Estimating Local Fiscal Multipliers

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Abstract

We propose a new identification strategy to measure the causal impact of government spending on the economy. Our methodology isolates exogenous cross-sectional variation in government spending using a novel instrument. We use the fact that a large number of federal spending programs depend on local population levels. Every ten years, the Census provides a count of local populations. Since a different method is used to estimate non-Census year populations, this change in methodology leads to variation in the allocation of billions of dollars in federal spending. Our IV estimates imply that government spending has a local income multiplier of 1.57 and an estimated cost per job of $30,000 per year. We also show that there are positive spillovers of federal spending across neighboring counties. Finally, we characterize the heterogeneity of the impacts of government spending and find that it has a higher impact in low growth areas.

Keywords: Government spending, fiscal multiplier, instrumental variables.

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1 Introduction

The impact of government spending on the economy is currently the object of a critical policy debate. In the midst of the worst recession since the 1930s, the federal government passed the American Recovery and Reinvestment Act (ARRA) in February 2009 at a cost of more than $780 billion in the hopes of stimulating a faltering US economy. The bill contained more than $500 billion in direct federal spending with a stated objective to "... save or create at least 3 million jobs by the end of 2010"(Romer and Bernstein, 2009). Despite the importance of this debate, economists disagree on the effectiveness of government spending at stimulating the economy. The endogeneity of government spending makes it difficult to draw a causal interpretation from empirical evidence as redistributive or counter-cyclical spending policies and automatic stabilizers likely biases OLS estimates towards zero. We contribute to this important discussion by proposing a new empirical strategy to identify the impacts of government spending on income and employment growth.

In this paper we propose a new instrumental variable that isolates exogenous variation in government spending at the local level. We use the fact that a large number of direct federal spending and transfer programs to local areas depend on population estimates. These estimates exhibit large variation during Census years due to a change in the method used to produce local population levels. Whereas the decennial Census relies on a physical count, the annual population estimates use administrative data to measure incremental changes in population. The difference between the Census counts and the concurrent population estimates therefore contains measurement error that accumulated over the previous decade. We use the population revisions which occurred following the 1980, 1990 and 2000 Censuses to estimate the effect of an exogenous change in federal spending across counties.[1] While we use this identification strategy to estimate local fiscal multipliers, one of the contributions of this study is the careful documentation of an instrument that can be used to analyze the impact of government spending on other outcomes as well.

In a first step, we document a strong statistical relationship between changes in population levels due to Census revisions and subsequent federal spending at the county level. This is consistent with the fact that a large number of federal spending programs use local population levels to allocate spending across areas. This dependence operates either through formula-based grants using population as an input or through eligibility thresholds in transfers to individuals

[1]Similar identifications strategies can be found in the literature. Gordon (2004) uses the changes in local poverty estimates following the release of the 1990 Census counts to study the flypaper effect in the context of Title I transfers to school districts. In contrast to Gordon (2004), our identifying variation emanates from measurement error rather than from changes in population between Censuses. In a paper looking at political representation in India, Pande (2003) uses the difference between annual changes in minorities' population shares and their fixed statutory shares as determined by the previous Census.

and families.[2] We also document the fact that it takes several years for different agencies in the federal government to update the population levels used for determining spending. Thus, even though the instrument we propose occurs once every decade, it provides many years of exogenous variation in federal spending.

Even thought the exogeneity assumption of the instrument is fundamentally untestable, the paper provides several indirect tests in support of it. For example, we show that the effects of the population shock start two years after a Census is conducted. This is consistent with the timing of the release of new population counts and provides strong support to the validity of our identification strategy. Moreover, we show that the instrument is broadly not correlated with past income, earnings and federal spending growth and is negatively correlated with past employment growth. This rules out explanations that posit that the instrument is simply identifying growing counties that will continue to grow in the future. Finally, we show that our estimates are robust to the inclusion of lagged outcome variables and known predictors of economic and population growth such as local demand shocks.

We use the exogenous variation in federal spending identified by our instrument to measure the causal impact of federal spending on economic outcomes at the local level. We find an estimate of the local income multiplier, the change in local aggregate income produced by a one dollar change in federal spending, of 1.57 and an estimated cost per job created of $30,000 per year. The IV results imply a return to government spending at the local level that is more than fifteen times larger than the corresponding OLS estimates and statistically different. This shows that not accounting for the endogeneity of federal spending leads to a large downward bias due to obvious concerns about endogeneity and reverse causality.[3]

Our paper is related to several recent papers using cross-sectional identification strategies to estimate government spending multipliers. Shoag (2010) uses differences in returns to state pension funds as windfall shocks to state finances that predict subsequent spending patterns. He estimates a state-level spending multiplier above 2 and a cost per job created of around $35,000. Chodorow-Reich et al. (2012) use formula-driven variation in federal transfers to states in 2009 associated with state-level Medicaid spending patterns before the Great Recession. They find a cost per job created of around $25,000 and an implied local spending multiplier of about 2. Wilson (2011) also uses state-level spending from the American Recovery and Reinvestment Act (ARRA) of 2009 instrumented with allocation formulas and pre-determined factors such as the number of highway lane-miles in a state or the share of youth in total population. He

[2] A review by the Government Accountability Office (GAO 1990) in 1990 found 100 programs that used population levels to apportion federal spending at the state and local level. Blumerman and Vidal (2009) found 140 programs for fiscal year 2007 that accounted for over $440 billion in federal spending; over 15% of total federal outlays for that year.

[3] For example, some categories of government spending are automatic stabilizers so that spending increases when the local economy experiences a slowdown.

finds a cost per job created of around $125,000. Fishback and Kachanovskaya (2010) study the effect of federal spending on aggregate state income, consumption and employment during the Great Depression. They instrument for federal spending at the state level using the interaction between a measure of swing voting in prior presidential elections and federal spending outside of the state. They find an income multiplier at the state level of around 1.1, with a higher impact on personal consumption but no significant impact on private employment. Nakamura and Steinsson (2014) use regional variation in US military spending to estimate a state-level multiplier of 1.5. Their identifying assumption requires that changes in military buildup are not correlated with relative regional economic conditions. A contribution of their paper is to develop a New Keynesian open-economy model to describes how their regional multiplier estimates relate to the traditional government spending multiplier at the national level. Finally, Clemens and Miran (2012) use state government spending cuts attributable to institutional rules on budget deficits to estimate a spending multiplier. Unlike the other studies mentioned here where spending changes come from windfall shocks that do not lead to changes in tax liabilities for states or regions, their reduced form estimates also reflect changes in tax liabilities. Consistent with a Ricardian effect, their multiplier estimate for income growth is around 0.8 at the annual level. We see our paper as a complement to these other contemporaneous approaches to estimating local fiscal multipliers. In particular, our use of county-level data as opposed to state-level data allows us to analyse a broader set of issues relating to spillovers across areas and characterize heterogeneneous effects of government spending using quantile regression methods. In addition, our larger sample size has the potential to generate more precise estimates of these important policy parameters.

The new cross-sectional literature on fiscal multipliers differs from the traditional empirical macroeconomics literature which relies on time-series variation (e.g. Ramey and Shapiro, 1997, Fatás and Mihov, 2001, Blanchard and Perotti, 2002, Ramey, 2010). This approach has many advantages. Foremost, it allows us to clearly identify the source of plausibly exogenous variation in government spending. Exploiting cross-sectional variation also allows for research designs with potentially much larger sample sizes. This can increase statistical power and the precision of our estimates. We show that a cross-sectional approach is particularly amenable to the study of the effects of government spending on local outcomes and can yield new results and insights. In particular, we measure the spillover effects of federal spending across counties. Our strategy also enables us to characterize the heterogeneity in the impact of government spending using a new method that uses instrumental variables in a quantile regression framework (Chernozhukov and Hansen 2008). We show that government spending decreases income growth inequality across counties.

Another key difference with time-series analysis is in the interpretation of our results. This is crucial because nation-wide effects of policy changes cannot be identified in cross-sectional regressions.[4] General equilibrium effects such as the Ricardian equivalence operating through the additional tax burden shared by all individuals cannot be measured by our approach. The same is true of the impact of monetary policy in response to a fiscal shock. These nation-wide effects imply that our estimates of local fiscal multipliers are not directly comparable to traditional national multipliers which have been the focus of the literature. For example, Nakamura and Steinsson (2014) show that the cross-sectional estimate of the local fiscal multiplier will coincide with the national multiplier only when nominal interest rates are unresponsive to a fiscal expansion such as when they are constrained by the zero-lower bound. Nevertheless, the estimates generated by this new literature are informative in their own right as they shed light on intermediate mechanisms and provide answers to important regional policy questions.

We also extend the analysis by directly measuring spillovers in federal spending. Positive spillovers across counties would lead us to underestimate the total regional effect of federal spending. On the other hand, if government spending crowds out private demand for labor and this effect is operating differently in the recipient and neighboring counties, our estimates at the local level could be overestimating the larger regional impact of government spending. We find that federal spending in neighboring counties positively affect economic growth in the home county.[5]

The following section provides background into the source of variation in population levels and the instrument. Section 3 describes the data used in the study. Section 4 describes the relation between the instrument and subsequent changes in federal spending as well as other properties of our Census shock instrument. Sections 5 presents the main instrumental variables results for local multipliers. Section 6 measures the spillovers of federal spending across neighboring counties while Section 7 analyzes heterogeneity in the impact of government spending. We finally conclude in Section 8.

2 Measurement of Population Levels

As mandated by the Constitution, the federal government conducts a census of the population every ten years. These population counts are used to allocate billions of dollars in federal spending at the state and local levels. The increased reliance on population figures has also led to the development of annual estimates that provide a more accurate and timely picture of the geographical distribution of the population. For the last thirty years, the U.S. Census Bureau has relied on administrative data sources to track the different components of population

[4] See Acemoglu, Finkelstein and Notowidigdo (2009) for a discussion in the context of health spending and local area income.

[5] Davis et al. (1997) find positive spillovers of demand shocks across states. Glaeser et al. (2003) develop a model in which the presence of positive spillovers leads to larger social multipliers than those implied by lower level estimates.

changes from year to year. These components are broadly defined as natural growth from births and deaths as well as internal and international migration. Natural growth is estimated from Vital Statistics data and migration flows are estimated using among other sources tax return data from the IRS, Medicare, school enrollment, and automobile registration data.[6]

A crucial feature of these estimates is that they are "reset" to Census counts once these data become available after a new Census is conduced. The difference between the two population measures in Census years is called "error of closure". The Census Bureau's objective is obviously to produce population estimates that are consistent over time. However, the use of two different methods for producing population figures necessarily leads to some discrepancy due to systematic biases and measurement errors in both the annual estimates and the decennial Census counts.

The error of closure has been substantial in recent Censuses. In 1980, the Census counted 5 million more people than the concurrent population estimate that had been derived by using the total population level from the 1970 Census and adding population growth throughout the decade. The 1990 Census counted 1.5 million fewer people than the national estimate. This was due to systematic undercounting of certain demographic groups. In 2000, the Census counted 6.8 million more people than the estimated population level based on the 1990 Census.[7] These errors of closure are even more important in relative terms at the local level due to the difficulty of tracking internal migration.

A few notable examples include Clark County, Nevada where Las Vegas is located. From an initial population of 756,170 people in 1990, the county grew by almost 85% over the following decade to reach 1,393,909 people in 2000. This growth rate was the 14th highest during the decade. The Census shock for Clark county in 2000 was also high at 8.8%, slightly above the 95th percentile in our sample for 2000. The counties of New York City also experienced a large Census shock in 2000 of 7.5% even though the city's population only grew by 8.5% over the previous ten years. Dade County, Florida where Miami is located also had large Census shocks of close to 6% compared to our sample average of 0.2%. San Diego County, on the other hand, had smaller shocks of 0.6% on average across all three Censuses. Census shocks in urban counties were positive and larger than those experienced by rural counties. This was caused by the fact that rural counties experienced more negative population shocks, i.e. Census enumerations consistently found fewer people than the contemporaneous administrative estimates. In absolute values however, the rural counties had larger shocks in every Census. Counties in the Midwest always had smaller average Census shocks and counties in the South had on average the largest shocks. Northeast counties also appeared to have the least discrepancy between Census enumeration and administrative estimate.

[6] See Long (1993) for details.

[7] See Census Bureau (2010a).

In Figure 1 we show the average county population growth rate across all counties by year. The series shows clear breaks in 1980, 1990 and 2000. We also show in Figure 2 the full distribution of county population growth rates for 1999 and 2000 separately. The figure clearly shows that the Census revisions affect the whole distribution of growth rates: the variance is also larger as more counties experience very high positive and negative growth in 2000 than in 1999.

Figure 1: Average County Population Growth Rate by Year

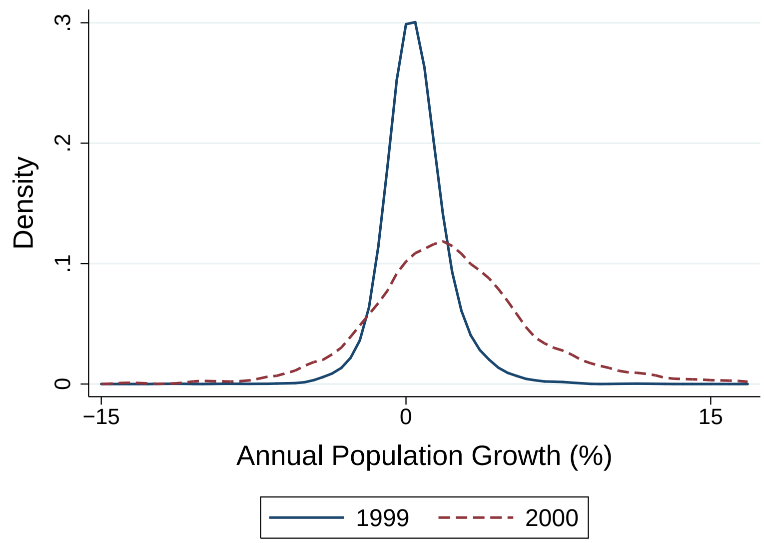
Notes: This figure plots the unweighted average population growth in percent across all counties by year using postcensal population estimates. See Data Appendix A for data sources.

These figures show that updating population estimates with new Census counts generates a large amount of cross-sectional variation. It is important to note though that population growth rates cannot be used as instruments for government spending as these are a combination of measurement error, which is a valid source of identifying variation, and true population growth, which is endogenous to economic factors that could confound the identification strategy. In order to implement our identification strategy, we need to isolate the component of population change that is exogenous to local economic conditions.

The instrument we use in the paper is the Census Bureau's error of closure at the local level. It is the difference between two concurrent estimates of the population in the same year: the Census counts and the administrative estimates derived by adding population growth to the population levels as determined by the previous Census.[8] To evaluate the suitability of the error of closure as an instrument for federal spending, it is necessary to determine to

[8]These administrative estimates are called postcensal estimates. See Appendix Table 1 for a definition of all variables used in the analysis.

Figure 2: Distribution of County Population Growth Rates 1999-2000



Notes: This figure plots the kernel density function of county population growth rates across all counties in 1999 and 2000 using postcensal population estimates. A half-width of 0.5 is used for the kernel. See Data Appendix A for data sources.

what extent the variation is driven by mismeasurement of population growth between Censuses or mismeasurement of population stocks during Census enumerations. If the variation is due primarily to the bias in the administrative estimates and the underestimation of growth, then high values of the instrument would identify counties that have grown more than expected in the past decade and are likely to keep growing relatively more in the future. These long-term growth dynamics are well documented in the literature (e.g. Blanchard and Katz 1992) and would cast serious doubts on the exogeneity assumption of the instrument. However, as we argue below the variation in the instrument is likely to come not only from the mismeasurement of population flows, but also from the mismeasurement of population stocks during Census enumerations. A we argue below, what determines the suitability of the Census shock as a valid instrument for federal spending changes at the local level is the relative biases of the two measurement methods.

