates that the multiplier is unambiguously negative. The same holds for the response of hours worked: when we find that labor supply increases following a spending shock—as we also do for some groups—we can unambiguously conclude that spending shocks raise hours worked, since the wealth effect works in the same direction.[2]

We find evidence of significant heterogeneity in our estimates of households' responses to positive spending shocks. For instance, lower-income households and households where the head works relatively few hours per week tend to cut consumption: since these estimates shut down the wealth effect, the cut in consumption is unambiguous. Instead, households with relatively higher income and households where the head has a full-time job tend to increase consumption—a result that in this case could be turned around by the presence of a wealth effect. Heads who on average work relatively few hours respond to the spending shock by immediately increasing their hours while those working full time do not adjust hours for many years after the shock. Once again, since the wealth effect goes in the same direction, we can unambiguously conclude that the labor supply response of these groups to a spending shock is positive. We also find significant differences in the effect of military spending shocks across states, depending on the state-specific unemployment rate. In states with relatively low unemployment, spending shocks have insignificant effects on consumption, suggesting that once you allow for wealth effects the multiplier could be negative. On the contrary, we estimate a positive response of consumption in high-unemployment states,

2. An alternative way to interpret our results is to think of them as the multiplier associated with an exogenous shift in export demand, as shocks to exports imply no wealth effect.

suggesting that the multiplier could be positive for a small enough wealth effect.

Our estimates suggest that the effects of a shift in government spending might vary over time depending, among other factors, on the state of business cycle and, at a lower frequency, on the composition of employment—for instance, the share of workers on part-time jobs. Shifts in spending could also have important distributional effects that are lost when estimating an aggregate multiplier. Aggregate fiscal multipliers conceal this wealth of information on the effects of shifts in fiscal policy; they also hamper the design of fiscal policies that are appropriate given the state of the business cycle. Finally, the more diverse are the effects of a fiscal impulse across different groups in the population, the more likely is the possibility that an economy-wide multiplier suffers from an aggregation bias (see, e.g., Stoker 2008).

The risks of relying on a single multiplier have recently been emphasized in the literature. Auerbach and Gorodnichenko (2011), using regime-switching models, find large differences in the size of spending multipliers in recessions and expansions, with fiscal policy being considerably more effective in recessions than in expansions. Favero, Giavazzi, and Perego (2011) compare fiscal multipliers across countries and find that they differ depending on the country's degree of openness to international trade, its debt dynamics, and its local fiscal reaction function. Interestingly, such differences concern not only the size of the multiplier, but sometimes also its sign.

We begin section 3.2 by describing our data. Section 3.3 discusses how the fiscal shocks we analyze are identified. Our results are presented in sections 3.4 and 3.5. Section 3.6 concludes.

3.2 Combining Household and State Data

We first detail the data that we use. We discuss the household-level data and in particular the approach to construct consumption data. We then explore the state-level data, especially the military procurement that provides the basis for our fiscal shocks instrument.

3.2.1 Constructing the Data for Individual Consumption, Hours, and Real Wages

In order to construct the panel of individual household data on consumption, we follow the approach of Blundell, Pistaferri, and Preston (2008a). The primary source of data is the PSID, a long-running (since 1968) panel series that includes a large number of socioeconomic characteristics of US households. These include data on income, hours worked,[3] wealth, and taxes, as well as other household characteristics such as family size and levels of

3. The 1983 questionnaire asks, "*How many weeks did you work in your main job in 1982? And, on the average, how many hours a week did you work on your main job in 1982?*"

education. However, it does not include data on total household consumption; instead there are measures of household expenditure on food.[4]

The CEX, collected by the Bureau of Labor Statistics (BLS), provides high-quality information on the purchasing habits of US consumers. While these data include numerous household characteristics, they are not collected in the form of a panel; specifically, different households respond in each year of the survey. Nonetheless, Blundell, Pistaferri, and Preston (2008a) impute estimates of both aggregate consumption as well as consumption of nondurables in the PSID using information from the CEX.

Their approach is detailed in their paper and in an unpublished appendix (Blundell, Pistaferri, and Preston 2008b): here we outline their imputation procedure. They estimate a demand function for food consumption (a variable that is available both in the PSID and CEX surveys but was not collected in the 1988 and 1989 surveys) using a total consumption variable (such as nondurable consumption expenditure),[5] a variety of household characteristics, and the relative prices of food and other types of consumption as regressors. They allow this function to have time- and characteristic-varying budget elasticities,[6] and they allow for measurement error in the total consumption variable by instrumenting it with cohort, year, and education-level demeaned hourly wages for the husband and wife. They then invert this consistently estimated demand function to derive the imputed PSID consumption measures.

Before we can make use of these data, they need to be carefully combined and merged to ensure that the timing of the PSID data matches the fiscal data that we discuss later. In particular, the questions used to construct the hours and income variables are retrospective: in the 1983 survey, the household is asked to report their working hours and income for 1982. With this in mind, and as shown in figure 3.1, the responses to the questions reported by the household during their interview in 1983 are recorded as head of household i's income earned and hours worked in 1982; these are denoted $y_{i,82}$ and $h_{i,82}$.

The questions referring to food expenditure, described in note 3, are much less clear in terms of their timing. The questions ask about food expenditure in an average week and we follow Blundell, Pistaferri, and Preston (2008a)

4. Again, using 1983 as a typical year, the question asked is, *"In addition to what you buy with food stamps, how much do you (or anyone else in your family) spend on food that you use at home? How much do you spend on that food in an average week? Do you have any food delivered to the door which isn't included in that? How much do you spend on that food? About how much do you (and everyone else in your family) spend eating out not counting meals at work or at school?"*

5. Nondurable consumption is defined as food; alcohol, tobacco, and expenditure on other nondurable goods, such as services, heating fuel, public and private transport (including gasoline), personal care, and semidurables, defined as clothing and footwear. It excludes housing (furniture, appliances, etc.), health, and education.

6. The budget elasticity is the elasticity of the food expenditure measure to the aggregate spending measure.

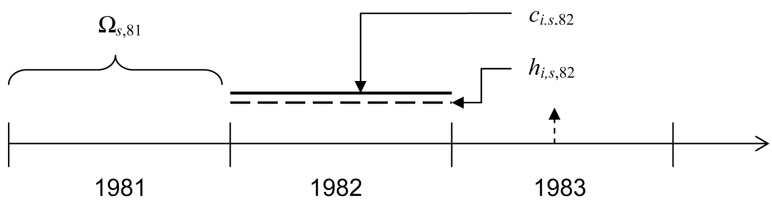


Fig. 3.1 A sample timeline of our data

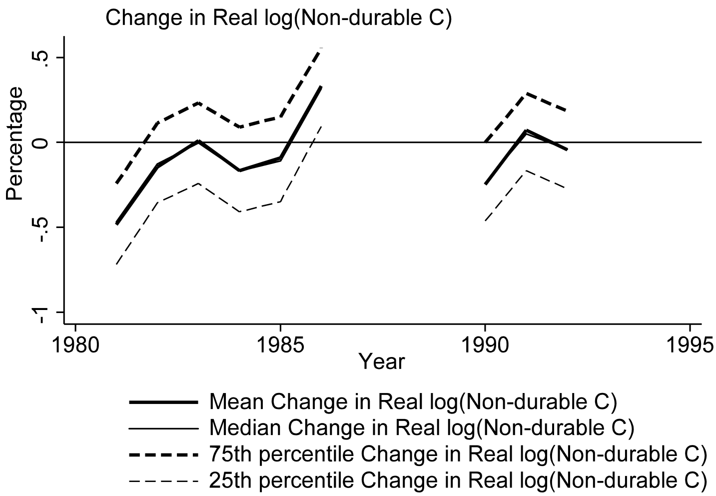


Fig. 3.2 The distribution of imputed household nondurable consumption growth

in assuming that this too refers to the previous calendar year. The imputed consumption variable, $c_{i,82}$, is therefore also the value from the 1983 survey.

Figure 3.2 shows a number of measures of the distribution of the (log growth) of the imputed nondurable consumption variable. We report the mean, median, 25th percentile, and 75th percentile for the cross-section in each year. As just discussed, the absence of the food expenditure variable for the years 1987 and 1988 (1988 and 1989 surveys) means that we lose the observations from those years. Additionally, the need to calculate a growth rate means we lose two further year's worth of observations: we lose the first year of data, as well as 1989 (the first year after the two-year break).

Figure 3.3 reports analogous statistics for the annual hours worked by the head of household. Three points are worth noting: (1) these data are continuous between 1967 and 1992 as the question was asked in each year of the PSID survey;[7] (2) the mean is below the median; (3) the median head

7. The survey started in 1968 but our retrospective treatment of the responses gives us data from 1967.

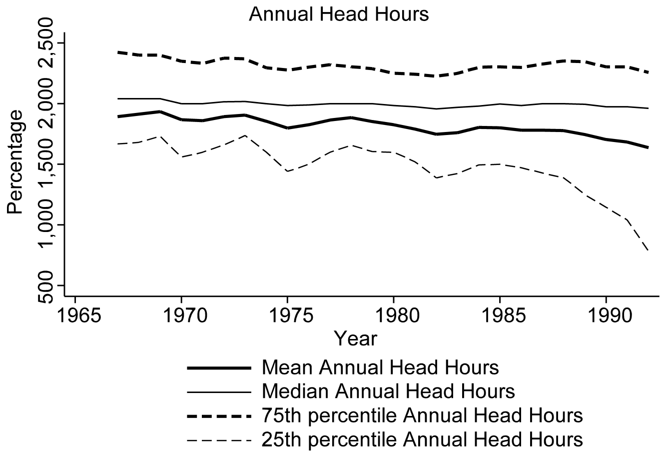


Fig. 3.3 The distribution of hours worked by head of household

of household works full time with about 2,000 hours per year (or nearly forty-two hours per week, based on forty-eight weeks of work), but there is a downside skew to the distribution caused by part-time and low-hours workers, as well those who do not work.

