

Can Government Purchases Stimulate the Economy?

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One of the few positive effects of the recent financial crisis has been the revival of interest in the short-run macroeconomic effects of government spending and tax changes. Before 2008, the topic of short-run effects of fiscal policy was a backwater compared to research on monetary policy. One reason for the lack of interest was the belief that the lags in implementing fiscal policy were typically too long to be useful for combating recessions. Perhaps another reason was that central banks sponsored many more conferences than government treasury departments. When the economy fell off the cliff in 2008 and the Fed reached the dreaded "zero lower bound" on interest rates, however, it became abundantly clear that more research was needed.

Given the upsurge in research on this topic, we now have many more resources to draw upon when asked "what is the government spending multiplier?" In this essay, I will begin by briefly reviewing what theory has to say about the potential effects. As I will discuss, "the multiplier" is a nebulous concept that depends very much on the type of government spending, its persistence, and how it is financed. I will then go on to review the aggregate empirical evidence for the U.S., as well as the cross-locality evidence on multipliers. I will conclude that the U.S. aggregate multiplier for a *temporary, deficit-financed* increase in government *purchases* (that enter separately in the utility function and have no direct effect on private sector production functions) is probably between 0.8 and 1.5. Reasonable people can argue, however, that the data do not reject 0.5 or 2.

I. Brief Review of the Theory

In this section, I briefly review the leading theories on the effects of government spending. An important point to keep in mind is that *all* of the theories hinge

fundamentally on the effect of government spending on equilibrium hours worked, and how those hours translate to output. Absent instantaneous adjustment of the capital stock, total output can only rise in the short-run if hours worked rise. Thus, the multiplier is intimately linked to the effect of government spending on equilibrium hours and to the extent of diminishing returns to labor .

A. Models in the Neoclassical Tradition

In neoclassical models, the key channels through which fiscal policy affects the private economy are wealth effects, intertemporal substitution effects, and distortions to first-order conditions (e.g. Barro and King (1984), Baxter and King (1993), and Aiyagari, Christiano, and Eichenbaum (1992)). To see this, consider first a standard neoclassical model with no distortionary taxes. The social planner maximizes the discounted utility of the representative household subject to the production function and resource constraints. Following Aiyagari et al (1992), we can write the standard Bellman equation to distinguish static from dynamic effects of government spending:

$$v(k, g^P, g^T) = \max_{k' \leq f(k, N) - g} \left\{ W(k, k' + g) + E(\beta v(k', g^{P'}, g^{T'}) | g^P) \right\}$$

where

$$W(k, k' + g) = \max_{c, n \in \{0 \leq n \leq N; 0 \leq c \leq f(k, n) - (g + k')\}} u(c, n)$$

In these equations, c is consumption, n is hours worked, k is the capital stock at the beginning of the period, g^T is the transitory component of government spending, g^P is the persistent component of government spending, g is total government spending, $u(c, n)$ is

the utility function and $f(k,n)$ is the production function. Primes denote the next period's value of variables. There exist unique solutions to the utility maximization subproblem, so that optimal labor supply and consumption can be written as:

$$n = h(k, k' + g) \quad \text{and} \quad c = q(k, k' + g)$$

It can be shown that the function h is strictly increasing in g since a rise in g represents a negative wealth effect (and leisure is assumed to be a normal good). For the same reason, the function q is strictly decreasing in g .

Aiyagari et al (1992) decompose the effect of government spending on hours as follows:

$$\frac{dn}{dg} = \frac{\partial h}{\partial g} + \frac{\partial h}{\partial k'} \frac{\partial k'}{\partial g}$$

The first term captures the static effect and the second term captures the dynamic effect. As discussed above, the first term is positive because of the negative wealth effect. Because g and k' enter symmetrically in the h function, $\partial h / \partial k' = \partial h / \partial g$. The size of $\partial k' / \partial g$ depends on whether the increase in g is transitory or persistent. Aiyagari et al (1992) show that

$$\left(\frac{\partial k'}{\partial g} \right)^P > \left(\frac{\partial k'}{\partial g} \right)^T$$

so a persistent increase in government spending raises next period's desired capital stock by more. Thus, a persistent increase in government spending raises hours more now.

