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Local Fiscal Multipliers, Negative Spillovers and the Macroeconomy*

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Abstract

This paper analyzes the impact of within-state military spending and national military spending on a state's employment. I estimate that, while within-state spending increases that state's employment (i.e., a positive local effect), an increase in national military spending *ceteris paribus* decreases employment in the state (i.e., a negative spillover effect). The combined local and spillover effects imply an aggregate employment effect that is close to zero. 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. I find support for this interpretation based on estimates of population changes by demographic groups in response to spending shocks.

1 Introduction

Recent research has used cross-regional variation in government fiscal policy to estimate the effects of the policy on state-level economic activity.[1] For example, researchers have estimated the causal impact of relative differences in government stimulus spending on relative differences in output across states.[2] The estimates resulting from these studies are known as "local multipliers." Existing local multiplier research has informed policymakers about the relative effects of policies across states or regions, but not their aggregate effects.[3]

The local multiplier will be only indirectly related to an object of particular interest to macroeconomists, the aggregate multiplier. This is because of spillovers across states. Sources of spillovers might include trade in goods, movements in factors of production, common monetary policy or common fiscal policy, among others. For example, if government purchases in state X lead to

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[1]See, for example, Chodorow-Reich et al. (2012), Clemens and Miran (2012), Shoag (2012) and Suarez Serráto and Wingender (2014).

[2]See Conley and Dupor (2013) and Wilson (2012).

[3]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."

iniflows by some state Y workers into state X, then there will be a negative spillover of spending in state X on state Y. This spillover channel will lead the local multiplier to be an upwardly biased estimate of the aggregate multiplier. Alternatively, 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.

In this paper, I extend the local multiplier approach to include the spillover effects of military spending in one state on the economic activity of other states.[4] I use state-level U.S. military expenditures covering most of the post-WWII period. I then combine the spillover effect and the local effect into a total multiplier. To the extent that the spillover variable encompasses the interaction between states, the total multiplier reflects the aggregate effect of military spending on economic activity.

I have two treatment variables: national military spending and state-level military spending.[5] Two econometric hurdles arise: endogeneity bias and anticipation effects. With regard to endogeneity, national military spending is plausibly exogenous with respect to the national business cycle because U.S. defense spending is driven by international geopolitical factors. National military spending, however, may be in part anticipated by businesses and households, which can lead to biased results. To avoid this bias, I instrument for national military spending using the Ramey (2011a) defense spending news shock series.

Next, state-level military spending may suffer from endogeneity if the allocation of defense contracts is influenced by economic factors. For example, federal legislators from currently worse-off economic regions may be more successful in steering military contracts to their states. Also, state-level military spending, like national military spending, may be in part anticipated. Because of these two concerns, I instrument for state-level military spending using state-by-state weighted values of the Ramey news shock series. The weights are determined by lagged values of state-specific variables in the spirit of Bartik (1991). I show that the national and state-level Ramey instruments are strong predictors of national and state-level military spending.

My primary findings concern the causal impact of state and national military spending on state employment. I estimate a positive effect of state military spending on state employment, holding fixed national military spending. I estimate a negative effect of national military spending on state employment, holding fixed state military spending. I call this second effect a negative fiscal policy spillover.

To preview the results, I temporarily put aside the issues of endogeneity and anticipation effects

[4]The two papers most closely related to mine, with respect to estimating spillovers, are Dupor and McCrory (2015) and Suarez Serráto and Wingender (2014). Those papers find positive spillovers between geographically neighboring states. The finding of negative "macro" spillovers in the current paper is not inconsistent with the finding of positive nearby-neighbor spillovers in those papers.