In order to explore the response of real wages, we take the real labor income of the head of household and divide it by annual hours. This gives us a measure of real labor income per hour worked, which we use as our measure of the real hourly wage. As with the hours data, this variable is available between 1967 and 1992. Overall, the sample contains between 1,500 households—for the early years in which we have only hours and real wage data—and nearly 3,000 households through the 1980s, when data for consumption can also be constructed. The time series of the number of observations per year, split between the hours and consumption variables, are displayed in figure 3.4. The main consumption regressions use 24,348 observations, while the hours and real wages regressions make use of 58,428 observations.

2.2.2 State-Level Data

In order to measure state-level fiscal shocks, we follow Nakamura and Steinsson (2011) and use state-level military spending data, which comes from the US Department of Defense's electronic database of military procurement (as reported in the DD-350 forms). They compiled these data for each state and year between 1966 and 2006. The spending covers all military purchases with value greater than \$10,000 (from 1966 to 1983) and greater than \$25,000 (1983 to 2006), and the form specifies the prime contractor as well as the location where the majority of the work was

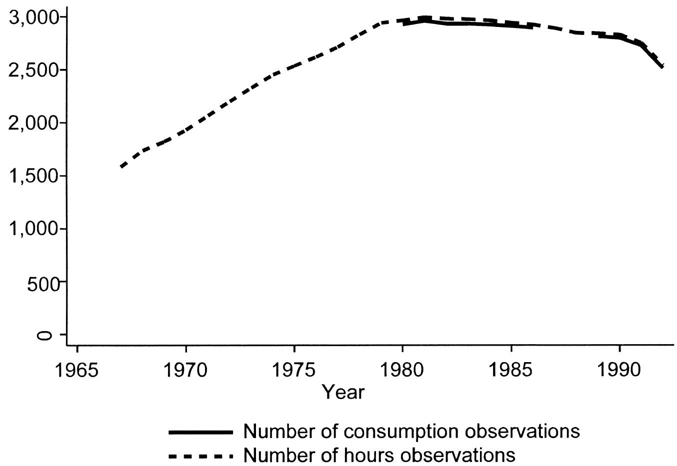


Fig. 3.4 The number of households with hours and consumption data

completed.[8] The DD-350 measure of government military spending in each state is denoted $G_{s,t}$, and it forms the basis of our fiscal policy instrument.

The macroeconomic literature generally agrees that aggregate military spending is exogenous to the economic decisions of US households and to the US business cycle (e.g., Ramey and Shapiro 1998). As such, a natural measure of the fiscal shock occurring in state s at time t, and resulting from changes in military spending in that state, is the percentage change in state military spending normalized by state GDP:

$$\Omega_{s,t} \equiv \frac{\Delta G_{s,t}}{Y_{s,t}}. \tag{1}$$

In the next section we discuss issues related to the potential endogeneity of this variable.

We use Gross State Product (GSP) compiled by the US Bureau of Economic Analysis (BEA) as the measure of state output ($Y_{s,t}$) used to normalize the level of fiscal spending. To convert this and other variables to per capita values we use US Census Bureau state population data. Nominal variables are converted into real series using the state-level Consumer Price Index (CPI) data computed by Del Negro (2002) and constructed aggregating a

8. Nakamura and Steinsson (2011) deal with the potential concern that these data are mismeasured due to interstate subcontracting using a newly-digitized data set from the US Census Bureau's Annual Survey of Shipments by Defense-Oriented Industries. This is an alternative measure of state-level shipments from defense industries to the government. Though the alternative series only runs up to 1983, the two series are very closely correlated over the coincident time periods, suggesting that cross-border subcontracting plays little role in the $G_{s,t}$ variable.

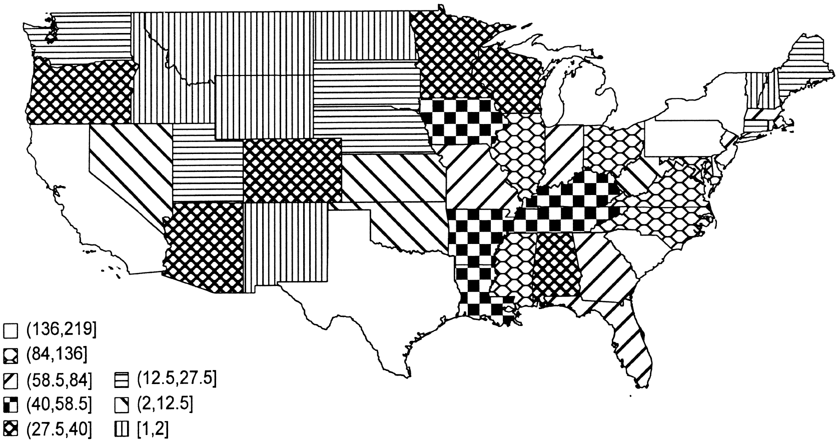


Fig. 3.5 The average number of households surveyed in each state per year

number of sources of state-level prices and costs of living. As these state-level data do not include CPI for the District of Columbia (DC), we assume that the price level there follows that of the overall United States in order to deflate nominal data from DC.

In terms of states, we use data from all fifty states as well as the District of Columbia. Of course, PSID sampling means that some states have much fewer households in each year. Figure 3.5 shows the median number of households per year in each state; to calculate this, we first calculate the total number of households in each state in each year and then calculate the median for each state. In figure 3.5 we show only the contiguous United States; this is simply to ensure that the map is easier to read. The median number of households per year is 4.5 in Alaska and 2.5 in Hawaii.

3.3 Econometric Identification of the Effects of Fiscal Shocks

The main advantage over aggregate studies of our use of state-level fiscal shocks is that we are able to control for those time effects that are common across states. Unfortunately this does not guarantee that we do not have endogeneity concerns: the variation in fiscal spending may not be completely random across states even if aggregate military spending is. Consider the possible factors that can drive the behavior of, for example, the change in hours of a head of household i who lives in state s at time t ($\Delta h_{i,s,t}$). As shown in equation (2), the movement of ($\Delta h_{i,s,t}$) will partly reflect factors that are common to all households at time t (for example, changes in monetary policy that affect the entire United States), factors common to all residents of state s (e.g., cross-state differences in working regulations), and then the idiosyncratic part related to household i. The latter two effects can be split into those effects that are time-invariant (such as the fact that certain people always work more hours than others) and those that are time-varying.

(2) $$\Delta h_{i,s,t} = \overbrace{\delta_t}^{\text{Time } t \text{ Effects}} + \overbrace{\bar{\gamma}_s + \tilde{\gamma}_{s,t}}^{\text{State } s \text{ Effects}} + \overbrace{\bar{\alpha}_i + \tilde{\alpha}_{i,t}}^{\text{Household } i \text{ Effects}} .$$

In our analysis, we are interested in the effect of changes in state-level military spending, $\Omega_{s,t}$, on the behavior of households in those states. Our baseline equation, which we estimate for the three main dependent variables of interest (consumption, hours, and real wages) is:

$$\Delta z_{i,s,t} = \alpha_i + \gamma_s + \delta_t + \sum_{k=0}^{K} \beta_k \Omega_{s,t-k} + \phi X_{i,s,t} + \varepsilon_{i,s,t}$$

where z_{it} is (log) of household's i consumption/hours/real wages at time t, $\Omega_{s,t-k}$ is the k period lag of government military procurements from supplier companies located in state s in period t expressed as a percentage of state output, and X_{it} is a vector of control characteristics such as whether the head of household is employed or retired. Variables α_i, γ_s, and δ_t are, respectively, household, state, and time fixed-effects.[9]

In order to analyze the effects of shifts in fiscal policy, the fiscal shocks should be exogenous and so uncorrelated with the error term. Relating this regression equation to (2), and assuming that no controls and only the contemporaneous shock ($k = 0$) are included, the estimated equation is:

$$\Delta z_{i,s,t} = \delta_t + \bar{\gamma}_s + \bar{\alpha}_i + \beta_0 \Omega_{s,t} + \overbrace{\tilde{\gamma}_{s,t} + \tilde{\alpha}_{i,t}}^{\varepsilon_{i,s,t}}.$$

The key for unbiased estimates of the β_0 coefficient is that $\Omega_{s,t}$ is uncorrelated with $\varepsilon_{i,s,t}$, which incorporates state-time fixed effects that are not controlled for elsewhere. This may not be the case if the amount of state-level military spending is related to the state economic cycle. Even though aggregate military spending has been shown to be exogenous, we may still worry that the allocation of this spending across states is correlated with the state cycle; in other words, spending associated with an exogenous military build-up is directed toward those states with weaker local conditions following lobbying and the resulting political decision.[10] Therefore, like Nakamura and Steinsson (2011), we build state-level fiscal spending shocks instrumenting $\Omega_{s,t}$. Specifically, we shall use the same logic that Nekarda and Ramey (2011) applied to industry shares. The share that state s receives of overall military spending in year t is $\eta_{s,t} = G_{s,t}/G_t$ so that:

(3) $$G_{s,t} = \eta_{s,t} G_t$$

9. Standard errors are clustered by household in all the household-level regressions.

10. For example, Mayer (1992) finds strong evidence of political business cycles in the distribution of military contracts, but suggests there is little evidence of the use of military contract awards for economic stimulus after 1965.

$$\Rightarrow \dot{G}_{s,t} = \eta_{s,t}\dot{G}_t + \dot{\eta}_{s,t}G_t \tag{4}$$

$$\frac{\dot{G}_{s,t}}{Y_{s,t}} = \frac{\eta_{s,t}\dot{G}_t}{Y_{s,t}} + \frac{\dot{\eta}_{s,t}G_t}{Y_{s,t}} \tag{5}$$

$$\Rightarrow \Omega_{s,t} \approx \frac{\eta_{s,t}\Delta G_t}{Y_{s,t}} + \underbrace{\frac{\Delta\eta_{s,t}G_t}{Y_{s,t}}}_{\text{Endogenous?}}. \tag{6}$$

Equation (6) shows that the overall change in military spending in state s in year t can be split between the fact that aggregate spending has changed and a share of this goes to state s, and the fact that the share of aggregate spending going to state s has changed. If our worry is that states in which there are weaker economic conditions increase their share more ($\Delta\eta_{s,t} > 0$), then the second term on the right-hand-side equation (6) is potentially endogenous. Of course, some of $\Delta\eta_{s,t}$ may be exogenous variation and so excluding it we potentially reduce the variability in our shocks, which would lead to less tight standard errors. However, given that using an endogenous regressor will bias our estimates, we choose to purge the shocks of this potential correlation with the residual at the expense of potentially less precise estimates of effects of fiscal shocks. Doing this, we concentrate on the first term on the right-hand side of (6), which can be rewritten as:

$$\frac{\eta_{s,t}\Delta G_t}{Y_{s,t}} = \frac{\Delta G_t}{G_t}\frac{G_{s,t}}{Y_{s,t}}.$$

As a result of the GSP term in the denominator of $G_{s,t}/Y_{s,t}$, $(\eta_{s,t}\Delta G_t)/Y_{s,t}$ is likely to be correlated with the state business cycle even if $G_{s,t}$ and $\Delta G_t/G_t$ are exogenous. We thus need instrument fiscal shocks using, rather than $\Omega_{s,t}$,

$$\widehat{\Omega_{s,t}}^R = \Delta\ln(G_t)\overline{\theta}_s, \tag{7}$$

where $\overline{\theta}_s$ is the time-average of the share of military spending in total output $(G_{s,t}/Y_{s,t})$ falling on state s.

