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Local and Aggregate Fiscal Policy Multipliers

Bill Dupor

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FEDERAL RESERVE BANK OF ST. LOUIS
Research Division
P.O. Box 442
St. Louis, MO 63166

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Bill Dupor†

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Abstract

In this paper, I estimate the effect of defense spending on the U.S. macroeconomy since World War II. First, I construct a new panel dataset of state-level federal defense contracts. Second, I 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, I 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, I find that the two differ dramatically. I infer that the local multiplier estimates alone do not provide useful information about the aggregate effects of policy. Finally, I 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) by including a spillover term in the panel regressions. My baseline aggregate findings are a long-run multiplier on income equal to 1.6, a moderate long-run effect on employment, and no effect on income or employment effect in the short run. The results suggest that lags in the effects of defense spending are so long that they render countercyclical spending policies ineffective. In addition, I find negative short-run spillovers on employment of spending across state borders.

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]

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†Federal Reserve Bank of St. Louis, william.d.dupor@stls.frb.org, billdupor@gmail.com.

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

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 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.

My paper compares and then integrates the local and aggregate multiplier approaches. In doing so, I make four contributions. First, I construct a new panel of annual federal defense contracts at the state level.[6] It is the most comprehensive dataset of its kind to date. Second, I aggregate the state-level data and use a narrative instrumental variables approach to estimate the effect of national defense contracts on national income and employment. I find that at shorter horizons, the aggregate income multiplier is less than 1. At longer horizons, the multiplier is greater than 1. I find no effect of defense spending on employment. Reassuringly, my estimates are consistent with previous work using solely macro data (e.g., Ramey (2011a) and Ramey and Zubiary (2014)).

Third, having estimated aggregate multipliers, I then use the state-level defense data to estimate

[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.

[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), Suarez Serráto 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, I 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).

local income and employment multipliers. I find that there is no systematic relationship between the estimated aggregate multipliers and the local multipliers. The aggregate multiplier can be greater or less than the corresponding local multiplier, depending on the particular horizon and on the variable being considered. The local multiplier estimates also tend to be sensitive to whether state and/or year fixed effects are included in the econometric specification. 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 do not provide reliable information about the aggregate effects of fiscal policy.[7]

Fourth, I 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, I extend the local multiplier approach to include the spillover effects of defense spending in one state on the economic activity of other states. I operationalize this by simultaneously estimating direct effect and spillover effect coefficients.[8] The sum of the two gives the aggregate effect of government spending. Of independent interest, I find a negative spillover effect of defense spending on employment in the short run. The estimates are consistent with a resource reallocation explanation: Persons take jobs in or move to a state with increased military spending, but they leave when increased out-of-state military spending creates opportunities elsewhere.

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, I am able to compare the two approaches. I find that the two approaches deliver similar point estimates. I also find a distinct advantage in using the approach based on state-level data: The estimated standard errors (SEs) are substantially smaller.

My benchmark estimates are that, at horizons less than 4 years following a defense spending increase, there is no aggregate effect on either income or employment. Over longer horizons, the aggregate income multiplier is approximately 1.5. Furthermore, over longer horizons, when aggregate defense spending increases by 1% of national income, aggregate employment increases by 0.5%. My results indicate that the lags in the macroeconomic effects of defense spending are so

[7]In a related paper, Kline and Moretti (2013) 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 the United States.

[8]The two papers most closely related to mine, with respect to estimating spillovers, are those by Dupor and McCrory (2015) and Suarez Serráto and Wingender (2014). Those papers find positive spillovers between geographically neighboring states.

long that they render countercyclical spending policy ineffective.

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. Also, 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 Zubiary (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. I follow this approach here.

I construct a new panel dataset of U.S. state-level defense contracts between 1951 and 2014.[9] My data add more than 20 years over otherwise comparable existing data. The longest panel of defense spending in previous research covers 1966 through 2006.[10]

The data are from four sources. The *Military Contract Awards by State* documents, issued by the U.S. Department of Defense Directorate of Information Operations and Reports, contain military contract data between 1951 and 1958. The U.S. Census Bureau (various years) *Statistical Abstract of the United States* provides the contract data between 1959 and 2009.[11] The Statistical Abstract data are not available for the years 1971, 1991 and 1993. For these years I use data from the DD350 military procurement forms available from the Department of Defense as collected by Nakamura and Steinsson (2014).[12] The data between 2010 and 2014 are from USAspending.gov.[13]

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

[9]I use the terms "contracts" and "spending" synonymously in this paper.

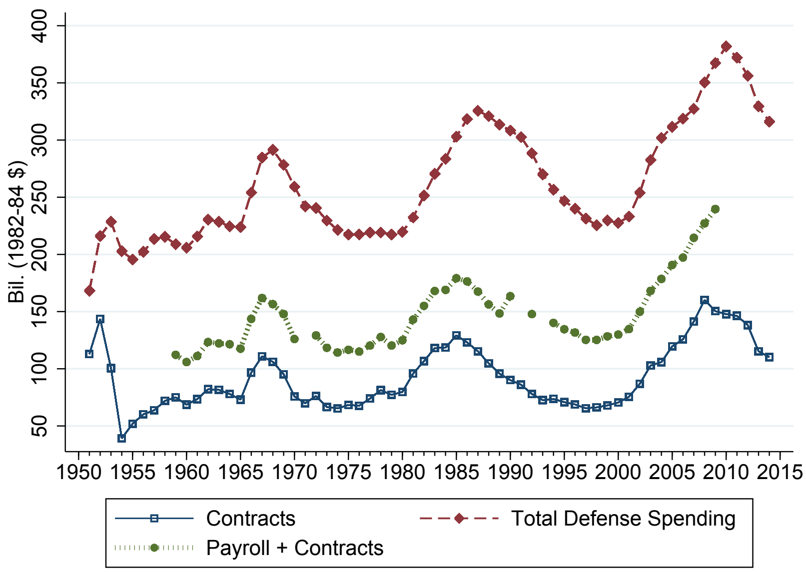
[10]See Nakamura and Steinsson (2014).

[11]The data itself were provided to the U.S. Census by the U.S. Department of Defense Office of the Secretary in the annual document *Prime Contract Awards by State*.

[12]The Nakamura-Steinsson data are available for download on the *American Economic Review* website.

[13]The USAspending.gov numbers include "Grants" and "Other Financial Assistance," which I am unable to disentangle from contracts.

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.

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, I 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, I 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.