[5]By state-level military spending, I mean federal military procurements and military employee compensation that occur within a state's geographic borders. Other papers that use state military spending to estimate the effects of fiscal policy include Hooker and Knetter (1997) and Davis, Loungani and Mahidhara (1997).

to demonstrate the finding graphically. Figure 1 contains a scatterplot with the 3-year growth rate of national military spending as a percentage of national output on the horizontal axis. The vertical axis contains the 3-year growth rate of state employment after conditioning on state military spending growth, macro shocks and state-specific time trends. The downward sloped line is the best linear predictor between the two variables. It has a slope equal to -0.97.[6] Thus, holding fixed a state's own military spending, a 1% increase in national military spending per capita causes a 0.97% decline in a state's employment.

In the remainder of the paper, I show that this negative conditional relationship between state employment and national military spending holds when I apply instrumental variables (IV). It also holds under alternative specifications, including adding further conditioning variables, controlling for aggregate shocks and conducting a subsample analysis and applying an alternative measure of defense spending.

My results can be explained by the cross-state reallocation of resources in response to the spending shocks. According to this explanation, *ceteris paribus* military spending within a state increases employment in that state as nonworking state residents accept jobs and individuals move from other states in response to new economic opportunities. When national military spending increases (holding fixed a state's spending), individuals in that state emigrate in search of opportunities created by military spending elsewhere.[7] I show that population counts, broken down by demographic groups, are consistent with the above explanation. The population of those more likely to move in response to economic opportunities (which I define as adults under 50 years of age) grows in states where defense spending increases and declines in states which, holding fixed state defense spending, have an increase in national defense spending.

2 An econometric model

Let $Y_{i,t}$ and $G_{i,t}$ denote the per capita year t, state i output and military spending, respectively. Let $N_{i,t}$ denote employment in state i during year t. My sample covers all 50 states from 1966 through 2009.

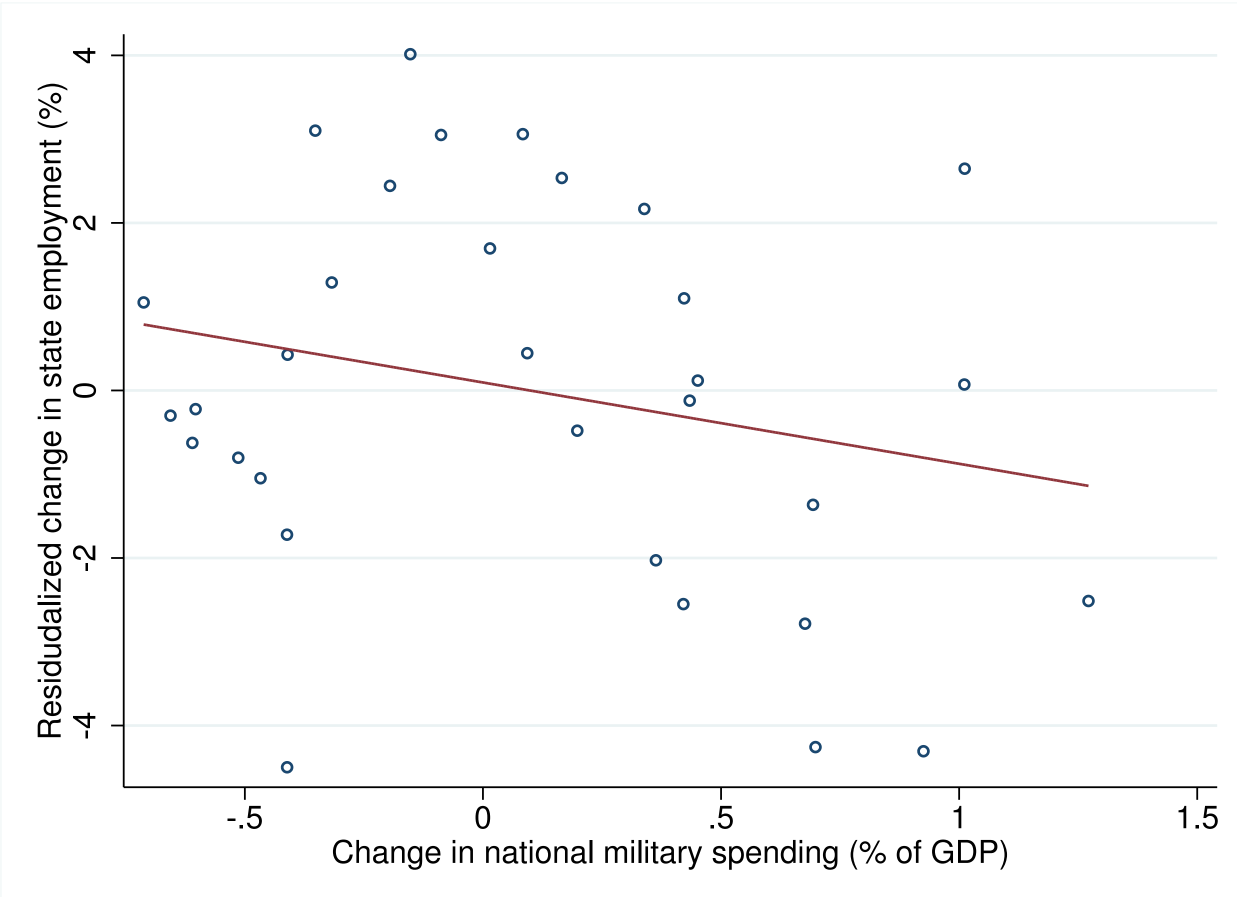
Let $N_{i,t}^{\delta}$ be the cumulative percentage increase in employment over a δ-year horizon relative to a year $t-1$ employment baseline:

