Local Fiscal Multipliers and Defense Spending[*]

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**Abstract**

I estimate county-level fiscal multipliers using shocks to military employment to instrument for local defense spending. Aggregate shocks to military employment are subject to the Base Realignment and Closure process, which is designed to isolate the recommendations of the Department of Defense from political influences. By exploiting variation in military employment, I address the endogeneity of government spending when using changes in defense spending to estimate fiscal multipliers. In addition, this method addresses the attenuation bias due to geographic measurement error that results from using data on military contracts alone with small geographic units. This extends the common method for estimating state- and national-level fiscal multipliers to more local geographic units. My estimates imply a local income multiplier between 0.5 and 0.8, which is smaller than existing estimates that use non-defense based sources of variation, but larger than the existing estimates based on variation in defense contract spending.

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# 1 Introduction

Local fiscal multipliers—the percentage change in local economic activity that results from a 1% increase in government spending—compactly summarize the effect of government spending on the local economy. In order to understand and implement area-specific fiscal policy it is critical to have well-identified estimates of local fiscal multipliers. However, measuring fiscal multipliers, whether at a local or a more aggregate level is complicated by the fact that changes in government spending are often purposefully tied to the state of the economy. For aggregate fiscal multipliers, using cross-sectional variation at the subnational-level for estimation has been a major development in the literature (Ramey 2011a; Chodorow-Reich 2019). Notably, Nakamura and Steinsson (2014) combine data on prime military contracts (purchase agreements for goods and services by the Department of Defense (DoD)) and compensation of military personnel at the state-level with aggregate variation in defense spending due to military buildups and drawdowns between 1966 and 2006 to construct a shift-share instrument for defense spending at the state-level. However, when this approach is applied to smaller geographic units the resulting estimates suffer from attenuation bias due to measurement error (Demyanyk, Loutskina, and Murphy 2019). This is due to the fact that prime military contracts are associated with a principal location of performance, but this is incomplete data in the case of multiple locations and is potentially the wrong location entirely in the presence of subcontracting (both of which are most likely to occur with the largest contracts). It follows that for existing estimates of local fiscal multipliers, instead of using defense spending as source of variation, researchers have sought out alternatives such as natural experiments that can be tied more directly to specific localities (Acconcia, Corsetti, and Simonelli 2014; Suárez Serrato and Wingender 2016; Gerritse and Rodríguez-Pose 2018).

In this paper, I estimate county-level fiscal multipliers using variation in local defense spending. However, I avoid the geographic measurement error that has complicated previous research by introducing data on military employment and compensation, in addition to prime military contracts. Data on military employment is linked to specific installations, and so does not suffer from geographic measurement error to the same degree as prime military contracts. However, like prime military contracts, changes in military employment in a location is potentially endogenous to local economic conditions. I address this concern by instrumenting for changes in local defense spending with a shift-share instrument that captures the local impact of nationwide changes in military employment. The identifying assumption is that changes in defense employment at the national-level are not based upon shocks to the local economies of locations with existing military installations conditional on county and year fixed effects. Here, initial shares of military employment are the basis for the differential impact of the national shock captured by the instrument. While this instrumental variable is functionally similar to the spendng-based instruments in the existing literature (Nakamura and Steinsson 2014; Demyanyk, Loutskina, and Murphy 2019), initial shares are based on employment at specific military installations which are well-defined spatially. Further, over the sample period (1983-2005), large shocks to military employment were made through the BRAC process. The BRAC process provides a method for the Department of Defense (DoD) to adjust the distribution of defense activity in a way that is designed to shield the decisions from political interference. In addition to a shift-share instrument based on the realized changes to the national-level of military employment implemented through the five rounds of BRAC between 1988 and 2005, I also construct a shift-share instrument where the national shock that differentially impacts counties through initial military employment shares is based on the proposed changes by the DoD to employment at military installations. These proposals were determined by the DoD based on their value to military objectives and not on local economic characteristics.

When using data on prime military contracts combined with military personnel compensation and the instrumental variables described above, I find a local income multiplier of 0.53, which implies that a 1% increase in per capita government spending in a county results in a 0.53% increase in the per capita income in that county. This estimate is smaller than most of the existing estimates of the county-level income multiplier based on non-defense sources of variation in government spending (Acconcia, Corsetti, and Simonelli 2014; Adelino, Cunha, and Ferreira 2017; Suárez Serrato and Wingender 2016). One notable exception are the results for the county-level income multiplier in Dube and Kaplan (2014). In contrast, a local income multiplier of 0.53 is larger than existing estimates based on variation in local defense spending (Demyanyk, Loutskina, and Murphy 2019; Auerbach, Gorodnichenko, and Murphy 2020). This is not surprising given the above discussion of the attenuation bias in these estimates due to their dependence on military contract data alone. This result is based on a two-year time horizon, as in Nakamura and Steinsson (2014). When the time-horizon is extended to a five-year window, the estimate of the local fiscal multiplier increases to 0.78.

This paper proceeds as follows. Section 2 provides an overview of the existing literature. Section 3 describes the data that are used in the analysis. Section 4 describes the empirical specification for estimating local fiscal multipliers and outlines the identification strategy. Section 5 presents the empirical results and Section 6 concludes.