Figure 3.6 shows, for four states, the raw shocks ($\Omega_{s,t}$) calculated according to equation (1) as well as the instrumented shocks ($\widehat{\Omega_{s,t}}^R$) as defined in (7). These data show, particularly in the case of Louisiana (top right frame), how the approach removes potential measurement error. The large spike up and then down in Louisiana in 1981 and 1982 is smoothed through when we use the instrumented approach. This noise seems to be less of an issue in some of the other states displayed. Comparing California (top left) to Wisconsin (bottom right) and New York (bottom left), it is clear that some states see much greater swings in the shock variable. In California the instrumented shocks are on average 0.14 percent of GSP and are as large (small) as 0.93 percent (–0.66 percent); in Wisconsin the mean is only .04 percent and the largest (smallest) shock was 0.25 percent (–0.18 percent) of GSP.

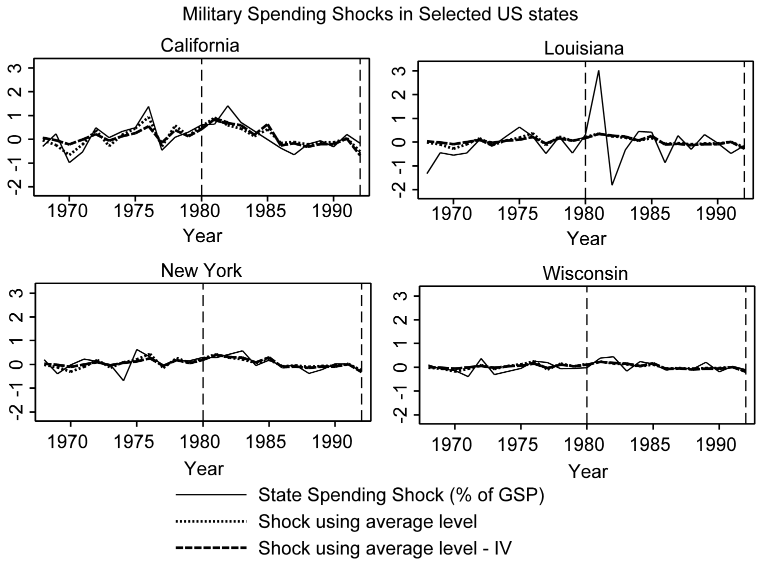


Fig. 3.6 State fiscal shocks in a selection of US states

Of course, figure 3.6 shows only a small sample of the states we use. To show the difference in variability across states in the main shock that we use, figure 3.7 shows the heat map (as in figure 3.5) of the interquartile range of $\widehat{\Omega_{s,t}}^{R}$;[11] California (0.7) is indeed one of the states with larger swings in military contracts. The most volatile are Missouri (1.0) and Connecticut (1.3). As before, we only show the contiguous United States; the interquartile range is 0.4 in Alaska and 0.5 in Hawaii.

As an alternative instrument, we also consider using Ramey's (2011) measure of defense news to instrument for aggregate US military spending. Specifically, we regress $\Delta \ln(G_t)$ on an annual sum of the news measure and generate $\widehat{\Delta \ln(G_t)}$ as the fitted value. We then create an alternative measure of our state-level shocks by applying the formula:

$$\widehat{\Omega_{s,t}}^{IV} = \widehat{\Delta \ln(G_t)}\overline{\theta}_s. \tag{8}$$

This gives a very similar pattern as shown in figure 3.6; the correlation between the two shock series is over 0.9 across all time periods and states. In appendix A, we show that the main results are robust to using this alternative measure of fiscal shock.

11. As variability in $\widehat{\Omega_{s,t}}^{R}$ is driven by the aggregate growth in military spending, this map captures differences in average military intensity across states ($\overline{\theta}_s$).

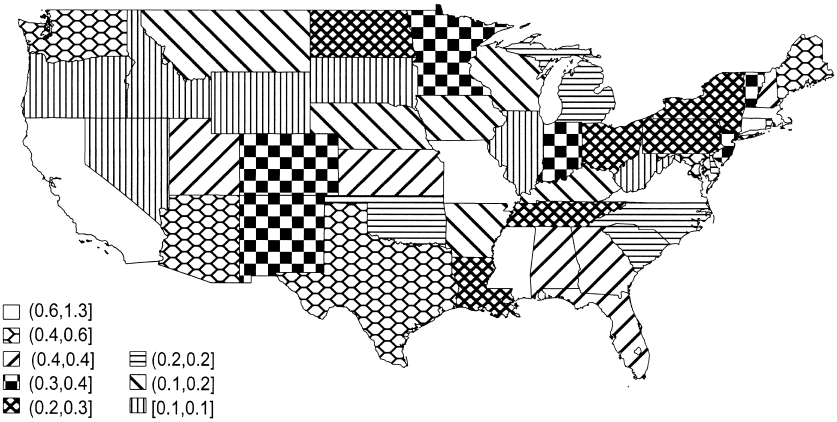


Fig. 3.7 The interquartile range of $\widehat{\Omega_{s,t}}^R$ by state

3.3.1 Household Heterogeneity

As mentioned before, an advantage of household data is that we can explore heterogeneity amongst households. Consider a simple dummy variable $D(A)_{i,s,t}$, which is 1 when the characteristic A applies to the head of household i in state s at time t. With this separation of households, we interact a particular set of household characteristics with the shock variables. The estimated regression is:[12]

$$\Delta z_{i,s,t} = \alpha_i + \gamma_s + \delta_t + \sum_{k=0}^{K} \beta_k \Omega_{s,t-k} + \sum_{k=0}^{K} \psi_k (D(A)_{i,s,t} \times \Omega_{s,t-k}) + \sigma D(A)_{i,s,t}$$

$$+ \phi X_{i,s,t} + \varepsilon_{i,s,t}.$$

In the remainder of the chapter we follow Romer and Romer (2010), who examine the effects of tax changes on the US economy, and choose a lag length that corresponds to three years ($K = 3$).

3.4 Results

Before describing our results it is useful to briefly summarize the predictions of a few models. In the (static) Investment-Saving/Liquidity Preference-Money supply (IS/LM) model an increase in government spending has no wealth effect and acts like a pure demand shock: because output is demand determined and prices do not respond, consumption increases, labor demand increases (although the model does not distinguish between

12. Where the characteristic is split into more than two groupings—for example, splitting the household into young, middle-aged, and older—we can use a similar but extended regression approach.

Table 3.1 **Effects of a positive spending shock in alternative models**

	Consumption	Labor supply	Real wages
Keynesian IS/LM model	+		+
Dynamic representative agent models	–	+	–
With nominal rigidities	–	+	+
With credit constrained consumers	+	+	+

an intensive and an extensive margin and thus has no predictions about the intensive margin), and so does the real wage.

Models based on a representative agent who makes optimal intertemporal decisions give the opposite result: the negative wealth effect associated with an increase in government spending lowers consumption and raises hours worked; this in turn lowers the real wage. The sharp difference between the results of the IS/LM and the intertemporal optimization models are attenuated in intertemporal models that allow for nominal rigidities, or introduce consumers subject to credit constraints: in the latter the response of consumption to a spending shock can be positive.[13] Table 3.1 summarizes these theoretical results.

When estimating the effects of a shift in fiscal policy one has two ambitions: (1) to control for anything that might have varied while fiscal policy was changing, so as to separate out the effects of other factors, such as shifts in monetary policy or the business cycle; (2) to construct an estimate of the total change in consumption (or hours worked, or the real wage) associated with the shift in fiscal policy. This will be the sum of the *direct* effect of the shift in fiscal policy, plus the indirect effect possibly arising from the change in wealth associated with the policy shift. In this chapter we achieve the first objective using time fixed effects and comparing the effects of shifts in government spending across different states in the same year. This, however, comes at the cost of shutting down the wealth channel—to the extent that one exists—that is, of overlooking any wealth effect associated with the shift in government spending. What we potentially estimate is simply the direct effect of the shift in government spending (i.e., excluding the wealth effect).

However, since we are interested in comparing the response of different households, we potentially run into an additional problem: the possibility that the wealth effect differs across households depending on their characteristics. For example, higher-income households might expect to pay a larger fraction of the future taxes than lower-income households. To understand what we estimate, the following might be useful.

13. See Leeper, Traum, and Walker (2011) for a detailed analysis of the multiplier implied by different models. The accompanying monetary policy obviously makes a difference, but remember that here we control for monetary policy that is the same across US states.

Assume the total wealth effect of the fiscal spending shock is, for a household belonging to group i, $\bar{w} + w^i$. That is, the wealth effect is comprised of two components: the average wealth effect, $\bar{w} < 0$, plus the specific wealth effect, which varies by household characteristic. Overall, the wealth effect should be nonpositive for both groups (which means that the average effect $\bar{w}$ is nonpositive and also that $\bar{w} + w^i$ is nonpositive), but—for instance, if taxes are progressive—the rich could expect to have a larger negative wealth effect than the poor. In this case, their specific wealth effect w^R (which measures the effect relative to the average), would be negative, while for the poor the specific effect would be positive, $w^P > 0$.