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

[6] Since there are over 1,500 state-year data points, I do not plot each point but rather separate the data into 20 bins based on their x-value. Each point on the graph represents the mean y- and x-value within a particular bin.

[7] Blanchard and Katz (1992) analyze mechanisms by which goods and factors adjust in response to regional business cycles, including worker mobility.

Figure 1: The effect of national military spending on state employment after controlling for state military spending



Notes: State employment is the 3-year percentage change after controlling for state military spending growth, state-specific time trends, and macro variables. National military spending is the 3-year change as a percent of output. The line represents the best linear predictor and has a coefficient equal to -0.97. To make the graph more legible, I place x-variables into 20 equally sized bins. Within each bin, I plot the mean values of the x-axis variable against the mean values of the y-variable.

Next,

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

This is the cumulative increase in military spending over a δ year horizon relative to a year $t-1$ military spending baseline, all of which is scaled by $Y_{i,t-1}$. Let N_t^{δ} and G_t^{δ} denote the aggregate analogs of $N_{i,t}^{\delta}$ and $G_{i,t}^{\delta}$.

Defining these variables as such permits me to estimate cumulative multipliers.[8] Cumulative multipliers report 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 $G_{i,t}^{\delta}$ implies that this variable should be interpreted as the change in military spending as a percentage of one year of output.

I estimate the model using the generalized method of moments (GMM) which, in this case, has a two-stage least-squares interpretation. The second-stage equation is

$$N_{i,t}^{\delta} = \alpha G_{i,t}^{\delta} + \phi G_t^{\delta} + \beta_i X_t + v_{i,t} \tag{2.1}$$

Here X_t consists of a linear time trend and four macro variables. The four macro variables are the growth rate of the price of oil, the real interest rate and one lag of each of these.[9] The coefficient on the linear time trend is state-specific. Equation (2.1) allows one to parse the distinct effects of state and national military spending on state employment. A number of authors have estimated the first of the two effects; however, to my knowledge, the current paper is the first to simultaneously estimate both effects.

Estimating (2.1) using least squares would suffer from two difficulties. First, some changes in military spending are likely to be anticipated. To address this issue, Ramey (2011a) constructed a time series for period-by-period changes in the expected present value of national U.S. military spending scaled by gross domestic product (GDP). I use this variable, R_t, as an instrument for G_t^{δ}.