# 2 Related Literature

Interest in fiscal multipliers has grown out of the macroeconomic literature on the general equilibrium effects of fiscal policy (Ramey 2011a). This has led to a focus on the aggregate multiplier, which is most often estimated using time series variation in government spending. However, two primary challenges arise for identification with this methodology. First, there is the expected complication that changes in fiscal policy at the national-level respond to the economic conditions of the nation. Given this reverse causality, it is necessary to find a source of changes in government spending that is independent of the state of the overall economy. One of the most effective sources of this variation is defense spending. Barro (1981), Ramey and Shapiro (1998), Hall (2009), Ramey (2011b), Barro and Redlick (2011), and Ramey and Zubairy (2018) all use variation due to military buildups and drawdowns over time to instrument for changes in spending. This approach to address the endogeneity of government spending is based on the assumption that national defense spending does not respond to economic conditions. However, it is not clear that wartimes are comparable with the peacetime conditions, which is related to the second challenge: other variables that respond simultaneously. For example, if monetary policy responds to changes in fiscal policy, then the estimated multiplier is the combined effect of both. This limits generalizability because the response of monetary policy may differ across time and location. Based on a review of this literature, Ramey (2011a) finds that the national fiscal multiplier falls between

0.8 and 1.5. One benefit of these estimates is that they are directly comparable to the fiscal multipliers in general equilibrium models.

Due to the identification challenges posed by using time series variation to estimate fiscal multipliers, additional strategies have been developed. Specifically, subnational fiscal multipliers estimated using cross-sectional variation enable researchers to exploit many natural experiments based on subnational variation for identification. Examples of spending shocks that create subnational variation include components of the 2009 American Recovery and Reinvestment Act (ARRA) that were based on pre-recession conditions (Chodorow-Reich et al. 2012), state-balanced budget rules (Clemens and Miran 2012), and shift-share instrument designs (Fishback and Kachanovskaya 2015). Notably, Nakamura and Steinsson (2014) make effective use of the same source of variation from the existing literature, defense spending, applied to subnational multipliers. Nakamura and Steinsson (2014) combine data on prime military contracts (purchase agreements for goods and services by the DoD) and compensation of military personnel at the state-level with aggregate variation in defense spending due to military buildups and drawdowns between 1966 and 2006 to construct a shift-share instrument for defense spending at the state-level. They find a state-level fiscal multiplier of approximately 1.5. A benefit of using cross-sectional variation in fiscal policy is that it addresses the generalizability concerns due to the response of monetary policy. This follows because the monetary response is constant across subnational geographies. However, fiscal multipliers estimated using cross-sectional variation have one primary drawback: it is not clear how these estimates relate back to the theoretically-defined multipliers in general equilibrium modeling. Chodorow-Reich (2019) provides a framework that bridges this gap and shows that subnational fiscal multipliers provide a lower bound for the closed-economy no-monetary-policy-response, deficit-financed national multiplier.[1] Combined, these factors make estimates of subnational fiscal multipliers useful for understanding the national fiscal multiplier.

However, subnational fiscal multipliers are also valuable for determining the efficacy of

[1] The main complication with this interpretation how the spending is financed. While the local economy bears a marginal burden of raising funds for federal spending, the burden is more than zero. So, if the government spending is not deficit-financed at the national-level, then these multipliers do not align with the theoretically-defined deficit-financed multipliers. However, this discrepancy decreases with the unit of geography used in the analysis because the share of the federal tax burden shrinks as well.

area-specific fiscal policy. Subnational government spending is used to address localized economic downturns. In addition, many national government policies do not specifically target locations, but have differential local effects (such as highway funding). Estimates of national multipliers from the existing literature are unlikely to be applicable to these questions. One reason for this is that subnational economies within a large developed nation, like states within the United States, are more akin to small open economies than the nation as a whole. In addition, subnational units have more limited fiscal policy options and do not have the benefit of monetary policy that responds to their economic conditions. These differences become more pronounced with increasingly smaller units of geography. While many estimates exist at the state-level, for more localized fiscal multipliers (where national and state-based estimates are the least relevant), there are not many estimates to draw from. Further, the existing estimates vary widely. In one example using Italian data, Acconcia, Corsetti, and Simonelli (2014) find a local fiscal multiplier (at the provincial-level) of 1.5 by instrumenting for public spending using shocks created by an anti-corruption law. Adelino, Cunha, and Ferreira (2017) use the recalibration of municipal bond ratings by Moody's to instrument for local government spending and find an income multiplier of 1.9. Suárez Serrato and Wingender (2016) instrument for federal spending at the county-level with the differences between estimated and census year population counts that are used to determine spending for many federal programs. Suárez Serrato and Wingender (2016) find a local income multiplier of government spending between 1.7 and 2. On the other hand, Dube and Kaplan (2014) apply a shift-share design to ARRA spending at the county-level and find smaller and statistically insignificant income multipliers. There are two notable studies that extended the framework in Nakamura and Steinsson (2014) to metropolitan areas. Both Demyanyk, Loutskina, and Murphy (2019) and Auerbach, Gorodnichenko, and Murphy (2020) also find small two-year income multipliers: 0.37 and 0.34, respectively. By using a source for data on military contracts that only contains more recent data, both studies are also able to estimate local output multipliers. However, neither find an estimate that is statistically distinguishable from zero at conventional levels. In addition, Demyanyk, Loutskina, and Murphy (2019) report an estimate of the two-year income multiplier for US counties of 0.09. For defense spending based estimation, geographic measurement error is a contributing factor to lower resulting point estimates (Demyanyk, Loutskina, and Murphy 2019). It follows that there is not a clear consensus in the literature for the size of local fiscal multipliers.

In this paper, I address the geographic measurement error of defense spending based estimates of local fiscal multipliers by exploiting variation in military employment to instrument for local defense spending. Military employment provides a bridge between military spending (with which it is highly correlated) and accurate measurements of geographic location because military personnel levels are associated with specific military installations, which avoids the potential for subcontracting and multiple locations. In addition, changes to military employment levels are subject to a unique policy: BRAC. The five rounds of BRAC between 1988 and 2005 have institutional characteristics that make them an ideal source of variation. While the effects of BRAC have been studied directly (Krizan et al. 1998; Poppert and Herzog Jr 2003; Hultquist and Petras 2012) it has not been applied in many contexts. One example of an application is Zou (2018) who uses a shift-share design constructed from actual changes in defense employment over the first four BRAC rounds to study the impact of base closures on local labor markets. Unlike the existing literature, in this paper, I instrument for actual changes in employment at military bases with proposals by the DoD.