3 Variable Definitions

My 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 I 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:

$$N^c_{i,t,\delta} = \left(\sum_{j=0}^{\delta} N_{i,t+j} - (\delta+1)\, N_{i,t-1} \right) / N_{i,t-1}$$

Next,

$$G^c_{i,t,\delta} = \left(\sum_{j=0}^{\delta} G_{i,t+j} - (\delta+1)\, G_{i,t-1} \right) / Y_{i,t-1}$$

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,

$$Y^c_{i,t,\delta} = \left(\sum_{j=0}^{\delta} Y_{i,t+j} - (\delta+1)\, Y_{i,t-1} \right) / Y_{i,t-1}$$

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

A few large outliers in the panel for $G^c_{i,t,\delta}$ suggest error in measurement. To mitigate the issue, I winsorize the top and bottom 0.5 percentile for this variable. This has a negligible effect on the aggregate results and the point estimates from the panel regressions. Winsorizing the data does improve the strength of the instruments in the panel case relative to the non-winsorized case.

Defining these variables as such permits me 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

[14] Employment data are missing for Michigan (before 1956), Alaska (before 1960) and Hawaii (before 1958). I 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 Zubiary (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.

of one year of income.

4 Aggregate Multipliers with Aggregate Data

4.1 The aggregate income and employment multipliers

Before working with these data at the state level, I aggregate the data to the national level and estimate national income and employment multipliers using a now standard framework: the Ramey "news shock" identification approach. This allows me to verify that my new dataset generates aggregate results similar to those in existing research.

To deal with potential anticipation effects of military spending, I use Ramey's narrative measure of defense news shocks, which I describe briefly next. 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. Note that failure to differentiate between actual spending or contracts versus news about spending means that the econometrician may miss important anticipation effects of fiscal policy.[16] I use this variable, denoted R_t, as an instrument for aggregate defense contracts.

Ramey's variable is quarterly, and I make her series applicable to my study by taking the average across the values for the four calendar quarters to construct an annual measure. Then I estimate the model using the generalized method of moments (GMM), which in this case has a two-stage least squares (2SLS) interpretation. Also, I report heteroskedasticity autocorrelation (HAC) corrected SEs throughout the paper.[17]

The second-stage equation for the income regression is:

$$Y^c_{t,\delta} = \phi_\delta G^c_{t,\delta} + \beta_\delta X_t + v_{t,\delta} \tag{1}$$

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] I 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.

At each successively longer horizon, I lose one additional observation (in order to calculate $Y_{i,\delta,t}$ and $G_{i,\delta,t}$). To make estimates comparable across horizons, I fix the sample and estimate the model

[16] See, for example Leeper, Walker and Yang (2013), for a discussion of various aspects of the fiscal foresight issue.

[17] I 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
4-year cumulative	0.18	-	0.03	-
multiplier	(0.31)		(0.37)	
10-year cumulative	-	1.52*	-	0.52
multiplier		(0.80)		(0.56)
Partial F statistic	3.54	3.87	3.54	3.87
N	53	53	53	53

Notes: Each specification includes two lags of the real interest rate and the change in the real price of oil as well as a linear trend. The SEs are robust with respect to autocorrelation. * $p < .1$, ** $p < .05$, *** $p < .01$

for each δ using the sample with containing the largest horizon (i.e., $\delta = 10$).

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

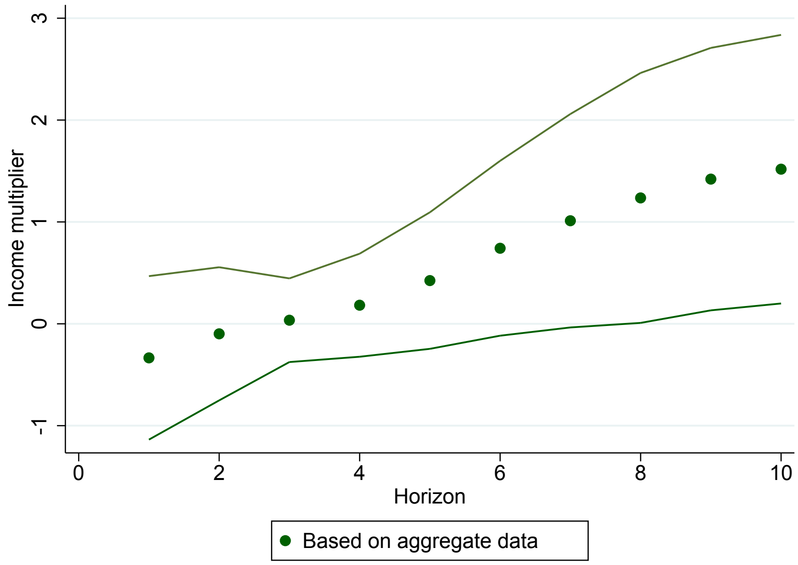
The income multiplier at the 4-year horizon is shown in column (1) of Table 1. The coefficient equals 0.18. Thus, if there is a cumulative increase in military spending equal to one percent of national income over a 4-year horizon in response to an exogenous news shock, then the cumulative change in national income equals 0.18%. The point estimate implies that the short-run national income multiplier is substantially less than 1. Note that the SE on the coefficient equals 0.31. Therefore, one can reject a multiplier greater than 1 with 90% confidence.

I assess the strength of the Ramey news instrument by reporting the Kleibergen-Paap partial F-statistic for each specification. The statistic equals 3.54 for the 4-year multiplier and 3.87 for the 10-year multiplier specifications. These values are less than the standard rule-of-thumb threshold of 10 required for the validity of the strong instrument approximation to hold. The reader should bear this in mind when interpreting the aggregate results.[19] The potential weak instrument problem for the aggregate data highlights one key advantage of using the state-level data to estimate the aggregate multiplier, described later in the paper. As shown in Section 6, the partial F-statistics indicate a stronger instrument when the state-level data are used.

Next, column (2) in 1 contains the 10-year income multiplier. The point estimate equals 1.52 with an SE equal to 0.80. The longer-run multiplier is substantially larger than the shorter-run multiplier, although given the statistical imprecision, one cannot reject that this multiplier is less

[19]The values reported here are lower than those in other studies that use the post-WII narrative defense news shock instrument, notably Ramey (2011a). The difference in partial F-statistics may be that others use quarterly data as opposed to my use of annual data.

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.

than 1 at a conventional confidence level.

The results in columns (1) and (2)in Table 1 will reflect a robust conclusion of this paper. Shorter-run income multipliers are estimated to be less than 1 and the longer-run income multipliers are greater than 1. Columns (3) and (4) contain the analogous results except employment is instead used as the dependent variable. The 4-year employment multiplier estimate equals 0.03 (SE = 0.37).