The response of interest, for testing between models and calculating multipliers, is the total effect

$$\frac{dC_i}{dg} = x_i + \bar{w} + w^i.$$

However, our estimation procedure controls for time fixed effects which, as we said, capture common factors such as the US business cycle and Federal Reserve policy stance, but also any common negative wealth effect that comes from the expected change in Federal taxes as a result of the spending shock. Therefore, we estimate:

- For the rich: $\beta^R = x^R + w^R$
- For the poor: $\beta^P = x^P + w^P$

Given that $\bar{w} < 0$, our estimate of the total effect is upward biased for both groups. If, however, we were interested in the direct effect, x^i, then if $w^R = w^P$ (i.e., if the two groups shared the same wealth effect), then our estimate of the direct effect would be unbiased. But it would not if instead $w^R < 0$, $w^P > 0$. In this case

- For the rich: $\beta^R < x^R$
- For the poor: $\beta^P > x^P$

In other words, if there are specific wealth effects as described earlier, these will cause us to overstate the x^P and understate x^R.

There are a few cases in which our results provide an upward bound for the total effect that is consistent with the intertemporal model. For instance, when, for consumption, we estimate $\beta^P < 0$ (that is, a negative response of consumption to the spending shocks—which we do for some groups, e.g., the relatively poor and part-time workers) our results are consistent with the intertemporal model because, for this group, our estimate of the direct effect is upward biased and $\bar{w} < 0$.

For hours, the analysis is similar except that the average wealth effect ($\bar{w} \geq 0$) and the specific wealth effects (relative to the average) under the progressive tax system described before, would be positive for the rich ($w^R \geq 0$) and negative for the poor ($w^P \leq 0$). Overall, the wealth effect should be

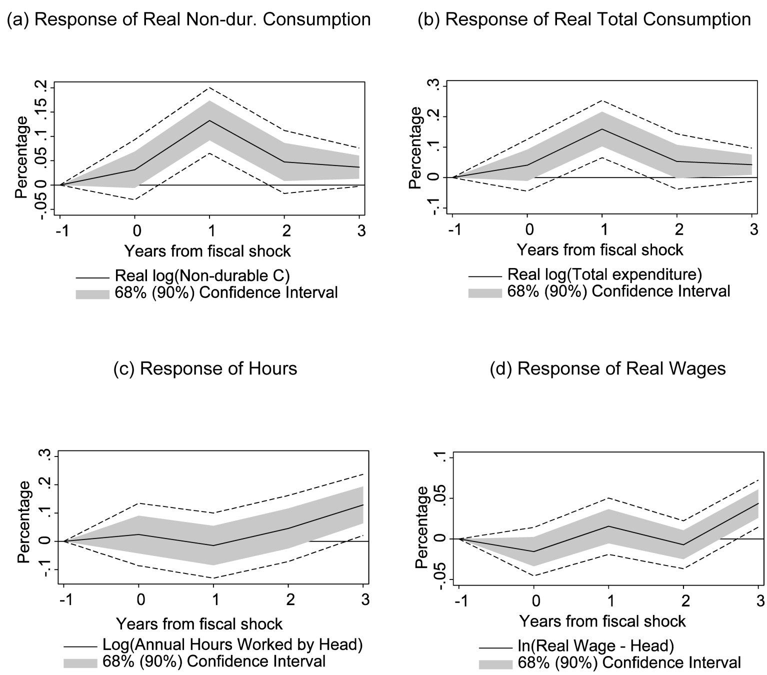


Fig. 3.8 IRFs to a 1 percent GDP state spending shock: The average response

nonnegative for both groups (they respond to the negative wealth effect by consuming less leisure) and, as aforementioned, the specific wealth effects reinforce the average wealth effect for the rich.

In this case, using similar logic, our estimates are downward biased estimates of the total effect for both groups, but we overstate the direct effect on hours and understate the direct effect on the poor. Where we estimate a positive response of hours for the rich ($\beta^R > 0$), we cannot conclude that the direct effect is positive, but we can state that the total effect is positive—since $\beta^R = x^R + w^R \geq x^R (w^R \geq 0$ and $\bar{w} \geq 0)$.

For the poor, where we find a negative ($\beta^P < 0$), as we do in the initial response, since $\beta^P = x^P + w^P \leq x^P (w^P \leq 0)$, we cannot conclude that the direct effect is negative nor can we conclude that the total effect is negative, as that depends on whether $|\bar{w}| > |\beta_P|$. If we estimate a positive effect ($\beta^P > 0$), as we do in the later years of the response, we can conclude that both the total effect and direct effect is positive.

We now illustrate our empirical findings. When we aggregate all households (figure 3.8) we find that following the increase in military spending, consumption increases right after the shock and remains higher for about two years; this is true for both durables and total consumption, which includes nondurables and services. (Given that the two categories of consumption seem to respond very similarly, in the rest of the chapter we only look at total consumption.) Hours worked and real wages initially do not move, although both increase significantly three years after the increase in spending: the long lag could be the result of off-setting positions by heterogeneous groups in the economy. Our estimates of the labor supply response focus on the intensive margin: longer hours by employed workers (we control for employment status in the regressions).[14] In section 3.5 we return to the issue of the extensive margin. The magnitude of these lagged effects is small. Since our shocks are equivalent to 1 percent of GDP, a point estimate of 0.16 for the percent change in aggregate consumption after the first year suggests that consumption increases by less than one-fifth, which is similar to the year-three response of hours, but four times as large as the percent change in real wages (0.04). In appendix A, we show that these results are unchanged if we use the alternative measure of the fiscal shock given by $\widehat{\Omega}_{s,t}^{IV}$ in equation (8).

As mentioned before, the evidence of a positive response of consumption is inconclusive, since it could be canceled or turned around by the presence of a wealth effect that our estimates capture in the time fixed effects.

As we mentioned, our data allow us to split the sample along a very large number of dimensions, although along some of them the resulting subsample included too few individuals. For instance, looking at splits based on the marital status of the head is problematic; over 70 percent of our more than 67,000 observations are married households (including permanently cohabiting), while only 11 percent are single and 19 percent are widowed, divorced, or separated. We thus have decided to look at six dimensions: the state of the local labor market, household income, workers in low-hours jobs, age, sector of employment, and gender.

3.4.1 The Effect of the State Cycle on Responses to Shocks

Using BLS data on state-level unemployment (available from 1976), we can derive measures of the state business cycle.[15] Auerbach and Gorodnichenko (2011) find that the effects of government purchases are larger in a recession: we can evaluate this with our data.

Our measure of the state cyclical conditions is the state unemployment gap, which we plot, along with the key components of the calculation, for the

14. All the regressions control for whether the head of household is employed or retired while the consumption regressions also control for real disposable income.

15. Using county-level unemployment data is problematic for two reasons. First, because many heads of household live outside the county in which they work and commute across county lines. Second, to protect the anonymity of respondents the PSID public-use files suppress the county identifier. As we wish to evaluate whether the local labor market is above or below its normal conditions, we cannot use the reported household measure of county unemployment because households may move to another county, meaning that the reported local unemployment rate can change with no meaningful change in labor market conditions relative to normal conditions.

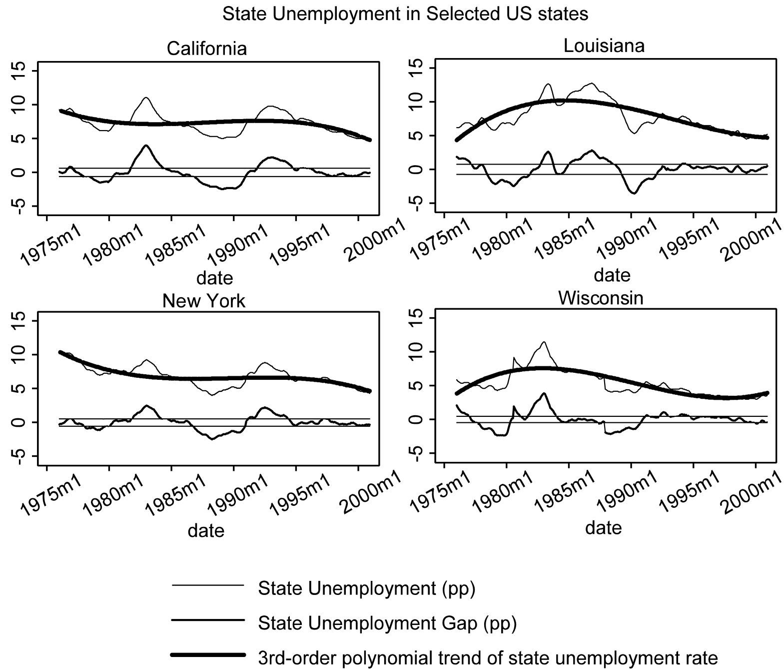


Fig. 3.9 State unemployment in a selection of US states

same four states used previously to illustrate the military spending shocks in figure 3.9. The calculation proceeds as follows. First, we take the time-series of state-level unemployment and calculate a trend unemployment rate by fitting a third-order polynomial trend. Second, we calculate the state unemployment gap as the difference between state unemployment and this fitted trend—the lower line in figure 3.9. Finally, we look across time comparing, within each state, periods of high and low unemployment where we define "tight" ("loose") labor market conditions as periods when the state unemployment gap is in the lower (upper) quartile.[16] A tight labor market is therefore one in which the state unemployment is far below its trend. We then include these dummy variables, as well as the appropriate interactions, in our regression equation, as described before.

The results (see figure 3.10) are consistent with Auerbach and Gorodnichenko (2011). Spending shocks seem to have different effects in periods of high and low unemployment. When the local labor market is tight, our estimates suggest that neither consumption nor hours respond, implying that wealth effects could make the consumption multiplier negative and

16. The quartiles are marked in the figure by the parallel lines that cut through the unemployment gap.

Fig. 3.10 IRFs to a 1 percent GDP state spending shock: The response by state labor conditions

the effect of hours worked positive. In periods of relatively high unemployment, we estimate a positive effect on consumption, which, however, could be canceled by the wealth effect.