2.1 Challenges of Counting the Population

The coverage of the Census enumeration has been a topic of intense research and debate among statisticians, demographers and policy makers in the last thirty years (see Brown et al. 1999 for a broad overview of this literature, Brunell 2002, Rosenthal 2000, Belin and Rolph 1994, Robinson et al. 1993, Fay et al. 1988, West and Fein 1990, Ericksen and Kadane 1985, Freedman 1993, Swanson and McKibben 2010). It is widely acknowledged that due to the many technical challenges associated with a physical enumeration, Census counts do not constitute an *a priori* better measure of true population than other statistical and administrative methods. In comparing postcensal estimates and population counts following the 1990 Census, Davis (1994) noted that "...ultimately we do not really know if the estimates are in error, or if it is the Census which is off the mark."

Conducting the U.S. Census is a relatively rare, technically challenging and costly endeavor. Unlike other Anglo-Saxon countries (Australia, Canada, England, Ireland and New Zealand) which conduct population censuses every 5 years, the American Census occurs only every 10 years. The United States also lacks universal population registration and health care systems such as those found in Scandinavian countries that facilitate the construction of national address lists. The Census Bureau only started maintaining and continuously updating a master address file following the Census 2000. These master files are a critical source of information to ensure that every household receives a questionnaire and is eventually counted (Swanson and McKibben 2010, National Research Council 1995). Incomplete or out-of-date master address files increase the likelihood that at-risk populations such as low-income households and movers will be missed.

Despite extensive follow-up work evaluating Census coverage over the last three decades, the Census Bureau has never used adjusted counts as the basis for congressional apportionment, federal spending allocation or administrative population estimates. This implies that the differential coverage of groups or regions between two consecutive Censuses has generated sizable variation in the error of closure. Research conducted by the Census Bureau established that for the Census of 2000, 60% of the error of closure was due to the differential coverage between Census 1990 and Census 2000, the remaining difference being due to under-estimation of national population growth (Robinson and West 2005). Other studies have found that the error of closure at the state level can be cut by more than half when administrative estimates are adjusted for under-coverage of Census counts (e.g. Shahidullah and Flotow 2005, Starsinic 1983), although others have also found mixed evidence (Murdock and Nazrul Hoque 1995).

Factors that make it hard to measure population changes through administrative data sources also make it hard to measure population stocks during Census enumerations. Several risk factors that are associated with the under-coverage of administrative data have also been related to the under-coverage of the Census: college students enumerated at their family home and their college address, children in joint custody, individuals with more than one residence, renters, multi-unit housing, population in rural areas, racial and ethnic minorities, foreign-born migration, legal emigration, Medicare under-enrollment, etc. (Robinson et al. 2002, Rosenthal 2000, Boscoe and Miller 2004, Judson, Popoff and Batutis 2004, Word 1997, Robinson 2001). Of particular concern for the measurement of population growth is the migration of low-income households. Since one of the main sources of information on internal migration comes from IRS tax records, low income households who do not have to file tax returns are more likely to be missed by administrative estimates. These groups however are also much more likely to be missed in Census enumerations than less mobile groups (Duane and Bradburn 1994).

2.2 Identifying Variation

In this section, we present a simple framework that formalizes the source of variation in the Census shock. This model relates the instrument to specific factors that could potentially challenge the exclusion restriction underlying our IV regressions, namely that the Census shock only affect locals economic growth through its impact on subsequent federal spending. A general model of the administrative or postcensal (PC) and Census (C) estimates of population can be written as follows:

$$Pop^i_{c,t} = g^i(Pop^*_{c,t}, u^i_{c,t}) \text{ for } i = C, PC,$$

for county c and year t where $Pop^*_{c,t}$ is actual population and $u^i_{c,t}$ are measurement errors. A specific yet flexible model of the population estimates is obtained by the following log-linear model

$$\log(Pop^i_{c,t}) = \alpha^i + \lambda^i \log(Pop^*_{c,t}) + u^i_{c,t} \text{ for } i = C, PC,$$

where the measurement error $u^i_{c,t}$ is independent of $\log(Pop^*_{c,t})$. In this model neither estimate gives the true population level $Pop^*_{c,t}$ but both contain an error term and might be biased to different degrees. These biases are characterized by the parameters α^i and λ^i.

The Census shock or error of closure is defined as the difference between these estimates in the year of a Census.

$$CS_{c,t} = \log(Pop^C_{c,t}) - \log(Pop^{PC}_{c,t}) = \Delta\alpha + (\lambda^C - \lambda^{PC}) \log(Pop^*_{c,t}) + \Delta\mu_{c,t}, \quad (1)$$

where $\Delta\alpha = \alpha^C - \alpha^{PC}$ and $\Delta u_c = u^C_{c,t} - u^{PC}_{c,t}$.[9] We can then express the exclusion restriction in the context of an IV regression as

$$\begin{aligned} 0 &= \mathbb{C}ov(CS_{c,t}, \varepsilon_{c,t}) \\ &= \mathbb{C}ov(\Delta\alpha + (\lambda^C - \lambda^{PC}) \log(Pop^*_{c,t}) + \Delta u_{c,t}, \varepsilon_{c,t}) \\ &= (\lambda^C - \lambda^{PC})\mathbb{C}ov(\log(Pop^*_{c,t}), \varepsilon_{c,t}) + \mathbb{C}ov(\Delta u_{c,t}, \varepsilon_{c,t}) \\ &= (\lambda^C - \lambda^{PC})\mathbb{C}ov(\log(Pop^*_{c,t}), \varepsilon_{c,t}), \end{aligned}$$

where $\varepsilon_{c,t}$ is the structural error term from a given outcome equation on income or employment such as in Equation (6) below. The third line assumes $\Delta\alpha$ is constant. The fourth line uses

[9] Note that the source of variation is coming from differences in population estimates and not from changes in actual population. This is important as population can be endogenous to economic factors that might confound the estimation strategy.

the fact that $\Delta u_{c,t}$ is the difference between measurement errors that are uncorrelated with the true population and the IV error term. The exclusion restriction is then satisfied when $\lambda^C - \lambda^{PC} = 0$ or when $\mathbb{C}ov(\log(Pop^*_{c,t}), \varepsilon_{c,t}) = 0$.

If both measurement methods approximate true population with added classical measurement error, we would have $\alpha^i = 0$ and $\lambda^i = 1$ for $i = C, PC$. In such a world, the Census shock would be the combination of two classical measurement errors and would be unrelated to any other factors that could confound the identification strategy. The model in Equation (1) suggests that the classical measurement error model, while sufficient, is overly restrictive. A sufficient, yet less restrictive condition, for the Census shock to be unrelated to true population and any other confounding factors is that $\lambda^C = \lambda^{PC}$. This condition states that both measurement methods may be biased but what is relevant for the instrument is the degree to which their correlation with true population differs. If this condition for our instrument is satisfied, i.e. the degree of bias in the Census counts and the administrative estimates are of the same magnitude, then the Census shock is plausibly exogenous.

This condition is not directly testable as it relies on knowledge of the true population $Pop^*_{c,t}$. However, it implies that the instrument should be unrelated to factors that would affect the outcomes of interest apart from its effect on federal spending. We therefore provide a number of checks to confirm the validity of the exclusion restriction. These checks consist of testing for any correlation between the error of closure and growth prior to the Census. Based on the model above, if one of the two methods is more biased than the other, one would expect the instrument to be correlated with past growth. If for example administrative estimates consistently underestimate true population growth, then the model would state that $\lambda^C > \lambda^{PC}$ and the correlation between the Census shock and past population growth would be positive. Again based on the model, a negative correlation between the Census shock and past growth would imply that the Census counts have a larger bias relative to the administrative estimates.

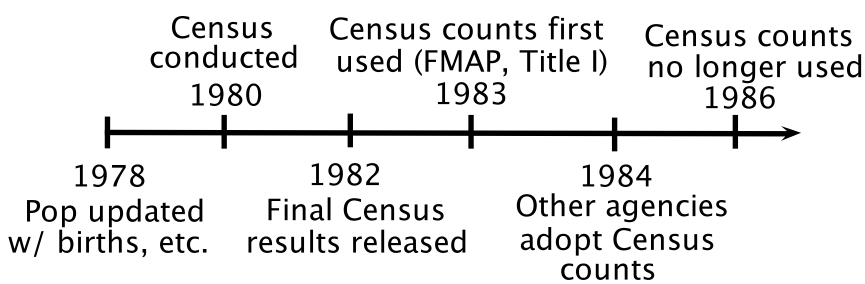
Another check of the exogeneity assumption relies on the timing of the impact of new Census figures on federal spending. We show that there is no positive correlation with local growth until after the publication of official Census counts. This is consistent with how we expect population revisions to start affecting federal spending allocation across counties and growth through the local fiscal multiplier. Finally we show that our estimates are robust to the inclusion of lagged outcomes and other drivers of economic growth at the local level.

2.3 Population and Federal Spending

Local population levels are used in the allocation of federal funds mainly through formula grants that use population as an input and through eligibility thresholds for direct payments to individuals (e.g. Blumerman and Vidal 2009, GAO 1987). Federal agencies use annual population estimates or Census counts depending on the availability and timeliness of the latter. The release of new Census counts therefore leads to a change in the population levels used for allocating spending that we exploit in our empirical design. However, this change does not occur in the year of the Census since it usually takes two years for the Census Bureau to release the final population reports.[10] The specific timing of the release of the final Census counts allows for a powerful test of our identification strategy as the Census shock should be uncorrelated with economic growth and federal spending at the local level before the release of the final Census counts.

Federal agencies also have some discretion in updating the population levels used to allocate spending. Variation in the year of adoption of Census counts across agencies suggests that the Census shock influences federal spending several years after the release of the final counts. One example is the Federal Medical Assistance Percentage (FMAP) used for Medicaid and Temporary Assistance for Needy Families (TANF) transfers to states. This percentage is a function of a three year moving average of the ratio of states' personal income per capita to the national personal income per capita.[11] The three-year moving average is also lagged three years so that the 2009 FMAP, the last year in our dataset, relies on population estimates dating back to 2004 (Congressional Research Service, 2008). We therefore would not expect the Census population shock to affect FMAP spending until three years after the Census is conducted. The moving average used in the FMAP implies that the population revision will be correlated with changes in the FMAP up until five years after the Census year. We illustrate a simplified timeline for the 1980 Census in Figure 3.

Figure 3: Timeline



Notes: This figure plots a stylized timeline of events surrounding a Census enumeration. See text for details.

Given the interest in the under-coverage of the Census, several attempts have been made to determine the effects of adjusting Census counts on the allocation of federal funds at the state level. For example, a review using statutory information for the 15 largest formula grants in

[10]See Census Bureau (2010b,c) and Census Bureau (2001).

[11]Per capita income depends on population estimates only through the denominator. See the Data section for further details.

1997 by the Government Accountability Office found that average federal spending in a given state would have increased by \$480 per year for every person added through adjustment had the 1990 Census state populations levels been adjusted for undercount (GAO 1999). Given the Census undercount at the state level in 1990, adjustment of population figures would have redistributed around 0.6% of federal spending. Other studies have also found similar estimates (Murray 1992, GAO 2006). Given that the error of closure is of comparable magnitude to the Census undercount, these results imply that we should also expect to find a relation of similar magnitude between the error of closure and subsequent changes in federal spending at the local level. We document these results in Section 4 below.

3 Data

Counties are a natural starting point for our analysis because of their large number and stable boundaries for the period under study. There are over 3,000 counties when excluding Hawaii and Alaska, which we do throughout the analysis. We use contemporaneous county population estimates published by the Census Bureau from 1970 to 2009. These are called postcensal estimates.[12] There were no postcensal estimates released in 1980, 1990 and 2000 because of the upcoming Censuses. Since our empirical strategy requires the comparison of administrative estimates and Census counts, we produce these postcensal estimates for census years using publicly-available data in an attempt to replicate the Census Bureau's methodology. We use annual county-level births and deaths from the Vital Statistics of the U.S. to generate our own estimates of county natural growth. The data used to estimate internal and international migration are from the County-to-County Migration Data Files published by the IRS's Statistics of Income.

Data on federal spending come from the Consolidated Federal Funds Reports (CFFR) published annually by the Census Bureau.[13] This dataset contains detailed information on the geographic distribution of federal spending down to the city level. In cases where federal transfers are passed through state governments, the CFFR estimates the sub-state allocation by city and county. Spending is also disaggregated by agency (from 129 agencies in 1980 to 680 in 2009) and by spending program (from 800 programs in 1980 to over 1500 in 2009). The specific programs are classified into nine broad categories based on purpose and type of recipient. We restrict our analysis to the following categories: *Direct Payments to Individuals*, *Direct Payments for Retirement and Disability*, *Grants* (Medicaid transfers to states, Highway Planning

[12] The Census Bureau also releases intercensal estimates, which are revised after new Census counts are available. See Census Bureau (2010a) for details on the revision procedure.

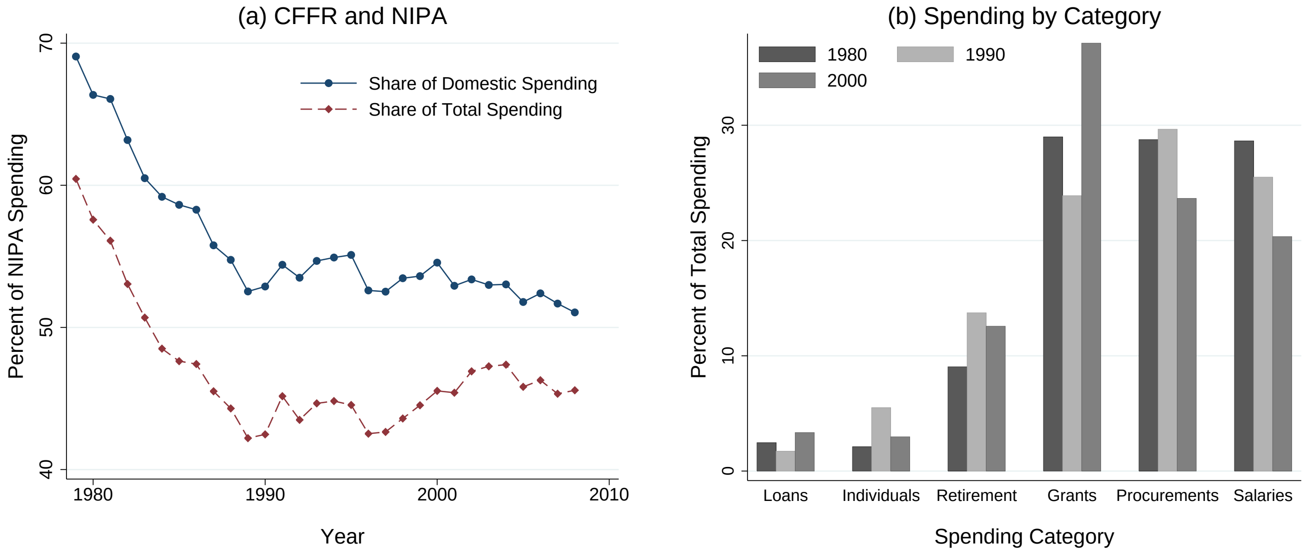
[13] The CFFR was first published by the Census Bureau in 1983. Predecessors to the CFFR are the Federal Outlays series from 1968 to 1980 and the Geographic Distribution of Federal Funds in 1981 and 1982.

and Construction, Social Services Block Grants, etc.), *Procurement and Contracts* (both Defense and non-Defense), *Salaries and Wages* of federal employees and *Direct Loans*. From these we exclude Medicare spending because federal transfers are based on reimbursements of health care costs incurred as well as Social Security transfers which are direct transfer to individuals that do not depend on local population estimates. We exclude *Direct Payments Other than for Individuals* which consist mainly of insurance payments such as crop and natural disaster insurance since these are not relevant in the context of our natural experiment and decrease the statistical power of our first stage. Finally, we exclude the *Insurance* and *Guaranteed Loans* categories because they represent contingent liabilities and not actual spending. Given the high variance of spending across years at the county level and the fact that some of the data represent obligations that are often subsequently revised, we use a three year moving average of total spending in these categories. Panel (a) in Figure 4 shows how our measure of federal spending at the national level compares to federal spending in the National Accounts. On average, we capture between 40 and 60% of total spending and between 50 and 70% of total domestic spending (total spending minus debt servicing and international payments). The decreasing coverage of our CFFR measure of spending compared to NIPA figures is mainly due to the exclusion of Medicare and Social Security spending, two of the largest and fastest growing federal spending programs. Panel (b) breaks down total federal spending by the broad categories used in the analysis for the three Census years.