# 3 Data

In order to analyze the effects of government spending on local economic activity, I construct a county-level panel over the sample period (1983-2005) that combines data on county-level economic variables, prime military contracts, and military employment.

## 3.1 Economic variables

In terms of economic variables, the primary data series that are collected over the sample period are county-level personal income and population, as well as the national CPI.

Data series for county-level GDP and personal income are available from the Regional Economic Accounts of the Bureau of Economic Analysis (BEA). However, county-level GDP is a relatively new measure and has only been constructed by the BEA for years since 2001. Since the county-level GDP data is not available for the majority of the window for which detailed military employment data is available and does not overlap with four of the five rounds of BRAC, I estimate an income-based multiplier instead. It follows that the primary measure of economic activity for each county is personal income, which is available for the entire sample period (1983-2005). This more closely aligns this paper with the previous literature on local fiscal multipliers, most of which also use instruments that require a different sample period (Suárez Serrato and Wingender 2016; Fishback, Horrace, and Kantor 2005; Dupor et al. 2021). Both Auerbach, Gorodnichenko, and Murphy (2020) and Demyanyk, Loutskina, and Murphy (2019) use MSA-level GDP measures from the BEA to estimate local fiscal multipliers, but the baseline results in both papers are not precise (at the 5% level of significance) and each paper primarily focuses on income-based multiplier estimates instead. Further, for the overlapping years (2001-2005), annual, 2-year, and 4-year percent changes in GDP are highly correlated with the same changes in personal income.

County-level population is also available from the Regional Economic Accounts of the BEA for the entire sample period. The national CPI series for the United States is collected from the Main Economic Indicators series of the Organization for Economic Co-operation and Development (OECD) in order to calculate the real values of any economic variables.

## 3.2 Prime military contracts

Data on prime military contracts is available through the Defense Contract Action Data System (DCADS). This database is composed of Individual Contracting Action Reports (form DD-350) which include a wide variety of information on the nature of each contract (such as the contracting office with the DoD, the principal product or service rendered, and even the method of solicitation). For the sample period (1983-2005), the data include military procurement contracts with a value of $25,000 or more. For the following analysis the variables extracted from these reports for each contract are the dollar amount, the Federal Information Processing System (FIPS) code, and the month the contract was formed. The dollar amount is reported in terms of whole dollars, the FIPS code is based on the “principal place of performance”, and the month of formation is based on the fiscal year calendar of the federal government.

How to map the timing of the contract actions to the economic data is an important consideration when constructing the county-level panel. The contract data includes two types of entries: initial contracts and modifications to existing agreements. Auerbach, Gorodnichenko, and Murphy (2020) combine information on new contracts and modifications to construct a variable that represents the contemporaneous spending associated with a contract. This is possible because they make use of a different source for more recent federal contracts (only since 2000) that includes the duration of each contract. With the explicit duration of each contract, Auerbach, Gorodnichenko, and Murphy (2020) can allocate the spending for a contract (accounting for modifications) evenly across years. However, approximating contemporaneous spending with a constant temporal allotment is a strong assumption. Instead, I follow Nakamura and Steinsson (2014) and use the formation date of contracts and modifications, as this date is a better reflection of the actual information shock about future government spending that the contract reflects. While this method does not fully capture the fact that changes in military spending may be anticipated (like is observed at the national-level (Ramey 2011b)), it is a closer approximation. Further, in fiscal year 2005, only 0.56% of prime military contracts were multiyear contracts. In addition, Demyanyk, Loutskina, and Murphy (2019) find similar results using both specifications.

One additional distinction is relevant for determining the timing of contract actions. Unlike Nakamura and Steinsson (2014), when constructing the county-level panel of prime military contract spending I adjust for the fiscal year of the federal government to better align with the economic variables. The DCADS data are collected for the federal government's fiscal year (which during the sample period ran from October to September). Specifically, I use the data on the month of formation for each contract to translate from fiscal years to calendar years before aggregating the data by year.

### 3.3 Military employment, compensation, and BRAC

Data on military base employment is collected from the annual reports on the Distribution of Personnel by State and Selected Locations from the DoD.[2] In order to combine military base personnel data with county-level economic data, each installation is mapped to a county using military data on the location of installations and mapping software. In addition, due to changes in county boundaries over the sample period, some counties in the quantitative analysis represent the aggregation of jurisdictions so that the definitions are consistent across the sample period.[3] Data on military compensation is available from the DoD for each state and year for eight different classifications of employees (active duty and civilian, for the following sub-categories within the DoD: Army, Navy and Marine Corps, Air Force, and Other). With this data, I calculate the average payroll for each type of employee in each state and year. This can then be combined with the actual level of military employment by installation to construct a measure of military compensation at the county-level. Further, data on proposed changes to employment for each round of BRAC by installation are also available from the DoD.[4]

## 4 Empirical Specification

In order to estimate the effect of government spending on local economic activity, I apply an empirical framework that extends the specification in Nakamura and Steinsson (2014). There are two primary differences to the empirical framework, first, the unit of geography is at the county-level instead of the state-level, and second, I construct an instrumental variable for DoD spending that is based on military employment data instead of military contract data. Variation in local military employment is used to create two separate instruments. Both instruments have a shift-share design with the same initial shares of military employment

[2] Documentation suggests that the smallest installations, such as recruitment centers, are not included in these reports. Given this, only counties with military base employment that is non-zero for more than one year and has a maximum value over the sample period of at least two-hundred combined civilian and active duty personnel are included in the panel to ensure consistency.

[3] The creation of Broomfield County in Colorado from a number of existing counties results in an single metropolitan Denver geography. This is the largest impact of the aggregation.

