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Local and Aggregate Fiscal Policy Multipliers☆

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Abstract

In this paper, we estimate the effect of defense spending on the U.S. macroeconomy since World War II. First, we construct a new panel dataset of state-level federal defense contracts. Second, we sum observations across states and, using the resulting time series, estimate the aggregate effect of defense spending on national income and employment via instrumental variables. Third, we estimate local multipliers using the state-level data, which measures the relative effect on economic activity due to relative differences in defense spending across states. Comparing the aggregate and local multiplier estimates, we find that the two deliver similar results, providing a case in which local multiplier estimates may be reliable indicators of the aggregate effects of fiscal policy. We also estimate spillovers using interstate commodity flow data and find some evidence of small positive spillovers, which explain part of the (small) difference between the estimated local and aggregate multipliers. Across a wide range of specifications, we estimate income and employment multipliers between zero and 0.5. We reconcile this result with the greater-than-one multipliers found in Nakamura and Steinsson (2014) by analyzing the impact of the Korean War episode in the estimation.

Keywords: fiscal policy, fiscal multipliers, spillovers

JEL: E62, H30, H56, H57

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1. Introduction

It would be difficult to overstate the need for economists and policymakers to understand the payoff of countercyclical fiscal policies. In large part, this is because these policies are typically very expensive. For example, the total budget impact of the most recent U.S. stimulus (i.e., the American Recovery and Reinvestment Act of 2009) was $840 billion. This is more than the congressional appropriations for military operations in Iraq since the 9/11 attacks, which totaled roughly $815 billion.[1]

The question of the effectiveness of these kinds of policies has received substantial empirical attention; recent research progress has advanced primarily along two fronts.[2] First, one set of studies analyzes macroeconomic time series using either narrative or structural vector autoregression (VAR) methods to infer the effect of exogenous identified shocks.[3] The benefits of this approach are that the resulting estimates capture general equilibrium effects and can be interpreted directly as the consequence of exogenous fiscal policy. Hurdles facing this literature include the endogeneity of fiscal policy, a limited number of observations, potentially weak instruments and potential anticipation effects caused by forward-looking firms and households.

More recently, a second set of studies uses cross-sectional variation in fiscal policies to estimate the effect of policy on regional economic activity.[4] The estimates resulting from these studies are known as "local multipliers." This approach often can overcome some of the first method's hurdles. By looking at regional data, the number of observations can be increased significantly. Also, the cross-sectional approach gives researchers greater scope

[1] See Belasco (2014) and Congressional Budget Office (2015).

[2] There is also a third front: using dynamic stochastic general equilibrium models to estimate the effects of government spending. Examples include Cogan, et. al. (2010) and Drautzburg and Uhlig (2015). Additionally considering this approach is beyond the scope of the current paper.

[3] See, for example, Blanchard and Perotti (2002), Edelberg, Eichenbaum and Fisher (1999), Mountford and Uhlig (2009), Ramey (2011a) and Romer and Romer (2010).

[4] See, for example, Chodorow-Reich et al. (2012), Clemens and Miran (2012), Conley and Dupor (2013), Nakamura and Steinsson (2014), Shoag (2012), Suárez Serrato and Wingender (2014) and Wilson (2012).

to find specific historical episodes and fiscal policy interventions from which to construct a statistically strong and conceptually credible instrument. The downside of the second approach is that it informs policymakers about the relative effects of a policy across regions, but not necessarily its aggregate effects.[5] If, for instance, stimulus spending in one state induces workers to immigrate from other states, the resulting local multiplier would be an upwardly biased estimate of the aggregate multiplier because it fails to account for the negative spillover on states that did not receive stimulus funds.

Our paper compares and then integrates the local and aggregate multiplier approaches. In doing so, we make five contributions. First, we construct a new panel of annual federal defense contracts at the state level.[6] Second, we aggregate the state-level data and use defense spending changes, following Hall (2009), in order to estimate the effect of national defense spending on national income and employment.[7]

Third, having estimated aggregate multipliers, we then use the state-level defense data to estimate local income and employment multipliers. We find that the estimated aggregate and local multipliers are similar to one another for both employment and income. By estimating both types of multipliers using the same dataset and identification scheme, these results provide the first empirical example in this literature to show that the local multipliers may provide reliable information about the aggregate effects of fiscal policy.

Fourth, we show how the disaggregate data can be used to improve our understanding of the aggregate effects of fiscal policy. For starters, it is important to recognize why local and aggregate multipliers might differ. This is because of spillovers across states. Sources

[5]This issue with the local multiplier approach has been recognized by several authors. See, for example, Nakamura and Steinsson (2014) and Ramey (2011b). In his description of this issue, Cochrane (2012) puts it succinctly: "Showing that the government can move output around does not show that it can increase output overall."

[6]By state-level defense contracts, we mean federal military procurement that occurs within a state's geographic borders. Other papers that use federal military procurement at the state-level are Hooker and Knetter (1997) and Davis, Loungani and Mahidhara (1997).

[7]Other papers that use military spending changes as an exogenous source of variation include Barro and Redlick (2011) and Sheremirov and Spirovska (2016).

of spillovers might include movements in factors of production, trade in goods, common monetary policy or common fiscal policy, among others. As an example, if government purchases in state X increase income of state X residents, who in turn import more goods from state Y, then the local multiplier will be a downward-biased estimate of the aggregate multiplier because of a positive spillover.

Bearing this in mind, we extend the local multiplier approach to include the spillover effects of defense spending in one state on the economic activity of another state. We operationalize this by considering changes in defense spending on a state's major trading partner and simultaneously estimating direct effect and spillover effect coefficients.[8] The sum of the two gives a multiplier that provides a better approximation of the aggregate effect of government spending.

We do find estimates of small positive spillovers between each state and its major trading partner. Summing the direct and spillover effect of government spending delivers an estimate with the state-level panel that is closer to the multiplier estimated with aggregate data.

The three estimation techniques explained above all point to one of the paper's main conclusions: Across a wide range of specifications, we estimate income and employment multipliers between zero and 0.5.

Fifth, we reconcile our finding of small income multipliers with the greater-than-one multipliers found in a related study by Nakamura and Steinsson (2014). We do so by analyzing the impact of the Korean War episode in the estimation. We make three key points: (1) The addition of these data turns out to be crucial in estimating aggregate multipliers because, without the Korean War years, there is too little variation in defense spending to deliver precise estimates. This point has been recognized in Hall (2009) and Ramey (2011a). (2) The inclusion of these data leads to significantly smaller income multipliers. (3) We argue that it is appropriate to use Korean War data to draw conclusions about how

[8]The two papers most closely related to ours, with respect to estimating spillovers, are those by Dupor and McCrory (2016) and Suárez Serrato and Wingender (2014). Those papers find positive spillovers between geographically neighboring states.

government spending affects the economy in normal times. In particular, we contend that wartime conditions during the conflict in Korea were not nearly as extreme as the exceptional circumstances that characterized World War II's command economy.

2. A New Defense Contract Dataset

There is a particularly powerful argument for using a nation's defense spending as a source of *exogenous* variation in government spending. Defense spending is plausibly exogenous with respect to a nation's business cycle because it is more likely driven by international geopolitical factors, rather than an *endogenous* countercyclical stimulus policy. The case is especially strong for the United States. Over the past century, U.S. military spending has not been associated with a war on domestic soil but rather engagement abroad. As such, researchers need not deal with the confounding effects of military spending and the associated destruction caused by wars fought at home.

If one focuses on macroeconomic post-WWII data (as many researchers have), then one butts up against the problem of a small sample size. A straightforward way to circumvent this problem, as taken by Owyang, Ramey and Zubairy (2013) and Ramey and Zubairy (2014) for example, is to include pre-World War II data. While the increase in the sample is beneficial, this approach relies on the assumption that the mechanism by which defense spending influences the economy is relatively unchanged over long spans of history. An alternative approach to increasing the number of observations is to exploit cross-sectional variation in addition to time series variation. We follow this approach here.

We construct a new panel dataset of U.S. state-level defense contracts between 1951 and 2014. Our data add more than 20 years over otherwise comparable existing data. The longest panel of defense spending in previous research covers 1966 through 2006.[9]

The data are from two sources. The first source consists of two sets of annual reports that were published by the same organization using the same underlying data: the *Prime Contract Awards by State* report and the *Atlas/Data Abstract for the US and Selected Areas.*

[9]See Nakamura and Steinsson (2014).

It was necessary to draw upon these two reports (as opposed to using one of them only) due to unavailability of documents for certain years; in general, the first report provides data for 1951 through 1980, and the second document was used for the years 1981 through 2009.[10] The second source, which provides data for the 2010-2014 period, is a U.S. government website: www.USAspending.gov. We now proceed to describe the nature of the data in detail.

The *Prime Contract Awards by State* report and the *Atlas/Data Abstract for the US and Selected Areas*—both published annually by the Directorate for Information Operations and Reports[11]—contain military contract data aggregated to the state level between fiscal years 1951 and 2009. These data cover military procurement actions over $10,000 up to 1983 and over $25,000 thereafter. The reports present data by principal state of performance: Manufacturing contracts are attributed to the state where the product was processed and assembled, construction and service contracts are attributed to the state where the construction or the service was performed. However, for purchases from wholesale firms and for transportation and communication services contracts, the contractor's business address is used.