Data on county personal income, salaries and wages and employment are taken from the Bureau of Economic Analysis' Regional Economic Information System (REIS). This data is compiled from a variety of administrative sources. Employment and earnings come from the Quarterly Census of Employment and Wages (QCEW) produced by the Bureau of Labor Statistics. The QCEW contains the universe of jobs covered by state unemployment insurance systems and accounts for more than 94% of total wages reported by the BEA. Personal income which also includes proprietors' and capital income, transfer receipts and supplements to salaries and wages uses IRS, Social Security Administration and state unemployment agencies data among other sources. An important feature of these data is that they do not depend on the change in population estimates that is the basis of our instrument (BEA 2010). In order to make these data comparable across counties, we use income, earnings and employment in per capita terms. The population figures used by the BEA are the revised intercensal estimates. These smoothed population estimates ensure that the outcome variables are not mechanically affected by the error of closure.[14] Finally, we express all dollar values in dollars of 2009 using the national Consumer Price Index published by the BLS.

[14]See Data Appendix A for further details about the definition and sources of population data.

Figure 4: Federal Spending in the CFFR



Notes: Panel (a) plots the share of domestic and total federal expenditure reported in the NIPA that is captured by the CFFR federal spending measure used in the estimations. Panel (b) plots the share of CFFR federal spending by major category and year for 1980, 1990 and 2000. Federal expenditures in NIPA Table 3.2 is from the Bureau of Economic Analysis. CFFR data is from the U.S. Census Bureau (2010d).

4 First Stage and Properties of the Instrument

This section documents how the instrument was generated, the dynamic effects of the Census shock on the federal spending, and, finally, properties of the instrument that give credence to the exclusion restriction in our main estimation. To implement our empirical strategy, we need both Census counts and concurrent population estimates. The Census Bureau however does not publish postcensal population estimates for years in which it conducts the Census. We therefore produce population estimates for Census years using publicly-available data on the components of change of population. Because we do not have access to all the data used by the Census Bureau, we estimate the following regression with the aim of approximating the methodology used to produce the estimates:

$$\Delta Pop_{c,t}^{PC} = \phi_1 Births_{c,t} + \phi_2 Deaths_{c,t} + \phi_3 Migration_{c,t} + u_{c,t}. \quad (2)$$

This calibration equation ensures that we can adequately replicate the Census Bureau's administrative estimates of the year-to-year population change using publicly-available data. The regression is estimated separately by decade on years for which population estimates are available (which excludes Census years).[15] The components of population change are taken from

[15]The results of the calibration regressions by decade are available in an online appendix.

the Vital Statistics and IRS migration data. The R-squared of these calibration regressions are 0.91 for years 1991 to 1999 and 0.78 for 1981 to 1988.[16] The correlation between estimated population growth and our predicted population growth is over 0.90. All the coefficients also have the expected signs and magnitudes.[17]

This procedure gives us estimated population growth rates from which we can extrapolate population levels in Census years.[18] For the 2000 Census for example, we calibrate the components of population change identity across counties using population growth during the 1990s. We then use the population estimates in levels for 1999 and the predicted population growth from actual births, deaths and migration in that year to produce population estimates for April 1st, 2000. The estimates are used to produce the counterfactual postcensal population levels $\widehat{Pop^{PC}_{c,\text{Census}}}$.[19] We then define the Census shock as[20]

$$CS_{c,\text{Census}} = \log(Pop^{C}_{c,\text{Census}}) - \log(\widehat{Pop^{PC}_{c,\text{Census}}}). \tag{3}$$

4.1 Census Shock and Federal Spending

This subsection documents how the instrument is related to federal spending growth at the county level. We first investigate the dynamics of this relationship to test whether it is consistent with statutory information on the publication of new Census population counts and their adoption by federal agencies. Specifically, since it takes around two years for the Census Bureau to compile and publish the Census counts at the local level, we shouldn't see any correlation between federal spending growth and the Census shock in years 0 and 1 following a Census. Moreover, there is a delay in the adoption of new population levels since federal agencies have some discretion in the way new population figures are used to allocate federal funds (GAO 1990). This suggest that the change in population due to the Census shock should affect spending for several years after the new Census count are released. Finally, once the new Census population levels have been fully incorporated in the allocation of federal funds across regions, the Census shock should no longer be correlated with spending growth.

Figure 5 below shows separately the average growth rates in federal spending per capita for counties that experienced positive and negative population shocks (relative to their state

[16]The Census Bureau did not publish postcensal estimates for 1979 and 1989.

[17]We report the results of these regressions in an online appendix.

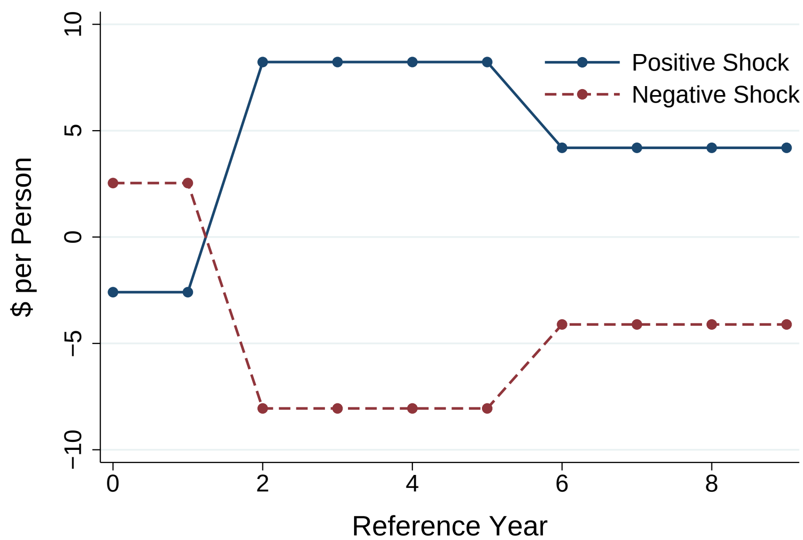
[18]Population growth is prorated in the year of the Census to account for the difference in end dates between population estimates (July 1st) and Census day (April 1st). Results are not materially affected by this transformation.

[19]Alternative methods of estimating the counterfactual postcensal population estimates, including a raw sum of the components of change (i.e. $\Delta Pop^{PC}_{c,t} = Births_{c,t} - Deaths_{c,t} + Migration_{c,t}$) and using an AR(3) time series model, produce similar estimates and do not alter our main results.

[20]Notice that while our instrument has been generated in an estimation step prior to the main estimations, it is not necessary to adjust the standard errors of our instrumental variable estimates (see e.g.Wooldridge 2002).

average) by year following a new Census enumeration. Consistent with information from the Census Bureau, we find that the average growth rate in federal spending goes up (down) markedly between years 0 and 1 and years 2 to 5 for counties that experience positive (negative) Census shocks. The differences in growth rates between the two groups of counties are not statistically different from zero in years 0 and 1 as well as in years 6 to 9 at which point we expect the new population figures to be fully incorporated in spending allocation and no longer affect growth rates. The difference in spending growth rates between the two groups of counties is around $16 per person per year during years 2 to 5 and is statistically different from zero at the 95% confidence level. We are able to precisely measure even such as small difference in means due to the large number of counties included in our sample during three consecutive Censuses. Given the average size of the Census shock in our sample, this difference implies an additional $250 per year in federal spending per additional person found by the Census count.

Figure 5: Average Federal Spending Growth for Positive and Negative Census Shocks

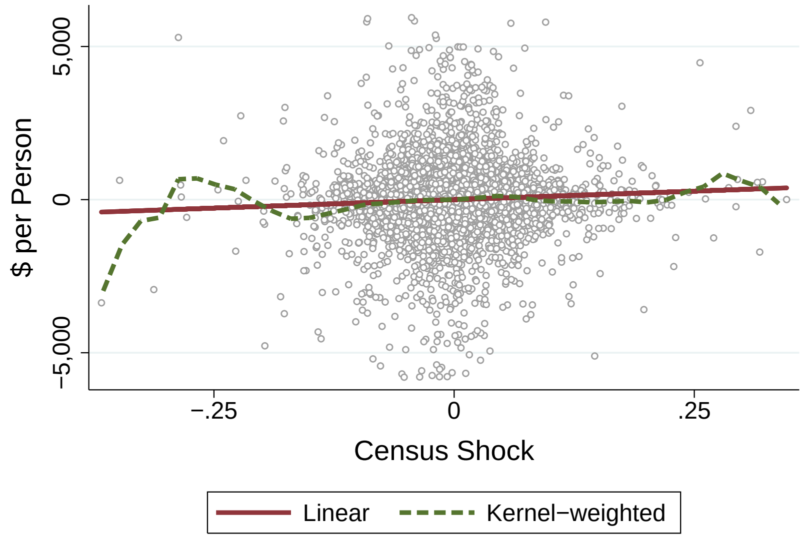


Notes: The figure shows the average growth rates in federal spending per capita for counties that experienced positive and negative population shocks relative to their state average by decade separately. Growth rates shown are the average for years 0 and 1, 2 to 5 and 6 to 9.

We also investigate how the Census shock affects spending allocation beyond this difference in means. Figure 6 plots growth in federal spending for years 2 to 5 over values of the Census shock by individual county. The figure also shows the linear relation between the two variables as well as the predicted values of a locally weighted non-parametric regression.

The average marginal effect of adding a person to the population estimate through the Census enumeration is an increase in federal spending of around $1,125 over four years or $280

Figure 6: Census Shock and Federal Spending Growth



Notes: The figure shows the scatter plot of average annual federal spending growth per capita from year 2 through 5 by value of the Census shock relative to state-decade averages. The figure also displays the linear relation estimated via OLS and the non-parametric relation using a locally-weighted polynomial of degree 1 with kernel half-width of 0.02.

per year.[21] The non-parametric estimator shows that this marginal effect varies somewhat across the range of values of the Census shock. Based on the slope of the local polynomial function (dashed line), we can see it is highest for very low values of the Census shock. The marginal effect is also negative along some ranges of the instrument but these values are not precisely measured. The scatter plot also highlights the fairly small variation in federal spending growth explained by the instrument. Whereas the standard deviation of growth in federal spending per capita is around $2,300 per year, the standard deviation of federal spending growth as predicted by the linear model is only $64 per year, around 3% of the total variation. Although these changes in federal spending are small, the fact that we use three distinct Census shocks for more than 3,000 counties provides us with the statistical power to precisely estimate this average marginal effect.

Finally we estimate the dynamic linear first-stage relationship between the Census shock and federal spending growth with the following regression:

$$\Delta F_{c,t} = \alpha_{s,t} + \gamma_t CS_{c,\text{Census}} + e_{c,t}, \tag{4}$$

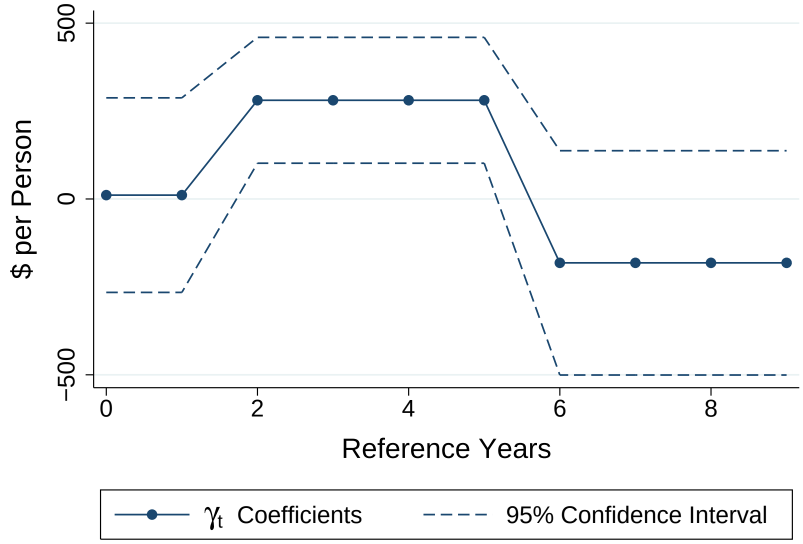
where $\Delta F_{c,t}$ is annual growth in federal spending per capita for county c in time period t, $\alpha_{s,t}$

[21] A GAO review of the 15 largest formula grant programs for fiscal year 1997 found that federal spending in a given state would increase by $480 per additional person per year had the 1990 Census state populations been adjusted for undercount (GAO 1999).

are state-decade fixed effects and $CS_{c,\text{Census}}$ the Census shock which occurs at the beginning of each decade. The first stage estimates γ_t are estimated jointly for separate periods based on prior information regarding the production and subsequent use of Census counts. The first sub-period t is for years 0 and 1 that occurs immediately after a new Census is conducted but before the new population data is released by the Census Bureau and used to update federal spending allocation across local areas. γ_t is then estimated for years 2 to 5 (e.g.1982 to 1985) when we expect the Census shock to affect federal spending allocation and growth at the county level and finally for years 6 to 9 when the new population counts have been fully integrated into federal spending allocation.

Figure 7 plots the γ_t's along with their respective 95% confidence interval with year 0 being the year in which the Census is conducted. Analogous to what we found in Figure 5, the graph shows that the Census shock is not correlated with federal spending growth in years 0 and 1 before the Census counts are released with an estimated marginal effect very close to zero. As explored in detail in the next subsection, this feature of the relation between the shock and federal spending is an important test of the validity of our identification strategy. The figure also shows that the Census shock is positively correlated with federal spending growth for years 2 to 5 with an estimated \$260 in additional federal spending per year for every person found in the Census. The figure also confirms that once all agencies have adopted the new population counts, these counts become obsolete and no longer affect federal spending growth. The estimated γ_t for years 6 to 9 is once again not statistically different from zero. We rely on the dynamics in this graph in our instrumental variables specification below and restrict the estimation to years 2 through 5 (i.e. 1982-1985) as these are the years in which our exogenous source of variation has a significant impact on the growth of federal spending. This graph demonstrates that the instrument we introduce in this paper isolates substantial variation in federal spending in a manner that is consistent with the timing of the release of Census counts and the slow adoption of population figures by government agencies. As discussed in the following subsection, the timing of this natural experiment as well as additional properties of the Census shock variable provide indirect tests of our main identification assumption and support the validity of exclusion restriction we invoke when using the Census shock as an instrument for federal spending growth.