The 10-year employment multiplier point estimate equals 0.52 (SE = 0.56), which is somewhat larger than the 4-year multiplier but substantially *smaller* than the corresponding income multiplier. This suggests that the increased income created by increased military spending, in the long run, comes from another factor besides labor, such as accumulated capital.

Next, I trace the dynamic paths of the two 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 dynamics are smooth. The multiplier is very close to zero in the first 4 years following a news shock, after which it begins a gradual increase. The multiplier crosses 1 at year 7 and then increases gradually until about year 9. For years beyond 9, the multiplier is roughly constant and equal to 1.5. Note that the confidence interval bands are fairly wide.

Since the sample is limited to 64 years, attempting to estimate multipliers beyond a certain horizon would be problematic. Reassuringly, (in results not reported here) the cumulative income and employment multipliers both level off at approximately the year-16 horizon.

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 news shock, which is often called the "reduced form." Second, the denominator is the cumulative response of spending to the news 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, I estimate the reduced form at each horizon, which is given by

$$Y^c_{t,\delta} = \alpha^Y_\delta R_t + \beta^Y_\delta X_t + v^Y_{\delta,t} \tag{2}$$

Figure 3 plots the coefficients α^Y_δ as a function of δ. To ease interpretation, I scale the news shock R_t such that the shock's cumulative effect on defense contracts at the 10-year horizon equals 10% of one year's income.

Over the first 4 years following the news shock, there is a near-zero response of income to the news. The negligible response is crucial in understanding why the shorter-run income multipliers plotted on Figure 2 are close to zero. Beginning around year 5, the effect on income becomes positive and increases monotonically over the horizons reported

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

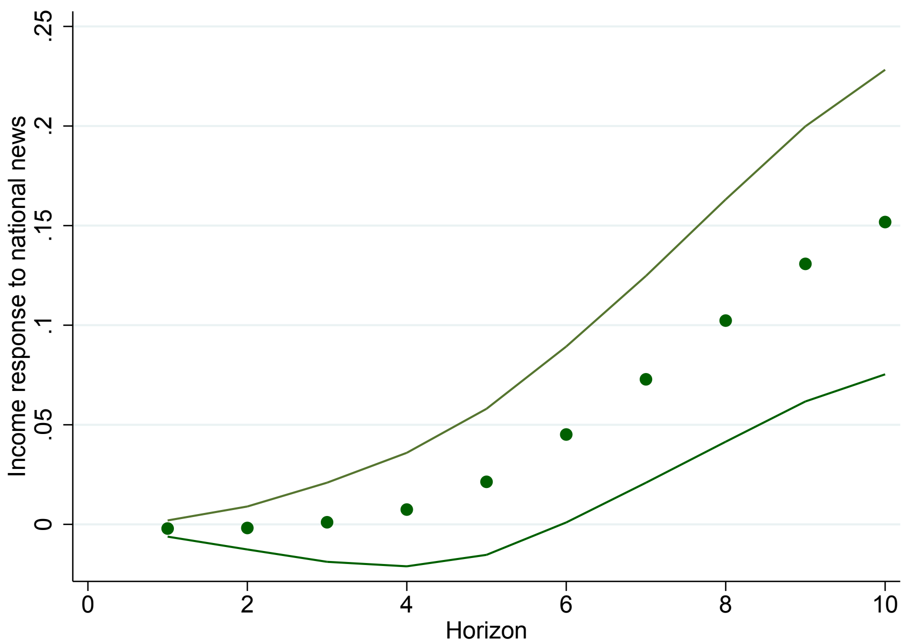
$$G^c_{t,\delta} = \alpha^G_\delta R_t + \beta^G_\delta X_t + v^G_{\delta,t} \tag{3}$$

This impulse response is plotted on Figure 4.

The increase in government spending is gradual and nearly linear in response to the news shock. The result that the spending increase is very long-lived is new to my paper, although it should not be surprising. When one thinks about defense spending shocks, it is natural to think in terms of 10- to 20-year intervals. For example, U.S. involvement in Vietnam began in roughly 1961 (this was the year of Ramey's largest positive defense news shock) but did not end until the early 1970s. As a second example, in 1979 and 1980, toward the end of the Carter administration and the start of Reagan's first term, the U.S. dramatically ramped up military spending as part of an effort to defeat Communism. This policy continued and began to reverse direction only roughly midway through George H.W. Bush's first term.

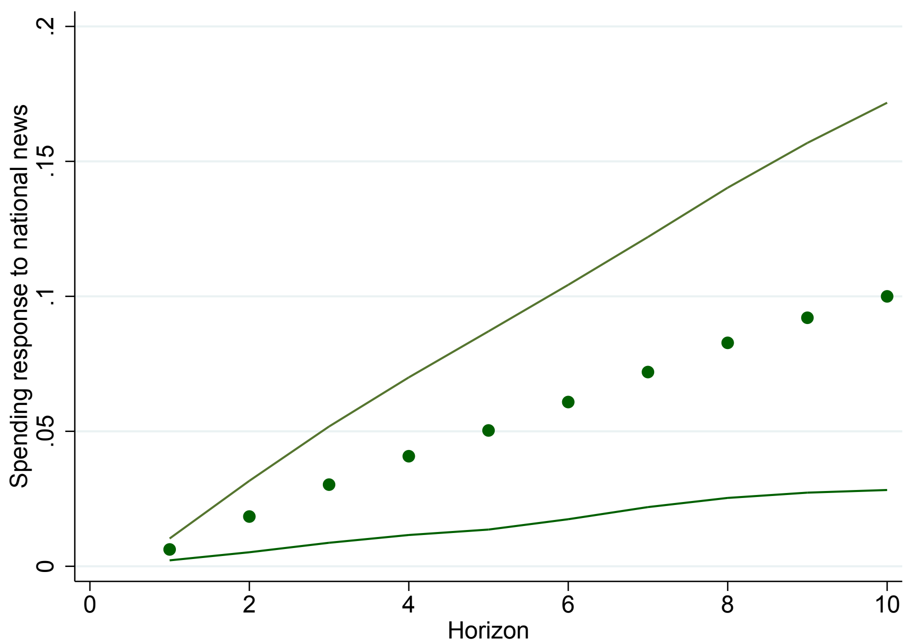
There are two reasons my finding of longer-run spending responses to news shocks is new to the literature. First, most of the relevant studies rely on VARs. VAR analyses of defense news shocks