The data between 2010 and 2014 are from USAspending.gov. The contracts data available from this source are also attributed to the state where the work is performed. The USAspending.gov numbers include "Grants" and "Other Financial Assistance," which we are unable to disentangle from contracts in the state level data. However, the other two components represent a very small portion of the funds awarded by the Department of Defense: at the national level (where the website does present the data by these three types of funds) contracts represent 99.99% of the funds awarded by the Department of Defense in 2010. Furthermore, the USAspending.gov data go back to 2007, which gives us three years

[10]For some years, we accessed the data directly from these sources, and for the remaining years we accessed the data via the Statistical Abstract of the United States, which in each issue cites one of these reports as the source.

[11]The Directorate for Information Operations and Reports is part of the Washington Headquarters Services, the essential services provider for the Department of Defense.

of overlap between our two data sources to check for consistency in the splicing procedure.

Our sources report data on prime contracts only and do not provide information on subcontract work. Thus, a valid concern is the extent of interstate subcontracting, that is, work that may have been done outside the state where final assembly or delivery took place. Nakamura and Steinsson (2014) faced the same issue and compared their prime contracts data to a dataset on shipments to the government from defense industries, reported by the U.S. Census Bureau from 1963 through 1983. They observed, on average, a one-for-one relationship between the prime contracts attributed to a state and the shipments data from this state. This suggests that the prime contracts data accurately reflect the timing and location of military production.[12]

Another valid concern with our data regards the timing of actual spending, since our sources only provide the fiscal year in which contracts were awarded, which might not necessarily coincide with the year(s) in which the work is performed. Moreover, the fact that our spending variable is reported in fiscal years while all other variables are reported in calendar years already introduces some error in the timing. Yet, these considerations should be mitigated because we estimate the effects of spending as 2-year or 4-year cumulative changes rather than one-year changes. A related concern is whether and how households and firms respond to anticipated changes in defense spending, that is, before contracts are awarded. This issue is explored in Section 4.

Our data, aggregated across states, are plotted in Figure 1 as the blue line with box markers. The time series evolves as one might expect. The dollar value of defense contracts at the start of the sample was high due to the Korean War. There is a decline in spending associated with the military drawdown that followed. The next two hump-shaped movements

[12]Even though there is historical data on prime contract awards at the county (and even metropolitan area) level, the Department of Defense warned that "because of the extent to which subcontracting occurs and because precise knowledge is lacking concerning the geographic distribution of these sub-contracts, any breakdown of prime contract awards below the State level must be considered to contain a built-in error so great as to obviate the validity of any conclusions" (Isard and Ganschow, 1961).

in spending occur in the 1960s and the 1980s, resulting from the Vietnam War and the Reagan military buildup. The final rise and then decline begin in 2001 due to the wars in Afghanistan and Iraq.

For comparison, we also plot contracts plus total U.S. Defense Department payroll (civilian and non-civilian defense personnel) as the green line with circles. Including payroll spending with contracts has the advantage of giving a more comprehensive indicator of defense spending; however, it suffers from the fact that it excludes the Korean War episode.

In addition, we plot total defense-related consumption and gross investment by the federal government (red line with diamonds) as measured by the Bureau of Economic Analysis (BEA). As shown in the figure and perhaps underappreciated in this literature, on average, reported military procurement and payroll data constitute only about half of total defense spending as reported in the national income and product accounts. This difference is explained by the fact that the BEA series includes consumption of fixed capital, employee benefits, and other expenses. As a robustness check, in Section 4.1 we replicate our aggregate estimations using the BEA-measured military spending.

3. Variable Definitions

Our analysis considers two different outcome variables: employment and personal income. Let $N_{i,t}$ denote employment in state i during year t. Employment consists of total nonfarm employment and is reported by the Bureau of Labor Statistics.[13] Similarly, let $Y_{i,t}$ and $G_{i,t}$ denote the real per capita year t, state i income and defense contracts, respectively. The raw state personal income data are nominal and available from the BEA. We use state personal income rather than gross state product because the latter data are not available for

[13]Employment data are missing for Michigan (before 1956), Alaska (before 1960) and Hawaii (before 1958). We impute these values by regressing the state employment-to-population ratio on the insured unemployment rate for each of the three states. Total nonfarm employment excludes the Department of Defense's military and civilian personnel. Our results are robust to the addition of military employment (see Section A in the online appendix).

years prior to 1963. The contract data are described in the previous section. Both personal income and defense contracts are scaled by the national Consumer Price Index (CPI) and state population.

Let $N^c_{i,t,\delta}$ be the cumulative percentage increase in employment over a δ-year horizon relative to a year $t-1$ employment baseline in state i:

$$N^c_{i,t,\delta} = \left(\sum_{j=1}^{\delta} N_{i,t+j-1} - \delta N_{i,t-1} \right) / N_{i,t-1} \tag{1}$$

Next,

$$G^c_{i,t,\delta} = \left(\sum_{j=1}^{\delta} G_{i,t+j-1} - \delta G_{i,t-1} \right) / Y_{i,t-1} \tag{2}$$

This is the cumulative increase in defense spending over a δ year horizon relative to a year $t-1$ military spending baseline, all of which are scaled by $Y_{i,t-1}$.[14] Finally,

$$Y^c_{i,t,\delta} = \left(\sum_{j=1}^{\delta} Y_{i,t+j-1} - \delta Y_{i,t-1} \right) / Y_{i,t-1} \tag{3}$$

Let $N^c_{t,\delta}$, $G^c_{t,\delta}$ and $Y^c_{t,\delta}$ denote the aggregate analogs of their state-level counterparts.

Defining these variables as such permits us to estimate cumulative multipliers.[15] Cumulative multipliers give the change in income (or employment) accumulated over a specific horizon with respect to the accumulated change in military spending over the same horizon. Also, scaling by $Y_{i,t-1}$ in $G^c_{i,t,\delta}$ implies that this variable should be interpreted as the change in military spending as a percentage of one year of income.

4. Aggregate Multipliers with Aggregate Data

Before working with these data at the state level, we aggregate the data to the national level and estimate national income and employment multipliers using a now standard frame-

[14]Scaling by $Y_{i,t-1}$ allows for a natural interpretation of the multiplier (dollar change in income per dollar of government spending). This specification is used in Hall (2009), Barro and Redlick (2011), Owyang, Ramey and Zubairy (2013), and Sheremirov and Spirovska (2016), among others. See Section B in the online appendix for alternative specifications.

[15]Ramey and Zubairy (2014) argue compellingly that cumulative multipliers are more useful from a policy perspective than other (sometimes reported) statistics, such as peak multipliers and impact multipliers.

work: the Hall defense spending approach. This allows us to verify that our new dataset generates aggregate results similar to those in existing research.

We estimate the model using the generalized method of moments (GMM), which in this case has a two-stage least squares (2SLS) interpretation. Also, we report heteroskedasticity and autocorrelation (HAC) corrected SEs throughout the paper.[16]

The second-stage equation for the income regression is:

$$Y^c_{t,\delta} = \phi_\delta G^c_{t,\delta} + \beta_\delta X_t + v_{t,\delta} \quad (4)$$

for $\delta = 0, 1, ..., D$. Here X_t consists of four macro variables. The variables are the growth rate of the price of oil, the real interest rate and one lag of each of these.[17] We include the real interest rate to reflect the influence of monetary policy and include the price of oil as a measure of "supply factors" influencing the economy. The coefficient ϕ_δ is then the cumulative percentage increase in national income through horizon δ in response to an increase in national military spending (cumulative through horizon δ) equal to 1 percent of national income. Thus, it is the cumulative aggregate income multiplier of defense spending. In the first stage, we use one-year innovations to defense spending ($G^c_{\delta,1}$) as an instrument for $G^c_{\delta,t}$, for reasons explained above.

At each successively longer horizon, we lose one additional observation (in order to calculate $Y^c_{\delta,t}$ and $G^c_{\delta,t}$). To make estimates comparable across horizons, we fix the sample and estimate the model for each δ using the sample containing the largest horizon (i.e., $\delta = 4$).

We also estimate the cumulative employment multiplier using equation (4), except that we replace $Y^c_{t,\delta}$ with $N^c_{t,\delta}$. Table 1 contains estimates of the income and employment multipliers at two different horizons.

The income multiplier at the 2-year horizon is shown in column (1) of Table 1. The coefficient equals 0.33 (SE = 0.12). Thus, if there is a cumulative increase in military spending equal to one percent of national income over a 2-year horizon in response to a

[16] We compute the estimates using Stata V.14 and the *ivreg2* command with the options *robust* and *bw*.

[17] The real interest rate is measured as the average 3-month Treasury Bill rate minus the year-over-year CPI growth rate.

defense spending shock, then the cumulative change in national income equals 0.33% over the same horizon. The point estimate implies that the short-run national income multiplier is substantially less than one. One can reject a multiplier greater than 1 with over 99% confidence.