Second, state-level military spending may suffer not only from anticipation effects, but also may be endogenous to local business cycle conditions. The second instrument is the Ramey news variable multiplied by a state-specific scaling factor.

$$Z_{i,t} = \left(s_{i,t}^{G} / s_{i,t}^{Y} \right) R_t$$

The scaling factor is the ratio of a state's share of national military spending, $s_{i,t}^{G}$, divided by the state's share of national output, $s_{i,t}^{Y}$. Both shares are computed as the average over years $t-2$ and

[8]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.

[9]The real interest rate is measured as the average federal funds rate minus the year-over-year consumer price index (CPI) growth rate.

Table 1: First-stage estimates: the impact of national and state news shocks on national and state military spending

	State spending (1) Coef./SE	National spending (2) Coef./SE
State news	28.25***	0.08
	(3.20)	(1.81)
National news	-4.36	23.71***
	(3.71)	(2.10)
N	1476	1476

Notes: The regressions are population weighted and include, as conditioning variables, state-fixed effects and state-specific linear trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

$t-1$. My use of lagged shares to construct state-specific time-varying instruments is motivated by Bartik (1991).

Population data are from the U.S. Census. Employment data are from the Bureau of Labor Statistics. State output measures GDP constructed by the U.S. Bureau of Economic Analysis. Military spending includes compensation to personnel and procurements and is from Nakamura and Steinsson (2014). Both state GDP and military spending are deflated by the national CPI. I estimate the model using population weights. I consider a 6-year horizon over which to compute the employment and military spending variables.

To assess instrument relevance, I first examine the least-squares regressions of the endogenous variables on the two news instruments and the conditioning variables (Table 1). Column (1) contains the state-level spending regression. State military news, $Z_{i,t}$, is a strong predictor of state spending, with a t-statistic greater than 9. Column (2) contains the national spending variable regression. National military news is a strong predictor of national military spending, with a t-statistic eqaul to 11.3. The Cragg-Donald partial F-statistic for the first stage is 53.8, which is further evidence of strong instruments.

3 Estimates

3.1 Benchmark Specification

Column (1) of Table 2 reports my benchmark estimates.[10] The coefficient on state spending equals 2.28 (SE=0.63). This indicates that if military spending in a state increases by 1% of state GDP over the first 6 years following a news shock (conditional on national military spending), then state employment increases by 2.28% over the same 6-year horizon. By itself, this might be taken as

[10]The estimates are computed with the STATA command *ivreg2* using population weights and the *gmm2s* option.

Table 2: Response of state employment to military spending shocks

	With spillover	Without spillover
	(1)	(2)
	Coef./SE	Coef./SE
State spending	2.28***	1.09***
	(0.63)	(0.36)
National spending	-1.70**	-
	(0.77)	
Total Response	0.59	1.09
	(0.44)	(0.36)
Partial F statistic	56.75	233.09
N	1476	1476

Notes: The model is estimated by two-step GMM. The regressions are population weighted and include, as conditioning variables, four macroeconomic variables and state-specific linear trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

evidence for the effectiveness of macro fiscal policy. However, it fails to reflect potential spillover of spending across states which can have important aggregate consequences.

The coefficient on national spending equals -1.70 (SE=0.76). Measured over the same horizons, if national military spending increased by 1% of national GDP, then (conditional on state military spending) the state's employment falls by -1.70%.

Thus, while 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. Later in the paper, I provide demographic population breakdowns of these regressions and show that they are consistent with this interpretation of Table 2.

The row labeled "Total response" sums the state and national spending responses. It equals 0.59 and is interpreted as the percentage change in national employment when state military spending increases by 1% of state output in every state. The coefficient implies a positive total effect on employment, although it is not statistically different from zero.

Column (2) provides the estimated effect if one omits national military spending from the regression. This corresponds to the traditional local multiplier approach. If one fails to control for national military spending, then an increase in within-state military spending has a positive and statistically significant effect on employment.

