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. Next, we use the panel aspect of the data to dramatically increase the precision of estimates of the aggregate multiplier (relative to using the aggregate data alone). Across a wide range of specifications, we estimate income and employment multipliers between zero and 0.5.

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 stud-

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[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 Drautzburg and Uhlig (2015) and Cogan, et. al. (2010). Additionally considering this approach is beyond the scope of the current paper.

ies 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 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 four 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.[8]

[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).

[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).

[8]In a related paper, Kline and Moretti (2014) study the effects of the Tennessee Valley Authority. While they find long-lasting localized gains in manufacturing, they also find that these gains were fully offset by losses elsewhere in

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 of spillovers might include movements in factors of production (as in the above example), trade in goods, common monetary policy or common fiscal policy, among others. As another 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 other states. We operationalize this by simultaneously estimating direct effect and spillover effect coefficients.[9] The sum of the two gives the aggregate effect of government spending.

Summing the direct and spillover effect of government spending delivers an estimate of *the aggregate multiplier based on disaggregate data*. Having already estimated the aggregate multiplier based on aggregate data, we are able to compare the two approaches. We find that the two approaches deliver similar point estimates. We also find a distinct advantage in using the approach based on state-level data: The estimated standard errors (SEs) are substantially smaller.

Our baseline findings are a multiplier on income of roughly 0.5 and a small positive effect on employment of government spending.

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

the United States.

[9]The two papers most closely related to mine, 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.

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.[10] 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.[11]

The data are from two sources. The first source consists of two reports that were published by the same organizations using the same underlying data: the *Prime Contract Awards by State* report and the *Atlas/Data Abstract for the US and Selected Areas*. It was necessary to draw upon these two reports (as opposed to using one of them only) due to availability issues with these historical documents. In general, the first report provides data for 1951 through 1980, and the second document was used for the years 1981 through 2009.[12] The second source, which provides data for the 2010-2014 period, is a an official website of the U.S. government: 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 Department of Defense, the Washington Headquarters Services, and the Directorate for Information Operations and Reports—contain military contract data aggregated at 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 negligible 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 goes back to 2007, which gives us three years of overlap between our two data sources to check for consistency in the splicing procedure.

[10] We use the terms "contracts" and "spending" synonymously in this paper.

[11] See Nakamura and Steinsson (2014).

[12] 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 cites either report as a source.

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.[13]

The addition of the 1951-1965 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). We also show that estimates of local fiscal multipliers change dramatically with the inclusion of this 15 year period. Specifically, NS find local output multipliers equal to roughly 1.5 without these years in their sample, whereas we find that extending the sample results in a local income multiplier equal to zero.

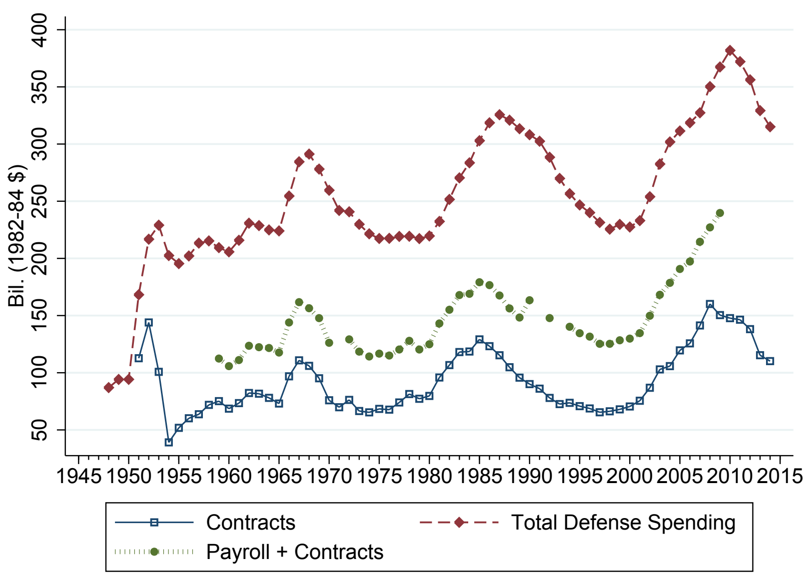
The data aggregated across states are plotted in Figure 1 as the blue line with box markers. The time series evolves as one might expect. The dollar value of defense contracts at the start of the sample was high due to the Korean War. There is a decline in spending associated with the military drawdown that followed. The next two hump-shaped movements 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 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, a large amount of U.S. military spending occurs outside the nation's borders. For example, such spending includes some military foreign aid as well as much of the cost of maintaining hundreds of military bases overseas.

[13] 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" (Walter Isard and James Ganschow, Awards of Prime Military Contracts by State, County, and Metropolitan Areas of the United States, Fiscal 1960).

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.

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.[14] 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 state gross domestic product because the latter data are not available for 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 $Y_{i,t-1}$. 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 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.

[14] 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.

[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.

4 Aggregate Multipliers with Aggregate Data

4.1 The aggregate income and employment multipliers

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 framework: the Hall defense spending approach.[16] 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.[17]

The second-stage equation for the income regression is:

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

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.[18] 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 with 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 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.