We assess the strength of the defense spending instrument by reporting the Kleibergen-Paap partial F-statistic for each specification. In all but one case, these values are well above the standard rule-of-thumb threshold of 10 required for the validity of the strong instrument approximation to hold.

Next, column (2) in Table 1 contains the 4-year income multiplier. The point estimate equals 0.07 (SE = 0.24). The results in columns (1) and (2) of Table 1 will reflect a robust conclusion of this paper. Aggregate income multipliers are estimated to be well below 1 and often statistically not different from zero.

Columns (3) and (4) contain the analogous results except employment is instead used as the dependent variable. The 2-year employment multiplier estimate equals 0.39 (SE = 0.11). Thus if military spending increases by one percent of national income, then employment increases by 0.39%. The 4-year employment multiplier estimate equals 0.24 (SE = 0.21). Both at the 2- and 4-year horizons, there is a muted response of employment to an increase in military spending.

Next, we trace the dynamic path of the income and employment multipliers as one varies the horizon δ. Panel (a) of Figure 2 plots the income multiplier; the dots represent the point estimates and the solid lines envelope the pointwise robust 90% confidence interval. The cumulative income multiplier path is smooth. The multiplier is between zero and 0.4 over the entire horizon. Apart from the first two years, the estimates are not statistically different from zero.

Panel (b) of Figure 2 plots the point estimates and 90% confidence interval of the employment multiplier (as a function of the horizon). The coefficient should be interpreted as the percentage growth in employment (accumulated over a particular horizon) in response to an exogenous defense spending increase (accumulated over the same horizon) equal to 1%

of national income. The estimate is stable between roughly 0.2 and 0.4 over every plotted horizon.

A potential concern, examined in depth by Ramey (2011a), is the importance of capturing anticipation effects when identifying government spending shocks. The idea is that businesses and households may react to expected changes in defense spending even before a contract is awarded. Failing to account for this might lead to biased results. To deal with potential anticipation effects, Ramey puts together a narrative measure of defense news shocks: From historical documents, she constructs a time series of innovations to the present discounted value of the sequence of future military expenditures, which she then scales by that year's nominal GDP.

To assess the timing issue in our data and potential anticipation effects not captured by our baseline specification, we reproduce our aggregate estimates using Ramey's news instrument. This exercise yields a 2-year cumulative income multiplier of 0.58, which is slightly higher but qualitatively similar to our baseline estimate of 0.33 presented in Table 1.[18] This result should mitigate concerns about failing to capture anticipation effects in the aggregate case. Unfortunately, there is not a similar news series to use at the state level. Nevertheless, if accounting for anticipation effects does not dramatically change the estimate of the aggregate multiplier, there is arguably no reason to believe that expectations would play a critical role at the disaggregate level.

4.1. Comparison with other military spending measures

One concern may be that our defense spending measure is not representative of overall U.S. military spending. To address this issue, we compare the income and employment multipliers based on the aggregated contract data with the same specification estimated using total BEA-measured defense spending.

[18]The use of Ramey's news instrument requires a longer sample to avoid a weak instrument problem, and thus we resort to using the BEA-measured defense spending data to make the comparison. As explored in Section 4.1, this alternative spending measure yields very similar multipliers compared to our aggregated contracts series. The regression results for this exercise are reported in Section C of the online appendix.

Panel (a) of Figure 3 plots the estimated income multipliers using the BEA defense measure (red "x" marker) and the associated 90% confidence interval (red dashed lines). For comparison, we plot the benchmark estimates—that is, using the aggregated contract data, using green circles and solid lines for the 90% confidence intervals. The figure shows that: (i) the point estimates are similar across the two specifications, and (ii) there is substantial overlap of the confidence intervals.

Panel (b) of Figure 3 plots the analogous estimates but for the employment rather than the income multipliers. The confidence intervals share a similar shape. Both result in employment multipliers between (roughly) 0 and 0.5. Figure 3 is reassuring in that our new measure of military spending gives income and employment multipliers that are similar to those based on a more traditional aggregate defense spending measure.

5. Local Multipliers with State-Level Data

In this section, we estimate income and employment multipliers using state-level data. As described in the introduction, these multipliers do not necessarily inform researchers about the aggregate effect of government spending. Rather, they tell us about the relative effect on income (or employment) across states due to relative differences in defense spending across states. These are known as "local multipliers" in the literature. These multipliers do not account for potential cross-state spillovers due to trade in goods, factor mobility or shared macroeconomic policies.

Many papers have estimated local multipliers; nearly all include the caveat that local multipliers cannot be interpreted as aggregate multipliers. Unfortunately, in public policy discussions, commentators regularly ignore this caveat and interpret local multiplier evidence to incorrectly infer the aggregate effects of fiscal policy.[19] To our knowledge our paper is the first to use the same dataset to estimate both local multipliers and aggregate multipliers.

It appears that the primary reason that this comparative analysis has, heretofore, not been done is because the existing studies primarily use cross-sectional data. Without suffi-

[19]See, for example, Boushey (2011), Glaeser (2013), Greenstone and Looney (2012) and Romer (2012).

cient time series variation, it is unclear how one might identify the spillover (and therefore the full aggregate) effect of fiscal policy without bringing significantly more economic structure to the problem.

The estimation equation is

$$Y^c_{i,t,\delta} = \psi_\delta G^c_{i,t,\delta} + \pi_{i,\delta} X_t + w_{i,t,\delta} \tag{5}$$

In our baseline specification, we also include both state and year fixed effects. X_t is the same set of control variables as in the aggregate regression. In each use of the panel data, we estimate the model using weights given by a state's share of the national population, averaged across every year.[20]

The coefficient ψ_δ is interpreted as the cumulative local income multiplier at horizon δ, or simply the local income multiplier at δ. It gives the relative change in state income between two states given a relative increase in government spending between those two states.

We require an instrument to estimate (5). The instrument should vary over both time and states. Some state-level changes in military expenditure may be endogenous to state-level business cycle conditions. For example, if states in severe downturns are more likely to receive military contracts relative to other states, then failing to correct for this endogeneity would likely bias our estimates of the multiplier downward.

We construct an instrument $Z_{i,t}$ that deals with both issues. It is given by

$$Z_{i,t} = \left(s^G_{i,t}/s^Y_{i,t}\right) G^c_{t,1}$$

This is the one-period national defense spending growth multiplied by a state-specific scaling factor. The scaling factor is the ratio of a state's share of national military spending, $s^G_{i,t}$, divided by the state's share of national income, $s^Y_{i,t}$. Both shares are computed as the state's averages in year $t-1$ and $t-2$. Our approach for generating a state-specific time-varying instrument is motivated by Bartik (1991). Using lagged shares of military spending reflects

[20] Our results do not change significantly if we remove the population weights. Regression results without weighting are available upon request.

the idea that the distribution of new future spending across states is related to how much spending each state will receive in the future. By using lagged values of the shares, we seek to mitigate the potential endogeneity resulting from the current state-specific business cycle in the cross-state allocation of contracts.

The punchline of the analysis in this section is that the aggregate and corresponding local multipliers do not vary substantially from each other. While the estimates differ somewhat, for example, the 2-year local and aggregate income multipliers all are between -0.01 and 0.33.

Panel (a) of Table 2 contains estimates of the 2-year local income multiplier from the state-level panel under various specifications. Column (1) reports the multiplier and partial F-statistic when we include neither state nor year fixed effects. The coefficient equals 0.23 (SE = 0.06).

Column (2) in Table 2 augments the column (1) specification by adding state fixed effects. This has a negligible impact on the multiplier estimate. Column (3) includes year fixed effects and no state effects, while column (4) includes both state and fixed effects. These last two specifications lead to declines in the income multiplier. The multiplier in column (4) equals -0.01. We also report the corresponding benchmark aggregate multiplier in column (5) estimated earlier in the paper. Note that the aggregate multiplier is very similar to the local multipliers in columns (1) and (2), but somewhat different from those in (3) and (4). The difference in estimates is likely due to the use of time fixed effects, which eliminate potential aggregate or "spillover" channel of the government spending shocks.

Panel (a) of Table 2 also contains estimates of the 4-year cumulative income multiplier. The aggregate multiplier reported in column (5) equals 0.07 (SE = 0.24). Two of the corresponding local multipliers, one with no fixed effects and one with state fixed effects only, are estimated to be 0.07 and 0.05. These estimates are encouraging in that these two local multipliers are similar to the aggregate multiplier; moreover, there is a more than 60% reduction in the SE.

The situation changes only somewhat with the inclusion of year fixed effects only (column

(3) in Table 2) or both state and year fixed effects (column (4)). The corresponding estimates of the local multipliers are 0.11 and 0.05.

Next, Panel (b) of Table 2 presents the 2-year and 4-year cumulative local employment multipliers.

At the 2-year horizon, the aggregate employment multiplier equals 0.39, while the local multipliers range from 0.03 to 0.30 depending on whether and how fixed effects are introduced. The local multipliers are also similar in magnitude to the aggregate employment multiplier estimate at the 4-year horizon.