Table 3: Response of state employment to state and national military spending shocks: alternative horizons for cumulative employment responses

	1-year (1) Coef./SE	2-year (2) Coef./SE	3-year (3) Coef./SE	4-year (4) Coef./SE	5-year (5) Coef./SE	6-year (6) Coef./SE
State spending	0.12***	0.33***	0.63***	1.05***	1.60***	2.28***
	(0.04)	(0.12)	(0.22)	(0.34)	(0.48)	(0.63)
National spending	-0.16***	-0.43***	-0.73***	-0.96**	-1.24**	-1.70**
	(0.05)	(0.15)	(0.27)	(0.42)	(0.59)	(0.77)
Partial F statistic	56.75	56.75	56.75	56.75	56.75	56.75
N	1476	1476	1476	1476	1476	1476

Notes: The model is estimated by two-step GMM. The regressions are population weighted and include, as conditioning variables, state-specific linear trends and four macro variables. In each specification, the spending variable is accumulated over the first 6 years following the shock, whereas the employment response is accumulated over the particular stated horizon.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.2 Dynamic Responses

In the presence of real-world adjustment costs and time-to-plan behavior by firms and households, one might expect both the local and spillover effects of unanticipated government spending shocks to accumulate gradually over time. Table 3 reports these dynamic responses. I consider a number of specifications in which each additional specification sequentially increases the horizon over which the accumulated employment response is computed by one year. The spending variables, on the other hand, are maintained at the benchmark 6-year accumulated change.[11]

Examining the 1-year horizon response to state spending (column (1) in Table 3), observe that when state military spending increases by 1% of state output (accumulated over 6 years), state employment increases by only 0.12% in the initial year. Thus, the employment adjustment is positive but small in the year of the news shock. As the horizon increases, the response to state spending increases monotonically.

The path of responses to national spending is very similar to that of state spending, apart from being of the opposite sign. The change in national spending has only a small negative effect in the short run. This may be because many workers face an adjustment period in moving to other states that have experienced an increase in government spending. As the horizon increases, the employment response remains negative but becomes monotonically larger in absolute value. When

[11] An alternative approach might be to vary the horizon of both the employment and military spending variables. Given the forward-looking nature of employment decisions, this may yield misleading results. Specifically, employment decisions made in early years are conditional on the expected path of military spending today and in the future. Thus, I contend that the total cost of spending over time should be included in calculating the short-run responses. Consider an extreme example. Suppose no additional military spending occurred in the first year following the shock; then even a tiny first-year increase in employment would generate an infinite causal impact, using the alternative measure.

Table 4: Responses of state employment to state and national military spending shocks: alternative specifications I

	Benchmark	no macro shocks	+spread	+lag news	+lag employ
	(1)	(2)	(3)	(4)	(5)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	2.28***	2.19***	2.30***	2.12***	2.19***
	(0.63)	(0.60)	(0.60)	(0.67)	(0.61)
National spending	-1.70**	-3.41***	-1.47**	-1.80**	-1.32*
	(0.77)	(0.68)	(0.75)	(0.77)	(0.77)
Partial F statistic	56.75	55.60	56.66	49.61	55.76
N	1476	1700	1476	1476	1476

Notes: The model is estimated by two-step GMM. Columns (2) through (5) each contain a single departure from the benchmark specification. Column (2) does not weight by population. Column (3) contains the OLS analog of the benchmark results. Column (4) uses the first 5 years of the sample to calculate share weights for the state-specific instrument.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

combined, the local and spillover effects of military spending largely cancel each other out with respect to the overall effect of the spending.

3.3 Alternative Specifications

Table 4 reports estimates for alternative specifications. Column (1) contains the benchmark estimates. In each of the remaining columns, I consider a departure from the benchmark one. Column (2) removes the macro shocks as conditioning variables. The results are similar to the benchmark case, except that the negative national spending response becomes stronger. Column (3) adds the spread between the 10-year Treasury bond and the effective federal funds rate, which is used to condition on the stance of monetary policy. The results are very similar to the benchmark case.