In this model with no distortionary taxation, Ricardian Equivalence reigns, so it does not matter whether government purchases are financed with current taxes or deficit spending. Results change considerably, though, when spending is financed with distortionary taxes. For example, a rise in current distortionary labor income taxes tends to depress output and hours.

Baxter and King (1993) catalog the possible range of government spending multipliers using a standard calibration of a DGE model. They find that the lowest multipliers result when (1) the increase in government spending is temporary and (2) governments raise distortionary taxes concurrently to keep the budget balanced. In this case, the multiplier can be as low as *negative* 2.5. Multipliers for temporary increases in government spending financed with deficits (to be paid with future lump-sum taxes) are somewhat higher, but are still substantially below unity. Permanent increases in government spending financed by current or future lump-sum taxes give larger multipliers because the greater negative wealth effect raises labor supply more and the steady-state capital stock rises, which leads to a rise in investment. In this case, the short-run multiplier is just below unity and the long-run multiplier is around 1.2.

As Burnside, Eichenbaum and Fisher (2004) note, on average in the post-WWII data, large increases in government spending are typically followed by hump-shaped rises in distortionary taxes. Although they do not discuss multipliers explicitly, the graphs from the analysis of models with paths of distortionary taxes lead to higher positive short-run multipliers than in the lump-sum tax case. The multiplier is higher because of intertemporal substitution effects: because individuals know that taxes will be higher in

the future, they intertemporally substitute more labor to the present when taxes are relatively low.

Thus, the neoclassical model predicts that the government spending multiplier can be negative or positive, depending on the extent and timing of distortionary taxes. For reasonable parameter values, the short-run multiplier can be as high as 1.2 or as low as -2.5, depending on the nature of the experiment.

B. Models in the Keynesian Tradition

The basic idea of the multiplier is illustrated in the so-called "Keynesian Cross Diagram" that is the staple of undergraduate macroeconomics. If interest rates are held constant, then the multiplier for government spending is given by $1/(1-mpc)$ and for taxes is given by $-mpc/(1-mpc)$, where mpc is the marginal propensity to consume. Allowing for open economy considerations (i.e. a marginal propensity to import) or rises in interest rates lowers the multiplier, whereas allowing for accelerator effects in investment can raise the multiplier. Even in extended models, the size of the multiplier is intimately linked to the marginal propensity to consume.

As Galí, Lopez-Salido, and Vallés (2007) and Cogan, Cwik, Taylor, Wieland (2010) discuss, the typical New Keynesian model (e.g Smets and Wouters (2007)) predicts a much smaller multiplier. Since the New Keynesian model builds a sticky-price edifice on a neoclassical foundation, neoclassical effects tend to mute the Keynesian multiplier. Cogan et al (2010) use the Smets-Wouters model to estimate multipliers that are equal to or less than unity. Galí et al (2007) are able to obtain multipliers as high as two, but only when they make the following two assumptions: (1) at least fifty percent of

consumers are rule-of-thumb consumers, so that the marginal propensity to consume is much higher than would be the case if consumers behaved optimally; and (2) employment is demand-determined, so that workers are always willing to supply as many hours as firms demand. These two assumptions essentially convert the New Keynesian model back into a traditional Keynesian model.

Within the new Keynesian model, however, there is one way in which multipliers can be made larger without resorting to widespread non-optimizing behavior. This is the case of the "Zero Lower Bound." Eggertsson (2001, 2011), Eggertsson and Woodford (2003), Christiano, Eichenbaum and Rebelo (2011) and Woodford (2011) explore fiscal policy in New Keynesian models in which the economy is caught in a deflationary spiral at the zero lower bound. A deficit-financed increase in government spending leads expectations of inflation to increase. When nominal interest rates are held constant, this increase in expected inflation drives the real interest rate down, spurring the economy. Christiano et al show that if interest rates are held constant for 12 quarters and government spending goes up during this time, the multiplier peaks at 2.3.

C. Other Considerations

Most of the models discussed above abstract from three potentially important features: (1) productive government spending; (2) transfers; and (3) underutilization of resources. I will briefly discuss how each of these might change the predictions.