The above results based on state-level data are encouraging for a researcher hoping to learn something about aggregate policy effects from disaggregate data. The main caution is that using time fixed effects sometimes reduces the local multiplier estimates towards zero in relation to the aggregate multipliers.

In the following section, we extend the usefulness of the panel data to show how one can (i) disentangle the direct effect from the spillover effect, and (ii) get a more accurate estimate of the national effect of fiscal policy using state-level data.

6. Estimating Spillovers

Earlier in the paper, we present evidence that the estimated local fiscal multiplier is similar to the aggregate estimate. Thus, this seems to indicate that the caveats that often arise in the discussion of local multiplier estimates, such as spillovers, common monetary policy, and factor mobility, are not quantitatively relevant in this particular case. In this section, we explore the role of interstate spillovers in the multiplier estimation.

We estimate the state-level regression except we add as an independent variable the accumulated change in defense contracts as a fraction of income of the state's major trading partner. The inclusion of this term is meant to capture potential spillovers of defense spending. The second-stage equation for the income regression is

$$Y^c_{i,t,\delta} = \gamma^Y_\delta G^c_{i,t,\delta} + \phi^Y_\delta \tilde{G}^c_{i,t,\delta} + \beta^Y_{i,\delta} X_t + v^Y_{i,t,\delta} \tag{6}$$

where $\tilde{G}_{i,t}$ is per capita defense spending in year t of state i's major trading partner.

We define a state's major trading partner using 2007 Commodity Flow Survey data as follows: State i's major trading partner is the destination state j with the largest total value of commodities that flow from i to j, divided by j's population. That is,

$$\text{Major trading partner}_i = \underset{j \neq i}{\text{argmax}} \frac{V_{i,j}}{Pop_j}, \text{ for } i, j \in \{\text{U.S. states}\} \tag{7}$$

where $V_{i,j}$ is the value of total shipments from state i to state j in 2007 and Pop_j is state j's population in that year. For instance, California is Hawaii's major trading partner because the per capita value of commodities that flow from Hawaii to California is greater than that of commodities that flow from Hawaii to any other state. Note that Nevada is California's major trading partner.[21]

In addition to the instrument $Z_{i,t}$ described previously, we also include $\tilde{Z}_{i,t}$ as an instrument for spending so that the new model is identified. State fixed effects are also included in our benchmark specification.

Since the Commodity Flow Survey has been conducted only five times, starting in 1993, we are unable to calculate a state's major trading partner for each year of our sample. Hence, we only use data from the 2007 survey and assume that the paired states have been important trading partners throughout the rest of our sample. This is a reasonable assumption as the definition in (8) results in major trading partners that share borders in almost all cases.[22]

In previous sections we have estimated the aggregate multiplier from aggregate data and the local multiplier from state-level data, we now present the results of an intermediate approach that uses state-level data to estimate a multiplier that partially accounts for interstate spillovers. That is, the estimates here should presumably be closer to the aggregate effect. The total multiplier from the state-level data is defined as the sum of the coefficient on state spending (i.e., the direct multiplier) and the coefficient on the partner's spending (i.e., the spillover multiplier). The thought experiment is to suppose that the government increases defense contracts by 1% of state income accumulated over a particular horizon in

[21]For a list of each state's major trading partner, see Section D in the online appendix.

[22]The only exceptions are Tennessee—whose major trading partner is Maryland—and, of course, Alaska and Hawaii, whose major trading partners are Washington and California, respectively.

every state. Then, from a state's perspective, we are estimating two effects: the effect of the increase in own-state contracts on own-state income and the spillover effect of the increase in contracts of the state's major trading partner on own-state income.

First, own-state contracts would increase and thus have an effect on own-state income. Second, contracts in the state's major trading partner would increase and have a second (spillover) effect on own-state income. The sum of these two effects is the national multiplier.

The income multiplier estimates appear in Panel (a) of Table 3. Column (1) gives the results for equation (6) at the 2-year horizon. The state spending coefficient equals 0.18 (SE = 0.06). The corresponding coefficient on the major trading partner's spending is 0.07 (SE = 0.07), though this effect is not statistically different from zero. The aggregate multiplier equals 0.25, the sum of the state and partner spending coefficients.

Panel (b) of Table 3 presents analogous results for the employment multiplier. At both horizons, we observe small positive spillovers and cumulative employment multipliers well below 1.

Table 4 summarizes the three methods we have used thus far to estimate income and employment multipliers. Columns (1) and (4) show the local multiplier approach with state-level data presented in Section 5, columns (2) and (5) also use state-level data but add a spillover term, and columns (3) and (6) present the aggregate multiplier estimated with aggregated data first shown in Section 4.

The results in Table 4 show that the inclusion of the spillover term shrinks the already small gap between the local multiplier and the aggregate multiplier. Although we have considered alternative definitions of the spillover region (all bordering states, all other 49 states, etc), any broader measure captures the national shock component and loads up all the effect of government spending in the spillover term. This is similar to what the inclusion of year fixed effects causes in Table 2.

While the multiplier from the state-level panel data and from the aggregated time series are not identical, they are quantitatively very similar. Both point estimates imply a 2-year cumulative multiplier that is close to 0.30.

Note also, the SEs are substantially lower using the state-level data. Specifically, the SE falls from 0.12 in column (3) to 0.07 in column (2). This is because there are many more observations of how an individual state responds to national spending than there are observations of how the nation as a whole responds to national spending.

The smaller SE's rely on the assumption of zero spatial correlation of the error term. To correct for spatial dependence, we report Driscoll-Kray standard errors in brackets in the state-level regressions of Table 4. With this correction the standard errors for the multipliers using the state-level data increase substantially and are very close to the standard errors of the aggregate multiplier based on aggregate data. Thus, the ability of using disaggregate data to improve the precision of national income multipliers relative to using aggregate data alone depends upon the stand one is willing to take regarding the spatial correlation between error terms present in the data.

We end this section by stressing that although the results presented seem to indicate that spillover effects are small and that the local multiplier is a good indicator of the aggregate effects of fiscal policy, this only reflects the findings of a particular case (US military spending data at the state level in the postwar era), and should not be regarded as a general conclusion.

7. Local Multiplier Estimates and the Influence of the Korean War

In this section, we show the influence that excluding the Korean War period has on the local multiplier estimate. Excluding this period dramatically increases the multiplier estimate. This is important because existing work by Nakamura and Steinsson (2014), that is based on post-1965 data, estimates a local multiplier that is greater than one.

To compare our results with Nakamura and Steinsson (2014), we first adopt a specification that closely mimics theirs. There are three substantive differences, besides their shorter sample, between their specification and ours. First, they use per capita output rather than income as the dependent variable. Second, they instrument by using an interaction of two-year national military spending growth with a state dummy.[23] Third, they draw their military

[23]Nakamura and Steinsson (2014) use two year growth rates for their dependent, endogenous and in-

contract data from a somewhat difference source.

Column (1) of Table 5 reports the local multiplier estimate based on equation (5) except we change the sample to match Nakamura and Steinsson (2014), use per capita output rather than income, use their contract data and adopt their instrument. The coefficient on spending equals 1.28, which is a two-year multiplier. This is close to the value 1.4, reported as the baseline specification in Nakamura and Steinsson (2014).

The column (2) specification is identical to that in column (1), except we move from GDP per capita to income per capita. We need to make this switch in order to extend the comparison to include the Korean War period because state-level GDP is not available for this period. The local multiplier equals to 1.04. This is to be expected because personal income is a fraction of GDP.

The column (3) specification is identical to that in column (2) except we switch from the Nakamura and Steinsson (2014) defense spending measure to ours. We emphasize that we continue to use the same years as used in the original Nakamura and Steinsson paper. There is a small change in the estimate by switching to our data; however the estimate remains well above zero.

Now, we are on square footing to ask how extending the dataset to include the additional years affects the local multiplier estimate. To this end, the column (4) specification is identical to the column (3) specification except we add the years 1951 to 1965 and 2007 to 2014 to the sample. The estimate of the local multiplier equals -0.04. The effect is precisely estimated and not statistically different from zero. Thus including these 23 years of data eliminates any causal impact of relative defense spending on relative state income. Note also that the inclusion of this episode dramatically increases the first stage partial F-statistic.

Now that it is clear that the extension of the sample explains the difference between our estimates and the multipliers estimated in related papers, we ask why the impact was so different in these years. For this analysis we look at the aggregate multiplier estimated with

strument variable rather than the cumulative growth rate. This difference has no important effect on the results.

aggregate data.[24]

Figure 4 shows a scatter plot of the two-year cumulative change in aggregate income (after controlling for macro variables) and the two-year cumulative change in aggregate defense spending. The two years that saw the largest decline in national military spending (1953-1954) stand out and are highlighted in red.[25] The remaining observations are shown in blue, as is the best linear predictor for that subsample (dashed line). The best linear predictor for the entire sample is shown in purple (solid line).