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



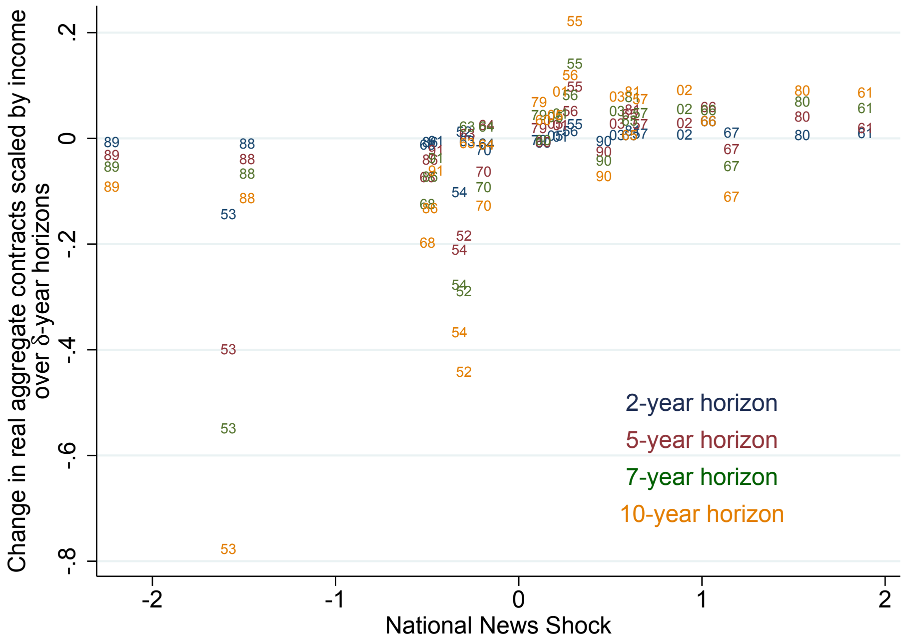
Notes: The scale of the news shock is selected such that the shock's 10-year cumulative effect on defense contracts equals 10% of one year's national income. The solid lines indicate pointwise 90% confidence intervals, which are robust with respect to autocorrelation.

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



Notes: The scale of the news shock is selected such that the shock's 10-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.

Figure 5: Defense spending over different horizons versus military news innovations



Notes: To improve readability, I only plot the data in which $|R_t| > 0.1$. The values shown represent the final two digits of the respective years of the shock.

find that, for the most part, the effect of defense news dies out after approximately 5 years.[20] The VARs used in those studies are typically based on quarterly data and stick to lag lengths between 2 and 4. As such, they do not take into account autocovariance properties of the data that are of longer lags. One advantage of the local projections approach is that impulse response functions are constructed in such a manner that longer lag autocovariances are implicitly built into the estimated relationships. Second, the studies that do use local projections to compute impulse responses via the local projections approach focus exclusively on shorter horizons.[21]

Next, I explicate why the estimates imply long-lived effects of news of defense spending. Figure 5 contains a scatterplot of $G^c_{t,\delta}$, i.e., accumulated changes in defense spending relative to national income, plotted against the military defense news innovation R_t. For each defense news value for a particular year, I plot the change in spending variable at four different horizons δ: 2, 5, 7 and 10 years.

Each of the four horizon points corresponding to a particular t is marked with the final two digits of that year. Each of the four horizon points for a particular year is distinguished by a

[20]See, for example, Ramey (2011a).

[21]These include Owyang, Ramey and Zubairy (2013) and Ramey and Zubiary (2014).

different color.[22] To interpret the figure, consider a particular year: 1953. This year witnessed the second largest (in absolute value) negative shock in my sample. Its value equals approximately -1.6. The 1953 shock occurred as a result of the armistice between the United States and North Korea, which ended the Korean War, and the significant drawdown in U.S. military involvement that followed. Because of the end of the war, there was a real defense spending decline in the years that followed. The total cumulative decline at the 2-year horizon (marked by the blue "53") equaled roughly 0.1% of one year's national income.

Next, observe that at the 5-year horizon (indicated by the number "53" in red) the accumulated change in military spending has fallen even further: equaling roughly 0.4%. Had the decline in spending run its course following news of the end of the Korean War in 1958, then the red "53" would lay approximately on top of the blue "53"; however, this is not the case. Instead, the value of $G^c_{1953,\delta}$ continues to fall with δ. Rather than having a 2- or 5-year impact on military spending, the news of the end of the Korean War still fed into decreased military spending even 10 years following the arrival of the original news.

This pattern can be seen in many other years as well. For a substantial number of years (t), the value of $G^c_{t,2}$ starts close to the horizontal line at zero and then drifts away, holding fixed t, as δ becomes larger. Its value either increases for positive news shocks or decreases for negative news shocks.

Thus, this scatterplot gives a more detailed explanation of the activity in Figure 4. The news shocks have gradual and long-lasting effects on U.S. defense spending during this period.

Next, I estimate (1) but I use the accumulated percentage change in employment as the dependent variable. Figure 6 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. At each horizon, one is unable to reject a zero employment effect with 90% confidence. The estimate stabilizes at roughly 0.5 after horizon 7.

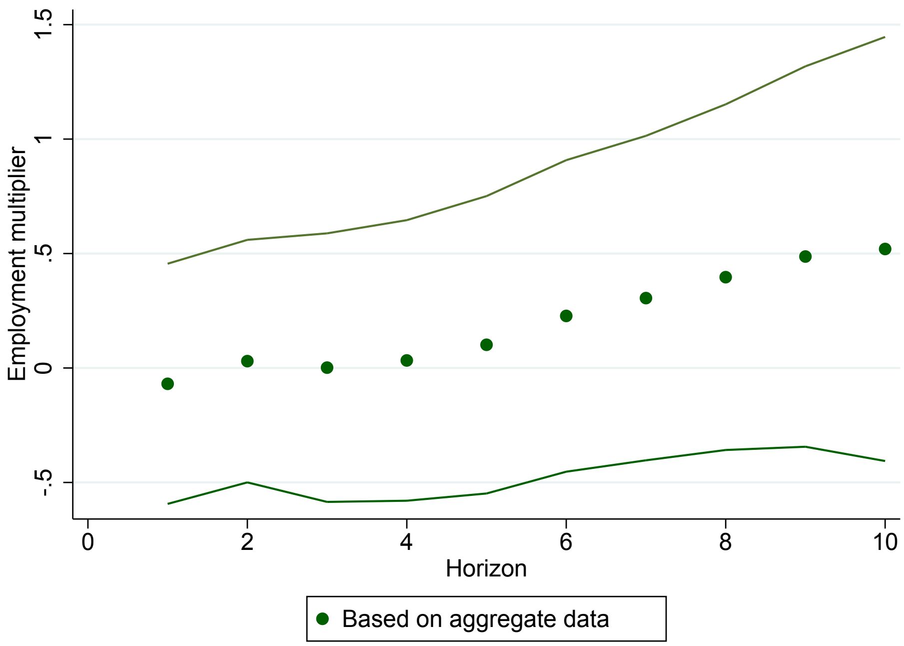
4.3 Comparison with other military spending measures

One concern may be that my 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, I compare the income and employment multipliers based on the aggregated contract data with the same specification estimated using total BEA-measured defense spending.