3.4.2 Responses by Income Group

In order to examine whether relatively richer and relatively poorer households react differently to a spending shock, we define two dummy variables using the distribution of real disposable income:

$$D(\text{low income})_{ist} = \begin{cases} 1 & \text{if in lower quartile of year } t \text{ income distribution} \\ 0 & \text{otherwise} \end{cases}$$

$$D(\text{high income})_{ist} = \begin{cases} 1 & \text{if in upper quartile of year } t \text{ income distribution} \\ 0 & \text{otherwise} \end{cases}$$

Our definition means that a household i will be marked as a low (high) income household with $D(\text{low income})_{ist} = 1$ ($D(\text{high income})_{ist} = 1$) if the household has real disposable income in year t that is at or below (at or above) the twenty-fifth (seventy-fifth) percentile of the US income distribution in year t.

Figure 3.11 shows that there is an important difference between the response of higher- and lower-income households according to our definition of relative income. Lower-income households respond to the spending shock lowering consumption and raising (although with a three-year lag) hours worked. The presence of a group-specific wealth effect would make such responses even stronger; as described earlier for a progressive tax system, these results are consistent with lower-income households cutting consumption (the true direct effect is more negative than our estimates, which potentially include the specific wealth effect) and raising hours (both the total and direct effects would be larger than the estimates presented). Thus lower-income households appear to behave consistently with the predictions of intertemporal models where households derive no benefit from the increase in government spending, but realize they will eventually have to pay for it. Their real wages, however, do not change significantly (as those models predict): this could be because there are regulatory reasons that make their wages relatively sticky (such as minimum wage laws).

The response of high- and middle-income households, instead, is inconclusive: we estimate a positive and significant direct response of consumption, which, however, could be overturned by the wealth effect. If anything, however, the military contracts we analyze seem to favor relatively higher income households, perhaps because they are concentrated in firms with relatively high-skilled workers, or because higher-income households are more likely to own shares in such firms.

One concern with this analysis is that our dummy variable could simply

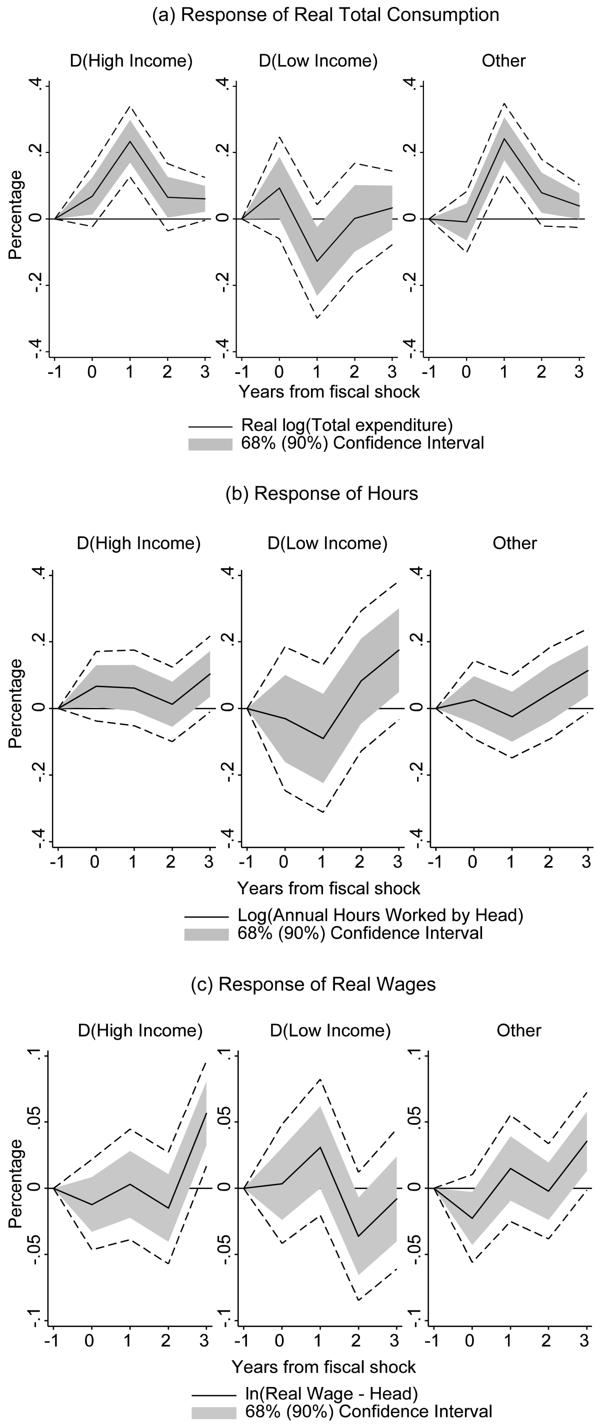


Fig. 3.11 IRFs to a 1 percent GDP state spending shock: The response by income relative to the US-wide distribution of income in period *t*

capture differences in levels of income across states: remember that we have identified those households with extreme (high or low) incomes within the entire distribution of income in the PSID in each year. Therefore, we repeat our analysis but use the following two alternative dummy variables:

$$D(\text{low income}^{A})_{ist} = \begin{cases} 1 & \text{if in lower quartile of states, year } t \text{ income distribution} \\ 0 & \text{otherwise} \end{cases}$$

$$D(\text{high income}^{A})_{ist} = \begin{cases} 1 & \text{if in upper quartile of states, year } t \text{ income distribution} \\ 0 & \text{otherwise.} \end{cases}$$

Now a household is a low (high) income household if the household has real disposable income in year t that is at or below (at or above) the twenty-fifth (seventy-fifth) percentile of the *state s* income distribution in year t. The potential worry about this approach is that some of the states, as just discussed, have relatively few households and therefore such a distribution is based on very few observations. Nonetheless, the results of the earlier analysis are little changed, as we show in figure 3.12.

3.4.3 Workers Who Work Low Hours

Heads of household working relatively few hours (most likely on part-time jobs) are likely to have more labor supply flexibility. In fact, in Giavazzi and McMahon (2010) we found that part-time German workers responded to an exogenous increase in uncertainty by working longer hours—a response we did not observe for workers in full-time employment. In order to check whether the response differs between full-time and part-time workers, we define a dummy variable:

$$D(\text{low hours})_{ist} = \begin{cases} 1 & \text{if the head regularly works less than 20 hours per week} \\ 0 & \text{otherwise.} \end{cases}$$

The choice of twenty hours per week is somewhat arbitrary. As aforementioned, we find that the median worker works about forty hours per week and so this number represents someone working about half the full-time worker's hours. We restrict the sample to heads of household who did not change their employment status during the year: since our data measure annual hours worked, if someone worked for six months and then lost their job and did not get a new one for the remainder of the year, their hours for the year would look like someone working about twenty hours a week but their position is not as a regular low hours worker.

Figure 3.13 shows that there is an important difference between the response of full-time and part-time workers. Heads working less than twenty hours per week initially respond to a spending shock increasing consumption, but they then soon reduce it (remember that these estimates likely over-

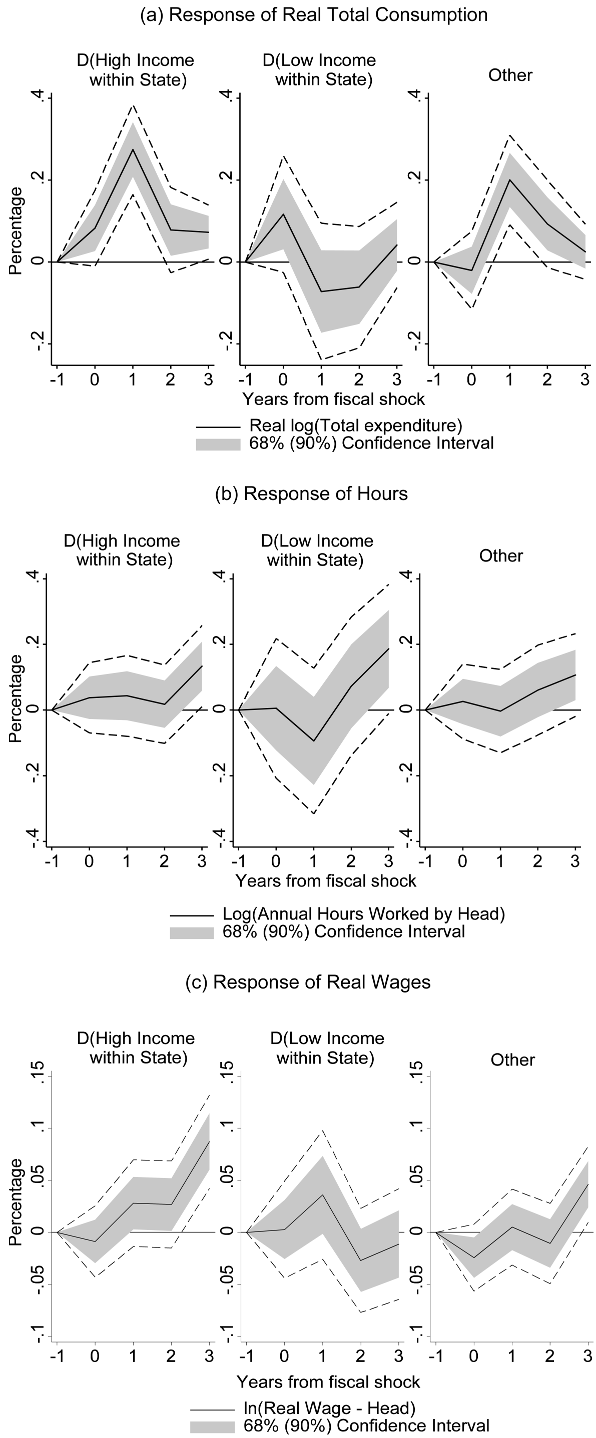


Fig. 3.12 IRFs to a 1 percent GDP state spending shock: The response by state income