Figure 7: First Stage Estimate by Year



Notes: The figure plots the estimated first stage coefficients from Equation (4) along with their 95% confidence interval. The point estimates are estimated and reported jointly for years 0 and 1, 2 to 5 and 6 to 9.

4.2 Properties of the Census Shock Variable

The objective of this paper is to provide a new means of measuring the effects of government spending on local economic growth. As argued in the previous subsection, the Census shock variable is a strong predictor of federal spending growth. In this subsection we characterize three additional properties of the instrument that support the assumption that the Census shock only affects local economic growth through its effect on federal spending.

First, we analyze whether the Census shock is geographically correlated. If the Census shock is strongly correlated across nearby counties in a given region, this might be evidence that the Census shock is related to a region-wide shock that might also explain the outcomes of interest and thus violate the exclusion restriction. However, an analysis of variance (ANOVA) shows that only 8% of the variation can be explained by MSA and state indicators. We also find on average a correlation of around 0.2 in values of the Census shock across counties in the same MSA. Therefore most of the variation in the instrument appears to be at the county level or below and not driven by region-wide economic shocks.[22]

A second potential concern is that time-invariant characteristics of particular counties might lead to large measurement errors in population and might also be determinants of economic development. For example, geographic, cultural, or political characteristics of a given region

[22] In Section 6 we analyze the spillover effects of shocks to nearby counties on local economic growth. The goal of that analysis is to explore the mechanisms through which additional spending leads to increased growth, not as a challenge to the exclusion restriction.

Figure 8: Serial Correlation of the Census Shock

Notes: The figure shows the scatter plots and estimated linear relation between each county's Census shocks across two consecutive Censuses after controlling for state-decade fixed effects.

might set counties on different growth paths and might also affect the likelihood that Census enumerators make errors in counting population or might affect how individuals respond to Census surveys. A similar concern is that counties might be subject to serially correlated shocks, such as the inflow of immigrant workers, that could be at the source of both our Census shock and the increase in economic activity. To explore the validity of these potential concerns, we consider whether the instrument is serially correlated. Figure 8 presents the scatter plots of the Census shocks across decades. These plots demonstrate that there is virtually no serial correlation in the shocks across Censuses. In both graphs, the slopes of the correlation are flat and not statistically different from zero. This feature of the Census shocks is consistent with measurement error being the source of the variation in the instrument. Importantly, it is evidence against confounding factors that could be driving the variation across areas and that are known to be strongly serially correlated such as illegal immigration in border states, for example.

A third and crucial concern is that the Census shock is correlated with underlying growth trends or previous local shocks that might directly affect the subsequent economic outcomes of interest. For example, if the postcensal population figures systematically underestimate economic growth or undercount true population levels, counties with previously higher growth trends would realize larger Census shocks and would likely maintain higher growth rates in the future. These local shocks could therefore be driving our results independently of the "true" effect of government spending on local growth. As discussed earlier the timing of our natural experiment provides an important test of this potential concern. Recall that while the Census shock is calculated in a given Census year, the results of the Census enumeration are not released until two years after it is conducted. This means that the instrument should only start affecting spending growth after an initial two years lag. By the same logic, the Census shock should also be uncorrelated with economic growth in any years before new Census population figures are published and used to update federal spending allocation.

Table 1 below shows the estimated coefficients for the following regressions of past local economic growth on the instrument $CS_{c,\text{Census}}$ and a full set of state by year fixed effects $\alpha_{s,t}$

$$\Delta^k y_{c,t} = \alpha_{s,t} + \beta CS_{c,\text{Census}} + \varepsilon_{c,t}. \tag{5}$$

The dependent variables $\Delta^k y_{c,t}$ are per capita income, earnings and employment growth over various periods k before the publication of the Census counts in year 2. For each panel, in column (1) we test whether the instrument can predict local growth in the two years between when the Census is conducted and when the final population counts are published; in column (2) we use as the dependent variable local economic growth over the three years preceding the Census; and in column (3) growth over the five years preceding the Census.

Table 1: Census Shock and Past Growth

(a) Income

Years	0 and 1	-3 to 0	-5 to 0
Census Shock	-201.8	-576.9**	-212.8
	(331.7)	(233.9)	(245.5)
R-squared	0.36	0.35	0.39

(b) Earnings

Years	0 and 1	-3 to 0	-5 to 0
Census Shock	-55.6	-135.3	-76.0
	(228.5)	(150.1)	(129.9)
R-squared	0.24	0.28	0.35

(c) Employment

Years	0 and 1	-3 to 0	-5 to 0
Census Shock	-8.4**	-18.2***	-8.0**
	(3.2)	(5.4)	(3.3)
R-squared	0.17	0.13	0.11

Notes: The table presents the OLS results from Equation (5). The number of observation is 9,204. State-decade fixed effects included. Standard errors clustered at the state level in parentheses. See text for details. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Consider the first column that presents the correlation between the Census shock and economic growth in the two years immediately following the Census. In both panels (a) and (b) we find that the Census shock is not correlated with income and earnings growth. Because the dependent variable is expressed in per capita terms and the instrument is defined as the log-difference between the Census count and the administrative estimate of the population, the β in Equation (5) can be interpreted as the marginal change in total annual county income growth (in thousands of 2009 dollars) associated with an additional person "found" by the Census enumeration. In panel (c), we find that there is a negative correlation with previous employment per capita growth. Given the definition of the instrument in Equation (1), if we were simply identifying counties that had grown more than expected between Censuses, we would expect to find larger Census counts than administrative estimates for the same year. In turn this would have resulted in positive point estimates in Table 5 everything else equal.[23] The sign of this correlation thus suggests that the variation in the Census shock is not driven by the mismeasurement of underlying growth trends by administrative estimates. The concern raised by these results is that Census shocks tend to be larger in areas in local recessions and that subsequent recovery in these areas could coincide with higher federal spending and thus bias our estimation towards finding a larger local multiplier. However, in Section 5 we present our main estimates of the multipliers and show that controlling for lagged income, earnings and employment growth does not affect our main conclusions as the estimated local multipliers change by less than 10% when adding lagged growth.

In columns (2) and (3), we explore whether the instrument predicts economic growth over two different periods before the Census is conducted. In panel (a), we find that the Census shock has a negative and statistically significant relationship at the 5% level with income growth over the three years preceding the Census, but again insignificant over a five-year period preceding the Census. Looking at per capita earnings growth in panel (b), we find that none of the point estimates are significant and all of them are also negative. Finally, the estimates for employment growth show a more systematic relation with the instrument, but once again of the opposite sign one would expect if the underestimation of growth inherent to the administrative estimates was the main source driving the variation of the Census shock. The results found here imply that counties where employment growth was relatively slower are more likely to have higher errors of closure. In our view these results do not pose an obvious threat to identification. First, we fail to find a systematic correlation between the Census shock and all lagged outcome variables. Second, the sign of the correlation between the Census shock and past growth does not provide a simple and intuitive explanation for how the instrument could be identifying long term growth patterns. The mean-reversion pattern documented in Table 5 coincides exactly

[23] Note that if administrative estimates overstated growth in low-growth counties, the same argument would apply and we would still expect to find a positive correlation between the Census shock and past growth.

with the timing of the publication of new Census counts two years after the Census is actually conducted and the population shock took place. Finally, the inclusion of past growth in our main IV estimations does not substantially affect the point estimates, suggesting that the effect Census shock on local growth operates independently from any past growth trends.

Taken together, the properties of the instrument, including lack of geographical correlation, lack of serial correlation, and the particular dynamics of the correlation between the Census shock, federal spending and local economic growth, provide strong indirect evidence of the validity of the exclusion restriction and the use of the Census shock as an instrument for federal spending growth at the local level.

5 Estimates of Local Fiscal Multipliers

This section presents our main estimates. We first present a reduced form version of the results that shows that our identification strategy is borne out in the raw data. This reduced-form relation is also illustrated via scatter plots of economic growth over values of the Census shock by individual counties along with the linear and non-parametric estimators. We then present OLS and IV regressions and interpret these results in terms of local multipliers and cost per job created.

5.1 Reduced Form Results

The estimates in Table 2 provide evidence of the impact of federal spending on local economic outcomes that does not rely on statistical models. The main idea is to compare growth in federal spending and economic outcomes across counties with large and small Census shocks. To this end, we group counties into bins based on quantiles of the Census shock. We then relate how each of these bins perform in terms of growth in the outcome variables and calculate the implied marginal impact of the Census shock on federal spending, the local spending multiplier on income and earnings and the cost per job created (obtained by the inverse of the local employment multiplier). To produce Table 2, we compute the average Census shock and growth in spending and outcomes for reference years 2 to 5 relative to all other counties in the same state for a given decade.

Panel (a) shows the average value of the Census shock and the growth rates of federal spending and outcome variables by quartile of the Census shock. Column (1) characterizes the variation in the instrument. Comparing the first and the last bin we see that the population shock varies by over 10 percentage points (or 10 people "found" through the Census enumeration per 100 people) in our sample. Column (2) shows how this population shock translates into annual per capita growth in federal spending. For the first bin containing counties with a

Table 2: Reduced Form Estimates

(a) Average Annual per Capita Growth

Quartile	(1) Census Shock	(2) Fed spend	(3) Income	(4) Earnings	(5) Employment
0-25%	-5.34	-18.59	-25.02	-19.11	-541.90
25-50%	-1.11	-2.54	-5.43	3.00	-196.14
50-75%	1.02	2.92	9.39	8.79	309.18
75-100%	5.43	18.21	21.06	7.32	428.87

(b) Implied Annual Marginal Effects

Quartile	Pop on Fed Spend	Fed Spend on Income	Fed Spend on Earnings	Cost per Job
0-25%	348	1.35	1.03	34,300
25-50%	229	2.14	-1.18	12,953
50-75%	286	3.22	3.01	9,435
75-100%	335	1.16	0.40	42,463
Mean	299	1.96	0.82	24,788

Notes: Panel (a) reports the average value of the Census shock and annual per capita growth for federal spending and the outcome variables by quartiles of Census shock values. Averages are relative to state-decade for reference years 2 through 5. Panel (b) reports the ratio of average federal spending growth to Census shock and average growth of the outcome variables to federal spending from Panel (a). The number of observations is 9,204.

Census shock in the bottom quartile, an average Census shock of -5.35%, yields an average annual decrease in federal spending of \$18.59 per person over 4 years.

The monotone ordering of the averages in column (1) is a mechanical effect from ranking the counties by values of the Census shock. The fact that changes in federal spending in column (2) are also ranked is evidence that our instrument is a good predictor of federal spending. In almost all cases for the other outcome variables, we find that positive (negative) Census shocks are associated with positive (negative) economic growth outcomes. Furthermore the fact that the magnitudes of these changes are all ranked in ascending order provides evidence that the identification strategy that we pursue in this paper does not rely on a particular statistical model.

Panel (b) of Table 2 shows the implied marginal effects by taking ratios of average growth rates from the first panel. These ratios imply that finding an additional person through the Census enumeration is associated on average across quartiles with an increase in government spending of \$299 per year (dividing column 2 by column 1). Panel (b) also shows the federal spending multiplier on income and earnings calculated by dividing the change in the economic outcome variable by the change in spending (dividing columns 3 and 4 by column 2). These effects are large in magnitude with average values of 1.96 for income and 0.82 for earnings.

The last column presents the cost of creating an additional job, which is calculated by taking the average federal spending growth (column 2) from panel (a) and dividing it by average employment growth (column 5). The average cost across the four bins is around \$25,000 per year. As we show below, our instrumental variables estimates are close to these values. Furthermore, it is reassuring to find that the marginal effects are relatively stable across bins.

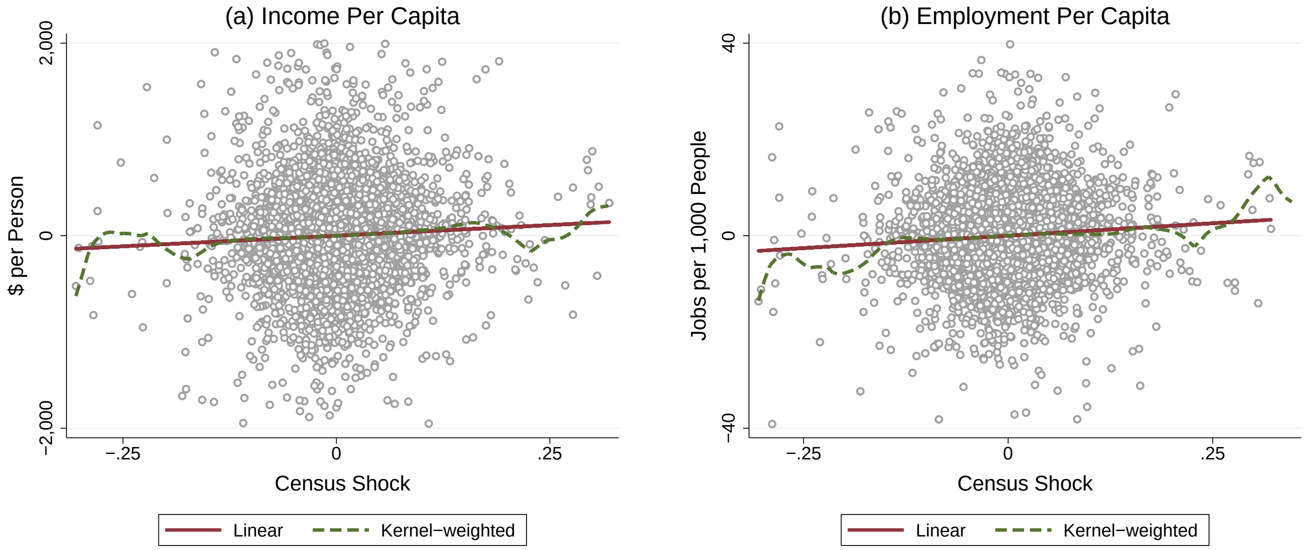
The average marginal effect of adding a person to the population estimate through the Census enumeration is an increase in federal spending of around \$1,125 over four years or \$280 per year.[24] The non-parametric estimator shows that this marginal effect varies somewhat across the range of values of the Census shock. Based on the slope of the local polynomial function (dashed line), we can see it is highest for very low values of the Census shock. The marginal effect is also negative along some ranges of the instrument but these values are not precisely measured. The scatter plot also highlights the fairly small variation in federal spending growth explained by the instrument. Whereas the standard deviation of growth in federal spending per capita is around \$2,300 per year, the standard deviation of federal spending growth as predicted by the linear model is only \$64 per year, around 3% of the total variation. Although these changes in federal spending are small, the fact that we use three distinct Census shocks for more than 3,000 counties provides us with the statistical power to precisely estimate this average marginal effect.

Panel (a) in Figure 9 plots the average annual per capita income growth in the 4 years following the publication of the new Census counts over the values of the Census shock. Similar to Figure 6, we also display the linear relation and a local polynomial estimate to describe how the conditional average varies over the range of Census shock values. The slope of the linear relation is around \$440 per year per additional person found. The standard deviation in annual income per capita growth of \$20 predicted by the OLS estimate represents around 5% of the total standard deviation in annual per capita income growth in our sample. The non-parametric estimator is much noisier at the tale ends of the distribution, but aligns very well with the linear estimator in the middle of the distribution. Panel (b) displays the same scatter plot and predicted values for the linear and non-parametric estimators for annual employment per capita growth. For both estimators, the slope of the curves in the middle of the Census shock distribution are close to 10 additional jobs per person per year.