Figure 7 plots the estimated income multipliers using the BEA defense measure (red "x" marker)

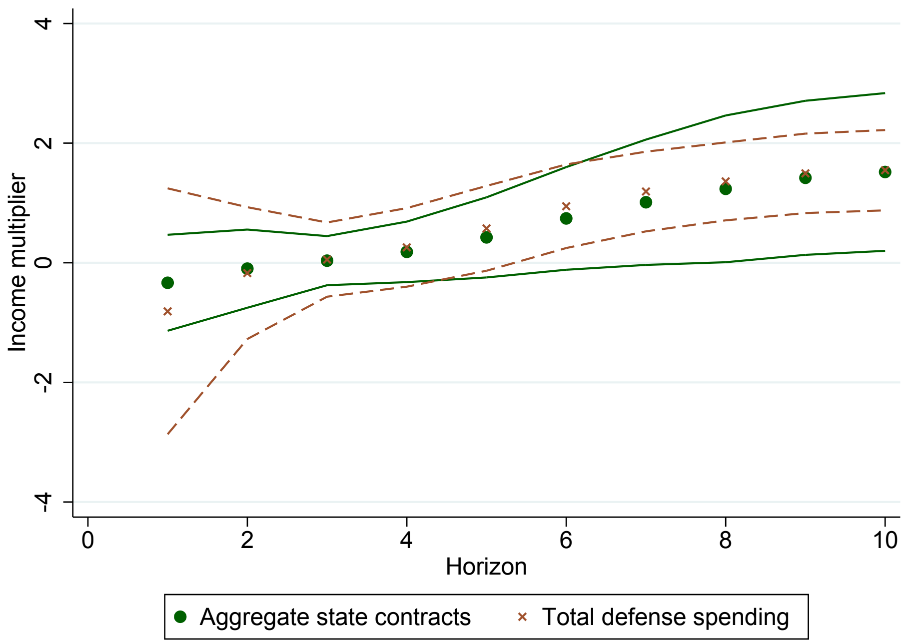
[22] In a number of years, R_t is close to or equal to zero. To avoid cluttering the figure, I do not plot observations that are zero or very small (in absolute values).

Figure 6: 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.

Figure 7: 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 BEA-measured robust 90% confidence interval based on total defense spending. The solid green lines show the robust 90% confidence interval based on aggregate state contract data.

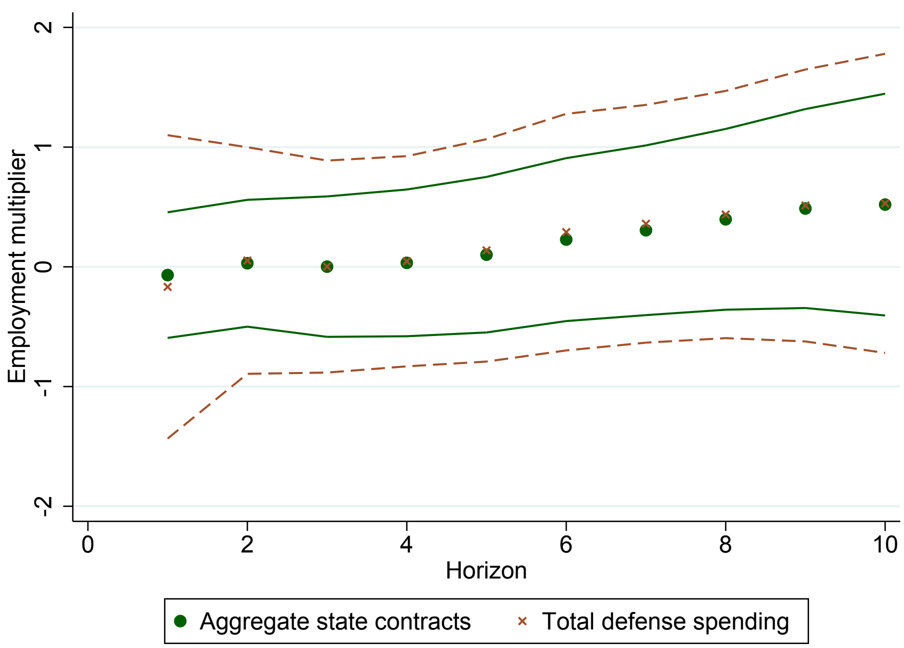
and the associated 90% confidence interval (red dashed lines). For comparison, I plot the benchmark estimates—that is, using the aggregated contract data, using green circles and solid lines for the 90% confidence intervals. The figure shows that: (i) the point estimates are similar across the two specifications, and (ii) there is substantial overlap of the confidence intervals.

Figure 8 plots the analogous estimates but for the employment rather than the income multipliers. At every horizon, the employment multiplier estimated using the BEA measure is very similar to that based on my aggregated contract data. The confidence intervals share a similar shape. Together, Figure 7 and 8 are reassuring in that my new measure of military spending gives income and employment multipliers very similar to those based on a more traditional aggregate defense spending measure.

5 Local Multipliers with State-level Data

In this section, I estimate income and employment multipliers using state-level data. My first result is that the additional data increase the precision of the estimates; however, these multipliers cannot

Figure 8: 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 BEA-measured robust 90% confidence interval based on total defense spending. The solid green lines show the robust 90% confidence interval based on aggregate state contract data.

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.

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.[23]

To my knowledge my paper is the first to use the same dataset to estimate both local multipliers and aggregate multipliers. This approach is useful because it tells us, in the context of a specific empirical example, the direction and magnitude of the difference between the local 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

$$Y^c_{i,t,\delta} = \psi_\delta G^c_{i,t,\delta} + \pi_{i,\delta} X_t + w_{i,t,\delta} \tag{4}$$

In my baseline specification, I also include both state and year fixed effects. X_t is the same set of control variables as in the aggregate regression, and the coefficients on the linear trend are state specific. In each use of the panel data, I 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.

I require an instrument to estimate (4). The instrument should vary over both time and states. As with its aggregate counterpart, $G^c_{i,t,\delta}$ may be partially anticipated. In addition, 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 correcting for this endogeneity would likely bias our estimates of the multiplier downward.