[16] Sheremirov and Spirovska (2016) also uses the Hall defense spending approach.

[17] We compute the estimates using Stata V.14 and the *ivreg2* command with the options *gmm2s*, *robust* and *bw*.

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

Table 1: Aggregate cumulative income and employment multipliers at various horizons, 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. * $p < .1$, ** $p < .05$, *** $p < .01$.

We assess the strength of the defense spending instrument by reporting the Kleibergen-Paap partial F-statistic for each specification. 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.

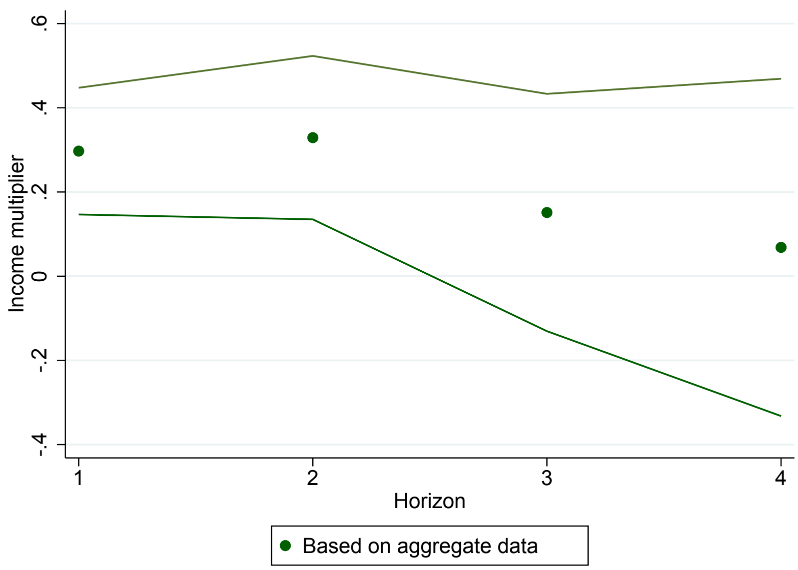
Columns (3) and (4) contain the analogous results except employment is instead used as the dependent variable. The 2-year employment multiplier estimate equals 0.39 (SE = 0.11). Thus if military spending increases by one percent of national income, then employment increases by 0.39%. The 4-year employment 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 multipliers as one varies the horizon δ. 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.

4.2 Decomposing the multipliers

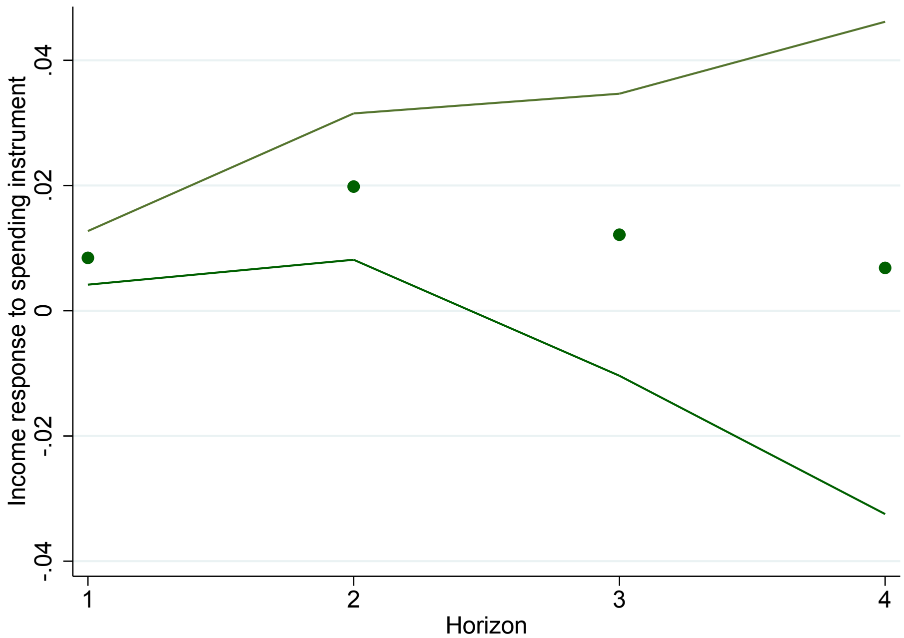
The cumulative income multiplier is the ratio of two cumulative responses. First, the numerator is the cumulative response of income to the defense spending shock, which is often called the "reduced form." Second, the denominator is the cumulative response of spending to the defense spending

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



Notes: The solid lines indicate pointwise 90% confidence intervals, which are robust with respect to autocorrelation and heteroskedasticity.

Figure 3: Cumulative impulse response of aggregate income to a spending shock (i.e., reduced form from 2SLS)



Notes: The scale of the defense spending shock is selected such that the shock's 4-year cumulative effect on defense contracts equals 10% of one year's national income. The solid lines indicate pointwise 90% confidence intervals, which are robust with respect to autocorrelation and heteroskedasticity.

shock (i.e., the first stage). To understand the dynamic properties of the multiplier, it is useful to decompose it into its two parts.