Finally, we provide in Figure 10 a graphical presentation of the reduced form relation between the Census shock and all three economic outcomes by year using the same specification as for Figure 7 (Equation 4). The dynamics of the reduced form results for all three outcome variables are similar to the first stage in that the two years following the Census display small

[24] A GAO review of the 15 largest formula grant programs for fiscal year 1997 found that federal spending in a given state would increase by \$480 per additional person per year had the 1990 Census state populations been adjusted for undercount (GAO 1999).

Figure 9: Census Shock and Economic Growth



Notes: The figure shows the scatter plot of average annual income and employment growth per capita from year 2 through 5 by value of the Census shock relative to state-decade averages. The figure also displays the linear relation estimated via OLS and the non-parametric relation using a locally-weighted polynomial of degree 1 with kernel half-width of 0.03.

and negative correlation between the Census shock and the outcomes variables (statistically insignificant for income and earning, see Table 1) before increasing to positive and statistically significant levels during years 2 to 5 for all three outcomes. Beyond year 5, the point estimates once again become small and statistically insignificant.[25]

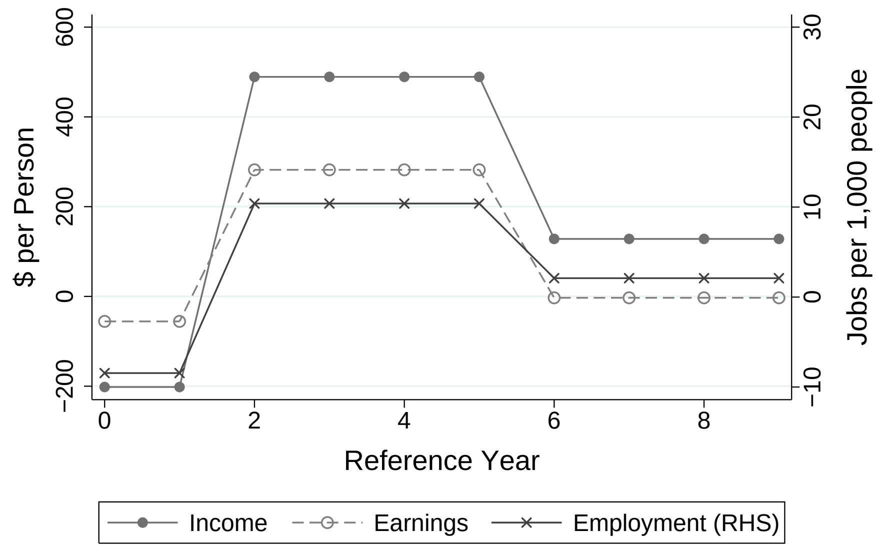
5.2 OLS and IV Estimates

This subsection presents our main estimates of the impact of government spending on income, earnings and employment growth. These multipliers are interpreted as the total impact of policy interventions that include direct impacts of government spending (such as government purchases or government hires) as well as impacts through indirect channels (such as the economic activity created by new government employees). As in the previous section, we restrict our analysis to reference years 2 through 5 as these are the years during which our instrument is expected to impact government spending. We quantify the relationships explored in the previous section by linear models of the form:

$$\Delta y_{c,t} = \alpha_{s,t} + \beta \Delta F_{c,t} + \Gamma X_{c,t} + \varepsilon_{c,t}, \tag{6}$$

[25]The confidence intervals are not shown to preserve clarity.

Figure 10: Reduced Form-Estimates by Year



Notes: The figure plots the estimated coefficients from the reduced form equation using local personal income, earnings and employment per capita growth analogous to Equation (4). The coefficients are estimated and reported jointly for years 0 and 1, 2 to 5 and 6 to 9. The coefficients for *Employment* are scaled by jobs per 1,000 people and reported on the right-hand side axis.

where $\Delta y_{c,t}$ is the average annual per capita growth of a given economic outcome over years 2 to 5 as a function of $\Delta F_{c,t}$, the average per capita change in federal spending over the same period, a vector $X_{c,t}$ that includes lagged values of income, earnings and employment growth, and local demand and supply shocks. We also include state-decade fixed effects $\alpha_{s,t}$. Finally, we allow for arbitrary correlation of the error term $\varepsilon_{c,t}$ at the state level across counties and decades. Given that both the dependent variable and federal spending are expressed in average annual per capita growth, the coefficient on federal spending β is interpreted as the local multiplier of federal spending on total personal income, earnings and employment. The inverse of the local employment multiplier can also be interpreted as the cost per job created.

As a prelude to our causal estimates of the impact of federal spending on economic outcomes we present OLS regressions that do not address the potential endogeneity of federal spending at the local level. Table 3 reports the results from the OLS regressions for income per capita. The OLS estimates are statistically significant but of small economic magnitude: they imply an average growth in total personal income of 10 cents for every additional federal dollar spent in the county. In column (2) we add lagged income, earnings and employment growth measured as the average annual growth per capita in the two years between the Census enumeration and the publication of the updated population counts. The point estimate on past income growth shows that county level income growth is serially correlated with a negative point estimate indicating mean-reversion in per capita income growth. However, adding past growth does not substantially alter the estimated relation between federal spending and local growth or increase the overall fit of the model. In column (3) we add an industry share-shift variable proposed by Bartik (1991). The variable calculates the county-level annual percentage growth in employment predicted by national employment growth at the 3-digit industry level and the base year industry composition of employment in each county. We also add a variable meant to capture a specific source of population growth by using a supply shock of immigrants developed by Card (2001). The variable is constructed in a similar way to the industry share-shift, but using levels of immigrant populations across Censuses by country of origin instead of industry employment levels. If, for example, there was a large influx of Eastern European immigrants in the US between 1990 and 2000, counties with larger Eastern European-born populations in 1990 would be likely to experience a larger influx of immigrants everything else equal. Card (2001) shows this variable is a good predictor of changes in total population. Adding these two sources of local economic and population growth as additional exogenous variables also does not affect the coefficient on federal spending. Column (4) which includes the full set of controls similarly shows a very small local spending multiplier.

Table 3: OLS Estimates of the Local Income Multiplier

	(1)	(2)	(3)	(4)
Federal Spending	0.10***	0.10***	0.09***	0.09***
	(0.02)	(0.02)	(0.02)	(0.02)
Past Income Growth		-0.09**		-0.10**
		(0.04)		(0.04)
Past Earnings Growth		-0.02		-0.02
		(0.08)		(0.08)
Past Employment Growth		0.87		1.06
		(1.11)		(1.11)
Industry Share Shifter			80.73***	82.19***
			(14.62)	(14.92)
Migration Share Shifter			-1.77**	-1.82**
			(0.83)	(0.75)
Observations	9,204	9,204	9,204	9,204
R-squared	0.21	0.23	0.24	0.26

Notes: The table reports the estimated coefficients from Equation (6). The dependent variable is the average annual growth in local personal income per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

We show the same OLS results with earnings and employment per capita growth in Tables 4 and 5. These estimates are also of small magnitude. The coefficient on federal spending in Table 4 shows that the response of labor earnings is twice as small to marginal changes in federal spending as total income. If these OLS coefficients were unbiased estimates of the true impact of federal spending at the local level, the results in Table 5 would imply that an additional million dollar in federal spending would only increases employment by 1.7 job per year in the average county. Taking the inverse of the employment multiplier gives the cost in terms of federal spending of increasing employment by one job. This small employment multiplier therefore implies a cost per job created of around $600,000 per year in the average county.

Table 4: OLS Estimates of the Local Earnings Multiplier

	(1)	(2)	(3)	(4)
Federal Spending	0.05***	0.04***	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
Past Income Growth		-0.14***		-0.14***
		(0.03)		(0.03)
Past Earnings Growth		0.19***		0.19***
		(0.07)		(0.07)
Past Employment Growth		1.23*		1.35*
		(0.67)		(0.72)
Industry Share Shifter			47.73***	47.01***
			(7.85)	(8.23)
Migration Share Shifter			-0.20	-0.57
			(0.59)	(0.47)
Observations	9,204	9,204	9,204	9,204
R-squared	0.37	0.38	0.38	0.40

Notes: The table reports the estimated coefficients from Equation (6). The dependent variable is the average annual growth in earnings per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

We now turn to our instrumental variables results. Table 6 shows the first stage results from estimating Equation (4). Column (1) only includes the Census shock and state-decade fixed effects. The coefficient implies that every additional person found through the Census enumeration increases annual federal spending in the average county by $280 per year over four years. A concern in instrumental variables estimation is that weak instruments can lead to large biases in the estimand whenever the errors are correlated with the instrument (e.g. Bound, Jaeger, and Baker 1995). To address this issue, we provide the F-statistic of the test that the instrument has a zero coefficient in the first stage equation. An F-statistic close to 10 is within the bounds of conventional levels of acceptance, suggesting that our instrument is not subject to the weak instrument problem. Adding past income growth and the share shift variables has a relatively small impact on both the estimated coefficient and its standard error. These results imply that for Clark County, Nevada for example, the Census shock of 8.8% in 2000 represented an unexpected increase in population of 118,000 people compared to

Table 5: OLS Estimates of the Local Employment Multiplier and Cost per Job

	(1)	(2)	(3)	(4)
Federal Spending	1.75***	1.78***	1.64***	1.67***
	(0.52)	(0.53)	(0.53)	(0.54)
Past Income Growth		-0.00		-0.00*
		(0.00)		(0.00)
Past Earnings Growth		0.00**		0.00**
		(0.00)		(0.00)
Past Employment Growth		-0.04		-0.03
		(0.03)		(0.03)
Industry Share Shifter			1.33***	1.30***
			(0.38)	(0.37)
Migration Share Shifter			-0.03**	-0.03**
			(0.02)	(0.02)
Cost per Job	570,993***	560,433***	609,466***	598,012***
	(169,176)	(165,052)	(198,532)	(192,500)
Observations	9,204	9,204	9,204	9,204
R-squared	0.14	0.15	0.17	0.17

Notes: The table reports the estimated coefficients from Equation (6). The dependent variable is the average annual growth in employment per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. The coefficient for federal spending is scaled up to show the marginal effect on employment per million dollars of spending. The *Cost per Job* is the inverse of the federal spending employment multiplier and is expressed in 2009 dollars. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

the contemporaneous postcensal estimate and a subsequent increase of $34 million in federal spending per year over four years. This spending shock represented a 1% increase in total federal spending for the county.

In the Table 7 we present our instrumental variables results for local personal income. Compared to Table 3, we find a much larger local spending multiplier that varies between 1.74 to 1.57 depending on the set of covariates included. These estimates are more than fifteen times larger than the corresponding OLS estimates in Table 3 and are statistically different from them in all cases. The direction of the bias in the OLS estimates therefore suggests that federal spending is directed to counties experiencing low growth.

We also consider the impact other covariates have on our estimates in columns (2) to (4). As discussed earlier, a potential confounder of our identification strategy is that the Census shock might be correlated with demand and supply shocks that can have a direct impact on the outcomes of interest. We attempt to address this concern by including lagged values of the dependent variables and other known sources of arguably exogenous variation in local economic and population growth. Similar to Table 3, lagged values of local income and earnings growth

Table 6: First Stage Estimates

	(1)	(2)	(3)	(4)
Census Shock	280.67***	289.69***	275.65***	284.80***
	(88.94)	(87.16)	(89.34)	(87.50)
Past Income Growth		-0.01		-0.01
		(0.02)		(0.02)
Past Earnings Growth		0.02		0.01
		(0.04)		(0.04)
Past Employment Growth		1.27**		1.30**
		(0.56)		(0.56)
Industry Share Shifter			11.70**	11.96**
			(4.95)	(5.28)
Migration Share Shifter			-0.07	-0.09
			(0.42)	(0.41)
Observations	9,204	9,204	9,204	9,204
R-squared	0.05	0.06	0.06	0.06
F-Stat Instr	9.96	11.05	9.52	10.59

Notes: The table reports the estimated coefficients from Equation (4) augmented with control variables. The dependent variable is the average annual growth in federal spending per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. The table reports the F-Statistic of a Wald test that the Census shock coefficient is equal to zero. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

in columns (2) and (4) are negatively correlated with the dependent variable. Although none of the coefficients for lagged growth are individually statistically significant, a Wald test strongly rejects the null hypothesis that they are all equal to zero. The coefficient on the Industry Share Shifter implies that a 1% increase in employment in a given county due to nation-wide industry growth leads to roughly \$60 increase in the growth rate of total income per capita. Given that the average income per capita growth in our sample is \$350 per person per year, this implies an elasticity of around 0.2. The estimated coefficients for the migration variable is negative and statistically significant which reflects its impact on income per capita growth both through direct changes in the denominator and also through the specific composition of migration-driven population growth. These results serve as a robustness check for the use of the Census shock as an instrument for federal spending. The IV estimates for the local multiplier are not very sensitive to the inclusion of the additional control variables–the estimated coefficient varies by less than 10% across specifications–even though these controls are themselves jointly very strongly correlated with the dependent variable.

We provide the IV estimates for earnings and employment in Tables 8 and 9. Once again, the point estimates are an order of magnitude larger than their OLS counterparts and statistically different at the 5% percent level in all but one case. When looking at labor earnings, we estimate

Table 7: IV Estimates of the Local Income Multiplier

	(1)	(2)	(3)	(4)
Federal Spending	1.74**	1.65**	1.66**	1.57**
	(0.80)	(0.71)	(0.80)	(0.71)
Past Income Growth		-0.07		-0.08
		(0.05)		(0.05)
Past Earnings Growth		-0.04		-0.04
		(0.08)		(0.08)
Past Employment Growth		-1.05		-0.81
		(1.50)		(1.42)
Industry Share Shifter			61.96***	64.13***
			(17.27)	(16.26)
Migration Share Shifter			-1.68**	-1.71**
			(0.78)	(0.75)
Observations	9,204	9,204	9,204	9,204
IV = OLS (p-value)	0.04	0.03	0.06	0.04

Notes: The table reports the estimated coefficients from Equation (6) with changes in federal spending per capita instrumented by the Census shock. The dependent variable is the average annual growth in local personal income per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. The table reports the p-value of a test of equality between the OLS and IV coefficients. All regressions include state-decade fixed effects. Standard errors clustered at the at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

that an additional dollar of federal spending increases annual earnings by 90 cents. Lagged income and earnings growth as well as the industry share-shifter variable are all individually statistically significant. The estimated local employment multiplier is around 35 jobs per million dollar of federal spending, which translates into an annual cost per job created of close to $30,000. This estimated employment multiplier is also robust to the inclusion of the additional controls, which are all strongly correlated with employment growth at the county level. This result does not imply that a new employee would be paid $30,000. Rather, it can be seen as the share of the cost per job that accrues to the federal government. The remaining share is paid by employers as a result of increased economic activity generated by government spending through direct and indirect channels. Combining the income and employment multipliers we could posit that the job created would have a total remuneration of 1.57*$30,000=$47,000.[26]

[26] We also ran the regressions using log-differenced data instead of levels which we report in the online appendix. We find a local spending multiplier on income of 2.18 and a cost per job of $35,000.