[4] Since the last full calendar year of prime military contract data is 2005, it is not possible to include the full implementation window for the most recent round of BRAC (2005).

to capture the differential exposure of counties to national shocks in military employment. These initial shares are based on the first year in the sample period (1983) and reflect the distribution of defense employment at the height of the Cold War. The first instrument uses realized changes in civilian employment by the DoD as the national shock and the second uses proposed changes as part of the BRAC process to civilian employment by the DoD instead. Both instruments address the geographic measurement error present in data on military contracts (which do not account for subcontracting or multiple locations of performance) which causes attenuation bias that increases with smaller geographic units.

## 4.1 Fiscal Multipliers

Following Nakamura and Steinsson (2014), the baseline empirical specification is

$$\frac{Y_{it} - Y_{i,t-2}}{Y_{i,t-2}} = \alpha_i + \gamma_t + \beta \frac{G_{it} - G_{i,t-2}}{Y_{i,t-2}} + \epsilon_{it}, \tag{1}$$

where $Y_{it}$ is real income per capita, $G_{it}$ is real defense spending per capita, $\epsilon_{it}$ is the error term, $\alpha_i$ are location fixed effects for each county, $i$, and $\gamma_t$ are time fixed effects for each year $t$. $\beta$ is the estimate of the fiscal multiplier, the percentage change in local income associated with an increase in government spending by one percent of local income, controlling for county-specific time-invariant characteristics and year-specific shocks (such as changes in monetary policy). Given the same denominator, lagged local income, $Y_{i,t-2}$, is used to scale both the change in local income, $Y_{it} - Y_{i,t-2}$, and the change in local government spending, $G_{it} - G_{i,t-2}$, the fiscal multiplier, $\beta$, is equivalent to the dollar change in local income for a dollar increase in local government spending. However, if changes in government spending as a share of local income respond to local economic conditions (which is expected given that the defense budget is determined through a political process), then it is expected that the direct estimate of $\beta$ will be biased downward as politicians from lagging areas have the greatest incentive to lobby for additional funding in their district. Unlike other forms of discretionary spending, defense spending is uniquely well suited for constructing instruments that isolate shocks to local government spending that are independent from political processes.

## 4.2 Identification strategy

In order to estimate $\beta$, I use instruments for changes to local defense activity that are determined at the national-level to isolate changes in local defense spending that are not dependent on local economic conditions.

The bulk of defense spending can be attributed to two distinct sources: prime military contracts and military personnel compensation. For example, in fiscal year 2000, the DoD had a discretionary budget authority of \$287 billion and spent \$123 billion on military contracts and \$103 billion on military compensation (42.9% and 35.9%, respectively). Further, since the DoD constitutes a large component of the total discretionary budget authority of the federal government, both military contracts and compensation do as well. For fiscal year 2000, military contracts and compensation represented 21.1% and 17.6% of the total discretionary budget authority.

In the existing literature, the primary methodology for constructing an instrumental variable for local defense spending is to focus exclusively on prime military contracts and isolate variation at the national-level with a shift-share design (Demyanyk, Loutskina, and Murphy 2019; Auerbach, Gorodnichenko, and Murphy 2020). Following Auerbach, Gorodnichenko, and Murphy (2020), define the average share of total government spending in $i$ over the sample period as $s_i = \frac{\sum_{t=1}^{T} G_{it}}{\sum_{t=1}^{T} G_t}$. Then the spending-based instrumental variable, $IV_{spend}$, for $\frac{G_{it}-G_{i,t-2}}{Y_{i,t-2}}$ is defined as follows,

$$IV_{spend} = \frac{s_i \left(G_t - G_{t-2}\right)}{Y_{i,t-2}}. \tag{2}$$

While this instrument has proven effective for estimating state-level fiscal multipliers (Nakamura and Steinsson 2014)), its weaknesses are exacerbated as the unit of geography for the analysis gets smaller. Data on DoD contracts is collected by contract, not by location. Specifically, locations are mapped to contracts by the "principal place of performance". However, this location will only partially align with the actual location of spending in the presence of a contract being executed at multiple locations and could be entirely spatially misaligned in the presence of subcontracting. Nakamura and Steinsson (2014) demonstrate that at the state-level, their measures of military spending are highly correlated with data on the value of shipments of physical goods. This suggests that at the state-level, geographic measurement error may be present but is relatively small. On the other hand, Demyanyk, Loutskina, and Murphy (2019) argue that at levels of geographic aggregation smaller than states the assumption in Nakamura and Steinsson (2014) is not likely to hold. Geographic measurement error will cause attenuation bias and it will worsen with the granularity of the geographic unit. Demyanyk, Loutskina, and Murphy (2019) estimate local fiscal multipliers using this instrumental variable at different levels of geographic definition (counties, CBSAs, and states) and find evidence of this attenuation bias. However, for the analysis in Demyanyk, Loutskina, and Murphy (2019), this is not an issue because the relationship of interest is the relationship between local fiscal multipliers and the local debt burden not the precise level of local fiscal multipliers. On the contrary, in this paper, the primary goal is the precise estimation of local fiscal multipliers. The other example of local fiscal multiplier estimation using this instrument (Auerbach, Gorodnichenko, and Murphy 2020) finds similarly small local income multipliers at the CBSA-level as in Demyanyk, Loutskina, and Murphy (2019). Consequently, relying on variation from military contract data alone is not well suited for estimating local fiscal multipliers.