First, we estimate the reduced form at each horizon, which is given by

$$Y^c_{t,\delta} = \alpha^Y_\delta G^c_{t,1} + \beta^Y_\delta X_t + v^Y_{\delta,t} \tag{5}$$

Figure 3 plots the coefficients α^Y_δ as a function of δ. To ease interpretation, we scale the shock $G^c_{t,1}$ such that the shock's cumulative effect on defense contracts at the 4-year horizon equals 10% of one year's income.

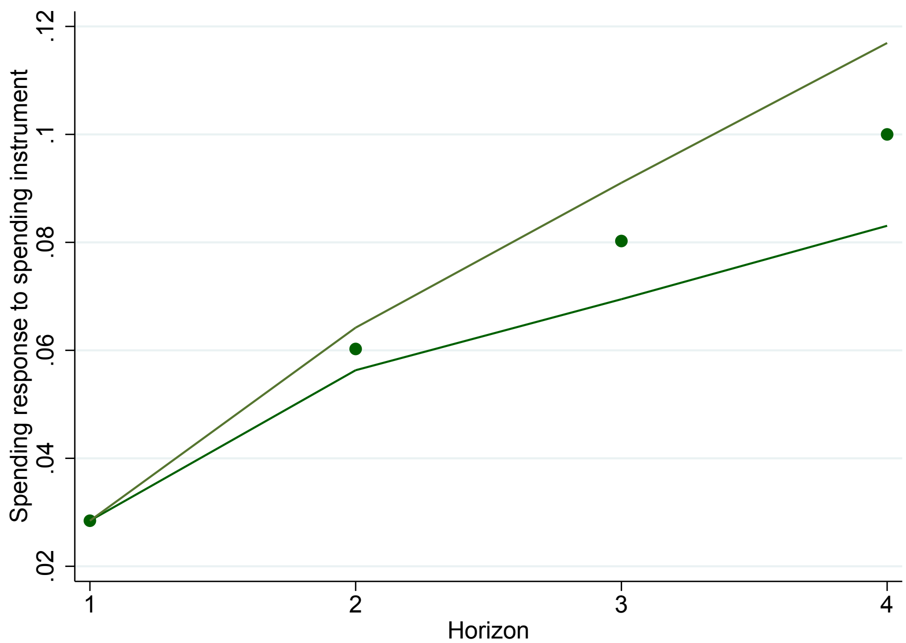
Next, we plot the first-stage estimate at each horizon using:

$$G^c_{t,\delta} = \alpha^G_\delta G^c_{t,1} + \beta^G_\delta X_t + v^G_{\delta,t} \tag{6}$$

This impulse response is plotted on Figure 4.

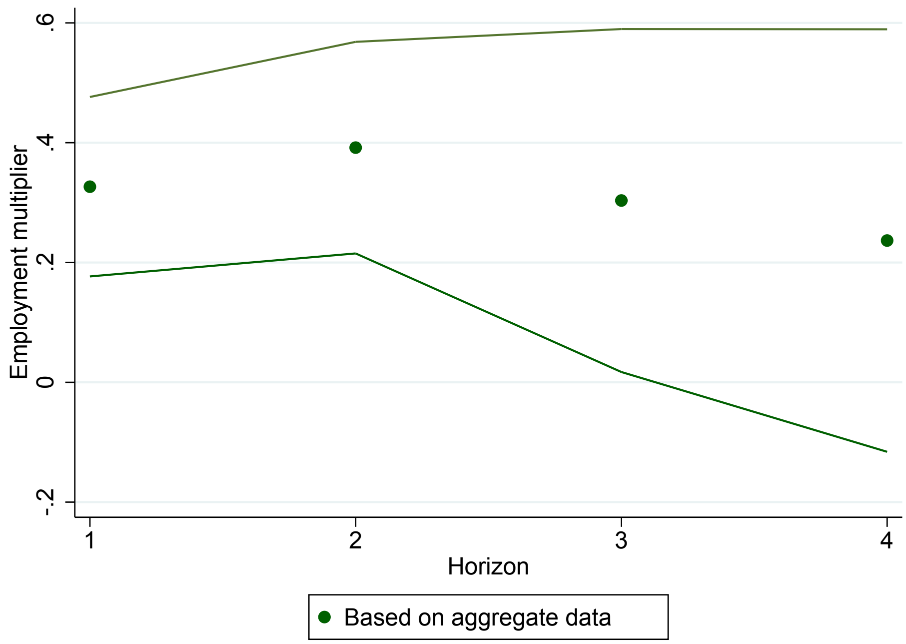
Next, we estimate (4) but we use the accumulated percentage change in employment as the

Figure 4: Cumulative impulse response of aggregate defense contracts to a spending shock (i.e., first stage from 2SLS)



Notes: The scale of the defense spending shock is selected such that the shock's 4-year cumulative effect on defense contracts equals 10% of one year's national income. The solid lines indicate pointwise 90% confidence intervals, which are robust with respect to autocorrelation and heteroskedasticity.