In the last section of their paper, Baxter and King (1993) consider the multiplier effects of an increase in investment in public capital. In the case of public capital that raises the marginal product of private inputs, the multiplier can be quite large, somewhere

between 4 and 13 in the long-run, but much lower in the short-run. Thus, considering productive government spending does not raise the predicted short-run stimulus effects in the neoclassical model.

As Oh and Reis (2010) and Cogan and Taylor (2011) point out, government purchases barely increased in 2009 and 2010 despite the large stimulus package. As both papers point out, most of the stimulus package was allocated to transfers. Most models, both neoclassical and New Keynesian, treat transfers like a negative lump-sum tax. In the typical homogenous agent model with perfect capital markets, a temporary rise in transfers now should have no effect because of permanent income hypothesis considerations and Ricardian Equivalence. Oh and Reis (2010) explore some simple models that relax these assumptions but are not able to generate much bigger effects.

All of the models discussed above assume the economy starts out in a steady-state in which capital is fully utilized and workers are fully employed. A key question is whether government spending multipliers can be greater if the economy starts out with underutilized resources, which is widely believed to be the case in 2009. It seems that this would be a promising area for more theoretical research. Below, I will discuss some empirical work that has found that the magnitude of the multiplier does seem to depend on the state of the economy.

To summarize this section, the theoretical work on government spending gives a wide range of possible values of the multiplier, depending on the type of model used, the assumptions about how monetary policy behaves, the type and persistence of government

spending, and how it is financed. It is necessary, therefore, to turn to the data to see if we can narrow the range.

II. Aggregate Time Series Evidence

All of the theories of the multiplier discussed above are general equilibrium theories, so aggregate data is the most natural place to study the strength of the multiplier. Numerous studies have been conducted on a variety of countries. In order to focus this section, I will concentrate on the U.S. evidence. Studies of multiple countries, such as by Perotti (2005), Beetsma, Giuiliodori, Klaasen (2008), Guajardo, Leigh and Pescatori (2010), Ilzetski, Mendoza, and Végh (2010), Beetsma and Giuiliodori (2011) and others, tend to find multipliers in the range of those discussed here for the U.S.¹

Since most aggregate studies measure what happens *on average* when government spending changes, it is very important to keep track of the characteristics of the experiments covered by the analyses. For example, to measure the effect of a deficit-financed increase in government spending, one needs to focus on periods in which taxes did not change significantly or one needs to control for tax effects. Tax multiplier estimates range from -0.5 to -5, so it is difficult to choose a single number to control for tax effects.² In addition, because stimulus packages are supposed to be temporary, we ideally would like to measure the effect of temporary changes. Also important is whether the economy had underutilized resources at the time of the government spending increase.

¹ However, there is also a literature that finds some evidence that fiscal contractions can be expansionary. See, for example, Giavazzi and Pagano (1990), Alesina and Perotti (1997) Alesina and Ardagna (2010).

² Examples of estimates of the tax multiplier are -0.5 (Favero and Giavazzi (forthcoming)), -1.1 (Barro and Redlick (2011)), -3 (Romer and Romer (2010)), and -5 (Mountford and Uhlig (2009)).

Table 1 gives a summary of just a few of the representative studies using aggregate data to estimate government spending multipliers; it is by no means meant to be exhaustive.³ The Evans (1969) paper is representative of the discussion of fiscal multipliers in the heyday of traditional Keynesianism and the big econometric models. Evans compared multipliers for sustained increases in government spending across the Wharton model, the Klein-Goldberger model, and the Brookings model. He found multipliers on government spending of about 2, both in the short-run and the long-run. He also discussed the estimated marginal propensity to consume in the models. In the Wharton and Klein-Goldberger models, the short-run marginal propensity to consume was estimated to be 0.55 and the long-run one was estimated to be 0.74.