The scatter plot reveals that the change in the estimate observed when extending the sample is due to the two observations that capture the military drawdown after the Korean War. In fact, the cumulative 2-year changes in defense spending experienced in 1953 and 1954 were 5.8 and 4.1 standard deviations away from the mean, respectively. As shown by the figure, these years were not associated with a significant decline in aggregate income. Excluding the observations for 1953 and 1954 yields an estimated multiplier of 1.00 (SE = 0.64), a value that drops to 0.25 (SE = 0.25) when the two observations are included.[26]

As Hall (2009) and Ramey (2011a) argue (and the Kleibergen-Paap partial F statistics and standard errors in Table 5 show), the inclusion of the Korean War years is crucial in estimating aggregate multipliers precisely, as there is too little time series variation in aggregate spending when the Korean War episode is excluded.

It is often argued that—due to abnormal wartime conditions—certain war episodes should be excluded when studying the effects of fiscal policy. Some of the atypical features in the U.S. during periods of war include: (i) an unusual increase in savings due to rationing and patriotism, (ii) a surge in taxes to finance military efforts, and (iii) a rise in the labor force participation rate due to conscription. We contend that the nature of economic conditions in the early Fifties does not justify disregarding the information provided by the Korean War

[24]The analogous analysis with state-level data leads to similar conclusions.

[25]Because of the way the cumulative variables are defined, these two observations correspond to data from 1952 to 1955.

[26]The small difference between the estimated multiplier here and those reported earlier in the paper is a result of using OLS instead of instrumental variables estimation.

episode. This becomes apparent when comparing how mild wartime conditions were during this period relative to the exceptional circumstances associated with World War II.[27]

First, we note that the increase in savings experienced during the Korean War was small compared to that of World War II. Hickman (1955) estimated that the ratio of personal saving to disposable personal income before both wars was roughly 5 percent. While this ratio surpassed 25 percent during World War II, it only increased to nearly 10 percent during the Korean War. This difference is partly explained by the fact that there was no rationing in the latter war. Although the U.S. government set price and wage controls during the Korean War, there were no policies comparable to those that led to the conversion from civilian manufacturing to war production during Second World War.[28]

Another factor that explains the difference in the U.S. savings behavior in these two conflicts is the fact that the Korean War, unlike the Second World War, was not financed by issuing debt; only 41 percent of the U.S. expenditures on World War II were financed by taxes, whereas the tax increases during the Korean War financed nearly 100 percent of the defense effort (Ohanian, 1997).

Second, Hall (2009) warns that the multipliers estimated using Korean War data may also be questionable because the war's buildup was accompanied by large increases in tax rates. One might then conjecture that the minimal reduction in income observed in the years of military drawdown after the war could be explained by an unusually large reduction in taxes following the conflict. Nevertheless, Ramey and Zubairy (2014) note that the average tax rate fell gradually after the Korean War; in fact, by 1955 (a year and a half after the

[27] There is no consensus in the literature as to whether World War II data should be used in the estimation of the multiplier. In fact, Brunet (2016) recommends against including World War II data due to the many unusual features of the wartime economy that significantly reduced the stimulative impact of wartime spending.

[28] As Brunet (2016) explains, conversion during World War II was incentivized by expressly prohibiting the production of certain civilian goods (such as civilian vehicles) and by setting up an allocation system for strategic materials, which precluded the obtention of raw materials with strategic value for any purpose other than war production.

end of the war), the average tax rate was still considerably higher than in 1950.

In their narrative analysis of postwar tax changes, Romer and Romer (2009) report that the Revenue Act of 1950, the Excess Profits Act of 1950, and the Revenue Act of 1951 implemented the increase in tax rates necessary to finance the war in Korea. While the tax increase of the first act was legislated to be permanent, the other two were temporary and allowed to expire in the first quarter of 1954. Though the expiration of these tax hikes were partly due to the decline in military spending after the war, Romer and Romer (2009) note that these tax cuts were also allowed to go into effect to offset the scheduled rise in Social Security taxes that quarter.

Third, Ramey and Zubairy (2014) recognize that the dramatic increase in the labor force participation rate during World War II due to conscription and patriotism allowed much more output to be produced relative to normal circumstances. Though there was a draft during the Korean War, its effect on the labor force was minor relative to the World War II experience. During the Second World War the armed forces' percent of the total labor force rose from 0.6 percent to over 18 percent, while the armed forces' share of the total labor force rose from 2.3 percent to only 5.5 during the Korean War. Thus, the effect of conscription on employment was relatively small during the Korean conflict.

8. Conclusion

Though past studies have conceptually addressed the difference between local and aggregate fiscal multipliers, this is the first paper to quantitatively examine this distinction. We do so by comparing state-level and national multipliers estimated with the same dataset. Our dataset is a newly constructed state-level panel of defense spending that extends existing datasets by more than 20 years. We find that the aggregate and state-level approaches deliver similar estimates, which are between zero and 0.5. These results provide the first empirical example in the literature in which local multipliers may be a reliable indicator of the aggregate effects of fiscal policy.

One of the caveats often mentioned in the discussion of local multipliers is that of inter-regional spillovers. In this paper we also estimate spillovers using interstate commodity flow data and find some evidence of small positive income and employment spillovers. This is consistent with our finding of similar state-level and national multipliers. Moreover, the addition of the spillover term shrinks the already small gap between the local and aggregate multipliers.

Finally, this paper reconciles our finding of small multipliers with the greater-than-one multipliers estimated in existing research. We achieve this by assessing the importance of including the Korean War years in our sample. These years provide important time series variation in defense spending for precisely estimating fiscal multipliers. Though the estimation of small multipliers is somewhat driven by two years that saw an enormous decline in military spending, we argue that there is no reason to disregard the information provided by the Korean War episode when studying the effects of defense spending on economic activity.

Our findings suggest several directions for future work. First, since we provide an example of similar local and aggregate multipliers estimated with the same dataset, it would be useful to find other historical periods and datasets toward which one can apply this approach. Perhaps the most promising direction would be to execute the approach taken in this paper for other countries with sufficiently disaggregated military spending data.

Second, we estimate spillovers of defense spending in a state's major trading partner, but one could refine our definition of the spillover region to better capture spillover effects. Moreover, although we find evidence of small spillovers between states, this need not be the case for a finer geographic division. Presumably, one would observe larger spillovers between counties.

Lastly, the evidence that state-level income and employment multipliers may be a reliable indicator of national multipliers opens the door to exploiting cross-sectional variation in fiscal policy research. For example, one can address the issue of whether the size of the multiplier depends on the state of the economy (i.e., the degree of slackness). With aggregate data, slackness can only modeled as a feature of the overall economy. With state-level data, slackness can be state specific. State-specific slackness is not only more realistic, but it also generates additional heterogeneity, which one can exploit in estimation.

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Online Appendix

A. Department of Defense Employment

The employment multiplier results presented in the paper use total nonfarm employment, as measured by the Bureau of Labor Statistics. This variable excludes the Department of Defense's military and civilian personnel. In this section, we examine the stimulative effects of defense spending on total nonfarm employment plus Department of Defense personnel.

A.1. The Data

We compiled the state-level data on the Department of Defense's civilian and active duty military personnel from several reports: *Selected Manpower Statistics (M01)*, *Distribution of Personnel (M02)*, *Atlas/Data Abstract of the United States and Selected Areas*, and *Statistical Abstract of the United States*. The first three documents were published by the Department of Defense, and the last was published by the Census Bureau. The data are available from 1956 through 2009.

In compiling these data we ran into several issues, which we briefly describe next. First, in September 2005, the *Distribution of Personnel (M02)* report began including Navy and Marine Corps personnel in afloat duty status as strength counts of their homeport locations. This led to sharp increases in the 2005 Navy personnel counts for several states. To correct for this, we estimated the number of personnel on afloat duty for the relevant years and subtracted it from the active duty military series. Second, from 1956 to 1966, the Maryland and Virginia counts in the *Selected Manpower Statistics* reports exclude the portion of each state that is part of the DC metro area. To fix this, we assumed that the changes in military employment in the Maryland and Virginia portions of the DC metro area can be approximated by changes in the overall DC metro area. Third, in 1985 the Portsmouth Naval Shipyard was reclassified from New Hampshire to Maine. This shipyard is the most important source of civilian personnel in the area. Thus, we swapped the civilian personnel series for these two states before 1985, effectively assigning the shipyard to Maine.

A.2. Results

Next, we report the results of adding Department of Defense personnel to the employment variable. Table A.1 is analogous to Panel (b) of Table 2 in the paper, except that the dependent variable is constructed using this more comprehensive measure of employment. Since we are restricted to a smaller sample size due to unavailability of Department of Defense personnel data, we also present the results for BLS-measured employment with this restricted sample for comparison.

Table A.1: Aggregate and state-level employment multipliers at a 2-year horizon: With and without Department of Defense Personnel

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Employment + DoD	0.63***	0.62***	1.06***	1.01***	0.49
Personnel	(0.19)	(0.17)	(0.32)	(0.26)	(0.56)
Employment	0.45**	0.43***	0.97***	0.90***	0.26
	(0.18)	(0.16)	(0.31)	(0.24)	(0.54)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	106.69	105.77	28.29	27.67	998.94
N	2592	2592	2592	2592	52

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table A.1 shows that the 2-year cumulative employment multiplier is somewhat higher if one takes into account employment by the Department of Defense. Note, however, that the employment multiplier is higher in this subsample even without the Department of Defense personnel addition. This is consistent with the higher multipliers estimated when excluding the Korean War episode (see Section 7 in the paper). The regression results at the 4-year horizon present a similar picture.