Figure 5: Aggregate cumulative employment multiplier over various horizons, based on aggregated state-level contract data



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

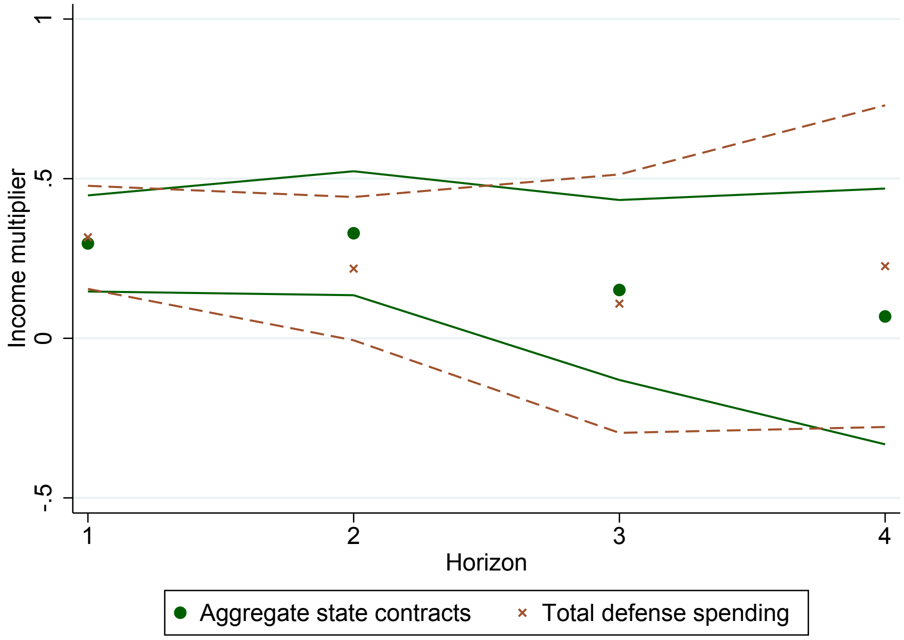
dependent variable. Figure 5 plots the point estimates and 90% confidence interval (as a function of the horizon). The coefficient should be interpreted as the percentage growth in employment (accumulated over a particular horizon) in response to an exogenous defense spending increase (accumulated over the same horizon) equal to 1% of national income. The estimate is stable between roughly 0.2 and 0.4 over every plotted horizon.

4.3 Comparison with other military spending measures

One concern may be that our defense spending measure is not representative of overall U.S. military spending. As explained in Section 2, in many years aggregated contracts within the 50 states made up less than half of the BEA-measured 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.

Figure 6 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

Figure 6: Cumulative aggregate income multiplier as a function of the horizon, estimated using aggregate contract data compared with using BEA-measured total defense spending



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.

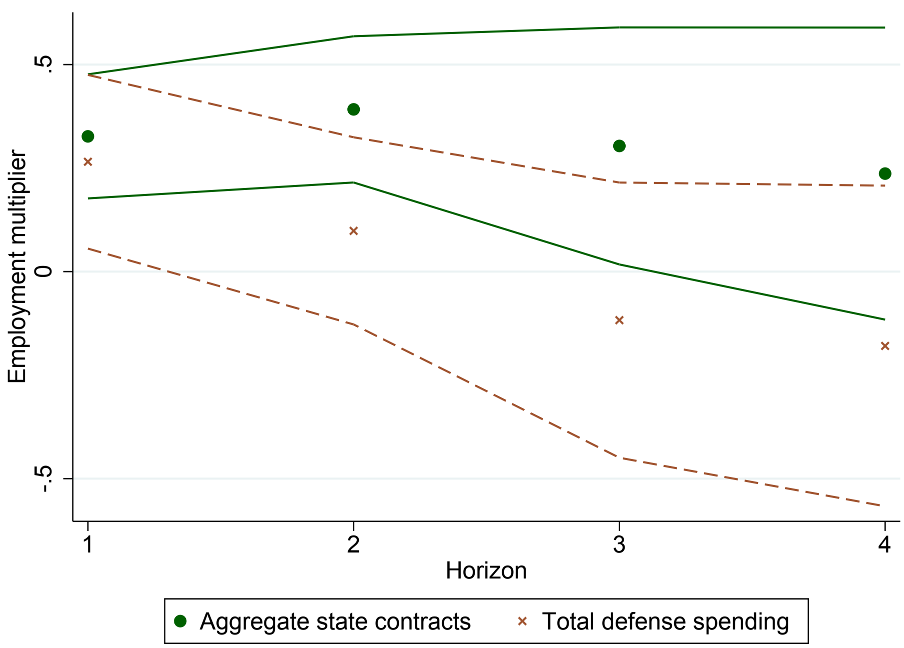
two specifications, and (ii) there is substantial overlap of the confidence intervals.

Figure 7 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. Together, Figure 6 and 7 are reassuring in that our new measure of military spending give 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, the new multipliers 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.

Figure 7: Cumulative aggregate employment multiplier as a function of the horizon, estimated using aggregate contract data compared with BEA-measured total defense spending



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.