Subsequent analyses have tried to come to terms with the Lucas' (1976) and Sims' (1980) critiques of this earlier literature. Most aggregate analyses of the last several decades have relied on vector autogressions (VARs) or dynamic simulations to estimate the effects of government spending and tax changes. None of these analyses is immune to potential problems of identification, though. For example, Barro (1981), Hall (1986), Ramey and Shapiro (1998), Hall (2009), Fisher and Peters (2010), Ramey (2011), and Barro and Redlick (2011) all focus on military buildups under the assumption that this type of government spending is the least likely to respond to economic events. Nevertheless, there is always the possibility that the events that lead to these buildups, such as the start of WWII and the start of the Cold War, could have other influences on the economy apart from the effects on government spending. For example, during WWII increased patriotism could have raised labor supply more than would be predicted by

³ Examples of other studies that use methods similar to some of the studies listed or investigate the robustness of those techniques are Fatás and Mihov (2001), Perotti (2005), Pappa (2005) Caldara and Kamps (2008), Caldara (2011), Monacelli, Perotti, Trigari (2010), and Tenhofen and Wolff (2011).

economic incentives and hence could raise the multiplier. In contrast, rationing and capacity constraints during the world wars could dampen the multiplier.

Numerous studies have followed Blanchard and Perotti (2002) by using Choleski decompositions to identify government spending shocks and by using assumptions on tax elasticities in a structural VAR (SVAR) to identify tax shocks. SVAR methods have the advantage that they are easy to implement and do not require extensive data gathering. Ramey and Shapiro (1998), Ramey (2011), and Leeper, Walker, and Yang (2011) have criticized traditional VAR methods, though, arguing that most changes in government spending and taxes are anticipated and showing that this can invalidate inferences from procedures that do not account for anticipations. Moreover, Caldara (2011) shows that small changes in the assumed elasticities of taxes and government spending in the structural VAR result in large differences in the estimated multipliers.

Table 1 shows, however, that despite significant differences in samples, experiments, and identification methods, most aggregate studies estimate a range of multipliers from around 0.6 to 1.8. Moreover, the range *within* studies is almost as wide as the range *across* studies, and the standard errors are always large. Thus, despite a healthy debate on methodology, most studies are giving similar answers.

These government spending multipliers do not necessarily represent deficit-financed increases in government spending, which is the type most likely to be used in a stimulus package. For example, the lower end of my multiplier estimates (Ramey (2011)) are from samples where the Korean War is dominant, and hence are samples in which much of the spending was financed by tax increases. Even during World War II, some of the increase in government spending was financed with taxes. Barro and

Redlick (2011) control for the average marginal tax rate and find government spending multipliers of only 0.6. Using the framework in Ramey (2011), I study the effect of holding marginal tax rates constant on the path of output and find no significant change from the original multiplier estimate of approximately unity.⁴

Fisher and Peters (2010) use excess returns of defense contractor stocks as news to estimate multipliers of 1.5 for the period 1960 - 2007. Their impulse response functions show no significant rise in taxes for their sample, so the increases in government spending they identify appear to be deficit-financed. However, their government spending shocks seems to be quite persistent. In particular, in contrast to the work by Ramey (2011) and others, which shows that government spending returns to normal by 16 quarters, Fisher and Peters' estimate suggests a very persistent increase in government spending, barely falling even after 20 quarters. (See the lower right panel of their Figure 5.). Given that permanent increases in government spending imply larger multipliers than temporary increases in a neoclassical model, their estimate of 1.5 may be somewhat above the relevant one for considering temporary stimulus packages.

Several recent aggregate studies consider the possibility that the multiplier may differ according to the state of the economy. Coutinho Pereira and Silva Lopes (2010) and Kirchner, Cimadomo, and Hauptmeier (2010) use time-varying parameters and Bayesian estimation techniques and find that government spending multipliers are not very different in expansions and contractions. In contrast, Auerbach and Gorodnichenko (2011) use a regime switching model to estimate multipliers that can differ according to whether the economy is in recession or not. Estimation of such a model is far from

⁴ In particular, I use the estimates from the VAR described on pages 29-30 of Ramey (2011). I then recompute the impulse response functions holding the Barro-Redlick tax rate constant and calculate the implied multiplier.

trivial, and many subtle issues arise in estimation. When Auerbach and Gorodnichenko do not allow the regime to change endogenously, they find identical impact multipliers in the two regimes, but different estimated dynamics that imply very large multipliers in recessions compared to expansions, 2.2 in recessions and -0.3 in expansions (see table 1 of their paper). The estimated dynamic behavior of some of the variables is odd in this experiment, which I suspect is caused by the assumption that the economy never switches regimes.⁵ Fortunately, Auerbach and Gorodnichenko also discuss results where they allow feedback, so that the economy can endogenously switch between regimes. Figure 3 of their paper shows the historical multipliers based on this experiment. In this case, they obtain multipliers between 0 and 0.5 during expansions and between 1 and 1.5 during recessions. The results from this more general model in which the economy is allowed to move between regimes seem more plausible.