Table 8: IV Estimates of the Local Earnings Multiplier

	(1)	(2)	(3)	(4)
Federal Spending	1.00**	0.95**	0.95**	0.90**
	(0.44)	(0.42)	(0.44)	(0.42)
Past Income Growth		-0.13***		-0.13***
		(0.03)		(0.03)
Past Earnings Growth		0.18***		0.17***
		(0.06)		(0.07)
Past Employment Growth		0.11		0.26
		(0.84)		(0.81)
Industry Share Shifter			36.89***	36.46***
			(8.59)	(8.16)
Migration Share Shifter			-0.14	-0.50
			(0.52)	(0.45)
Observations	9,204	9,204	9,204	9,204
IV = OLS (p-value)	0.03	0.04	0.04	0.05

Notes: The table reports the estimated coefficients from Equation (6) with changes in federal spending per capita instrumented by the Census shock. The dependent variable is the average annual growth in earnings per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. The table reports the p-value of a test of equality between the OLS and IV coefficients. All regressions include state-decade fixed effects. Standard errors clustered at the at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

6 Spillovers

In this section, we present the results when we include neighboring counties in the estimation of local spending multipliers. This extension to the baseline results is important since there might be externalities in the effects of fiscal shocks across local areas. Depending on the sign of these spillovers, we could be underestimating or overestimating the total effect of government spending at a local or regional level. For example, if federal spending goes to building a road in a county and some of the workers are hired from other areas or materials are purchased elsewhere, the increased demand for inputs and labor could have positive effects outside the targeted county. The county-level results would then be underestimating the total impact of federal spending in a given local area. If, however, the increase in federal spending leads to in-migration from neighboring areas and higher wages due to a decrease in labor supply, this could potentially reduce the number of firms in other counties. This kind of effect could then lead to negative spillovers and our county-level results would overestimate the total impact of federal spending at a regional level. Note that our cross-sectional estimation methodology will not allow us to recover the national-level multiplier since the inclusion of year effects will absorb any national variation in growth that is common to all areas.

The spillover analysis consists of adding to our baseline Equation (6) the covariates of every

Table 9: IV Estimates of the Local Employment Multiplier and Cost per Job

	(1)	(2)	(3)	(4)
Federal Spending	37.04***	34.69***	35.84***	33.56***
	(13.74)	(12.41)	(13.43)	(12.05)
Past Income Growth		-0.00		-0.00
		(0.00)		(0.00)
Past Earnings Growth		0.00		0.00
		(0.00)		(0.00)
Past Employment Growth		-0.08***		-0.07***
		(0.03)		(0.03)
Industry Share Shifter			0.92**	0.91***
			(0.38)	(0.35)
Migration Share Shifter			-0.03*	-0.03**
			(0.02)	(0.02)
Cost per Job	26,996***	28,830***	27,904***	29,796***
	(10,010)	(10,311)	(10,455)	(10,700)
Observations	9,204	9,204	9,204	9,204
IV = OLS (p-value)	0.00	0.00	0.00	0.00

Notes: The table reports the estimated coefficients from Equation (6) with changes in federal spending per capita instrumented by the Census shock. The dependent variable is the average annual growth in employment per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. The coefficient for federal spending is scaled up to show the marginal effect on employment per million dollars of spending. The *Cost per Job* is the inverse of the federal spending employment multiplier and is expressed in 2009 dollars. The table reports the p-value of a test of equality between the OLS and IV coefficients. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

counties' closest neighbors. This is done by duplicating our sample to include each pairwise combination between a county and its neighbors. We use two different definitions of neighbors to provide robustness checks.[27] For every individual county, we define neighbors as: 1) the 10 closest counties based on highway miles between county centroids and 2) all the other counties within the same MSA (and grouping all counties not included in an MSA in the same rest-of-state area). For closest geographic neighbors, this approach generates a sample size ten times larger than the original sample. For MSA neighbors, each observation is duplicated $(n_m - 1)$ times where n_m is the number of counties in MSA m. We weigh each observation by the inverse of the number of times the observation was duplicated. This along with a degrees-of-freedom correction to the variance matrix, allows us to recover the same baseline estimate and standard errors as in the original sample when only the counties' own covariates are included as in

[27] We also used a different approach where variables were aggregated at the MSA and state levels. This approach however does not allow enough cross-sectional variation in the instrument to accurately estimate the impact of the Census shock on local outcomes. The problem of weak instrument at higher levels of aggregation is particularly acute.

baseline Equation (6).

We estimate the following spillover equation via 2SLS

$$\Delta y_{\tilde{c},t} = \alpha_{s,t} + \beta \Delta F_{\tilde{c},t} + \beta^n \Delta F^n_{\tilde{c},t} + \Gamma X_{\tilde{c},t} + \Gamma^n X^n_{\tilde{c},t} + \varepsilon_{\tilde{c},t}, \tag{7}$$

where annual federal spending change per capita in own and neighboring counties $\Delta F_{\tilde{c},t}$ and $\Delta F^n_{\tilde{c},t}$ are instrumented with own and neighboring counties' Census shocks $CS_{\tilde{c},\text{Census}}$ and $CS^n_{\tilde{c},\text{Census}}$.

Table 10 reports the first stage coefficients for the Census shock variable from the spillover estimating equation.[28] Column (1) shows the estimated coefficients for the instruments in both first stage equations when defining neighbors as the 10 closest counties by highway miles. The Census shock in own-county remains a strong predictor of own federal spending growth, unlike the Census shock of neighboring counties. The magnitudes and significance levels are inverted in the second equation using federal spending changes in neighboring counties as the dependent variable, although the coefficient for neighbors' Census shock is not as precisely estimated. In column (2), we define as neighbors all other counties in the same MSA. Since many MSAs and rest-of-state areas have a large number of counties, the duplicated sample is much larger. The coefficients for own Census shock on own spending and neighbors' Census shock on neighbors' federal spending are of very similar magnitude but with the first relation more precisely estimated. Once again, the Census shocks do not explain spending changes across neighboring counties.

In Table 11 we present the estimates from Equation (7). The estimated local multipliers for own federal spending across the two specifications are very close to those estimated in our baseline IV with the full set of covariates. We also find in both columns that federal spending in neighboring counties has a positive impact on income in own-county, although the point estimates are not statistically significant. From previous Table 10 we also know that the endogenous federal spending in neighboring counties is particularly subject to weak instrument bias. Finally, the total local spending multipliers are shown at the bottom of the table. They are equal to 1.77 in both columns, slightly higher than the baseline estimate of 1.57.

The results for employment are shown in Table 12. We once again find a similar although slightly smaller own employment multiplier to the one estimated in our baseline IV regressions. The impact on employment of federal spending changes in neighboring counties is once again positive and much smaller than own-county spending. The cost per job created (calculated as the inverse of the sum of the two local employment multipliers) is now lower than the single county estimate across all three specifications, around $25,000. Taken together, these spillover results suggest there are positive externalities across neighboring counties and federal spending

[28] The full set of results with all the covariates is available upon request.

Table 10: First Stage Spillover Estimates

	(1) 10 Closest Counties		(2) Same MSA	
	Own	Neighbors	Own	Neighbors
Census Shock				
Own	283.14***	48.79	285.94***	1.91
	(86.76)	(36.60)	(87.33)	(34.01)
Neighbors	-3.20	192.75*	-16.00	285.09**
	(45.73)	(95.83)	(35.74)	(126.56)
Observations	92,040	92,040	550,908	550,908
R-squared	0.06	0.06	0.06	0.06
Angrist-Pischke F-Stat	11.28	4.12	10.92	5.12

Notes: The table reports the estimated coefficients for the Census shock of the first stage equations for the endogenous federal spending variables from Equation (7). The dependent variables are the average annual growth in federal spending per capita in own county and neighboring counties from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. All regressions include state-decade fixed effects and all the covariates included in Table 6 for both own and neighboring counties. Standard errors clustered at the state level with degrees-of-freedom adjustment to account for duplicated observations in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 11: Spillover Estimates for Local Income

	(1) 10 Closest Counties	(2) Same MSA
Federal Spending		
Own	1.53**	1.58**
	(0.65)	(0.71)
Neighbors	0.24	0.19
	(0.46)	(0.22)
Sum of Multipliers	1.77*	1.77**
	(0.99)	(0.86)
Observations	92,040	550,908

Notes: The table reports the estimated coefficients for Federal Spending from Equation (7). The dependent variable is the average annual growth in local personal income per capita in own county from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. All regressions include state-decade fixed effects and all the covariates included in Table 7 for both own and neighboring counties. Standard errors clustered at the state level with degrees-of-freedom adjustment to account for duplicated observations in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

has a beneficial impact on the economic outcomes of areas beyond the initial recipient counties. We also finally note that once again beyond the issue of spillovers, the other fundamental difference between cross-sectional analyses and time-series designs is the fact that we cannot identify the effects of fiscal shocks common to all areas. For example, including year fixed effects in an attempt to control for unrelated macroeconomic shocks will also capture any nation-wide effect of the spending change itself in a particular year. As mentioned earlier, candidates for such nationwide shocks related to our instrument are the impact of future taxes on the current behavior of consumers and firms and the effect of the monetary policy response to a fiscal expansion.

Table 12: Spillover Estimates for Employment

	(1) 10 Closest Counties	(2) Same MSA
Federal Spending		
Own	31.69***	33.52***
	(11.05)	(12.04)
Neighbors	8.63	5.67
	(10.20)	(4.81)
Cost per Job	24,799**	25,515***
	(10,980)	(9,651)
Observations	92,040	550,908

Notes: The table reports the estimated coefficients for Federal Spending from Equation (7). The dependent variable is the average annual growth in employment per capita in own county from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. The *Cost per Job* is the inverse of the sum of the federal spending employment multipliers in own and neighboring counties and is expressed in 2009 dollars. All regressions include state-decade fixed effects and all the covariates included in Table 9 for both own and neighboring counties. Standard errors clustered at the state level with degrees-of-freedom adjustment to account for duplicated observations in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

7 Heterogeneity

This section characterizes the heterogeneity of outcomes using a quantile regression framework that describes how the impact of government spending differs throughout the distribution of county growth rates. This heterogeneity of the impacts of government spending is estimated using an instrumental variable quantile regression approach recently developed by Chernozhukov and Hansen (2008).

Our main regression estimates show that government spending has large impacts on the conditional means of income, earnings and employment across counties. A more complete characterization of the impacts of government spending over the entire distribution of income and employment growth rates is also possible. This could answer the question as to whether faster or slower growing counties are more impacted by government spending. This could also address the potential for government spending to reduce inequality in economic outcomes across counties. Quantile regression provides an appealing approach to characterizing the impact of government spending on different parts of the outcome distribution. However, methods that combine quantile regression with instrumental variables have only recently been proposed in the literature.[29] We implement the instrumental variable quantile regression (IVQR) procedure developed by Chernozhukov and Hansen (2008) that takes advantage of our identification strategy to produce causal estimates.

Before introducing the IVQR approach, we consider a quantile regression estimate that does not account for the endogeneity of government spending. For a given quantile q of the outcome distribution of $\Delta y_{c,t}$, we estimate the conditional quantile function

$$Q_q(\Delta y_{c,t}) = \alpha_t^q + \beta^q \Delta F_{c,t} + \Gamma X_{c,t}, \tag{8}$$

with α_t^q decade fixed effects, $\Delta F_{c,t}$ the per capita change in federal spending and county covariates $X_{c,t}$. We do not include state fixed effects as we are interested in comparing counties relative to the national distribution. Including state fixed-effects would change the interpretation of the results by limiting the comparison to counties within the same state. Figure 11 plots the β^q's from these estimations for 11 values of q for each of our main outcomes. Panel (a) shows the coefficients for income that are of a similar magnitude than the OLS estimates but that are generally larger for counties at the bottom of the growth distribution. Panel (b) shows both the employment multiplier (the number of additional jobs created per million dollars of federal spending) and the cost per job created. Similar to the local income multiplier, the employment multiplier is highest for counties displaying lower per capita growth. Inversely, the cost per job created is highest in the fastest growing counties, reaching over 1.5 million dollars per year for counties with the highest per capita growth rates. These low multiplier results would lead us to believe government spending has a modest impact across the distribution of outcomes and does relatively little to reduce the inequality in income and and employment across counties.

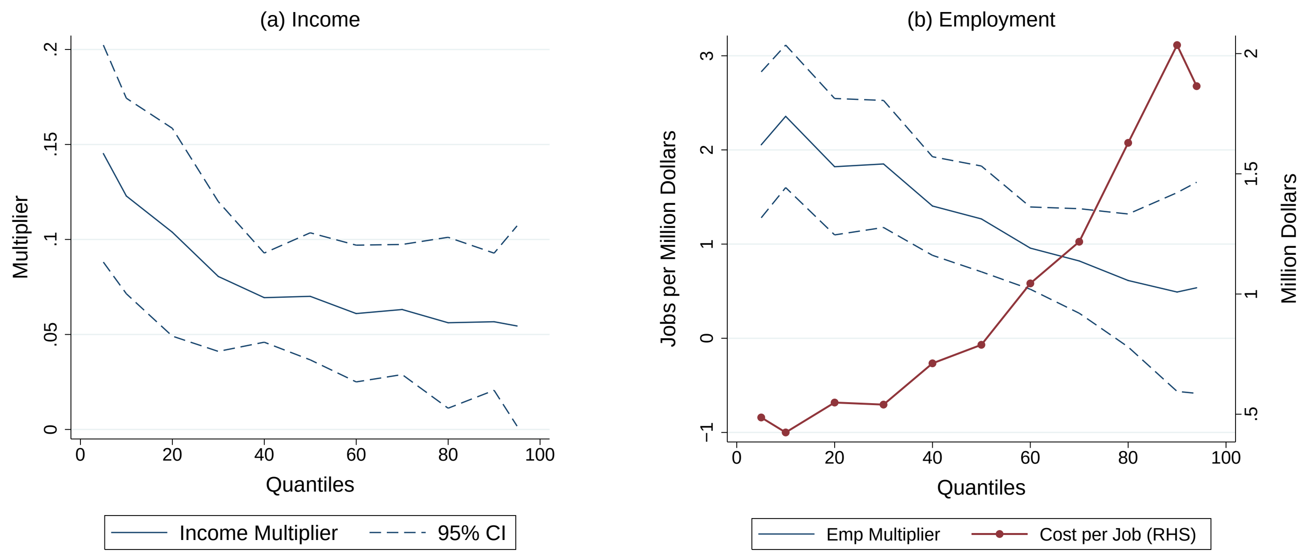
The IVQR we implement acknowledges the endogeneity of government spending and provides consistent estimates of the β^q's that are not subject to endogeneity bias. Consider the alternative quantile function

$$\tilde{Q}_q(\Delta y_{c,t}) = \alpha_t^q + \beta^q \Delta F_{c,t} + \gamma^q CS_{c,t} + \Gamma X_{c,t}, \tag{9}$$

where we add the county-level Census shock $CS_{c,t}$. The IVQR framework uses the insight that, at the true value of the structural parameter β^q, the instrumental variable will not influence the

[29]See Angrist and Pischke (2009) for a review of recent developments.

Figure 11: Quantile Regression Estimates of Spending Multipliers



Notes: The figure plots the estimated coefficient for federal spending from Equation (8) along with its 95% confidence interval for 11 quantiles of the distribution of the dependent variable. Panel (a) uses the average annual growth in local personal income per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Panel (b) uses the average annual growth in employment per capita. Panel (b) also reports the cost per job created in dollars of 2009 on the right hand side axis at the corresponding quantiles.

conditional quantile, so that $\gamma^q = 0$. To compute estimates of β^q, the IVQR framework finds values of β^q such that γ^q is as close to zero as possible. Distance from zero, in this context, is measured using the F-statistic for testing $\gamma^q = 0$.[30]

Figure 12 presents the result of these estimations for income and employment for 7 values of q. These figures confirm our previous findings that instrumental variable estimates suggest a much larger effect of government spending on local income and employment than do methods that do not account for the endogeneity of government spending.