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 sufficient 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

$$Y^c_{i,t,\delta} = \psi_\delta G^c_{i,t,\delta} + \pi_{i,\delta} X_t + w_{i,t,\delta} \tag{7}$$

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.

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 (7). 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 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

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

Table 2: Response of income to defense spending shock: aggregate and state-level panel analysis at a 2-year horizon

	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)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	74.37	75.21	31.22	30.76	519.26
N	2934	2934	2934	2934	60

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

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.

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.

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

Next, Tables 4 and 5 present 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 mul-

Table 3: Response of income to a defense spending shock: aggregate and state-level panel analysis at a 4-year horizon

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
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)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	74.04	74.35	32.45	31.46	5.53
N	2934	2934	2934	2934	60

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

Table 4: Response of employment to a defense spending shock: aggregate and state-level panel analysis at a 2-year horizon

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
2-yr cumulative	0.30***	0.27***	0.13*	0.03	0.39***
employment multiplier	(0.06)	(0.06)	(0.08)	(0.06)	(0.11)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	74.37	75.21	31.22	30.76	568.64
N	2934	2934	2934	2934	60

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

Table 5: Response of employment to a defense spending shock, aggregate and state-level panel analysis, 4-year horizon

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
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)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	74.04	74.35	32.45	31.46	84.97
N	2934	2934	2934	2934	60

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

tipliers range from 0.03 to 0.30 depending on whether and how fixed effects are introduced. As seen in Table 5, 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 put the state-level data to good use in estimating aggregate multipliers.

Here is the idea. As explained at the beginning of the current section, aggregate and local multipliers differ because of spillovers across states. Spillovers could have many origins, including fiscal policy, monetary policy as well as interstate movements in goods and factors of production. Fortunately, we have sufficient variation to estimate this spillover effect. This will involve including both state-level defense spending as well as national defense spending in the state-level regressions. We will call the former the direct effect of spending and the latter the spillover effect. The total effect will be the sum of the direct and spillover effects.

Moreover, once we make the adjustment for the spillover effect, then the state-level based total multiplier estimates will be very similar to the national data based aggregate multiplier estimates. While the two point estimates will line up closely, the state-level based estimates will have much smaller standard errors.

Table 6: Cumulative income multipliers based on state-level data and on aggregate data: 2-year and 4-year horizons

	2-year horizon		4-year horizon	
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	-0.00	-	0.06	-
	(0.06)		(0.08)	
National spending	0.31***	0.33***	-0.01	0.07
	(0.07)	(0.12)	(0.09)	(0.24)
Total Multiplier	0.31***	0.33***	0.05	0.07
	(0.05)	(0.12)	(0.07)	(0.24)
Partial F statistic	15.99	519.26	16.29	5.53
N	2934	60	2934	60

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

6 Aggregate Multipliers using State-level Data

In this section, we estimate the state-level regression except we add as an independent variable the accumulated change in *national* defense contracts as a fraction of national income. The second-stage equation for the income regression is

$$Y^c_{i,t,\delta} = \gamma^Y_\delta G^c_{i,t,\delta} + \phi^Y_\delta G^c_{t,\delta} + \beta^Y_{i,\delta} X_t + v^Y_{i,t,\delta} \tag{8}$$

We also include state fixed effects in our benchmark specification.

Equation (8) allows one to parse the distinct effects of state and national military spending on state income. As explained previously, several authors have estimated the first of the two effects; however, to our knowledge, this paper is the first to estimate both effects.

In addition to the instrument $Z_{i,t}$ described previously, we also include $G^c_{1,t}$ as an aggregate instrument so that the new model is identified.

The aggregate 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 national 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 every state. Then, from a state's perspective, there would be two effects.

First, own-state contracts would increase and thus have an effect on own-state income. Second, national contracts 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 Table 6. Column (1) gives the results for specification

(8) at the 2-year horizon. The state spending coefficient equals 0.00. The corresponding coefficient on national spending is 0.31. Thus, holding fixed state spending, an increase in national spending increases state income. The aggregate multiplier equals 0.31, the sum of the state and national spending coefficients. This constitutes an important positive spillover between states.

For comparison, column (2) of Table 6 reports the estimate of the aggregate multiplier based on the aggregated state-level data. This is the same estimate reported in Table 1. The coefficient on national spending is 0.33. By construction, the aggregate multiplier is equal to the coefficient on national spending, so we simply report the same number in both entries.

While the aggregate 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.

Since the two estimates deliver similar results, the curious reader may ask "Why go to the trouble of using the disaggregate data at all?" The payoff is that the SEs are substantially lower using the state-level data. Specifically, the SE falls from 0.12 to 0.05. 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.

Next, columns (3) and (4) in Table 6 contain the analogous estimates for the multipliers at the 4-year horizon. The 4-year aggregate multiplier is nearly zero.

Figure 8 plots the cumulative aggregate income multiplier (green "x") at each horizon based on the aggregated state-level data. The cumulative aggregate income multipliers (purple circles) based on the state-level data are also plotted. The 90% confidence intervals for both sets of estimates are also plotted on the figure. At each horizon, the point estimates from the two different methods are relatively similar. Yet, the 90% confidence bands are much narrower for the estimates based on the state-level approach.