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

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

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

Table 2: Response of income to defense spending news 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	0.26**	0.26**	0.38*	0.38**	0.18
income multiplier	(0.13)	(0.12)	(0.20)	(0.19)	(0.31)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	30.62	31.04	11.54	12.51	3.54
N	2634	2634	2634	2634	53

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

This is the Ramey news variable 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 $t-2$. My 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, I 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 can vary substantially from each other. Whether these two differ—and the direction of the difference—depends on the horizon of the cumulative multiplier, the dependent variable under consideration, and the fixed effects assumption used in the state-level analysis. In short, these estimates suggest that *no reliable information about aggregate multipliers can be acquired solely from estimating local multipliers.*

Table 2 contains estimates of the 4-year local income multiplier from the state-level panel under various specifications. Column (1) reports the multiplier and partial F-statistic when I include neither state nor year fixed effects. The coefficient equals 0.26 (SE = 0.13).

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 a significant increase in the income multiplier. The multiplier in column (4) equals 0.38 and is statistically different from zero at a 95% confidence level. I also report the corresponding benchmark aggregate multiplier in column (5) estimated earlier in the paper. Note that the aggregate multiplier is less than half of the local multiplier in columns (3) and (4).

Table 3: Response of income to defense spending news shock: aggregate and state-level panel analysis at a 10-year horizon

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
10-yr cumulative	1.49***	1.49***	0.51	0.52	1.52*
income multiplier	(0.32)	(0.30)	(0.42)	(0.32)	(0.80)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	27.73	29.17	8.60	10.19	3.87
N	2634	2634	2634	2634	53

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

Table 3 contains estimates of the 10-year cumulative income multiplier. The aggregate multiplier reported in column (5) equals 1.52 (SE = 0.80). Two of the corresponding local multipliers, one with no fixed effects and one with state fixed effects only, are both estimated to be 1.49. By itself, these estimates are encouraging. The point estimates between these two local multipliers are similar to the aggregate multiplier; moreover, there is a more than 50% reduction in the SE.

The situation changes 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 fall to 0.51 and 0.52. For these two later specifications, the local multipliers are dramatically less than the aggregate multiplier.

The hit-or-miss nature of the local multiplier as a good estimate of the aggregate multiplier exists for the employment multipliers as well. Tables 4 and 5 present the 4-year and 10-year cumulative local employment multipliers.

At the 4-year horizon, the aggregate multiplier equals 0.03, while the local multipliers range from 0.17 to 1.16 depending on whether and how fixed effects are introduced. As seen in Table 5, the local multipliers are similarly unreliable estimates of the aggregate multiplier for employment at the 10-year horizon.

The above results based on state-level data may, by themselves, be discouraging for a researcher hoping to learn something about aggregate policy effects from disaggregate data. In the following section, I will 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, I have sufficient variation to estimate this spillover effect. This will involve including

Table 4: Response of employment to defense spending news 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	0.28**	0.17	1.16***	0.73***	0.03
employment multiplier	(0.12)	(0.11)	(0.37)	(0.28)	(0.37)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	30.89	31.04	11.54	12.51	3.54
N	2634	2634	2634	2634	53

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

Table 5: Response of employment to defense spending news shock, aggregate and state-level panel analysis, 10-year horizon

	State-level panel data				Aggregate data
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
10-yr cumulative	0.94***	0.69***	2.41***	1.37**	0.52
employment multiplier	(0.24)	(0.22)	(0.76)	(0.54)	(0.56)
State FE	No	Yes	No	Yes	No
Year FE	No	No	Yes	Yes	No
Partial F statistic	27.96	29.17	8.60	10.19	3.87
N	2634	2634	2634	2634	53

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

both state-level defense spending as well as national defense spending in the state-level regressions. I 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 I 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 estimates will line up closely, the state-level based estimates will have much smaller standard errors.

6 Aggregate Multipliers using State-level Data

In this section, I estimate the state-level regression except I 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

$$Y^c_{i,t,\delta} = \gamma^Y_\delta G^c_{i,t,\delta} + \phi^Y_\delta G^c_{t,\delta} + \beta^Y_{i,\delta} X_t + v^Y_{i,t,\delta} \tag{5}$$

I also include state fixed effects in my benchmark specification. Only the linear trend is allowed to be state specific.

Equation (5) 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 my knowledge, this paper is the first to estimate both effects.

In addition to the instrument $Z_{i,t}$ described previously, I also include R_t as an aggregate instrument in order 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 (5) at the 4-year horizon. The state spending coefficient equals 0.49, which implies that holding fixed national spending, a $1 increase in state defense spending increases state income by roughly $0.49. The corresponding coefficient on national spending is -0.25. Thus, holding fixed state spending, an increase in national spending decreases state income.

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

Table 6: Cumulative income multipliers based on state-level data and on aggregate data: 4-year and 10-year horizons

	4-year horizon		10-year horizon	
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.49**	-	0.92	-
	(0.25)		(0.57)	
National spending	-0.25	0.18	0.74*	1.52*
	(0.19)	(0.31)	(0.42)	(0.80)
Total Multiplier	0.25**	0.18	1.65***	1.52*
	(0.12)	(0.31)	(0.27)	(0.80)
Partial F statistic	7.30	3.54	5.77	3.87
N	2634	53	2634	53

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

on national spending is 0.18. By construction, the aggregate multiplier is equal to the coefficient on national spending, so I 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 similar. Both point estimates imply a 4-year cumulative multiplier that is positive and not greater than 1.

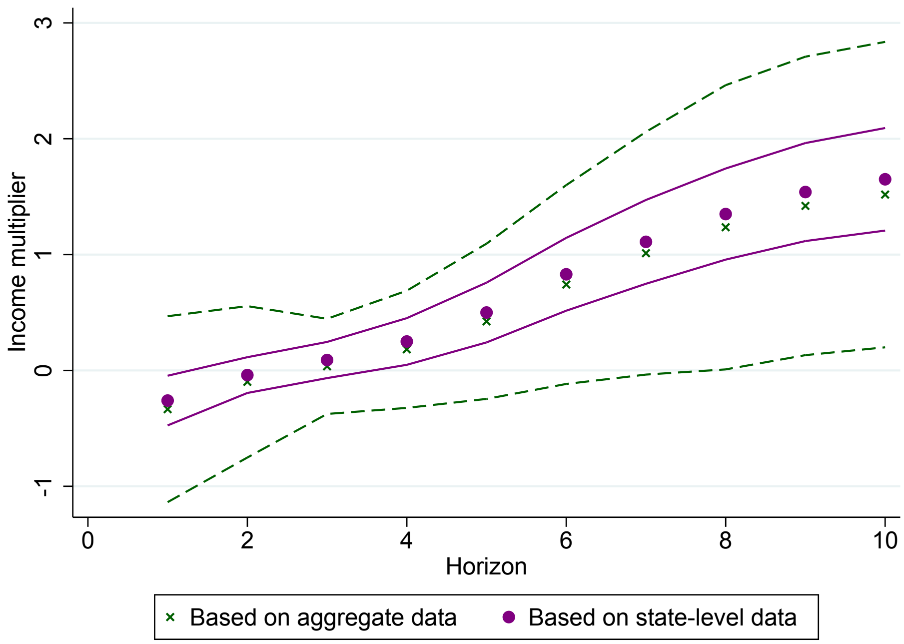
Since the two estimates deliver similar results, the curious reader may ask "Why go to the trouble of using the disaggregate data at all?" The payoff is that the SEs are substantially lower using the state-level data. Specifically, the SE falls from 0.31 to 0.12. This is because there are many more observations of how an individual state responds to national spending than there are observations of how the nation as a whole responds to national spending. Also, there is a substantial increase in the partial F-statistic for the panel data case.