[30] For a given quantile q, the algorithm used in the estimation is as follows

1. Use a grid search method to find the value of $\tilde{\beta}^q$ that minimizes the F-statistic for testing $\gamma^q = 0$. The F-statistic is computed by first fixing a value of $\tilde{\beta}^q$, estimating the quantile regression

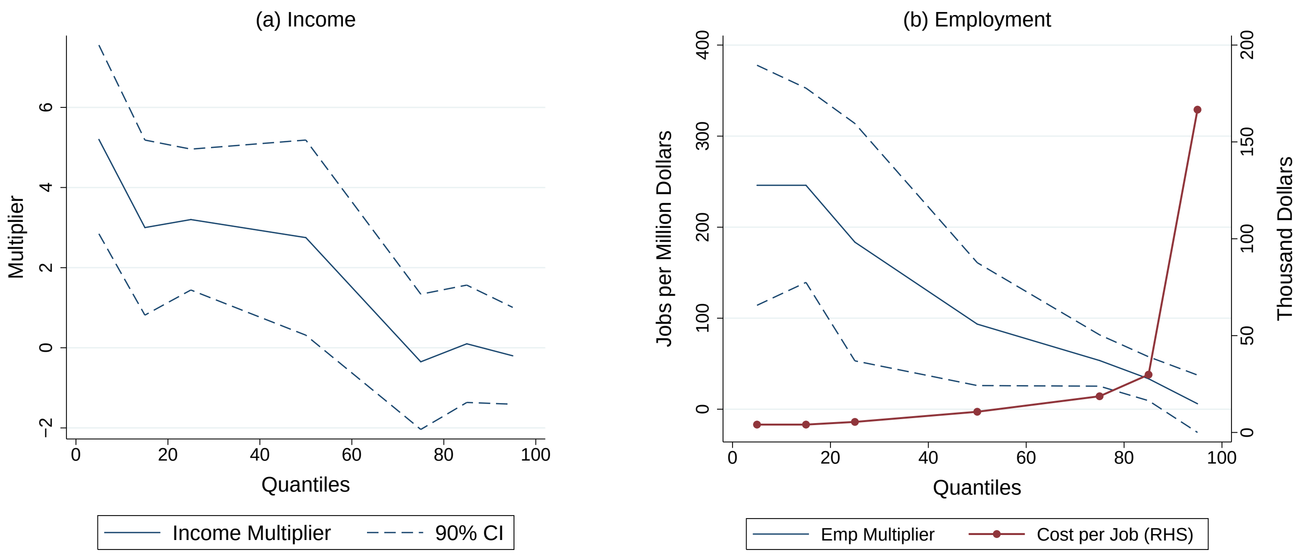
$$\tilde{Q}_q(\Delta y_{c,t}) = \alpha^q_{s,t} + \tilde{\beta}^q \Delta F_{c,t} + \gamma^q CS_{c,t} + \Gamma X_{c,t},$$

and testing $\gamma^q = 0$.

2. Confidence intervals and standard errors are computed using a paired-bootstrap of step 1 to account for intra-cluster correlation at the state level. The dual inference approach of Chernozhukov and Hansen (2008) was also implemented and yielded similar results.

Note that the inference procedure for the IVQR is robust to weak instruments. An important caveat, however, is that the results we estimate are consistent estimates of the structural parameters in Equation (8) only if the model is correctly specified. Alternative methods that are robust to model misspecification have been proposed by Chen and Pouzo (2009).

Figure 12: IVQR Estimates of Spending Multipliers



Notes: The figure plots the estimated coefficient for federal spending from Equation (9) along with its 95% confidence interval for7 quantiles of the distribution of the dependent variable. Panel (a) uses the average annual growth in local personal income per capita from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Panel (b) uses the average annual growth in employment per capita. Panel (b) also reports the cost per job created in dollars of 2009 on the right hand side axis at the corresponding quantiles.

These graphs further show that counties with lower income growth are more impacted by changes in government spending than counties with higher income growth. This differential effect can be interpreted either as a "redistributional effect," i.e. poor areas benefit more from federal spending, or as a "stabilizing effect." The latter highlights the view of fiscal federalism as providing insurance against local shocks. Because federal spending has such a large impact in low growth counties, it could be an effective way to help areas experiencing temporary negative shocks. Since we do not include dynamics in our analysis, we cannot differentiate between counties which are experiencing temporary shocks and those which are permanently better-off. Regardless of these interpretations, the downward-sloping profiles in Figure 12 (a) shows that increasing government spending not only raises income but also decreases inequality of income growth rates across counties.

The results for employment growth also indicate a higher multiplier at the lower end of the growth distribution, but with a spike for counties around the median. The estimates are similar to the IV estimates and are much larger than the quantile regression estimates. The cost per job created also varies widely across the distribution with very little federal spending required to create an additional job in very low growth counties and very high cost in the fastest growing counties.

8 Conclusion

Now several years into a slow recovery from the Great Recession, the impact of government spending on the economy is one of the most important policy questions we face. The federal government spent vast amounts of money in the hope of stimulating the economy, but many economists and policy analysts claim fiscal policy has a limited impact in the short term and cripples long term growth prospects. In this paper, we propose a new methodology to estimate critical parameters. We rely on cross-sectional instead of time-series variation and propose a new instrumental variable to identify the causal impact of federal spending. This new approach is a powerful yet transparent way to measure several important parameters such as the income multiplier, the cost per job created, and the inequality-reducing effect of government spending.

We find a large effect of government spending on local economic outcomes. The timing of the impact on both economic growth and federal spending is consistent with the release of the new Census counts and our estimates are robust to the inclusion of potential confounders, thereby strengthening the case for causal identification. We have shown that there are positive spillovers across counties and that government spending provides higher returns in depressed areas, which has contributed to reducing inequality in income and employment across counties.

Future work could focus on the interaction of federal spending with local business cycles, since recent papers have shown that the income multiplier might be larger during recessions (Auerbach and Gorodnichenko 2010, Christiano et al. 2009, Woodford 2010). It would also be of interest to document the dynamic relationship between the new measure of spending shocks and economic outcomes by using more flexible estimation specifications. This would make the current results more comparable to macroeconomic estimates of impulse response functions and would allow the estimation of the long term effects of fiscal shocks on local economies.

The instrument we introduce in this paper is also relevant for the field of urban and regional economics. The exogenous variation in government spending we propose constitutes a shock to local labor and housing markets that can be used to test general spatial equilibrium models where agents move across locations to benefit from higher wages or cheaper amenities (Roback 1982, Kline 2010). The empirical strategy we proposed can be used to further our understanding of agglomeration effects as well as migration, wages and housing price responses to government spending shocks. Such models can also be used to estimate the deadweight loss of federal spending as a place-based policy due to the potential distortions in the locational decisions of individuals (Glaeser and Gottlieb 2009, Glaeser 2008, Moretti 2010). We address some of these questions in a follow up paper on the incidence of federal government spending (Suárez Serrato and Wingender 2011).

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A Online Appendix Not For Publication

A.1 Data Sources

In order to construct the panel of county population and the instrument, we use postcensal population estimates published by the Census Bureau from 1971 to 2009. This distinction between postcensal and intercensal is important. The latter are retrospectively revised to account for the error of closure in Census years whereas the former are the contemporaneous estimates produced every year to tract population growth. Intercensal population estimates are not relevant for our study since federal spending only depends on the contemporaneous estimates. Most of the earlier data are archived at the Inter-University Consortium for Political and Social Research (ICPSR) (http://www.icpsr.umich.edu/). For the years 1971 to 1974, we use the *Population Estimates of Counties in the United States* (ICPSR 7500). For years 1975 to 1978, we use the data from the *Federal-State Cooperative Program: Population Estimates* study (ICPSR 7841 and 7843). No postcensal population estimates were published for 1979, 1980, 1989, 1990 and 2000. For 1981 to 1988, we use population data from the *County Statistics File 4* (CO-STAT 4) (ICPSR 9806). Data for Census years and from 1991 onward were taken directly from the Census Bureau's website (http://www.census.gov/popest/estimates.html) since the postcensal estimates are still available. Local and state population estimates are produced jointly by the Census Bureau and state agencies. The Federal-State Cooperative Program has produced the population estimates used for federal funds allocation and other official uses since 1972.

Birth data from Vital Statistics are taken from the micro data files available at the NBER (http://www.nber.org/data/) for the years 1970 to 1978. We use the Centers for Disease Control and Prevention's (CDC) *Compressed Mortality Files (*http://wonder.cdc.gov/) for years 1979 to 1988 and tables published in the Vital Statistics, *Live births by county of occurrence and place of residence* for years 1989 and 1990. Data for 1991 to 2009 are taken directly from the Census Bureau's components of growth data files available on the Census website. Data on county level deaths are taken from the NBER's *Compressed Mortality* micro data files from 1970 to 1988 and from the CDC's *Compressed Mortality* tabulated files from 1989 to 2006. County level deaths for 2007 to 2009 were taken directly from the Census Bureau's components of growth files.

Migration data come from the IRS Statistics of Income. Years 1978 to 1992 were taken from the *County-to-County, State-to-State, and County Income Study Files, 1978–1992* (ICPSR 2937) and *Population Migration Between Counties Based on Individual Income Tax Returns, 1982-1983* (ICPSR 8477). The most recent years are available directly from the IRS SOI's website (http://www.irs.gov/taxstats/).

Data on Federal spending were taken from the Census Bureau's *Consolidated Federal Funds Reports*. These reports have been produced annually since 1983 and provide a detailed account of the geographic distribution of federal expenditures. 1983 and 1984 data are available on CD-ROM from the Census Bureau and for downloading from the SUDOC Virtualization Project at the University of Indiana (http://www.cs.indiana.edu/svp/). Data from 1985 to 1992 are available for download individually by year at the ICPSR. The Census Bureau's website has CFFR releases from 1993 onwards. Data on federal spending prior to 1983 is available from the *Geographic Distribution of Federal Funds* for fiscal years 1981 and 1982 (ICPSR 6043 and 6064) and from the *Federal Outlays* dataset from 1976 to 1980 (ICPSR 6029). Note that debt servicing, international payments and security and intelligence spending are not covered in the CFFR. See Census Bureau (2010d) for further details.

The Industry share shifter variable was calculated using the Quarterly Census of Employment and Wages database produced by the Bureau of Labor Statistics. The Migration share shifter variable was calculated using Census tabulations from 1970 to 2000 on foreign-born population by country of birth. The tables were downloaded from the National Historical Geographic Information System at the University of Minnesota (http://www.nhgis.org/).

County-to-County Distance information was downloaded from the Oak Ridge National Laboratory (http://cta.ornl.gov/transnet/SkimTree.htm).

A.2 Variable Definitions

Census Shock	Log-difference between the Census count and the postcensal county population estimate in the year of the Census.
Postcensal population estimate	Annual population estimate derived by using the last Census count available and updated with annual administrative data to account for population growth such as number of births, deaths and migration from IRS tax return data.
Intercensal population estimate	Revised population estimate that is obtained by redistributing the error of closure (i.e difference between the Census count and the postcensal estimate) across all years of the previous decade. This variable is produced by the Census Bureau for the previous decade only once the final census counts are published. See Census Bureau for formula and details.

Federal Spending Per capita total annual federal spending by county as recorded by the Consolidated Federal Funds Report. This measure excludes *Direct Payments Other than for Individuals* and *Insurance and Guaranteed Loans*. Debt servicing, international payments and security and intelligence spending are also not covered in the CFFR. See main text for details. All variables expressed in per capita terms use intercensal population estimates provided by the BEA as the denominator.

Personal Income Total personal income per capita as reported by the BEA. Personal income is the sum of labor earnings, dividends, interests and rental income and personal transfer receipts.

Earnings Net earnings per capita by place of residence. This is computed by the BEA as earnings by place of work (the sum of wage and salary disbursements, supplements to wages and salaries, and proprietors' income) less contributions for government social insurance, plus an adjustment to convert earnings by place of work to a place-of-residence basis.

Employment Total employment per capita reported by the BEA. Total employment is the sum of full-time and part-time employment for both employees and sole proprietors.

Industry Share-Shifter Predicted annual employment growth by county using the weighted sum of national employment growth rates by industry (74 2-digit SIC categories until 1999 and 95 3-digit NAICS categories for 2000 to 2009). The county-specific weights are determined by the employment share of each industry by county in the base year. We include use as controls in our main regression the Industry Share Shifter in the Census year and the two previous years.

Migration Share-Shifter Predicted immigrant population growth computed in a similar way as the *Industry Share-Shifter*. The migration variable uses instead national changes in populations levels by country (or region) of birth across Censuses with the county-specific weights given by the share of immigrant populations by country of origin measured in the base Census year. This variable only has one distinct observation per decade.

A.3 State Government Spending

A potential concern with our results is that we only use variation in federal spending following the Census shock to estimate local spending multipliers. State and local government spending could also respond in a similar way to new information about local population levels and ommiting it could potential lead us to overestimate the effect of federal spending on local economic outcomes. However, it could also be the case that federal spending crowds-out spending by other levels of government, which would then lead us to underestimate the impact of government spending at the local level.[31]

The main reason we limit the analysis to federal spending is that there does not exist to our knowledge a comprehensive dataset that tracks state spending by local areas similar to the federal CFFR data. It is worth noting that the CFFR actually captures a significant share of state spending that consists of federal transfers passed through state governments. In the aggregate, this amounts to roughly one quarter of total state spending over the time period. Even though we don't have a comprehensive measure of state spending at the local level, we use below two sources of data to see whether partial measures of state spending do in fact respond to the Census shock.

In Table A.1, we look at the response of one particular type of government spending that is available for state and local governments at the county level. We use data on government salaries and wages from the BEA to see if we can detect a response in state spending to variation in the Census shock. We show in column (1) for reference the first stage coefficient for our Census shock variable in the regression using federal spending from the CFFR. The second column reports the coefficient for the Census shock in the regression using federal wages instead of total federal spending. The size of the point estimate is smaller and less precisely estimated than column (1). In column (3) we report the coefficient of the regression using salaries and wages from state governments. State wages do not appear to respond to the Census shock as the coefficient is very small and not statistically different from zero. The negative sign would also suggest some crowding out. Finally, column (4) shows the responses of local government wages. The point estimate is also close to zero and not statistically significant. Interestingly the last two point estimates are much smaller than the response of federal wages to the Census shock even though state and local wages are higher on aggregate and in the average county than federal wages.

Our second indirect test uses data from the Annual Survey of Governments (ASG). The ASG collects data annually from a sample of governments from all levels on various financial items. We use information on intergovernmental revenues of county and city governments to see if federal and state transfers to local governments vary in response to the Census shock.

[31] There is no concensus in the fiscal federalism literature on the crowding out effect of federal spending on state spending. Recent examples include Gordon (2004) and Knight (2002).