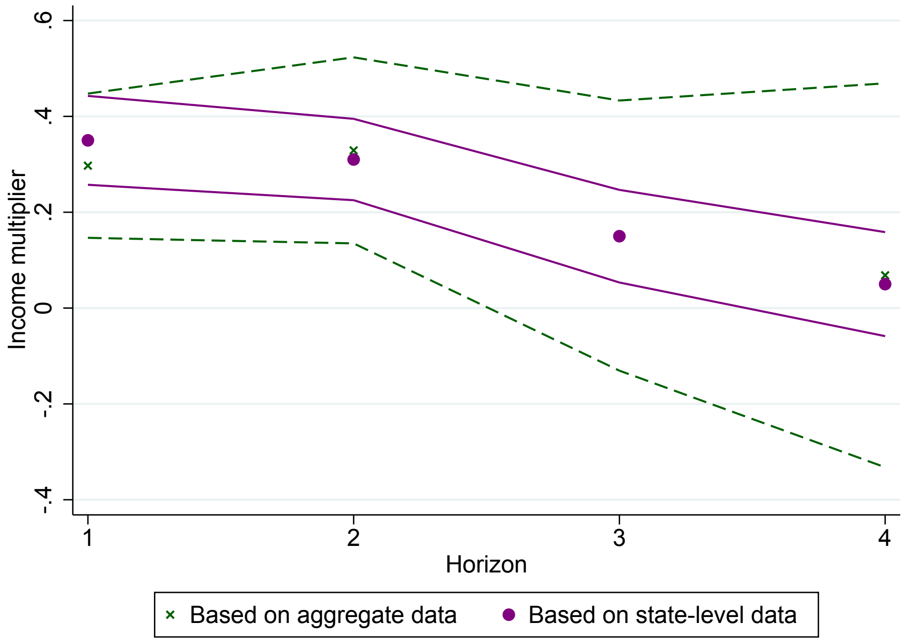
Next, we plot the spillover and direct cumulative multiplier coefficients as a function of the horizon on Figure 9. Observe that the spillover coefficient at short horizons (through year 2) is positive and statistically distinguishable from zero. Thus, at short horizons, there is a small positive spillover from national defense spending on a state's income, after controlling for state defense spending. The direct effect is nearly zero at every horizon.

Table 7 contains the analogous estimates to Table 6 but for employment. At both horizons, the cumulative aggregate employment multiplier is substantially less than 1. As discussed above, the use of state-level data results in smaller standard errors relative to using aggregate data alone.

Figure 10 presents the analogous information as in Figure 8 but for employment instead of income. First, the multiplier is stable between roughly 0.2 and 0.4 over the entire horizon. Second, as with the income multiplier, using state-level instead of aggregate data greatly sharpens the precision of the estimates.

Next, we report a few robustness checks on the results. Tables 8 and 9 compare the benchmark

Figure 8: Aggregate cumulative income multiplier based on state-level data and aggregate data: various horizons



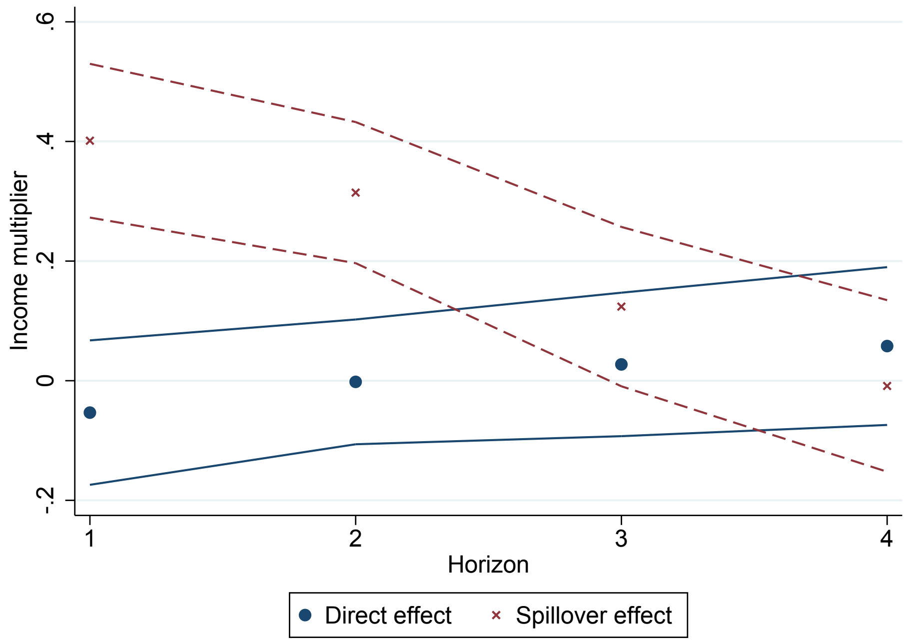
Notes: Solid lines indicate the robust pointwise 90% confidence interval based on state level data. Dashed lines indicate the robust pointwise 90% confidence interval based on aggregate data.