Column (4) of Table 4 adds two lags of the state news instrument to account for potential serial correlation in that variable. Column (5) adds two lags of the state employment growth rate. Both sets of estimates are very close to the benchmark specification.

One alternative specification I do not consider is adding year fixed effects to the benchmark specification. In that case, any effect of national military spending would be soaked up by these dummy variables. In other words, I would lose identification. One of the key benefits of using a panel, by using the time-series variation in national spending, is to go beyond merely comparing *relative* outcomes between states. It is along these same lines that cross-sectional data do not permit considering national spillovers of the kind studied in this paper.

Table 5 provides additional alternative specifications. Again, column (1) contains the benchmark results. Column (2) contains the benchmark specification, except I estimate the model without population weighting. Column (3) contains the ordinary least squares (OLS) estimates. Column

Table 5: Responses of state employment to state and national military spending shocks: alternative specifications II

	Benchmark (1) Coef./SE	no weights (2) Coef./SE	OLS (3) Coef./SE	Bartik inst. (4) Coef./SE
State spending	2.28***	2.79***	0.95***	5.17***
	(0.63)	(0.92)	(0.18)	(1.07)
National spending	-1.70**	-3.66***	-0.93***	-4.58***
	(0.77)	(1.02)	(0.29)	(1.19)
Partial F statistic	56.75	23.96		25.30
N	1476	1476	1476	1476

Notes: The model is estimated by two-step GMM. Columns (2) through (4) each contain a single departure from the benchmark specification.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(4) modifies the instrument used in the estimation. In constructing the state-specific instrument, I scale the Ramey defense news shock by a state's share of defense spending relative to its share of GDP where those shares are calculated over the first 5 years of the sample. Across the specifications, I continue to find statistically significant positive direct and negative spillovers effects.

Table 6 splits the sample by time periods in several ways. Column (1) restates the benchmark estimates for comparison purposes. Columns (2) and (3) contain the estimates depending on whether monetary policy is in an "active" regime—that is, whether it has increased inflation in a greater than 1-for-1 manner in response to an increase in inflation, or alternative "passive" regime. The distinction is important because, as several authors have noted, in some models, the response of economic activity to a government spending shock depends on how responsive the monetary authority is to inflation.[12] Often unresponsiveness is tied to the zero lower bound on interest rates; however, as Dupor and Li (2015) explain, in the above class of models, larger multipliers should also arise when monetary policy is passive relative to when it is active. Dupor and Li (2015) dub this (positive effect on the multiplier) the "expected inflation channel of government spending."

For the passive policy sample (column (3)), note that that partial F-statistic is very small. As such, there is a weak instrument problem, making interpreting the second-stage results problematic. For the active policy sample (column (2)), the state spending coefficient is positive as in the benchmark case. Most interestingly, the national spending coefficient is also positive (although not statistically different from zero). Column (2) is problematic for the expected inflation channel of government spending hypothesis. According to that theory, the direct and spillover effects of government spending should be smaller than they are over the entire sample.

Column (4) in Table 6 excludes the Volcker disinflation years (1979-1982). The state spending

[12]See, for example, Christiano, Eichenbaum and Rebelo (2011) and Woodford (2011).

Table 6: Responses of state employment to state and national military spending shocks: alternative years included

i

	Benchmark (1) Coef./SE	Active policy (2) Coef./SE	Passive policy (3) Coef./SE	Excl. disinflation (4) Coef./SE	Excl. oil shock yrs. (5) Coef./SE
State spending	2.28***	1.51**	-2.96	1.76**	1.63***
	(0.63)	(0.72)	(3.89)	(0.70)	(0.62)
National spending	-1.70**	6.30***	-3.73	0.38	-1.43*
	(0.77)	(1.06)	(4.40)	(0.87)	(0.76)
Partial F statistic	56.75	46.34	0.39	39.44	60.49
N	1476	976	350	1500	1250