Gordon and Krenn (2010) also discuss the role of underutilized capacity on government spending multipliers. They create a new quarterly data set extending back to 1913 and study the role of government spending in increasing output in 1940. They find a multiplier of only 0.9 if they extend the sample to the fourth quarter of 1941, but a multiplier of 1.8 if they stop the sample in the second quarter of 1941. They give arguments and detailed evidence that the U.S. economy started hitting capacity constraints in some sectors after the second quarter of 1941.

My narrative analysis of this period (Ramey (2009)), however, suggests that some of what Gordon and Krenn measure as a multiplier may actually be an anticipation effect. Since Gordon and Krenn use a standard Choleski decomposition in a VAR, they do not

⁵ For example, the impulse response functions suggest that a shock to government spending during a recession leads to a permanently higher level of government spending and an *ever-increasing* path of output (relative to trend). See Figure 2 of their paper

control for anticipations of future increases in government spending. Thus, they observe a large increase in output in response to what appears to be a modest increase in current government spending. An alternative interpretation is that the large increase in output is the result of firms gearing up for anticipated large future increases in government spending. In fact, Barro and Redlick (2011) show that including my news variable eliminates interaction effects with unemployment in their specification.

Yet another issue is the possibility that the multiplier is greater at the zero lower bound, as discussed in the theoretical section above. Some of the authors of these papers have argued that since most of the estimates of multipliers have been over time periods in which interest rates were not at the lower bound, they do not apply to the current situation.

In fact, we do have historical evidence from periods with very low interest rates. From 1939 to the second quarter of 1947, the rate on Treasury bills never rose above 0.38 percent although the average annual rate of inflation was six percent over this time period. In Ramey (2011, p. 38), I describe results showing that when I limit the sample to the period covering 1939 to 1949, I find a multiplier of 0.7 (but with even larger than normal standard errors due to the reduced sample). Thus, I find no evidence of larger multipliers during the extended period in which interest rates were held virtually constant at the zero lower bound.

Based on these considerations and the estimates available, I would argue that despite significant differences in methodology, the range of plausible estimates for the multiplier in the case of a temporary increase in government spending that is deficit financed is probably 0.8 to 1.5. As discussed above, I truncated the lower estimates

because they were usually affected by concurrent increases in distortionary taxes. I truncated the very highest estimates because of the various concerns I expressed above. If the increase is undertaken during a severe recession, the estimates are likely to be at the upper bound of this range. It should be understood, however, that there is significant uncertainty involved in these estimates. Reasonable people could argue that the multiplier is 0.5 or 2 without being contradicted by the data.

III. Cross-State Evidence

In their recent review of empirical economics, Angrist and Pishke (2010) praised the increase in empirical standards and the many advances in applied microeconomics, but bemoaned the fact that macro and industrial organization were slow to adopt some of these new approaches. The exciting new literature on cross-state effects of government spending is both an answer to Angrist and Pishke, but also an explanation for why the techniques used in applied microeconomics are not always suitable for macroeconomics.

One reason that the "natural experiment" techniques have been slow to diffuse in macroeconomics is that it is difficult to use them to answer *macroeconomic* questions. As I will discuss shortly, there have been numerous recent papers using panel data or state cross-section data to estimate the effects of government spending on state economies. These studies estimate government purchases or transfers multipliers, holding national effects constant. Thus, the studies that look at government transfers are answering the question: "When the federal government redistributes \$1 more to Mississippi than to other states (with taxes liabilities imposed on all states), what happens to income (or employment) in Mississippi *relative* to other states?" The answer to this

question is only indirectly related to the aggregate multiplier. To see the difference, suppose the economy behaves according to a simple traditional Keynesian model. In this case, if the government transfers \$1 to Mississippi and finances it by increasing lump-sum taxes across all states, the true aggregate multiplier is 0, since the taxes and transfers cancel in the aggregate. However, if we run a panel regression with time fixed effects (which net out the economy-wide rise in tax liabilities), we will estimate a multiplier of $mpc/(1-mpc)$, where mpc is the marginal propensity to consume. If the marginal propensity to consume were 0.6, then we would estimate a multiplier of 1.5 at the state level, even though the aggregate multiplier for this experiment is 0.