Table 7: Cumulative employment multipliers based on state-level data and on aggregate data: at 2-year and 4-year horizons

	2-year horizon		4-year horizon	
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.04	-	0.14	-
	(0.07)		(0.11)	
National spending	0.32***	0.39***	0.06	0.24
	(0.08)	(0.11)	(0.14)	(0.21)
Total Multiplier	0.36***	0.39***	0.20**	0.24
	(0.05)	(0.11)	(0.08)	(0.21)
Partial F statistic	15.99	568.64	16.29	84.97
N	2934	60	2934	60

Notes: Robust SEs are reported. * $p < .1$, ** $p < .05$, *** $p < .01$.

Figure 9: Direct and spillover cumulative income multipliers based on state-level data: at various horizons



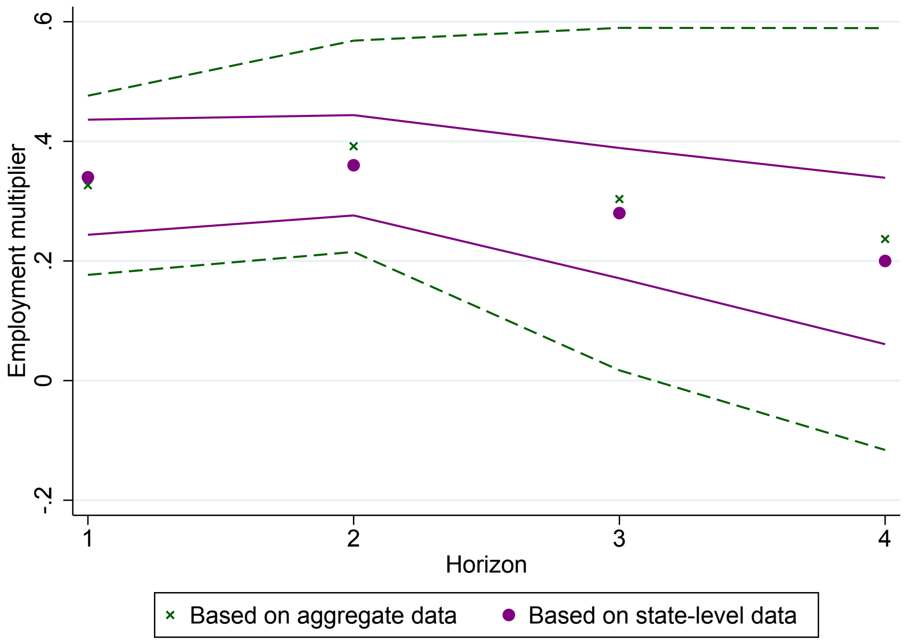
Notes: The solid blue lines indicate the robust pointwise 90% confidence interval for the direct multipliers. The dashed red lines indicate the robust pointwise 90% confidence interval for the spillover multipliers.

Table 8: Four-year cumulative income multipliers with and without state fixed effects

	Instrumental variables		Least squares	
	With FEs	Without FEs	With FEs	Without FEs
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.06	0.12	0.08	0.12
	(0.08)	(0.08)	(0.08)	(0.08)
National spending	-0.01	-0.07	-0.02	-0.06
	(0.09)	(0.09)	(0.08)	(0.09)
Total Multiplier	0.05	0.05	0.06	0.06
	(0.07)	(0.07)	(0.07)	(0.07)
Partial F statistic	16.29	16.79		
N	2934	2934	2934	2934

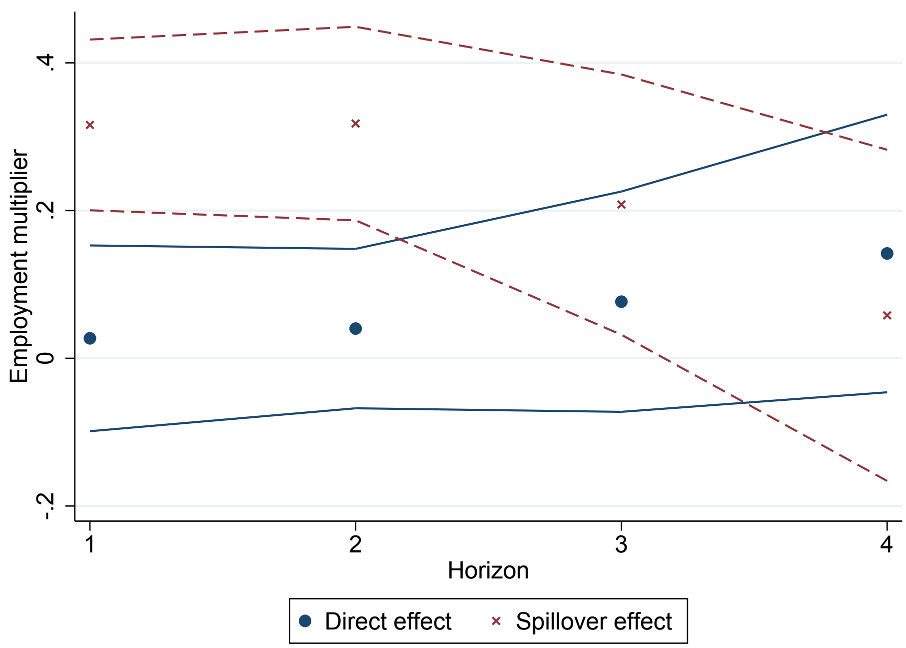
Notes: Robust SEs are reported. Least squares and instrumental variables based on state-level data and on aggregate data are reported. FEs, fixed effects. * $p < .1$, ** $p < .05$, *** $p < .01$.