In this paper, I address the geographic measurement error concerns by adjusting the above methodology in three ways. First, I extend defense spending to include not only military contracts, but also military personnel compensation. Nakamura and Steinsson (2014) use both definitions of defense spending to estimate fiscal multipliers and find similar results for both definitions at the state-level. Second, following Goldsmith-Pinkham, Sorkin, and Swift (2020), I use initial shares instead of average shares when constructing shift-share design instruments to make interpretation of the identification clearer. Third, and most significantly, instead of constructing a shift-share instrument based on DoD contract spending alone, I use military employment. The aggregate trends in spending on military contracts and military employment are presented in Figure 3 (normalized to an index value of 100 at the start of the sample period, 1983). At the national-level, military contracts and military employment have similar secular trends. The sample period, 1983-2005, starts at the height of the Cold War and captures the large military drawdown (in terms of contracts and employment) that followed the collapse of the Soviet Union as well as the start of the military buildup in the early 2000s. However, changes in military employment noticeably lag behind changes in military spending; in addition, DoD contracts have more variation over the sample period. In Figure 1, initial (1983) shares of military contract spending by county are presented. Most counties have some baseline amount of military contract spending, but only a small number have spending equal to a significant share of their local economic activity. In Figure 2, initial shares of military employment by county are presented. Fewer counties are home to a military installation that employs civilians, but a larger share of locations with a military installation have a substantial amount of local economic activity tied to that installation. Like with military contract spending, the counties are spread broadly across the country, capturing urban, suburban, and rural locations as well as at least some locations in every state.

For the shift-share instrument based on military employment, $IV_{emp}$, let $L^m_{it}$ be the number of civilians employed by the military in county $i$ and year $t$ and let $N_{it}$ be the population of county $i$ in year $t$. I use employment of civilians instead of active duty members because civilians are integrated into their local labor market and better reflect the real trade-off of government spending (in the absence of the military installation they can be employed in a different role locally) and because there is less noise overtime due to civilian DoD employees not being subject to deployments. In particular, active duty employment at a base can fluctuate if they are deployed to a foreign country, even though they are still connected to the local economy (for example, by paying rent). Define the initial (1983) share of military employment of civilians in county $i$ as $s^L_i = \frac{L^m_{i,1983}}{\sum_{i=1}^{N} L^m_{i,1983}}$. Then a shift-share instrument based on national trends and initial local shares of military employment is defined as

$$IV_{emp} = \frac{s^L_i \left(L^m_t - L^m_{t-2}\right)}{N_{i,t-2}}. \tag{3}$$

The identification for $IV_{emp}$ is based on the initial (1983) share of military employment by county. The identification is built upon the assumption that changes in military spending and employment related to the military drawdown at the end of the Cold War and the military buildup in the early 2000s were decided upon independently of changes in the local economies of counties with disporportionate shares of military employment in 1983. So long as the DoD did not wind down defense spending and employment at the national-level because impacted counties were economically prospering and did not start to build back up in the early 2000s because the same counties were economically lagging, then the initial shares can be thought of as a measure of the differential exposure of counties to an exogenous common shock (Goldsmith-Pinkham, Sorkin, and Swift 2020). More concretely, conditional on county and year fixed effects, the initial shares from 1983 cannot be correlated with the idiosyncratic shock for each county to its per capita income growth rate (note, $\epsilon_{it}$, is in terms of changes, not levels).

Returning to Figure 3, the fact that changes in military employment at the national-level lags behind changes in defense spending is likely the result of differences in how changes to DoD spending and employment are determined. For both contract and payroll obligations, the funding must first be appropriated by Congress. However, unlike contracts, since 1977 there have been strong limitations on the ability to close or realign military installations. Instead, changes to the distribution of defense activity are made through the BRAC process. There have been five rounds of BRAC: 1988, 1991, 1993, 1995, and 2005. Each round has a 5-year implementation window from its approval. The process is designed to limit direct interference by politicians on the impact to their district by first having the DoD submit proposals that are legally mandated to be based primarily on the military value of each installation, then having these recommendations evaluated by an independent commission with the power to make changes, and finally by forcing the President and Congress to approve or reject the final list in its entirety.

Based on the limitations of the DoD outside of the BRAC process, the changes in the observed trend of military employment over the sample period are mostly the result of the first four BRAC rounds. Thus, more directly than military spending, military employment meets the identifying assumption that changes in military drawdowns and buildups are based on geopolitical factors (like the fall of the Soviet Union) that change defense priorities (less need to defend northern airspace) and result in realized changes (multiple high-latitude Air Force bases closing). However, there is one component of the BRAC process that has the most potential to introduce non-defense related priorities into the decision process: the independent BRAC commission. The BRAC commission takes the list of recommendations based on military value from the DoD and then makes adjustments that are still based primarily on military value, but also have secondary considerations of other factors. While the independent commission could act as a formality, it has actually made major revisions to the DoD proposals in the past. It follows that the original recommendations by the DoD, which are not implemented one-for-one, may be a better source of variation for the exogenous common shock that the instrumental variable captures the differential exposure of counties to. This is a novel approach in the literature on the local economic consequences of military employment, which generally makes one of two assumptions, (i) observed changes in military employment are exogenous, or (ii) the final list of changes implemented in a round of BRAC is exogenous (e.g., Hooker and Knetter 1997; Krizan et al. 1998; Hooker and Knetter 2001; Poppert and Herzog Jr 2003; Hultquist and Petras 2012). One notable exception, Zou (2018), uses a shift-share instrument like $IV_{emp}$ to estimate the reduced-form impact of additional workers on local labor markets. It follows that the military employment based instrument using proposed changes through the BRAC process as the national shock, $IV_{BRAC}$, is defined as follows

$$IV_{BRAC} = \frac{s_i^L \left(\Delta_h BRAC\right)}{P_{i,t-2}}, \tag{4}$$

where $\Delta_h BRAC$ is the national total of the DoD's individual proposed changes to the BRAC commission over the time horizon $h$. Save for the replacement of the realized changes in military employment over the time horizon with the proposed changes to military employment by the DoD through the BRAC process over the time horizon, the interpretation and identification is the same with $IV_{BRAC}$ as with $IV_{emp}$.