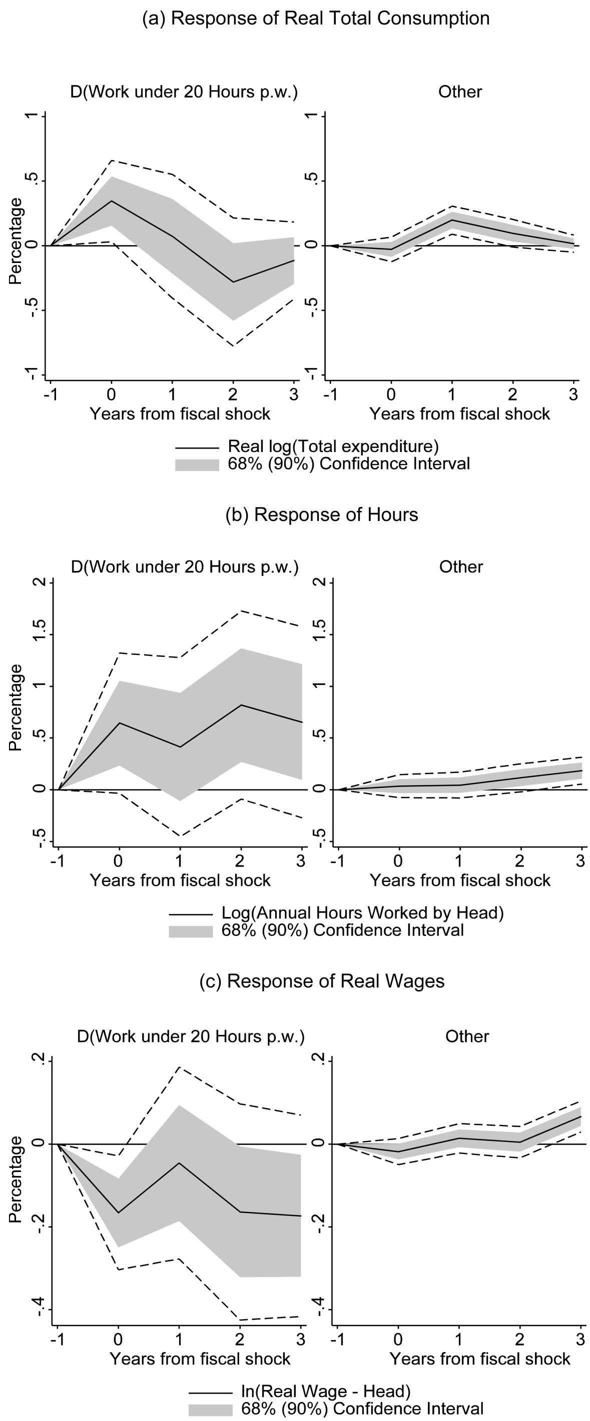


Fig. 3.13 IRFs to a 1 percent GDP state spending shock: The response by part-time workers

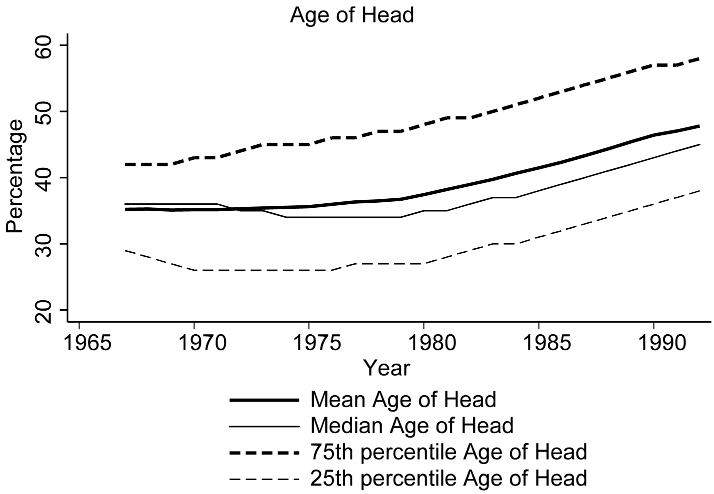


Fig. 3.14 Time series of the age distribution

state the multiplier). They also work longer hours, precisely as we observed for lower-income households. As in that case, our estimates understate the increase in hours because the average wealth effect would indicate an increase in hours, meaning that the hours of part-time workers unambiguously increase. And like the lower-income households, if they expect to pay less of the taxes, then the increase in hours and fall in consumption is further reinforced. Hours, which average about ten per week for this group, actually increase by between 50 and 75 percent, meaning the average worker would now work fifteen to eighteen hours per week. Finally, those working less than twenty hours also see their real wages fall, which is consistent with the increase in their labor supply.

The response of heads working more than twenty hours per week is instead closer to the response obtained using aggregate data.

3.4.4 Age

We have also looked at different age groups. In order to split the sample into different age groups, we do as we did for income and use the by-year distribution of ages as the point of reference. This is shown in figure 3.14 and we will define anyone above (below) the seventy-fifth (twenty-fifth) percentile in a given year as the high (low) age group:

$$D(\text{low age})_{ist} = \begin{cases} 1 & \text{if in lower quartile of age distribution in } t \\ 0 & \text{otherwise} \end{cases}$$

$$D(\text{high age})_{ist} = \begin{cases} 1 & \text{if in upper quartile of age distribution in } t \\ 0 & \text{otherwise.} \end{cases}$$

The response of older workers, relative to younger ones, should depend, in principle, on their life horizon and on the extent to which they internalize the well-being of their children. If they do not, and expect that someone else will bear the taxes that will be raised to pay for the additional spending, the negative wealth effect associated with the increase in government spending will be smaller. Instead, if they expect that some of these taxes will fall upon themselves, they will cut consumption and increase hours, the more so the fewer the active years they have left. However, because our time fixed effect captures the average wealth effect, any differential wealth effect should be reflected in the estimated response of the older workers.

The results are shown in figure 3.15. While all age groups seem to increase consumption (as in the aggregate response), the youngest workers tend to increase by the least. The response of hours is more striking: relatively young heads increase hours, while the oldest workers actually reduce their hours. These findings are consistent with the older workers experiencing a negative wealth effect that is smaller relative to the mean wealth effect captured in the time fixed effect; this (relative) positive wealth effect is reflected in the estimated response as more positive consumption and lower hours worked. Younger workers seem to experience a relatively larger negative wealth effect. Intriguingly, the middle-aged heads tend to both increase consumption and hours worked.

3.4.5 Responses by Workers from Different Industries

We are also able to follow which industry a head of household works for between 1976 to 1992. This is the response to a question in which the head is asked to report the "kind of business" that the head of household considers themselves to work in. The categorization uses the three-digit industry codes from the 1970 Census of Population Classified Index of Industries and Occupations. We use these data and classify workers according to two dummy variables, which we define only for those who are employed:[17]

$$D(\text{manufacturing})_{ist} = \begin{cases} 1 & \text{if head is employed in manufacturing industry} \\ 0 & \text{otherwise} \end{cases}$$

$$D(\text{services})_{ist} = \begin{cases} 1 & \text{if head is employed in services sector} \\ 0 & \text{otherwise.} \end{cases}$$

We use these two industries as Nekarda and Ramey (2011) discuss the effects of military spending on US manufacturing while services account for about 70 percent of the US economy; the residual includes "Agriculture,

17. Manufacturing industries include both durable and nondurable industries given by codes 139–169, 177–209, 219–238, 107–138, 239–259, 268–299, and 307–398. The "Services" sector is identified by codes 707–718, 757–759, 727–749, 769–798, 807–809, 338–389, 828–848, 857–868, 869–897, and 849.

Fig. 3.15 IRFs to a 1 percent GDP state spending shock: The response by age

Forestry, and Fishing," "Mining and Extraction," "Construction," "Retail or Wholesale," "Transport, Communication & Utilities," and "Government" industries.

The results of our sectoral split are reported in figure 3.16. The sectoral response of consumption matches the aggregate response: there is no difference across sectors. But the (positive) response of hours is concentrated in the service sector, confirming what we had found looking at heads working less than twenty hours: flexibility is higher where part-time jobs are more frequent (3.2 percent of heads who work in the services sector work low hours compared with only 1.4 percent of those in other sectors).

We also compared government employees (including those working for states and cities) with heads of households working in the private sector. Interestingly, spending shocks have no effect on government employees: neither their consumption nor their hours move.

3.4.6 Gender Split

Finally, we look at whether there are differences in the reaction of households in which the head is a female. Such households make up 26 percent of all observations. While 12 percent of male heads are in the lower income quartile, 40 percent of female heads are. Female heads are disproportionately not employed; half of not employed heads are female. Of those female heads in employment, they are underrepresented (in the sense of less than 25 percent share) in all sectors of employment except for services; they make up 37 percent of the services sector.

Given this information, it is not surprising that their response to a spending shock matches that of heads working in the service industry (see figure 3.17). While the effect of the spending shock on consumption is independent of gender, the response of hours is concentrated on women. Also their real wages increase more than those of nonfemale heads.