Figure 10: Aggregate cumulative employment multiplier based on state-level data and aggregate data: various horizons



Notes: Solid and dashed lines indicate robust pointwise 90% confidence intervals.

Figure 11: Direct and spillover cumulative employment multipliers based on state-level data: at various horizons



Notes: Solid and dashed lines indicate robust pointwise 90% confidence intervals.

Table 9: Four-year cumulative employment multipliers with and without state fixed effects

	Instrumental variables		Least squares	
	With FEs	Without FEs	With FEs	Without FEs
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.14	0.32**	0.12*	0.21***
	(0.11)	(0.16)	(0.06)	(0.08)
National spending	0.06	-0.10	0.06	-0.02
	(0.14)	(0.20)	(0.09)	(0.13)
Total Multiplier	0.20**	0.21*	0.18**	0.19*
	(0.08)	(0.11)	(0.08)	(0.10)
Partial F statistic	16.29	16.79		
N	2934	2934	2934	2934

Notes: Robust SEs are reported. Least squares and instrumental variables are based on state-level data and on aggregate data. FEs, fixed effects. * $p < .1$, ** $p < .05$, *** $p < .01$.

results with cases when state fixed effects are dropped and the least squares method is used instead of instrumental variables.

7 Local Multiplier Estimates and the Influence of the 1950s

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 NS, that is based on post-1965 data, estimates a local multiplier that is greater than one.

To compare our results with NS, we first adopt a specification that closely mimics theirs. There are three subsantive differences, besides their shorter sample, between the NS and our specifications. First, they use per capita output rather than income as the dependent variable. Second, they instrument by using an interaction of $G_{i,t,2}$ with a state dummy.[20] Third, they draw their military contract data from a somewhat difference source.

Column (1) of Table 10 reports the local multiplier estimate based on equation (7) except we change the sample to match NS, 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 NS.

Next, column (2) reports the aggregate multiplier based on the aggregated data. Again, we use per capita output, their contract data and their sample period. Since this specification uses

[20]NS use two year growth rates for their dependent, endogenous and instrument variable rather than the cumulative growth rate. This difference has no important effect on the results.

Table 10: Effect on the government spending multiplier of extending the sample to include 1950-1965, two-year horizon

	(1) Coef./SE	(2) Coef./SE	(3) Coef./SE	(4) Coef./SE	(5) Coef./SE
Local multiplier	1.28***	-	1.04***	0.71**	-0.04
	(0.41)		(0.36)	(0.28)	(0.03)
Aggregate multiplier	-	0.57	-	-	-
		(1.26)			
Partial F statistic	5.68		5.68	5.29	20.77
N	1950	39	1950	1950	2734
Aggregated data?	No	Yes	No	No	No
Dependent variable	Output	Output	Income	Income	Income
Defense measure	NS	NS	NS	Our Data	Our Data
Starting year	1966	1966	1966	1966	1951
Est. Procedure	IV	OLS	IV	IV	IV

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

aggregated data, we use least squares. The aggregate multiplier estimate equals 0.57 (SE = 1.26). The large SE is due to the exclusion of the Korean War period, which is discussed in Hall (2009). The remaining columns of the table work with the state-level data.

The column (3) 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 (4) specification is identical to that in column (3) except we switch from the NS defense spending measure to mine. We emphasize that we continue to use the same years as used in the original NS 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 data set to include the additional years affects the local multiplier estimate. To this end, the column (5) specification is identical to the column (4) specification except we add the years 1951 to 1965 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 15 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.

8 Conclusion

In this paper, we adapted the local multiplier approach to allow for cross-regional spillovers in a way that permits researchers to use cross-sectional variation in variables to help identify and more precisely estimate the aggregate effects of fiscal policy. We also compared the estimates of local multipliers and national multipliers using a common data set and identification scheme.

Our findings suggest several directions for future work. First, one can apply this method to 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 only 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.

Second, since we have shown that one can substantially sharpen the precision of aggregate multiplier estimates relative to those using aggregate data alone, it would be useful to find other historical periods and datasets toward which one can apply this approach. The method relies on cross-sectional variation to find the local effects of government spending and time series variation to estimate the magnitude of the spillover channel. At the same time, one must address the endogeneity of fiscal policy, along both the aggregate and the cross-sectional dimension. Perhaps the most promising direction would be to execute the approach taken in this paper for other countries with sufficiently disaggregated military spending data.

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