# 5 Results

In this section I present baseline estimates of local fiscal multipliers as well as estimates at a longer time horizon and additional findings to test the robustness of the baseline results.

## 5.1 Baseline estimates

In Table 1, I present the baseline estimates of local income multipliers using Equation 1. These estimates are based on a 2-year time horizon. The table consists of four different specifications for estimating the local (county-level) income multiplier. Each specification includes county and year fixed effects. The county fixed effects control for any time-invariant characteristics of the county and the year fixed effects control for common year-specific shocks (such as changes in monetary policy). Given that the error terms for counties may be correlated over time, the standard errors are clustered by county.

In the first column of Table 1, I present ordinary least squares (OLS) results for the impact of changes in per capita defense spending on per capita personal income. The estimated coefficient of $\beta$ is 0.0172 and is not statistically distinguishable from zero at the 5%-level. As discussed in Section 4, geographic measurement error in defense spending creates attenuation bias for this result. Further, it is expected that defense spending is endogenous to local economic conditions, increasing more in lagging counties due to the political nature of how it is determined. If defense spending is endogenous in this way, this would result in a downward bias in the estimated local income multiplier.

The three remaining columns each present the two-stage least squares (TSLS) results of the same basic specification with a different instrumental variable for changes in per capita local defense spending as a share of local per capita income. For each TSLS specification, one additional statistic is included: the Kleibergen-Paap rk Wald F-stat.

The local income multiplier estimated using $IV_{spend}$ is presented in column 2 of Table 1. The estimated coefficient of $\beta$ is 0.1501. While this fiscal multiplier is larger than the OLS result, it is similarly not statistically significant. Under the identifying assumption the shift-share instrument can address the endogeniety concerns, but $IV_{spend}$ is still based on initial shares of defense spending that have geographic measurement error and this estimate likely reflects the resulting attenuation bias. In addition, a Kleibergen-Paap rk Wald F-stat equal to only 4.390 suggests that the first-stage strength of this instrument is low which could introduce further weak instrument bias.

The two final columns of Table 1 present TSLS estimates of the local income multiplier using instruments based on variation in military employment. Unlike defense contracts, military employment is well-defined spatially because it is associated with specific military installations that can unambiguously be mapped to different units of geography. In column 3, the estimated coefficient of $\beta$ is 0.4173 and is highly statistically significant. The instrumental variable in this column is a shift-share instrument where initial shares of military employment by county are combined with realized changes in the national-level of civilian employment at military installations. A Kleibergen-Paap rk Wald F-stat equal to 156.940 suggests a strong first-stage relationship that alleviates concerns of potential weak instrument bias. In column 4, an estimated local income multiplier using an alternative military employment based instrumental variable is presented. Here, instead of using realized changes in domestic military employment of civilians, the same initial shares are combined with proposed changes in military employment of civilians at the national-level from the DoD's BRAC recommendations. The estimated coefficient of $\beta$ is 0.5327, which is larger than using the more direct military employment instrument. However, it is similar to $IV_{emp}$ in that it is a relevant instrument as it exhibits a strong first-stage relationship with local defense spending. Taking this as the preferred specification, this estimate suggests that a one percent increase in per capita DoD spending as a percent of per capita income results in a 0.53% increase in the per capita income in a county.[5] A local income multiplier of 0.53 is larger than existing estimates using defense spending data in the literature which are approximately 0.35 at the CBSA-level and 0.09 at the county-level (Demyanyk, Loutskina, and Murphy 2019; Auerbach, Gorodnichenko, and Murphy 2020). This difference suggests that using instruments based on differential exposure to military employment shocks alleviates the attenuation bias found in previous estimates that used instruments based on variation in geographically misspecified defense contracts alone. However, a local income multiplier of 0.53 is still smaller than existing estimates that are based on alternative sources of local variation in government spending (Acconcia, Corsetti, and Simonelli 2014; Suárez Serrato and Wingender 2016; Adelino, Cunha, and Ferreira 2017).

[5]Note that any county that did not receive any DoD spending over the entire sample period (1983-2005) is excluded from the analysis. However, the findings are robust to different sets of included counties (see Section 5.3).

## 5.2 Longer time horizon

In this section, I present estimates of local income multipliers using Equation 1, but with a longer time horizon of 5 years. In particular, using 5-year changes aligns the specification with the full implementation window of each round of BRAC which captures the full adjustment for the proposals that $IV_{BRAC}$ is based upon. In Table 2, the same basic specifications are reproduced to estimate the local income multiplier at a the 5-year time horizon. Across each specification, the estimated coefficients of $\beta$ are larger with the longer time horizon (as is expected). The same pattern emerges in terms of the statistical significance of the estimates as was observed for the 2-year time horizon. With the instrumental variables based on military employment, the estimated coefficients are both statistically significant at the 1%-level whereas the OLS estimate and the TSLS estimate using $IV_{spend}$ are both imprecise. At the 5-year time horizon, the first-stage relationship as captured by the Kleibergen-Paap rk Wald F-stat for $IV_{spend}$ and $IV_{BRAC}$ are both stronger, whereas $IV_{emp}$ is weaker. However, unlike at the 2-year horizon all three TSLS results exhibit strong first-stage relationships that alleviate concerns of potential weak instrument bias. For the specification using $IV_{emp}$, the estimated coefficient is 0.5501, and for the specification using $IV_{BRAC}$ the coefficient is 0.7796. Once again taking the estimating found using the BRAC-based instrument as the preferred result, this estimate suggest that a one percent increase in per capita DoD spending as a percent of per capita income results in a 0.78% increase in per capita income in a county.

## 5.3 Robustness

In this section, I provide additional findings to determine if the baseline results are robust to two different specification choices: the use of combined military contract and compensation data and the inclusion of counties that never receive any military funding.