Next, columns (3) and (4) in Table 6 contain the analogous estimates for the multipliers at the 10-year horizon. Examining column (3), there is now a positive spillover effect on income. Holding fixed own-state defense spending, if national military spending increases by 1% of national income, then own-state income increases by 0.92%. When this positive spillover effect is combined with the positive direct effect, the resulting aggregate multiplier is substantial, equaling 1.65.

Column (4) in Table 6 reports the aggregate multiplier using the aggregate data. As in the 4-year case, the aggregate multiplier estimated in either manner gives a quantitatively similar picture of how defense spending influences national income. The results imply that positive spillovers become quantitatively important at longer horizons following a news shock.

Figure 9 plots the cumulative aggregate income multiplier (green "x") at the various horizons based on the state-level data. The cumulative aggregate income multipliers (red circles) based

Figure 9: Aggregate cumulative income multiplier based on state-level data and aggregate data: various horizons



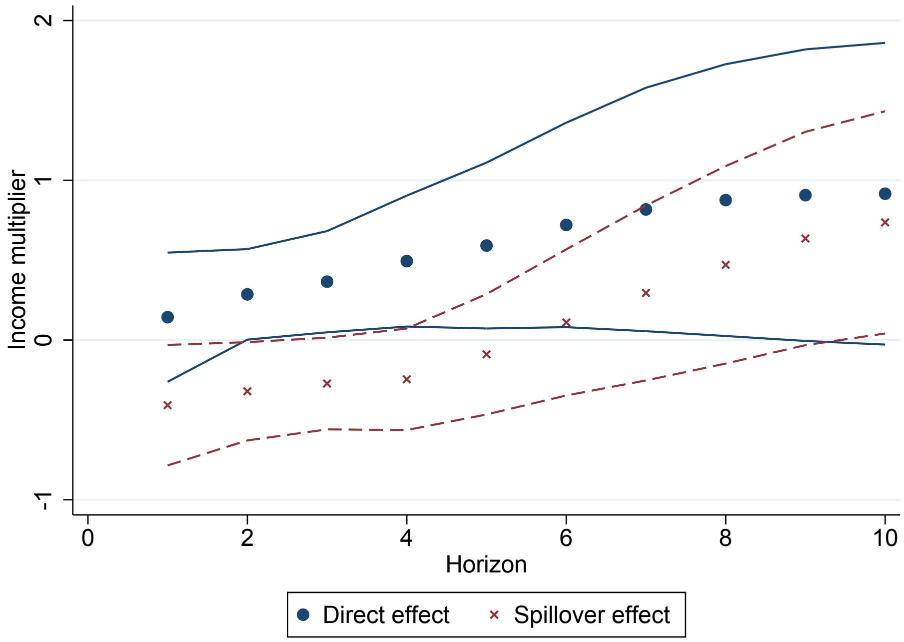
Notes: Solid lines indicate robust pointwise 90% confidence intervals.

on the aggregated 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. For example, at the 10-year horizon one cannot reject a cumulative multiplier that equals 2.5 or one that equals 0.5 with 90% confidence based on the aggregate data. For the method based on the state-level data, at the same horizon one can easily reject a multiplier greater than 2.5 or less than 1 at the same level of confidence.

Next, I plot the spillover and direct cumulative multiplier coefficients as a function of the horizon on Figure 10. Observe that the spillover coefficient at short horizons (through year 4) is negative, but not statistically distinguishable from zero. At short horizons, there is either a small positive or a negative spillover from national defense spending on a state's income, after controlling for state defense spending. The direct effect is positive at short horizons. Over the first few years, the positive direct and negative spillover effects largely offset, each other, which leaves the total effect equal to approximately zero. At longer horizons, the spillover coefficient for income becomes positive. Combined with a positive direct effect, the aggregate effect becomes more substantial.

Figure 10: Direct and spillover cumulative income multipliers based on state-level data: at various horizons



Notes: The solid red lines indicate the robust pointwise 90% confidence interval for the state-level based data. The dashed green liens 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 4-year and 10-year horizons

	4-year horizon		10-year horizon	
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.55**	-	1.01**	-
	(0.27)		(0.49)	
National spending	-0.49*	0.03	-0.46	0.52
	(0.29)	(0.37)	(0.50)	(0.56)
Total Multiplier	0.06	0.03	0.56***	0.52
	(0.13)	(0.37)	(0.21)	(0.56)
Partial F statistic	7.30	3.54	5.77	3.87
N	2634	53	2634	53

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

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	0.49**	0.39	0.10	0.17**
	(0.25)	(0.25)	(0.07)	(0.07)
National spending	-0.25	-0.15	0.16**	0.10
	(0.19)	(0.21)	(0.07)	(0.08)
Total Multiplier	0.25**	0.24*	0.25***	0.26***
	(0.12)	(0.13)	(0.07)	(0.07)
Partial F statistic	7.30	6.59		
N	2634	2634	2634	2634