Shoag (2010) and Nakamura and Steinsson (2011) explore in detail what these experiments mean if we interpret states as small open economies in a currency union. As the various versions of their model show, translating the state-level estimates to aggregate estimates depends importantly on the type of spending and the assumptions of the theoretical model. Clemens and Miran (2011) present a very useful econometric framework for evaluating the economic context of the various natural experiments. Their discussion highlights many of the complications that arise in most of the cross-state empirical work on the subject.

Table 2 lists some of the papers that have estimated state or region multipliers. This literature has focused as much on employment effects as income effects, which is important in this era of jobless recoveries. Many (though not all) papers find positive employment effects. A notable exception is the Cohen, Coval and Malloy (2011) paper, which finds that an increase in earmarks (induced by shifts in political power) lead to a decline in corporate employment in the state.

In terms of income multipliers, most estimates lie in the range of 0.5 to 2. As with the aggregate papers, the ranges within papers are sometimes as large as the range across papers. Fishback and Kachanovskaya (2010) study New Deal outlays, which are of particular interest because of parallels between the Great Depression and the Great Recession. According to their estimates, the types of outlays that had the highest multiplier were public works and relief, with a multiplier of 1.7. In contrast, payments to farmers to take their land out of production had an income multiplier of -0.5.

Despite using very different identification methods, many of these cross-state studies find multipliers on purchases or transfers of about 1.5 to 1.8 for income and an implied cost of around \$35,000 per job created. Several studies also find that the multiplier is significantly higher during times of higher slack (e.g. Shoag (2010), Serrato and Wingender (2011), Nakamura and Steinsson (2011)). These findings suggest that some types of stimulus spending that redistribute resources from low unemployment states to high unemployment states could result in sizeable aggregate multipliers. More research is needed, however, to understand how these local multipliers translate to aggregate multipliers.

IV. Conclusions

We now have many more estimates of fiscal multipliers than we did in Fall 2008 and early 2009, when policy-makers were trying to decide whether to use fiscal policy to try to stimulate the economy. Many of the studies are so recent, however, that the profession needs more time to interpret results and to check their robustness before coming to any firm conclusions. At this point, it seems that the bulk of estimates imply

that the aggregate multiplier for a temporary rise in government purchases not accompanied by an increase in current distortionary taxes is probably between 0.8 and 1.5.

Despite the increase in the number of estimates, there is still no consensus on the mechanism by which government spending raises GDP. Some of the papers find that government spending leads consumption to decline, consistent with the negative wealth effect of the neoclassical model. Others find that consumption increases, consistent with rule-of-thumb consumers. Household studies, such as the work by Parker, Souleles, Johnson and McClelland (2011), can help shed light on this issue. Nekarda and Ramey (2011) present evidence that industry markups do not change in response to government spending, as required by the New Keynesian model. Thus, more research is required before we understand the mechanism.

It is important to note that none of these estimates sheds light on the *welfare* consequences of temporary increases in government spending to stimulate the economy. Such an analysis would require a better understanding of the mechanisms, as well as assumptions about whether government purchases enter the utility function.