3.4.7 In Sum

Our main findings from the various splits can be summarized as follows:

1. The spending shocks we have analyzed seem to have important distributional effects. There is a difference between the response of higher- and lower-income households. Lower-income households match the predictions of standard intertemporal representative agents models: they cut consumption (unambiguously because the wealth effect if anything reinforces our results) and work longer hours (also unambiguously), precisely as we would expect from households that receive no benefit from higher public spending but realize they will eventually have to pay for it. The response of higher-income households is more muted and we are unable to say whether the positive response of consumption we estimate is reversed by the wealth effect. Of course, our results may be specific to the military contracts that

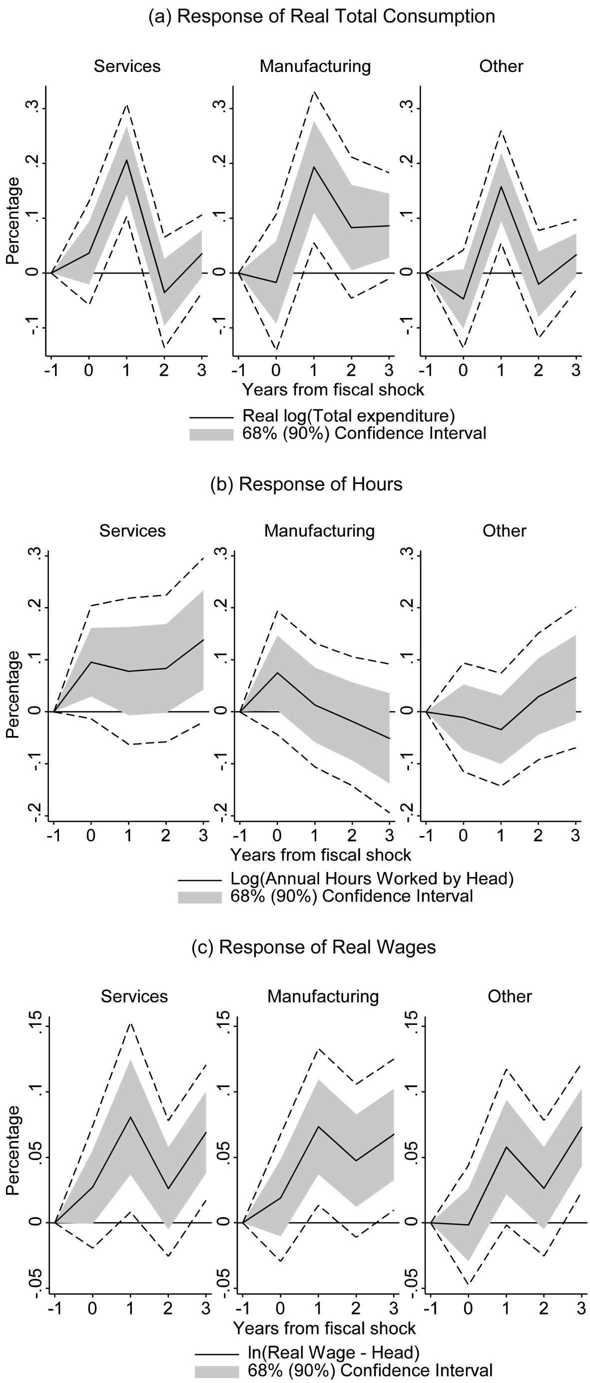


Fig. 3.16 IRFs to a 1 percent GDP state spending shock: The response by industry

Fig. 3.17 IRFs to a 1 percent GDP state spending shock: The response by gender

we consider and so other types of fiscal spending may have very different distributional effects.

2. There is also an important difference between the response of full-time and part-time workers. Differently from full-time workers, part-timers respond to a spending shock cutting consumption, although perhaps not immediately. They also work longer hours, precisely as we observed for lower-income households. But differently from lower-income heads, those working less than twenty hours also see their real wages fall, which is consistent with the increase in their labor supply. Thus the response of part-time workers matches that predicted by a model in which households make optimal intertemporal decisions and government spending is pure waste, at least from their viewpoint.

3. Our results suggest that increases in military spending tend to be more effective in states with relatively high local unemployment. Although we cannot say whether in such states consumption increases, it certainly does not (and could very well decrease) in states with low local unemployment.

4. The positive response of hours worked to a spending shock is concentrated among households headed by a woman, among heads employed in the service sector, and among relatively younger workers).

5. There is not much our results can say about the aggregate effects of these spending shocks. At the aggregate level our estimates indicate an increase in consumption, which, however, could be overturned by the working of a wealth effect.

3.5 The Extensive Margin of Employment

So far we have analyzed the intensive labor supply margin: hours worked by employed workers. A separate question is the effect of the spending shocks on the extensive margins—employment. Specifically, we estimate a linear probability model and regress a dummy variable for whether the worker is employed on state, time, and household fixed effects. For this regression we include only those households in the labor force. The regression is analogous to those estimated before. For the aggregate results reported in figure 3.18 the estimated equation is:

$$D(\text{employed})_{i,s,t} = \alpha_i + \gamma_s + \delta_t + \sum_{k=0}^{K} \beta_k \Omega_{s,t-k} + \varepsilon_{i,s,t}.$$

While the point estimate is for an increase in the likelihood of employment for a household in a state receiving a positive fiscal spending shock, the result is only marginally significant after two years.

Figure 3.19 reports the results for a variety of the classifications just used; we cannot, obviously, do the industry breakdown as it is only classified for those that are employed. A positive spending shock increases the likelihood of employment for almost all households. Strikingly, households headed by relatively poorer workers see their probability of employment fall; this

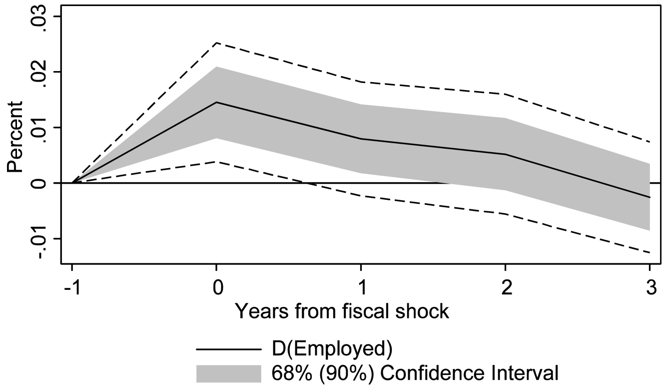


Fig. 3.18 Change in the probability of employment following a 1 percent fiscal shock

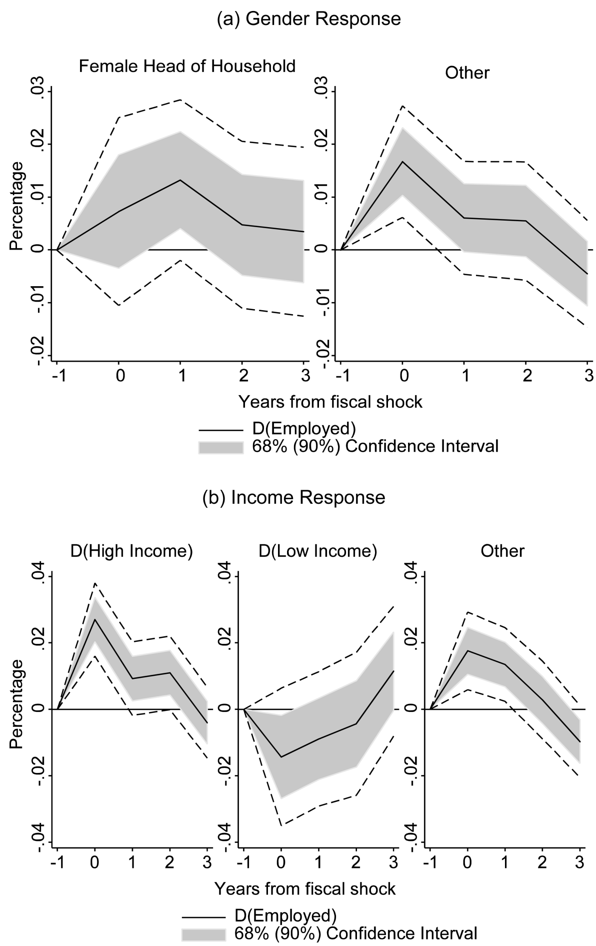


Fig. 3.19 Δ in probability of employment following a 1 percent fiscal shock

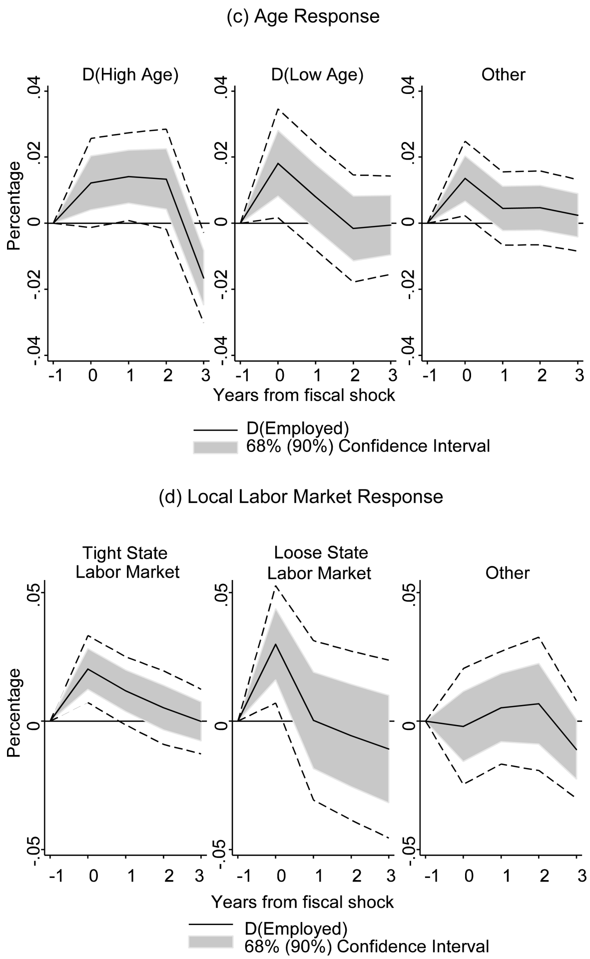


Fig. 3.19 (cont.)

effect tends to accentuate the relative decline in the intensive margin for these households. In periods of relatively high unemployment, spending shocks have no effect on hours worked nor on the likelihood of being employed.

3.6 Conclusion

Observing significant differences across the responses of various groups does not necessarily imply that aggregate estimates are biased: they could simply reflect the average of group-level responses. Aggregation theory suggests, however, that the large differences we have documented are likely to result in biased aggregate estimates. In our results there are no instances of a consistent response among all groups that disappears at the aggregate level, which would be clear evidence of an aggregation bias. If aggregation bias exists, it is likely to be attenuated.

Our results could be used to design the allocation of military contracts across states, so as to increase their macroeconomic effect: the answer here is simple—you want to spend in states with relatively high unemployment. They also suggest that military spending has significant distributional effects: the group more negatively hit appears to be part-time workers. They cut consumption, work longer hours, and see their real wages fall. Of course, it would also be interesting to explore the effects of other types of government spending, and so care should be taken in extrapolating from the identified fiscal spending shocks in this chapter to all other types of fiscal spending.