Notes: The model is estimated by two-step GMM using the benchmark specification, apart from the years included in the sample. Columns (1) restates the benchmark estimates. Column (2) contains post-1981 data. Column (3) contains pre-1979 data. Column (4) excludes 1979 through 1982. Column (5) excludes 1972 though 1980.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

coefficient equals 1.76 and the national spending coefficient is close to zero. Evidently, the weak performance of the economy during the Volcker disinflation that occurred alongside the military buildup in the early Reagan years plays an important role in my estimate of a negative national spillover effect on employment. Once those years are excluded from the sample, the negative spillover effect is no longer present. Column (5) excludes 1972 through 1980, which span the two largest postwar oil shocks. Excluding the oil shock years generates no qualitative difference from the benchmark specification.

3.4 Responses for Alternative Demographics Groups

One possible explanation for the negative spillover effect is the relocation of labor from states when out-of-state military spending generates employment opportunities elsewhere. If this is the operative mechanism, then one may expect relocation to be more prevalent among younger individuals. Columns (1) and (2) of Table 7 contain the estimates for females and males under 50 years of age. In each case, state spending has a positive and statistically significant effect on the state's population growth for the group. Thus, persons tend to move into states where military spending exogenously increases. In each case, national military spending has a negative and statistically significant effect on the state's population growth for the group. As such, younger individuals, on net, tend to move out of states when there is a shock to national military spending (holding fixed state military spending).

Columns (3) and (4) of Table 7 contain the estimates for persons aged 50 years and older by gender. Among these two groups, within state spending has a much smaller, and statistically

Table 7: Responses of population to state and national military spending shocks: by demographic group

	Female (under 50)	Male (under 50)	Female (over 49)	Male (over 49)
	(1)	(2)	(3)	(4)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE
State spending	0.68**	0.83***	0.04	0.12
	(0.27)	(0.31)	(0.18)	(0.18)
National spending	-0.83**	-0.85**	2.03***	2.16***
	(0.33)	(0.38)	(0.22)	(0.22)
N	1476	1476	1476	1476

Notes: The model is estimated by two-step GMM. The regressions are population weighted and include, as conditioning variables, state-specific linear trends and four macro variables.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

insignificant, effect of population growth. Net migration into a state may be low because workers in these demographic groups may already be satisfied with jobs in their home states, in part because wages typically rise with job tenure. Also, retirees (also contained within these two demographic groups) do not need to move in response to new job opportunities arising from military spending.

The response of population growth among older individuals to national spending, on the other hand, is roughly 2%. Thus, older individuals tend to move into states that do not experience higher military spending when national military spending increases. One potential explanation is that, as younger individuals, on net, move out of these states, the cost of housing and other goods in relatively fixed supply may fall, generating lower cost-of-living opportunities for persons in retirement.

3.5 An Alternative Defense Spending Measure

Next, I redo the analysis using an alternative panel for military spending. The new series are taken from the Department of Defense's "Atlas/Data Abstract for the United States and Selected Areas" hereafter referred to as the DDAD series. These data are available in the U.S. Census *Statistical Abstract of the United States* for most years from 1959 through 2009. The DDAD series cover four additional recessions beyond those in the primary series used in the previous sections. The series also include the additional coverage of the run up to the Vietnam War and much of the Bush military expansion during the Second Gulf War. The two panels have somewhat different coverage in terms of expenditures. For example, the DDAD series include the payroll's of nonmilitary personnel, whereas the primary series do not. Despite a few differences in coverage, the two measures are closely related. The correlation between the growth rate of real per capita military spending for the two measures (in years when both measures are available) equals 0.95.