Table 1. Examples of Aggregate Analyses on U.S. Data

| Study | Sample | Identification | Implied spending multiplier |
|--|--|---|--|
| Evans (1969) | Quarterly, 1948-1962 | Based on estimates of equations of Wharton, Klein-Goldberger, and Brookings models | Slightly above 2 in all models |
| Barro (1981), Hall (1986), Hall (2009), Barro-Redlick (2011) | Annual, various samples, some going back to 1889 | Use military spending as instrument for government spending. | 0.6 - 1 |
| Rotemberg-Woodford (1992) | Quarterly, 1947 - 1989 | Shocks are residuals from regression of military spending on own lags and lags of military employment | 1.25 |
| Ramey-Shapiro (1998), Edelberg, Eichenbaum, and Fisher (1999), Eichenbaum-Fisher (2005), Cavallo (2005) | Quarterly, 1947 – late 1990s or 2000s | Dynamic simulations or VARs using Ramey-Shapiro dates, which are based on narrative evidence of anticipated military buildups | 0.6 – 1.2, depending on sample and whether calculated as cumulative or peak. |
| Blanchard-Perotti (2002) | Quarterly, 1960 - 1997 | SVARS, Choleski decomposition with G ordered first | 0.9 to 1.29, depending on assumptions about trends. |
| Mountford-Uhlig (2009) | Quarterly, 1955 - 2000 | Sign restrictions on a VAR | 0.65 for a deficit-financed increase in spending. |
| Romer-Bernstein (2009) | Quarterly | Average multipliers from FRB/US model and a private forecasting firm model | Rising to 1.57 by the 8 th quarter |
| Cogan, Cwik, Taylor, Wieland (2010) | Quarterly, 1966 – 2004 | Estimated Smets-Wouters Model | 0.64 at peak |
| Ramey (2011) | Quarterly, 1939 - 2008 and subsamples | VAR using shocks to the expected present discounted value of government spending caused by military events, based on | 0.6 to 1.2, depending on sample. |

| | | | |
|---|---------------------------|---|---|
| | | narrative evidence | |
| Fisher-Peters (2010) | Quarterly, 1960 – 2007 | VAR using shocks to the excess stock returns of military contractors | 1.5 based on cumulative effects. |
| Auerbach- Gorodnichenko (2011) | Quarterly, 1947 - 2008 | SVAR that controls for professional forecasts, Ramey news. Key innovation is regime switching model | Expansion: -0.3 to 0.8 Recession: 1 to 3.6 |
| Gordon-Krenn (2010) | Quarterly, 1919 - 1941 | Choleski decomposition in VAR | 1.8 if no capacity constraints |

Table 2. Examples of Cross-State Analyses

| Study | Type of Data | Identification | Results |
|---|---|--|---|
| Davis, Loungani, Mahidhara (1997) | Military prime contracts, military personnel, panel of states 1956 - 1992 | Panel VAR, with military variables ordered after oil but before other variables. | Cost of job created ranges from \$34,000 to \$400,000 (in \$2010), depending on employment data source and allowance for spillovers. Decreases in military spending have larger effects than increases. |
| Hooker-Knetter (1997) | Military procurement contracts, panel of states 1963-1994 | Assume military procurement contracts uncorrelated with state economy | Elasticity of nonfarm payroll employment to real military contracts per capita is 1.8; decreases in military spending have larger effects than increases. |
| Fishback-Kachanovskaya (2010) | Various types of New Deal Spending, panel of states, 1930-1940 | Interaction of swing voting and aggregate government spending | Income multiplier of -0.57 to 1.67, depending on type of spending; negligible impact on employment |
| Cohen, Coval and Malloy (2010) | Federal earmarks, Panel of states, 1967-2008 | Whether state senators and representatives control powerful committees | Decrease in corporate employment, investment and R&D, suggesting crowding out of private activity |
| Chodorow-Reich, Feiveson, Liscow and Woolston (2010) | Medicaid spending from ARRA in cross-section of states, Dec. 2008 - June 2010 | Variations due to pre-recession medicaid spending | \$100,000 in spending results in 3.5 job years |
| Wilson (2011) | Total ARRA spending in cross-section of states, | State ARRA spending instrumented by | Approximately \$25,000 per job created, but job is |

| | | | |
|-------------------------------------|--|---|--|
| | Feb. 2009 - Feb. 2010, with Oct. 2010 follow-up. | Wall Street Journal forecasts | short-lived. |
| Shoag (2010) | State government spending, panel of states, 1987-2008 | Changes in state spending caused by excess returns to state pension fund returns | Income multiplier around 2; each \$35,000 generates one additional job |
| Clemens & Miran (2011) | State government outlays, panel of states, 1988-2004 | Interaction of state balanced budget rules with business cycle. | 0.3 to 3, depending on specification. Standard errors are large. |
| Nakamura-Steinsson (2011) | Military prime contracts, panel of states | State-specific sensitivity to aggregate changes in military spending | 1.5 income multiplier |
| Serrato and Wingender (2011) | Federal spending on localities, panel of counties, 1970-2009 | Changes in federal spending on states caused by updates of population estimates based on the Census | 1.88 income multiplier; \$30,000 per job created |

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