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

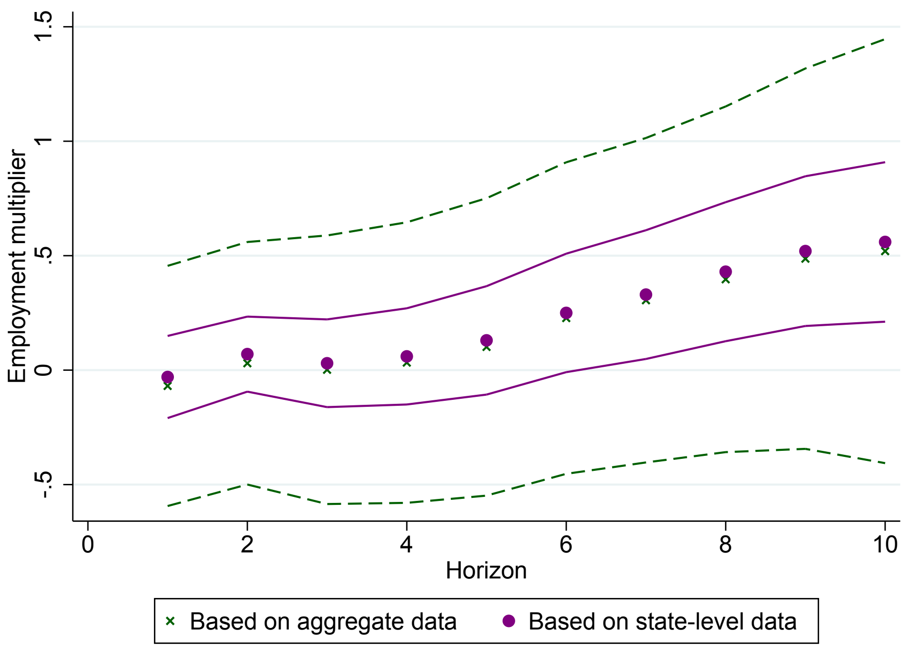
Table 7 contains the analogous estimates as Table 6 but for employment. At both horizons, the cumulative aggregate employment multiplier is never substantially greater than 1 and less than 1 in three of the four cases. In particular, at the 4-year horizon, both methods estimate a multiplier less than 0.5. This calls into question the ability of defense spending to substantially increase employment in the short run.

One noteworthy aspect of the table is the negative coefficient on national spending, despite the fact that the coefficient on state spending is positive. Thus, while within-state military spending increases state employment, federal military spending by itself crowds out state employment. The first effect is straightforward. Increasing military spending locally increases economic activity locally. The second effect is consistent with the following explanation. As military spending elsewhere in the nation increases, this makes work in other states more attractive. As workers in the state respond to these out-of-state opportunities, within-state employment falls.

Figure 11 presents the analogous information as in Figure 9 but for employment instead of income. There are three things to note. First, at shorter horizons the cumulative multiplier (estimated either way) is approximately zero. It then increases at longer horizons until it levels off at approximately year 7. Second, as with the income multiplier, using state-level instead of aggregate data greatly sharpens the precision of the estimates.

Next, I report a few robustness checks on the results. Tables 8 and 9 compare the benchmark results with cases when state fixed effects are dropped and the least squares method is used instead of instrumental variables. Comparing columns (1) and (2) of Table 8 shows that the exclusion of fixed effects has a negligible effect on the coefficients of the income multiplier. Comparing columns (1) and (3) of that table shows that using the least squares method has a negligible effect on the

Figure 11: 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 12: Direct and spillover cumulative employment multipliers based on state-level data: at various horizons

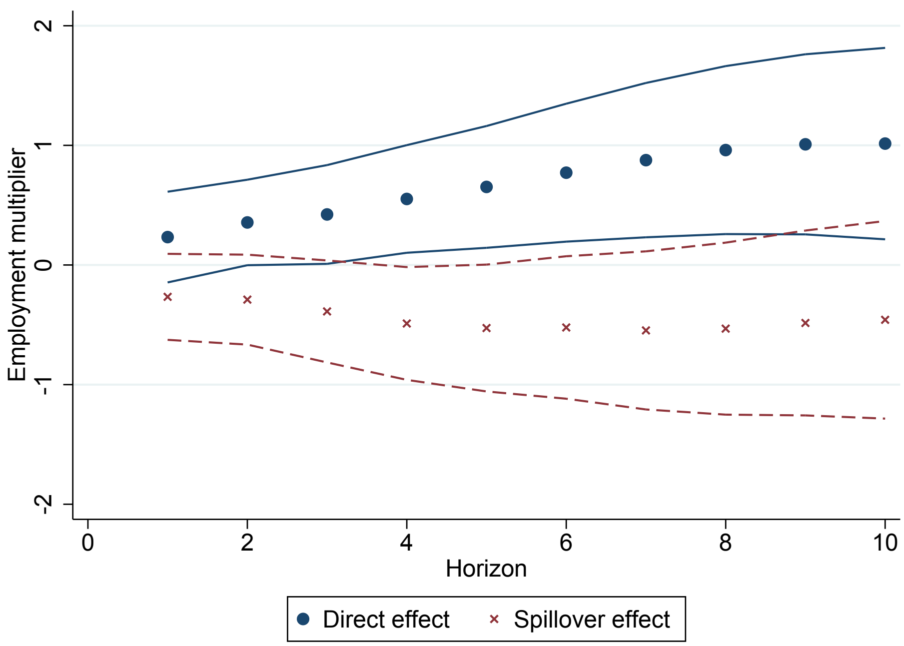


Table 9: Four-year cumulative employment multipliers with and without state fixed effects

	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	0.55**	1.12***	0.10	0.29***
	(0.27)	(0.35)	(0.07)	(0.08)
National spending	-0.49*	-0.99***	0.32***	0.16
	(0.29)	(0.34)	(0.07)	(0.12)
Total Multiplier	0.06	0.13	0.42***	0.45***
	(0.13)	(0.16)	(0.06)	(0.10)
Partial F statistic	7.30	6.31		
N	2634	2634	2634	2634

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

estimate of the total multiplier; however, there is a change in the direct and spillover coefficients. The direct effect becomes less positive and the spillover effect switches from negative to positive. This may be related to Ramey's finding, based on aggregate data, that failing to account for anticipation effects can lead to an upwardly biased estimate of the aggregate output multiplier.

With respect to employment, Table 9 shows that the inclusion of fixed effects in the instrumental variables estimates has little effect on the total multiplier, although it does change the sizes of the direct and spillover multipliers in a way that they offset one another. Comparing columns (1) and (3) of 8 shows the total employment multiplier is substantially larger in the least squares case. Again, this may be related to the inability of least squares to account for potential anticipation effects of fiscal policy.

7 Conclusion

In this paper, I 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. I show that without accounting for spillover effects, local multiplier estimates are not informative about the aggregate multipliers.

My 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, by decomposing the aggregate multiplier into direct and spillover effects, I establish the existence of interesting spillovers. At shorter horizons, *ceteris paribus* there is a negative effect of national spending on state employment. At longer horizons, *ceteris paribus*, the spillover effect on state income is positive. It would be useful to build microfounded models in which the spillover mechanisms might operate.

Finally, since I 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 potential bias from anticipation effects, as well as the endogeneity of fiscal policy, along both athe 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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