Table 8: Responses of state employment to state and national military spending shocks: DDAD defense spending measure

	Benchmark (1) Coef./SE	Exclude Disinfl. (2) Coef./SE	Passive years (3) Coef./SE	OLS (4) Coef./SE	Unweighted (5) Coef./SE
State spending	2.35***	1.71**	5.43**	1.05***	3.46***
	(0.62)	(0.76)	(2.47)	(0.11)	(1.18)
National spending	-2.91***	-1.26	-5.67**	-1.51***	-3.90***
	(0.71)	(0.87)	(2.73)	(0.20)	(1.14)
Partial F statistic	41.38	26.13	3.12		11.47
N	2194	1994	894	2243	2194

Notes: The model is estimated by two-step GMM using the Department of Defense Abstract/Data Analysis (DDAD) data. Column (1) restates the benchmark estimates (apart from the DDAD variable change). Column (2) excludes 1979 through 1982. Column (3) includes only pre-1979 data. Column (4) contains the OLS estimates. Column (5) reports unweighted estimates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

One drawback of the DDAD panel is that three years of data are missing. To maximize the number of observations, in Table 8 I report estimates using the DDAD data, buti replace missing values with those from the primary defense spending source.[13]

Table 8 reports the estimates using the DDAD defense series. The upshot is that the table buttresses my baseline findings. Across each specification, I find a positive local effect and a negative spillover effect of defense spending. Column (1) contains the estimates using the benchmark specification, apart from the change in the treatment variable. The coefficient on state spending equals 2.35 and the coefficient on national spending equals -2.91. Both are statistically different from zero at a 1% confidence level. The sum of the two coefficients gives the total employment multiplier. It is negative but not statistically different from zero.

Next, column (2) in Table 8 contains results when the Volcker disinflation is excluded. Both the state and national spending coefficients are of the expected sign, although each is smaller in absolute value relative to the corresponding values in column (1). Moreover, the coefficient on national spending is not statistically different from zero. Column (3) reports results for the passive monetary policy era. Unfortunately, the partial F-statistic indicates a weak instrument problem, rendering the estimates unreliable. Column (4) contains the OLS estimates, which find a positive local effect and negative spillover effect. Note that the coefficient on state spending is smaller than the corresponding IV instrument, indicating that the exogeneity bias works in the expected direction for state spending. Column (5) reports the unweighted IV estimates. Once again, these results confirm my baseline findings.

[13]The missing years are 1971, 1991 and 1993. Also, I scale state military spending by state personal income rather than state GDP because the latter is not available prior to 1963.

4 Conclusion

This paper has identified a cross-state negative spillover channel of defense spending. Conditional on military spending in a state, an increase in military spending nationwide reduces employment in that state. Moreover, the negative spillover effect of national spending is of roughly the same magnitute as the positive direct effect of state spending on state employment. Thus, the economy-wide employment effect of a nationwide increase in military spending is small. These results are robust across nearly all of the specifications considered. Regressions using demographic groups as outcome variables suggest that the negative spillover is due to reallocation of labor across states.

I see three sharp implications of my findings. First, while many researchers have told cautionary tales about interpreting local fiscal multipliers as macroeconomic fiscal multipliers,[14] my paper completes the story by showing that, in the postwar United States, local employment multipliers may overstate the aggregate effect of government spending because of negative spillovers.[15]

Second, my paper may help reconcile aggregate vector autoregression evidence,[16] which often finds small effects on private consumption and employment of government spending with cross-sectional, i.e. local multiplier, studies which often find large effects.[17] Again, this may be due to negative spillovers present because of labor reallocation as evidenced by the population demographic results in my paper.

Third, the exercise in the paper highlights the stringent data requirements necessary to glean information about the aggregate effects of government spending from disaggregated data. 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, the strategy must address potential bias from anticipation effects, as well as the endogeneity of fiscal policy, along both at 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.[18]

[14] See the discussion in the introduction.

[15] Along this dimension, my paper is related to Kline and Moretti (2013), who 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 U.S.

[16] See, for example, Ramey (2011a) and Ramey (2013).

[17] See, for example, Chodorow-Reich et al. (2012) and Shoag (2011).

[18] The major stumbling block may be whether a “defense news shock” series, such as the one constructed by Ramey (2011a), exists for countries besides the U.S.

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