Finally our estimates, despite the potential problem of missing any wealth effect, can in some cases still allow us to discriminate between alternative models. We find it interesting that some groups (lower-income and low-hours workers in particular) appear to behave consistently with the predictions of models in which households respond to government spending shocks making optimal intertemporal decisions.

Appendix A
Robustness

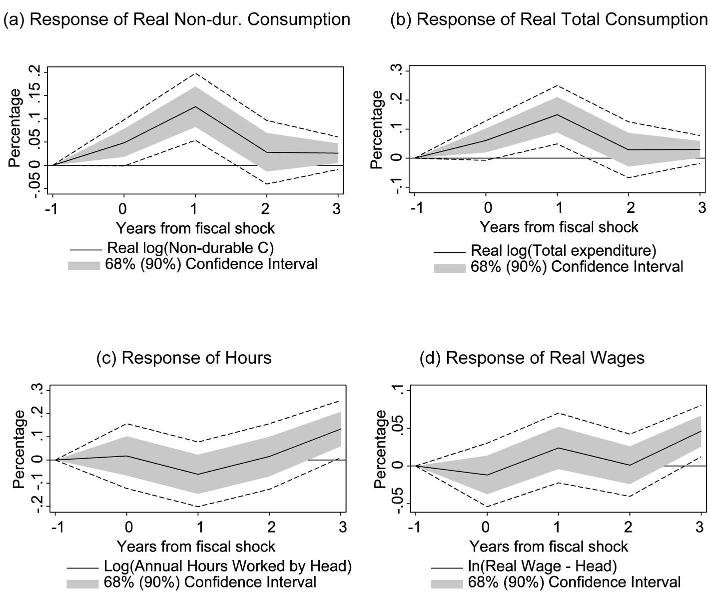
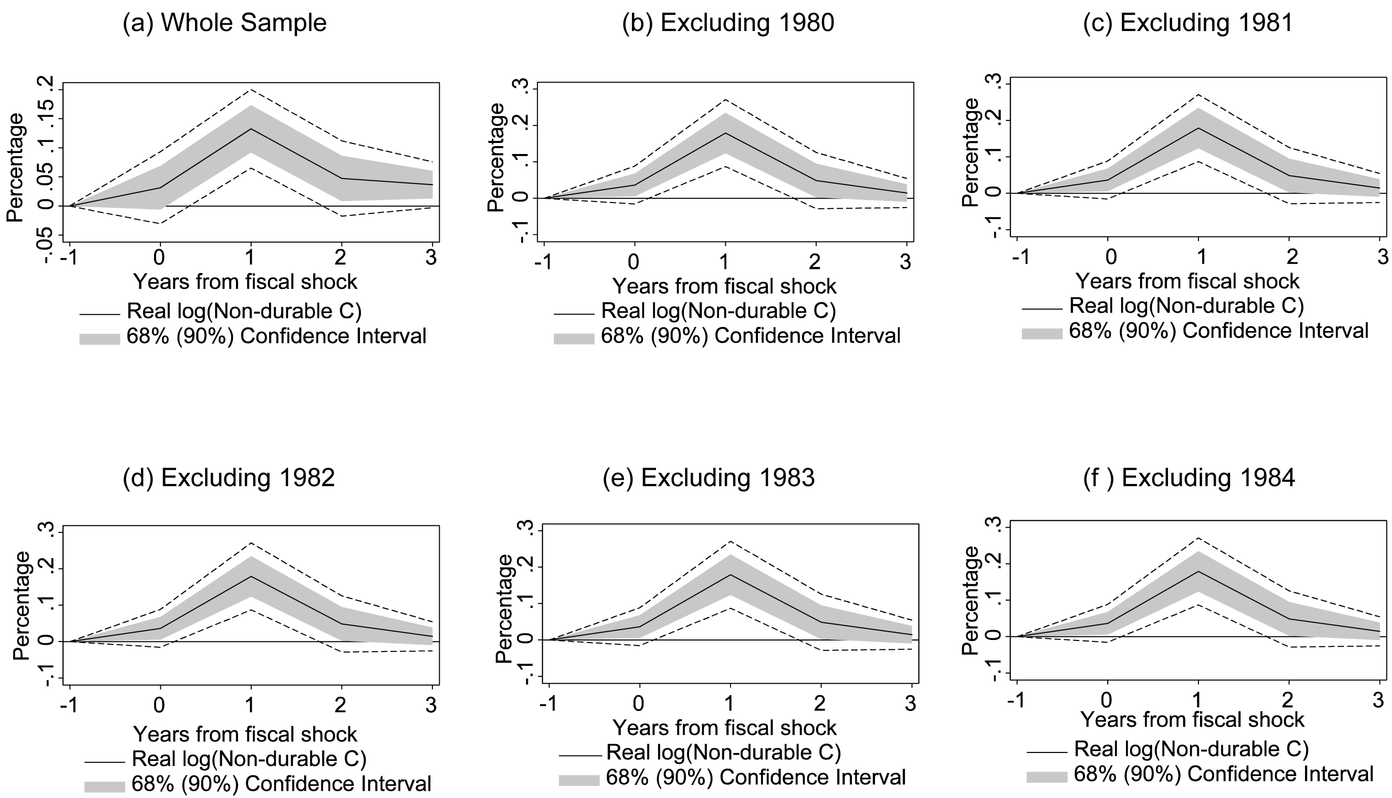


Fig. 3A.1 IRFs to a 1 percent GDP state spending shock: The average response using the alternative measure of fiscal shock

Appendix B

Robustness to Excluding Individual Years



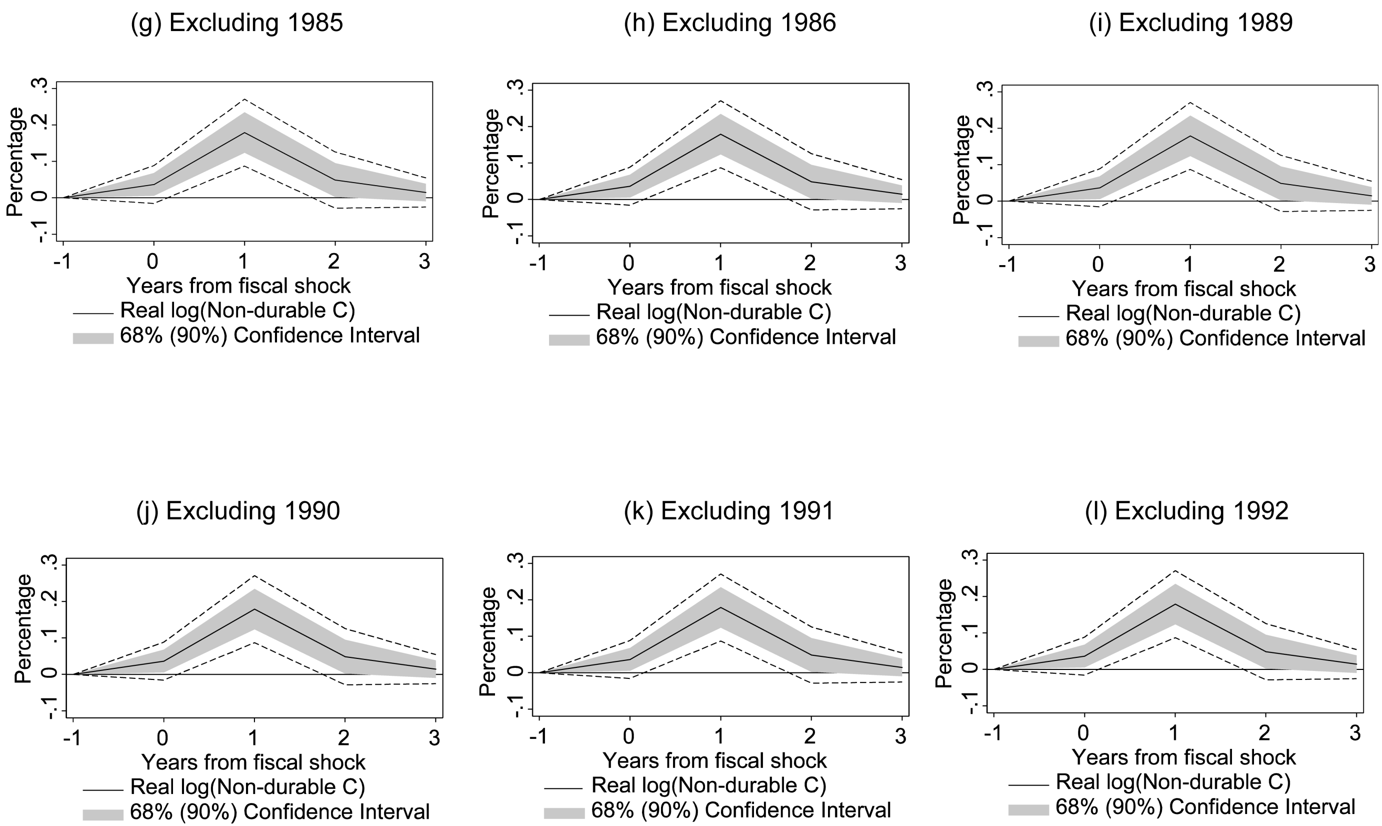


Fig. 3B.1 IRFs to a 1 percent GDP state spending shock: Response of real nondurable consumption

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Comment Lawrence J. Christiano

This is an excellent chapter on the effects of government spending that is well worth studying. Most of my discussion focuses on the background and motivation for the analysis. I begin by describing what it is about the current economic situation in the United States and other countries that motivates interest in the economic effects of government spending. Perhaps the natural place to look for information on the effects of government spending is the time series data. I review the information in the US time series data since 1940 using the different approaches taken by Ramey and Hall in this volume. I show that whatever information there is in the data about the effects of government spending primarily stems from the Korean War and World War II

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I am very grateful for discussions with Benjamin Johannsen. I am particularly grateful for his assistance on the computations for Valerie Ramey's vector autoregression, reported in this comment. For acknowledgments, sources of research support, and disclosure of the author's material financial relationships, if any, please see http://www.nber.org/chapters/c12637.ack.