B. Alternative Specifications

In this section we perform some robustness checks by modifying our baseline specification. These results should alleviate any concern that our finding of substantially small multipliers is driven by an attenuation bias.[29] Such bias could arise when dividing both the dependent and independent variables by Y_{t-1} in the presence of a measurement error in Y. In addition, first-differencing I(0) variables could bias our estimates toward zero. As the tables below show, our main results are robust to alternative specifications.

Table B.2 reports the results from our baseline specification (exactly as shown in Table 2 from the paper). To address the concern of a potential attenuation bias in the presence of a measurement error in Y, Table B.3 reports the results of modifying our baseline specification such that we no longer divide both sides by Y_{t-1}. That is, this alternative specification differs from the baseline in that the dependent variable and the main regressor are defined as $Y^d_{i,t,\delta} = \sum_{j=1}^{\delta} Y_{i,t+j-1} - \delta Y_{i,t-1}$ and $G^d_{i,t,\delta} = \sum_{j=1}^{\delta} G_{i,t+j-1} - \delta G_{i,t-1}$, respectively. Similarly, the state-level instrument is constructed using $G^d_{i,t,\delta}$ instead of $G^c_{i,t,\delta}$.

Now the coefficient should be interpreted as the cumulative increase in income in response to a one dollar increase in accumulated defense spending. Although the estimated coefficients increase in all five columns, our main conclusions still hold: the income multiplier is well below one, and the aggregate and corresponding local multipliers do not vary substantially from each other. Interestingly, with this new specification we can reject a four-year cumulative multiplier of zero estimated with state-level data. Yet, in all cases, the four-year cumulative multiplier is substantially less than one.

To address the issue of first-differencing a variable that is potentially I(0), we follow the approach taken in Ramey (2016) and modify our baseline specification by including some lagged level variables to mop up any serial correlation. In particular, we add two regressors:

[29] We thank an anonymous referee for bringing this issue to our attention.

Table B.2: Aggregate and state-level income multipliers at a 2-year and 4-year horizons: As shown in the paper

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-yr cumulative	0.23***	0.22***	0.02	-0.01	0.33***
income multiplier	(0.06)	(0.06)	(0.05)	(0.05)	(0.12)
Partial F statistic	74.37	75.21	31.22	30.76	519.26
4-yr cumulative	0.07	0.05	0.11	0.05	0.07
income multiplier	(0.06)	(0.06)	(0.07)	(0.06)	(0.24)
Partial F statistic	74.04	74.35	32.45	31.46	5.53
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
N	2934	2934	2934	2934	60

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table B.3: Aggregate and state-level income multipliers at a 2-year and 4-year horizons: Without dividing by Y_{t-1}

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-yr cumulative	0.40***	0.40***	0.11	0.10	0.45**
income multiplier	(0.08)	(0.08)	(0.07)	(0.07)	(0.19)
Partial F statistic	109.68	110.81	47.98	46.89	502.49
4-yr cumulative	0.31***	0.31***	0.18**	0.17*	0.28
income multiplier	(0.09)	(0.09)	(0.09)	(0.09)	(0.31)
Partial F statistic	112.79	112.61	52.13	49.62	71.67
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
N	2934	2934	2934	2934	60

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

$ln(Y_{t-1})$ and $ln(G_{t-1})$. The results are presented in Table B.4. This exercise yields similar results to those reported in Table B.2. Once again, our main conclusions hold. Though we do not report the employment regressions here, those results are not sensitive to different specifications either.

Table B.4: Aggregate and state-level income multipliers at a 2-year and 4-year horizons: With mop up variables ($ln(Y_{t-1})$ and $ln(G_{t-1})$)

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-yr cumulative	0.35***	0.36***	-0.01	-0.08*	0.39***
income multiplier	(0.08)	(0.08)	(0.05)	(0.05)	(0.14)
Partial F statistic	71.22	77.10	30.76	32.73	155.64
4-yr cumulative	0.29***	0.29***	0.04	-0.07	0.26
income multiplier	(0.08)	(0.08)	(0.07)	(0.06)	(0.31)
Partial F statistic	70.65	75.71	31.60	33.27	15.48
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
N	2927	2927	2927	2927	60

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

C. Alternative Instrument

At the end of Section 4.1 in the paper, we discussed the importance of anticipation effects when identifying government spending shocks. Table C.5 presents the regression results of using Valerie Ramey's defense news shocks series as an instrument.

To evaluate the magnitude of the role played by anticipation effects in the estimation of aggregate multipliers, we would ideally modify our baseline aggregate specification to use

Ramey's news shocks as an instrument for changes in defense spending. The results of this exercise are presented in column (1) of Table C.5 for the two-year cumulative multiplier. However, the low Kleibergen-Paap F statistics and the large standard errors frustrate our comparison task. We must therefore resort to a longer time series to avoid the weak instrument problem and get a more precise estimation with the news shocks instrument. We use the BEA data on national defense spending introduced in Section 4.2 of the paper.

Columns (2) and (3) both use the BEA data and differ only in the instrument: column (2) uses Ramey's news shocks and column (3) uses the one-year change in defense spending. The similar point estimates (close to 0.5) should mitigate concerns about the impact of anticipation effects. Since the BEA series starts in 1929, and thus includes unusual historical episodes like the Great Depression and World War II, it is important to examine whether this result holds if we restrict the BEA data to match the years available in our sample. Column (4) shows that the point estimates do not change by much. Finally, column (5) reiterates that the estimates obtained using our data match those calculated with the BEA data. This last point confirms that it is valid to draw conclusions about the correct timing of our data by examining BEA series.

Table C.5: 2-year cumulative income multiplier based on aggregate data with an alternative instrument

	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-year cumulative	-0.32	0.58***	0.44***	0.32*	0.34***
multiplier	(0.60)	(0.05)	(0.02)	(0.18)	(0.13)
Partial F statistic	3.12	14.23	635.05	465.08	563.86
N	60	81	81	60	60
Defense measure	Dupor/Guerrero	BEA	BEA	BEA	Dupor/Guerrero
Starting year	1951	1929	1929	1951	1951
Instrument	News Shoocks	News Shocks	$G^c_{t,1}$	$G^c_{t,1}$	$G^c_{t,1}$

Notes: Column (5) does not exactly match our baseline specification because we omitted oil price controls in this table due to data availability issues before 1946. The SEs are robust with respect to autocorrelation and heteroskedasticity. * $p < .1$, ** $p < .05$, *** $p < .01$.

D. List of Major Trading Partners

Table D.6 shows the list of each state's major trading partner. As explained in the paper, we define a state's major trading partner using 2007 Commodity Flow Survey data as follows: State i's major trading partner is the destination state j with the largest total value of commodities that flow from i to j, divided by j's population. That is,

$$\text{Major trading partner}_i = \underset{j \neq i}{\text{argmax}} \frac{V_{i,j}}{Pop_j}, \text{ for } i, j \in \{\text{U.S. states}\} \tag{8}$$

where $V_{i,j}$ is the value of total shipments from state i to state j in 2007 and Pop_j is state j's population in that year.

Table D.6: Major trading partners

State	Major Trading Partner	State	Major Trading Partner
AK	WA	MT	WY
AL	GA	NC	SC
AR	OK	ND	MN
AZ	NM	NE	SD
CA	NV	NH	MA
CO	WY	NJ	NY
CT	RI	NM	AZ
DE	PA	NV	UT
FL	GA	NY	NJ
GA	AL	OH	KY
HI	CA	OK	KS
IA	MN	OR	WA
ID	UT	PA	NJ
IL	WI	RI	MA
IN	KY	SC	NC
KS	MO	SD	ND
KY	TN	TN	MD
LA	MS	TX	LA
MA	RI	UT	WY
MD	VA	VA	MD
ME	NH	VT	NH
MI	IN	WA	OR
MN	ND	WI	MN
MO	KS	WV	VA
MS	LA	WY	UT

References

Directorate for Information Operations and Reports (various years), *Atlas/Data Abstract for the US and Selected Areas.*

Directorate for Information Operations and Reports (various years), *Distribution of Personnel (M02).*