In Table 3, I present the resulting estimates of $\beta$ from Equation 1 using only data on the value of military contracts in a county instead of the combined defense spending measure of contracts and compensation. There are three primary and related differences between these results and the baseline. First, the first-stage strength of the instruments based on military employment are not as strong with Kleibergen-Paap rk Wald F-stats of 16.416 and 5.266 for the TSLS specifications using $IV_{emp}$ and $IV_{BRAC}$, respectively. Second, the estimated coefficients for $\beta$ are larger for the same two specifications. Finally, the estimated coefficients for TSLS specifications with instruments based on military employment are less precise. While military employment and defense contract spending are strongly related, conditional on county and year fixed effects, it is clearly not as strong as the relationship with the combined measure of defense spending based on contracts and compensation. This directly results in less precise estimates, which explains the larger coefficients. Specifically, even though the estimated coefficients of $\beta$ are larger (for the specification using $IV_{BRAC}$ the coefficient is 2.2886), the more precise estimate using both contracts and compensation for the defense spending measure falls well within the 95% confidence interval. Indeed, the entire 95% confidence interval for the more precise estimate of $\beta$ using $IV_{BRAC}$ from Table 1 fits within the equivalent 95% confidence interval from this specification. Thus, while the coefficients are larger under this specification, due to their increased imprecision it is clear that they are not meaningfully different.

An additional robustness check is for the inclusion of counties that did not receive any military spending over the entire sample period. This subset includes 130 out of 3,061 time-consistent counties. I present results for estimating Equation 1 including these counties in Table 4. The results are similar in terms of coefficients and statistical significance; however, the point estimates of the fiscal multipliers are smaller when the additional counties are included. When considering the confidence intervals, they essentially overlap and are similarly not meaningfully different from the baseline estimates.

# 6 Conclusion

In this paper, I exploit the differential exposure of counties to national changes in defense spending to estimate local income multipliers. Specifically, I use data on military contracts, compensation, and employment to extend the empirical framework of Nakamura and Steinsson (2014) to the county-level. Employment of civilians at military installation is well-defined geographically and thus avoids the attenuation bias in estimates of local fiscal multipliers based on variation in defense contracts alone. This attenuation bias is the result of geographic measurement error in the data on military contracts due to contractors producing goods and services in multiple locations as well as subcontracting work to entirely different entities. This bias has made the direct application of the framework in Nakamura and Steinsson (2014) to more granular units of geography difficult (Demyanyk, Loutskina, and Murphy 2019). I construct two different instrumental variables based on military employment, one standard shift-share instrument where the national changes are based on observed changes in domestic military employment and an alternative instrument where national changes are based on changes to defense employment that were proposed by the DoD as part of the BRAC process, which better isolates changes due to changing defense needs.

I find that the local income multiplier is between 0.53 and 0.78 depending on the length of the time horizon. These estimates of local income multipliers are larger than existing estimates using defense spending as a source of variation at the county- and CBSA-level. However, they are smaller than estimates of local income multipliers based on other sources of local variation in government spending. Given that at the national and state-level estimates of fiscal multipliers based on military spending are generally smaller than those based on other sources of variation, these results are not surprising. Since defense spending is uniquely well-suited as a source of changes in government spending that is not directly linked to economic conditions, addressing the geographic measurement error concerns represents an addition to the small but growing literature of estimates of local fiscal multipliers. Having a precise estimate of the impact of government spending at the local-level can help determine the effectiveness and design of policies and programs that result in spatial redistribution.

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# Tables

Table 1: Local Income Multipliers [2-year horizon]

| | OLS | IV | | |
|---|---|---|---|---|
| | | Spend. | Mil. Emp. | BRAC |
| Prime contracts plus military compensation | 0.0172 | 0.1501 | 0.4173*** | 0.5327*** |
| | (0.009) | (0.214) | (0.072) | (0.148) |
| Year F.E. | Yes | Yes | Yes | Yes |
| County F.E. | Yes | Yes | Yes | Yes |
| N | 61551 | 61551 | 61551 | 61551 |
| Kleibergen-Paap rk Wald F-stat | | 4.390 | 156.940 | 34.735 |

Standard errors in parentheses

Standard errors clustered by county.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Local Income Multipliers [5-year horizon]

| | OLS | IV | | |
|---|---|---|---|---|
| | | Spend. | Mil. Emp. | BRAC |
| Prime contracts plus military compensation | 0.0223 | 0.1559 | 0.5501*** | 0.7796*** |
| | (0.012) | (0.116) | (0.109) | (0.191) |
| Year F.E. | Yes | Yes | Yes | Yes |
| County F.E. | Yes | Yes | Yes | Yes |
| N | 52758 | 52758 | 52758 | 52758 |
| Kleibergen-Paap rk Wald F-stat | | 19.445 | 116.252 | 39.702 |

Standard errors in parentheses

Standard errors clustered by county.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Local Income Multipliers [2-year horizon]

| | OLS | IV | | |
|---|---|---|---|---|
| | | Spend. | Mil. Emp. | BRAC |
| Prime contracts | 0.0029 | 0.1613 | 1.4137** | 2.2886* |
| | (0.006) | (0.230) | (0.474) | (1.153) |
| Year F.E. | Yes | Yes | Yes | Yes |
| County F.E. | Yes | Yes | Yes | Yes |
| N | 61551 | 61551 | 61551 | 61551 |
| Kleibergen-Paap rk Wald F-stat | | 4.015 | 16.416 | 5.266 |

Standard errors in parentheses

Standard errors clustered at the county

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Local Income Multipliers [2-year horizon/All counties]

| | OLS | IV | | |
|---|---|---|---|---|
| | | Spend. | Mil. Emp. | BRAC |
| Prime contracts plus military compensation | 0.0164 | 0.1010 | 0.4063*** | 0.4623*** |
| | (0.009) | (0.214) | (0.071) | (0.140) |
| Year F.E. | Yes | Yes | Yes | Yes |
| County F.E. | Yes | Yes | Yes | Yes |
| N | 64281 | 64281 | 64281 | 64281 |
| Kleibergen-Paap rk Wald F-stat | | 4.424 | 158.120 | 34.951 |