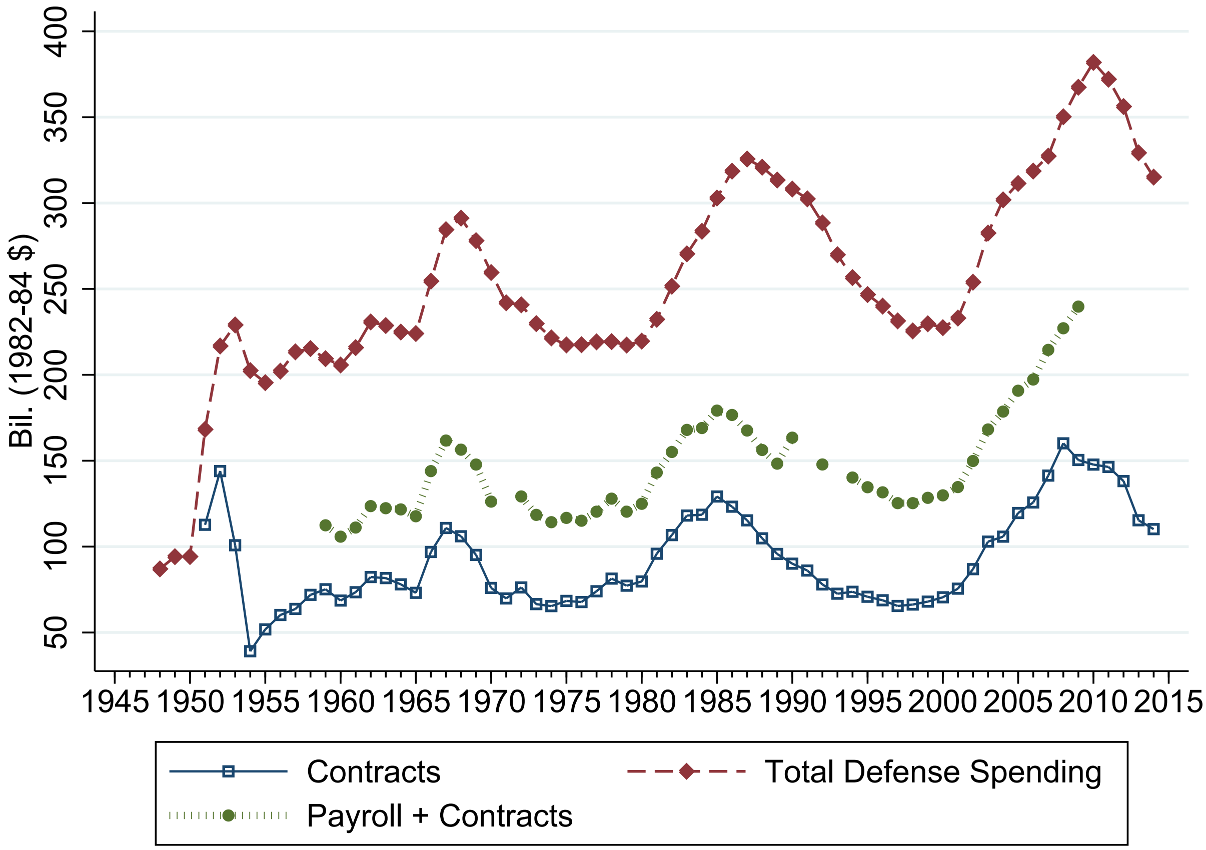
Directorate for Information Operations and Reports (various years), *Selected Manpower Statistics (M01).*

Ramey, V. (2016), "Macroeconomic Shocks and their Propagation," ch. 2 in John B. Taylor and Harald Uhlig (eds.), *Handbook of Macroeconomics*, Amsterdam; Oxford: North-Holland: Elsevier, pp. 71-162.

U.S. Census Bureau (various years), *The Statistical Abstract of the United States.*

Tables and Figures

Figure 1: Three measures of real U.S. defense expenditures



Notes: Contracts are the sum of awarded military contracts added across U.S. states (see text for description of data). Payroll plus contracts includes payroll to both civilian government and military defense employees. Total defense spending is government consumption plus gross investment in defense from the Bureau of Economic Analysis.

Table 1: 2-year and 4-year aggregate cumulative income and employment multipliers, based on aggregated state-level contract data

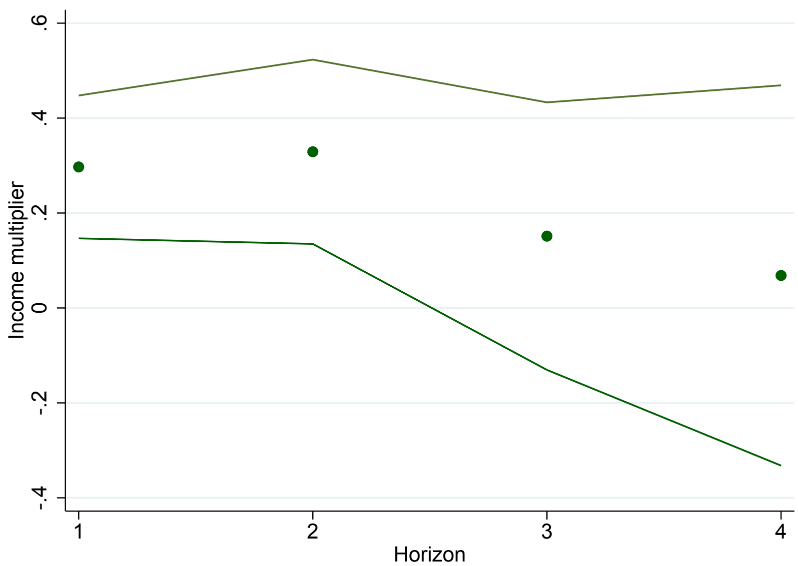
	Income		Employment	
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-year cumulative multiplier	0.33***	-	0.39***	-
	(0.12)		(0.11)	
4-year cumulative multiplier	-	0.07	-	0.24
		(0.24)		(0.21)
Partial F statistic	519.26	5.53	568.64	84.97
N	60	60	60	60

Notes: Each specification includes two lags of the real interest rate and the change in the real price of oil. The SEs are robust with respect to autocorrelation and heteroskedasticity.

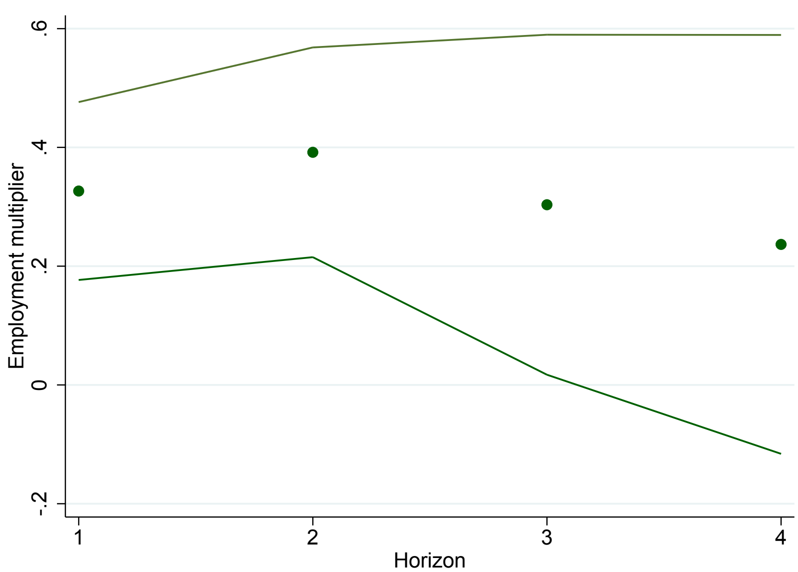
* $p < .1$, ** $p < .05$, *** $p < .01$.

Figure 2: Aggregate cumulative income and employment multipliers over various horizons, based on aggregated state-level contract data

(a) Income



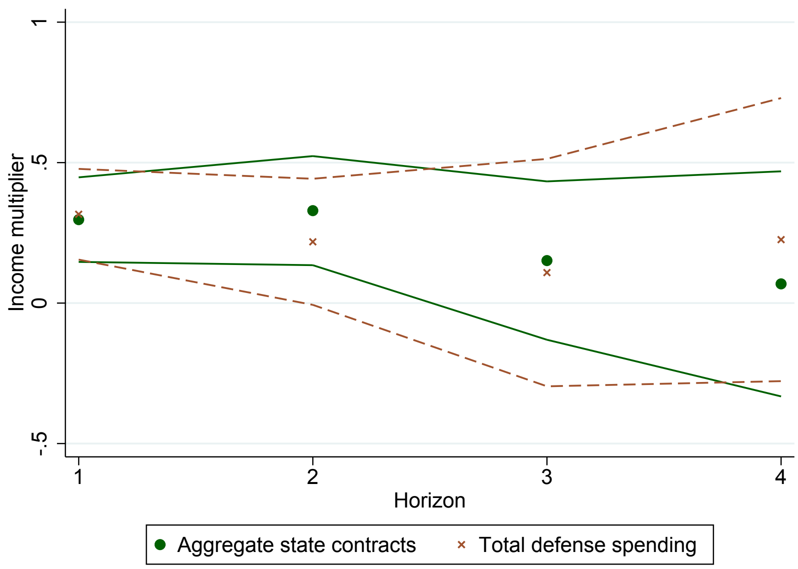
(b) Employment



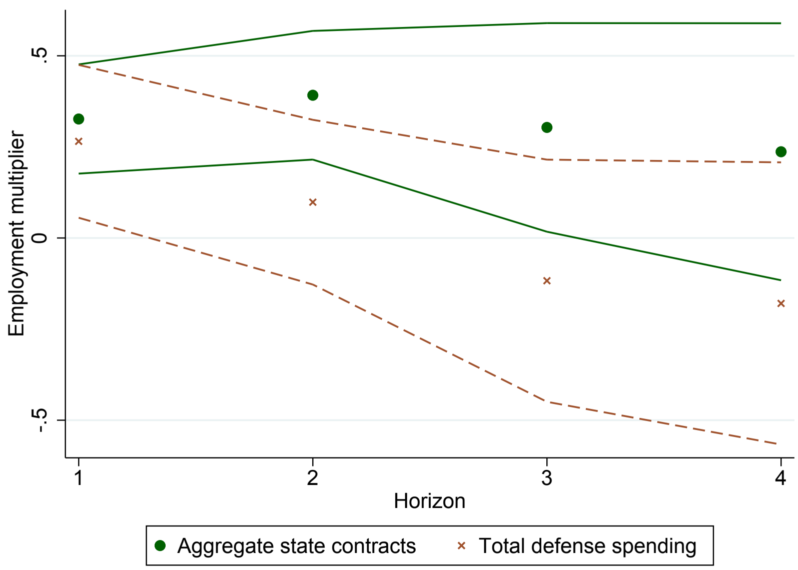
Notes: The solid lines indicate the robust pointwise 90% confidence interval.

Figure 3: Cumulative aggregate income and employment multipliers as a function of the horizon, estimated using aggregate contract data compared with using BEA-measured total defense spending

(a) Income



(b) Employment



Notes: The dashed red lines show the robust 90% confidence interval based on total BEA-measured defense spending. The solid green lines show the robust 90% confidence interval based on aggregate state contract data.

Table 2: Aggregate and state-level multipliers at a 2-year and 4-year horizons

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
	(a) Income				
2-yr cumulative	0.23***	0.22***	0.02	-0.01	0.33***
income multiplier	(0.06)	(0.06)	(0.05)	(0.05)	(0.12)
Partial F statistic	74.37	75.21	31.22	30.76	519.26
4-yr cumulative	0.07	0.05	0.11	0.05	0.07
income multiplier	(0.06)	(0.06)	(0.07)	(0.06)	(0.24)
Partial F statistic	74.04	74.35	32.45	31.46	5.53
	(b) Employment				
2-yr cumulative	0.30***	0.27***	0.13*	0.03	0.39***
employment multiplier	(0.06)	(0.06)	(0.08)	(0.06)	(0.11)
Partial F statistic	74.37	75.21	31.22	30.76	568.64
4-yr cumulative	0.24***	0.18***	0.31**	0.14	0.24
employment multiplier	(0.09)	(0.07)	(0.15)	(0.11)	(0.21)
Partial F statistic	74.04	74.35	32.45	31.46	84.97
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
N	2934	2934	2934	2934	60

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation.

* $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3: Cumulative income and employment multipliers based on state-level data with spillover term: 2-year and 4-year horizons

	2-year horizon	4-year horizon
	(1)	(2)
	Coef./SE	Coef./SE
	(a) Income	
State spending	0.18***	0.09
	(0.06)	(0.06)
Partner spending	0.07	-0.06
	(0.07)	(0.07)
Total Multiplier	0.25***	0.03
	(0.07)	(0.07)
	(b) Employment	
State spending	0.20***	0.16
	(0.06)	(0.11)
Partner spending	0.11	0.04
	(0.07)	(0.10)
Total Multiplier	0.31***	0.20***
	(0.06)	(0.07)
Partial F statistic	27.13	29.58
N	2934	2934

Notes: The partial F statistics are the same in the income and employment regressions. SEs are robust with respect to heteroskedasticity and autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 4: 2-year cumulative multipliers estimated with three different methods

	Income			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef./SE/DK	Coef./SE/DK	Coef./SE	Coef./SE/DK	Coef./SE/DK	Coef./SE
State spending	0.22***	0.18***	-	0.27***	0.20***	-
	(0.06)	(0.06)		(0.06)	(0.06)	
	[0.10]	[0.08]		[0.10]	[0.11]	
Partner spending	-	0.07	-	-	0.11	-
		(0.07)			(0.07)	
		[0.05]			[0.05]	
National spending	-	-	0.33***	-	-	0.39***
			(0.12)			(0.11)
			-			-
Total Multiplier	0.22***	0.25***	0.33***	0.27***	0.31***	0.39***
	(0.06)	(0.07)	(0.12)	(0.06)	(0.06)	(0.11)
	[0.10]	[0.12]	-	[0.10]	[0.10]	-
Partial F statistic	75.21	27.13	519.26	75.21	27.13	568.64
N	2934	2934	60	2934	2934	60

Notes: Heteroskedasticity and autocorrelation robust standard errors are shown in parentheses. Driscoll-Kraay standard errors are presented in brackets. * $p < .1$, ** $p < .05$, *** $p < .01$.

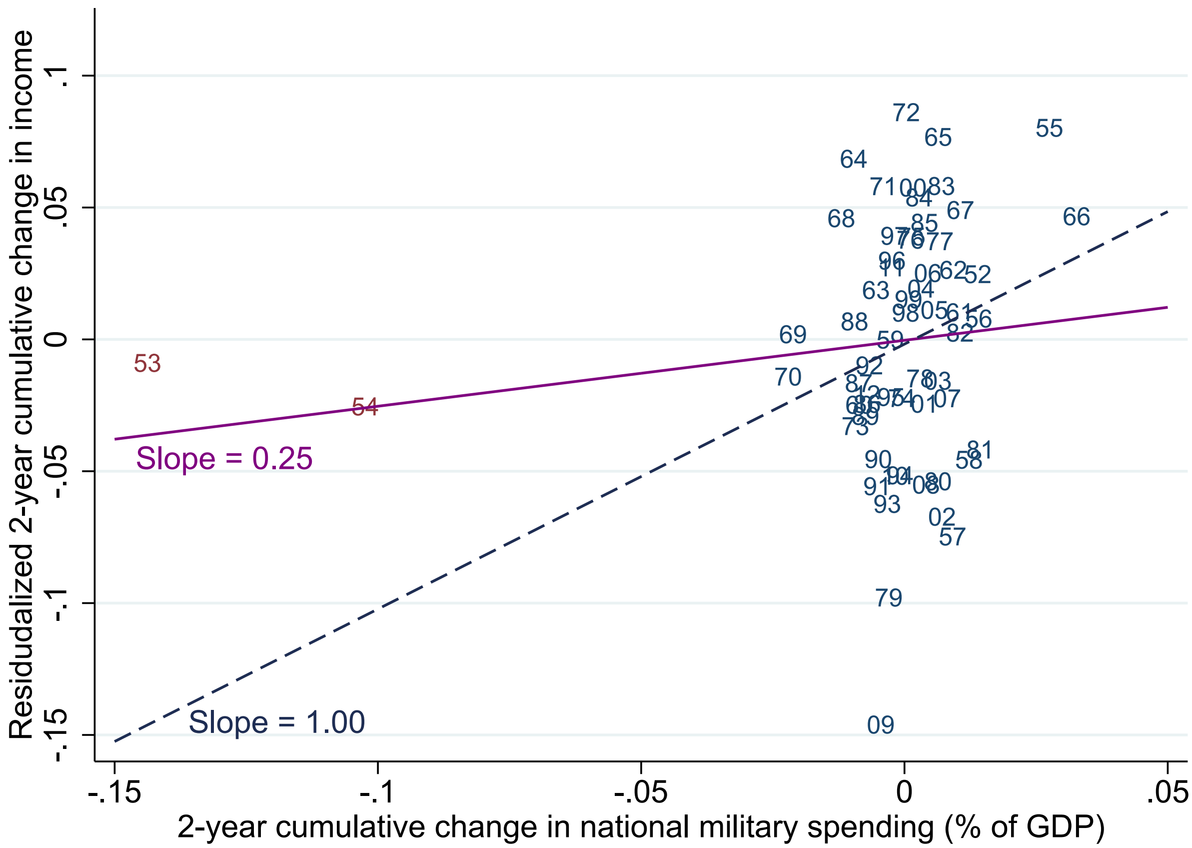
Table 5: Effect on the government spending multiplier of extending the sample to include 1950-1965, two-year horizon

	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
Local multiplier	1.28***	1.04***	0.71**	-0.04
	(0.41)	(0.36)	(0.28)	(0.03)
Partial F statistic	5.68	5.68	5.29	20.23
N	1950	1950	1950	3084
Dependent variable	Output	Income	Income	Income
Defense measure	NS	NS	Our Data	Our Data
Sample	1966-2006	1966-2006	1966-2006	1951-2014

Notes: SEs are robust with respect to heteroskedasticity and autocorrelation.

* $p < .1$, ** $p < .05$, *** $p < .01$.

Figure 4: The effect on aggregate income of aggregate defense spending after controlling for macro variables (OLS)



Notes: The best linear predictor of the subsample that excludes the outliers (1953-1954) is shown in a dashed blue line, while that of the entire sample is shown in a solid purple line.