Standard errors in parentheses

Standard errors clustered by county.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

# Figures

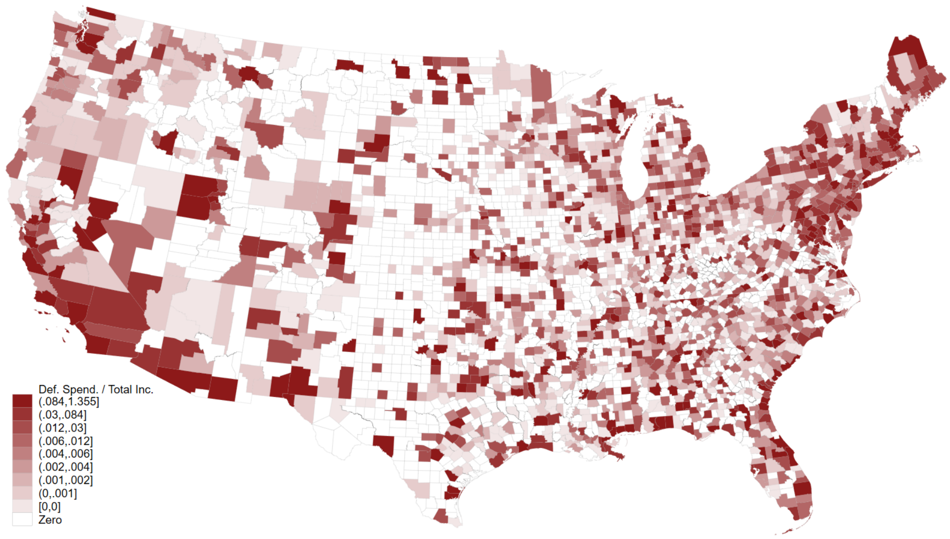


Figure 1: Military contract spending by county as a share of income in 1983.

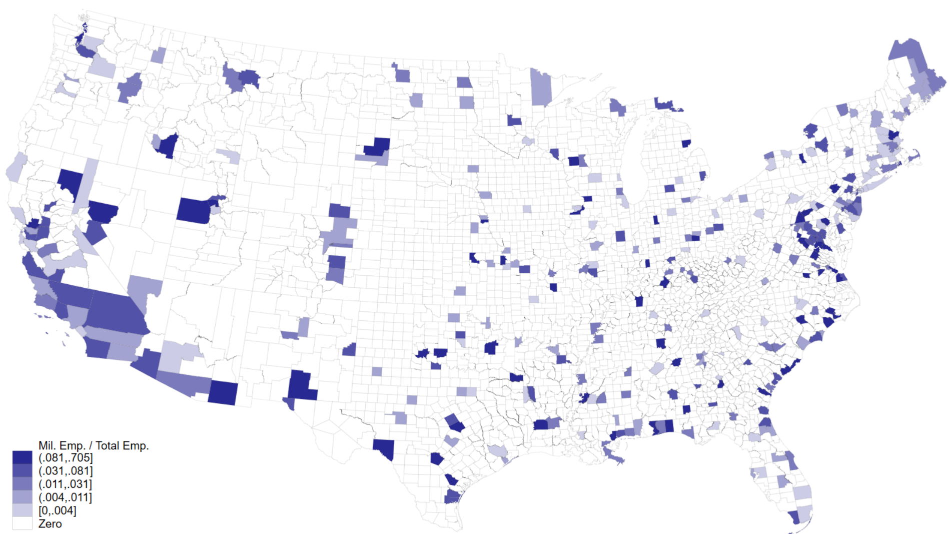


Figure 2: Military employment by county as a share of the population in 1983.

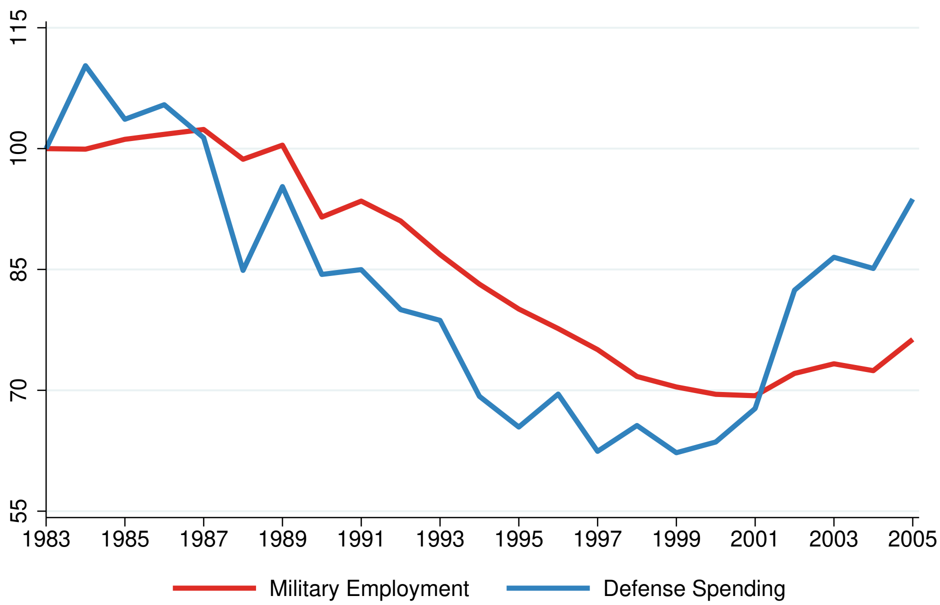


Figure 3: Secular trends in military spending and employment