Table A.1: OLS Estimates of Census Shock on Spending and Wages

	(1) Federal Spending	(2) Federal Wages	(3) State Wages	(4) Local Wages
Census Shock	280.67***	61.00**	-12.84	-7.36
	(88.94)	(23.08)	(12.58)	(17.74)
Observations	9,204	9,196	8,789	8,789
R-squared	0.05	0.05	0.07	0.13

Notes: The table reports the estimated coefficients from Equation (4) using average annual growth in federal spending per capita from the CFFR in column (1), federal wages only from the BEA in column (2), state wages from the BEA in column (3) and local government wages from the BEA in column (4). Data for the dependent variables are from 1982 to 1985, 1992 to 1995 and 2002 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Column (1) in Table A.2 reports the coefficient on the Census shock using data from the CFFR as the dependent variable. In columns (2) and (3), we use respetively federal and state transfers to local governments. The point estimates become very small and insignificant. These results suggest that intergovernmental transfers to local governments do not respond to the Census shock and shouldn't therefore play a large role in identifying the total marginal impact of government spending changes in our estimation framework.

Table A.2: OLS Estimates of Census Shock on Spending and Transfers

	(1) Federal Spending	(2) Federal Transfers	(3) State Transfers
Census Shock	280.67***	3.89	2.31
	(88.94)	(5.25)	(9.49)
Observations	9,204	8,065	8,065
R-squared	0.05	0.07	0.28

Notes: The table reports the estimated coefficients from Equation (4) using average annual growth in federal spending per capita from the CFFR in column (1), federal transfers from the ASG in column (2) and state transfers from the ASG in column (3). Data for the dependent variables are from 1982 to 1985, 1992 to 1995 and 2002 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

We conclude from these results that we cannot find evidence that state and local government spending changes in any systematic way in response to the Census shock. The fact that there appears to be no correlation between the Census shocks and state spending could be due to two opposing effects: a direct effect through formula transfers and a crowding-out effect from increased federal spending.

A.4 Detailed Lists of the Largest Census Shocks

Table A.3: Largest Census Shock - 1980

	State	County	Population	Census shock
		Positive Shocks		
1	Maryland	St. Mary's	60,176	0.904
2	South Dakota	Jackson	3,399	0.794
3	Georgia	Chattahoochee	21,379	0.427
4	Nevada	White Pine	8,289	0.355
5	Texas	Shackelford	3,939	0.345
6	Texas	Hemphill	5,387	0.337
7	Oregon	Harney	8,232	0.328
8	Colorado	Gunnison	10,713	0.328
9	Washington	San Juan	7,899	0.325
10	Colorado	Ouray	1,944	0.316
11	Nevada	Eureka	1,219	0.288
12	Texas	Starr	27,666	0.278
13	Florida	Hernando	45,715	0.270
14	Oklahoma	Johnston	10,395	0.270
15	Colorado	Rio Blanco	6,349	0.237
16	Oklahoma	Wagoner	42,146	0.235
17	Florida	Monroe	63,721	0.229
18	North Carolina	Dare	13,523	0.224
19	Texas	Hood	17,859	0.223
20	Idaho	Elmore	21,685	0.221
21	Oregon	Sherman	2,171	0.215
22	Colorado	Summit	8,960	0.213
23	Texas	Live Oak	9,666	0.211
24	Texas	Maverick	31,738	0.207
25	Texas	Frio	13,791	0.204
		Negative Shocks		
1	Maryland	Somerset	19,131	-1.030
2	New Mexico	Valencia	61,128	-0.646
3	Colorado	Hinsdale	414	-0.360
4	Georgia	Peach	18,989	-0.317
5	Oregon	Wheeler	1,502	-0.279
6	Kansas	Geary	30,083	-0.273
7	Colorado	Park	5,419	-0.271
8	Idaho	Clark	798	-0.270
9	Nebraska	Gosper	2,136	-0.258
10	Utah	Daggett	777	-0.244
11	Montana	Treasure	987	-0.234
12	Texas	Culberson	3,333	-0.233
13	North Dakota	Divide	3,472	-0.225
14	South Dakota	Dewey	5,371	-0.210
15	South Dakota	Sully	1,978	-0.195
16	Colorado	Jackson	1,862	-0.190
17	Colorado	Mineral	819	-0.190
18	Texas	Oldham	2,287	-0.189
19	Nebraska	Hayes	1,348	-0.187
20	North Dakota	Steele	3,081	-0.186
21	New Mexico	De Baca	2,433	-0.185
22	Kansas	Hamilton	2,501	-0.184
23	Montana	Judith Basin	2,662	-0.184
24	Utah	Wayne	1,924	-0.176
25	South Dakota	Moody	6,681	-0.172

Table A.4: Largest Census Shock - 1990

	State	County	Population	Census shock
		Positive Shocks		
1	Nevada	Storey	2,535	0.254
2	Colorado	Crowley	3,946	0.210
3	Missouri	De Kalb	9,975	0.202
4	South Carolina	McCormick	8,876	0.196
5	Maryland	Somerset	23,469	0.189
6	Florida	Gilchrist	9,751	0.187
7	Texas	Kinney	3,130	0.170
8	Texas	Glasscock	1,443	0.163
9	Colorado	Eagle	22,297	0.163
10	Idaho	Camas	739	0.161
11	Georgia	Crawford	9,071	0.159
12	Illinois	Brown	5,851	0.158
13	Texas	Concho	3,084	0.156
14	Georgia	Liberty	52,906	0.155
15	Texas	Hudspeth	2,905	0.155
16	Florida	Liberty	5,594	0.150
17	Colorado	Douglas	61,670	0.149
18	Nevada	Lander	6,291	0.136
19	Arizona	Santa Cruz	29,854	0.133
20	Montana	Park	14,643	0.130
21	North Carolina	Onslow	150,098	0.126
22	Kansas	Riley	67,212	0.125
23	Idaho	Boise	3,568	0.125
24	Georgia	Camden	30,734	0.116
25	Kentucky	Carlisle	5,218	0.114
		Negative Shocks		
1	North Dakota	Mercer	9,754	-0.304
2	South Dakota	Shannon	9,937	-0.222
3	Nebraska	Hooker	799	-0.209
4	North Dakota	Slope	894	-0.196
5	Nebraska	Banner	860	-0.194
6	Texas	Mcmullen	816	-0.192
7	Colorado	San Juan	749	-0.191
8	Montana	Petroleum	519	-0.190
9	Oklahoma	Cimarron	3,294	-0.190
10	Texas	Oldham	2,273	-0.188
11	Oklahoma	Roger Mills	4,113	-0.182
12	Louisiana	Sabine	22,487	-0.173
13	North Dakota	Sioux	3,777	-0.169
14	North Dakota	Billings	1,090	-0.168
15	North Dakota	Mckenzie	6,348	-0.168
16	Louisiana	La Salle	13,621	-0.167
17	Montana	Golden Valley	911	-0.167
18	Utah	Rich	1,731	-0.166
19	Wyoming	Lincoln	12,710	-0.163
20	Kansas	Geary	30,558	-0.160
21	New Mexico	McKinley	61,414	-0.160
22	South Dakota	Jackson	2,796	-0.160
23	North Dakota	Sheridan	2,131	-0.158
24	Montana	Rosebud	10,473	-0.157
25	Mississippi	Issaquena	1,923	-0.155

Table A.5: Largest Census Shock - 2000

	State	County	Population	Census shock
		Positive Shocks		
1	Georgia	Echols	3,782	0.376
2	Mississippi	Issaquena	2,258	0.344
3	Nevada	Pershing	6,672	0.342
4	Texas	Concho	3,963	0.285
5	Texas	Dickens	2,724	0.277
6	Florida	De Soto	32,196	0.265
7	Florida	Hardee	26,769	0.248
8	Georgia	Wheeler	6,174	0.244
9	Georgia	Calhoun	6,325	0.240
10	Wyoming	Teton	18,381	0.211
11	Utah	Daggett	926	0.204
12	Colorado	Crowley	5,509	0.203
13	South Carolina	Edgefield	24,586	0.201
14	Texas	Llano	17,077	0.201
15	Texas	Live Oak	12,233	0.200
16	Florida	Hendry	36,255	0.199
17	Colorado	Lake	7,815	0.198
18	California	Mono	12,921	0.196
19	New Mexico	Catron	3,567	0.195
20	Florida	Sumter	53,738	0.193
21	New Mexico	Sierra	13,209	0.190
22	Idaho	Boise	6,702	0.186
23	Florida	Glades	10,579	0.185
24	Georgia	Crawford	12,408	0.182
25	Colorado	San Miguel	6,609	0.174
		Negative Shocks		
1	Texas	Edwards	2,143	-0.530
2	Texas	Loving	65	-0.523
3	Texas	Polk	41,539	-0.269
4	Texas	Presidio	7,355	-0.210
5	North Dakota	Billings	876	-0.182
6	North Dakota	Slope	760	-0.153
7	Nevada	Esmeralda	978	-0.149
8	Nebraska	Logan	773	-0.147
9	Tennessee	Fayette	29,083	-0.133
10	Texas	Reagan	3,290	-0.129
11	Georgia	Chattahoochee	15,047	-0.129
12	Nebraska	Thomas	733	-0.126
13	Kentucky	Meade	28,189	-0.119
14	Nevada	Lander	5,702	-0.111
15	Montana	Prairie	1,179	-0.109
16	Missouri	Wright	17,926	-0.109
17	Texas	Jeff Davis	2,233	-0.105
18	Idaho	Power	7,484	-0.105
19	Kentucky	Owsley	4,852	-0.102
20	Nevada	Eureka	1,632	-0.098
21	Michigan	Ionia	61,712	-0.097
22	Wyoming	Niobrara	2,396	-0.096
23	Virginia	Rappahannock	6,980	-0.096
24	West Virginia	Mingo	28,007	-0.094
25	Illinois	Pope	4,411	-0.093

A.5 Replication of the Census Methodology

Table A.6: Components of Population Growth Calibration

	(1) 1982-1988	(2) 1991-1999
Births	1.83***	1.45***
	(0.16)	(0.08)
Deaths	-2.03***	-1.23***
	(0.37)	(0.20)
Net Migration	0.76***	1.03***
	(0.13)	(0.06)
Observations	12,312	27,684
R-squared	0.78	0.91

Notes: The table reports the estimated coefficients from the calibration regression given by Equation (2). Column (1) regresses biennial county population growth from 1982 to 1988 on the number of births, deaths and net migration in the previous two years. Column (2) regresses annual population growth on annual number births and deaths, and net migration from 1991 to 1999. Standard errors clustered at the state level in parentheses. * p<0.1, ** p<0.05, *** p<0.01

A.6 Log-Difference Data

The following tables report the effects of federal government spending on local outcomes using log-differenced data instead of per capita changes directly in levels. The first stage regression is given by the following equation:

$$\Delta ln(F_{c,t}) = \eta_{s,t} + \gamma CS_{c,t} + \Pi X_{c,t} + e_{c,t}, \tag{A.10}$$

where $\Delta ln(F_{c,t})$ is the average annual log change in federal spending per capita from year 1 to year 5 in county c and decade t, $\eta_{s,t}$ are state-by-year fixed effects, $CS_{c,t}$ values of the Census shock by decade t and $X_{c,t}$ the full set of control variables as in Table 7 but expressed here in log per capita change. Similarly the second stage is now expressed in terms of log changes in income and employment per capita as a function of log changes in federal spending per capita:

$$\Delta ln(y_{c,t}) = \alpha_{s,t} + \beta \Delta ln(F_{c,t}) + \Gamma X_{c,t} + \varepsilon_{c,t}, \tag{A.11}$$

Table A.7: First Stage Elasticity Estimates

	(1)	(2)	(3)	(4)
Census Shock	0.079***	0.082***	0.077***	0.080***
	(0.023)	(0.024)	(0.021)	(0.023)
Past Income Growth		0.117		0.075
		(0.181)		(0.191)
Past Earnings Growth		-0.085		-0.062
		(0.082)		(0.086)
Past Employment Growth		0.061*		0.071**
		(0.035)		(0.032)
Industry Share Shifter			0.437***	0.442***
			(0.150)	(0.154)
Migration Share Shifter			-0.014	-0.013
			(0.012)	(0.013)
Observations	9,204	9,204	9,204	9,204
R-squared	0.09	0.09	0.09	0.09
F-Stat Instr	12.18	11.81	12.97	12.27

Notes: The table reports the estimated coefficients from Equation (A.10) The dependent variable is the log-difference in federal spending per capita from 1981 to 1985, 1991 to 1995 and 2001 to 2005. The table reports the F-Statistic of a Wald test that the Census shock coefficient is equal to zero. All regressions include state-decade fixed effects. Standard errors clustered at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.8: IV Estimates of the Elasticity of Local Income

	(1)	(2)	(3)	(4)
Federal Spending	0.213**	0.227**	0.204**	0.217**
	(0.099)	(0.092)	(0.103)	(0.097)
Past Income Growth		0.044		0.029
		(0.083)		(0.080)
Past Earnings Growth		-0.094		-0.086
		(0.061)		(0.059)
Past Employment Growth		0.001		0.005
		(0.012)		(0.012)
Industry Share Shifter			0.186***	0.183***
			(0.071)	(0.066)
Migration Share Shifter			-0.006	-0.004
			(0.004)	(0.004)
Observations	9,204	9,204	9,204	9,204

Notes: The table reports the estimated coefficients from Equation (A.11) with log-changes in federal spending per capita instrumented by the Census shock. The dependent variable is the log-difference in local personal income per capita from 1981 to 1985, 1991 to 1995 and 2001 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Since these results use log-differenced data, the estimated coefficients are elasticities and must be transformed to recover local fiscal multipliers. Fiscal multipliers are usually understood to measure the marginal effect on economic outcomes of a one dollar change in federal spending. Given the definition of the elasticity, we multiply the coefficient β from Equation A.11 by the ratio of federal spending to total income by county to recover the marginal effect of federal spending on total county income. We report in Table A.9 the local fiscal multiplier on personal income evaluated at the mean and various quantiles of the distribution of this ratio. The table also reports our main estimate of the local fiscal multiplier using data in levels.

Table A.9: Local Income Multiplier Estimates

Log Estimation				Level Est.
p25	p50	p75	Mean	
1.59	2.18	2.96	2.44	1.57

Notes: The local income multiplier is the product of the coefficient for Federal Spending from Table A.8 and the ratio of total county income to federal spending. The local income multiplier is given for various quantiles and the mean value of this ratio across all counties in the sample. The multiplier from the Level Estimation is taken from Table 7.

Table A.10: IV Estimates of the Elasticity of Local Employment

	(1)	(2)	(3)	(4)
Federal Spending	0.312***	0.287***	0.306***	0.279***
	(0.086)	(0.082)	(0.086)	(0.082)
Past Income Growth		-0.001		-0.015
		(0.055)		(0.056)
Past Earnings Growth		0.010		0.017
		(0.023)		(0.023)
Past Employment Growth		-0.075		-0.072
		(0.051)		(0.049)
Industry Share Shifter			0.142**	0.147**
			(0.069)	(0.065)
Migration Share Shifter			-0.004	-0.005
			(0.005)	(0.005)
Observations	9,204	9,204	9,204	9,204

Notes: The table reports the estimated coefficients from Equation (A.11) with log-changes in federal spending per capita instrumented by the Census shock. The dependent variable is the log-difference in employment per capita from 1981 to 1985, 1991 to 1995 and 2001 to 2005. All regressions include state-decade fixed effects. Standard errors clustered at the at the state level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.11: Estimates of the Cost per Job

Log Estimation				Level Est.
p25	p50	p75	Mean	
26,256	35,995	48,808	40,364	29,796

Notes: The *Cost per Job* is obtained by dividing the ratio of federal spending to total county employment by the employment multiplier from Table A.10 and is expressed in 2009 dollars. The *Cost per Job* is given for various quantiles and the mean value of the distribution of this ratio across all counties in the sample. The *Cost per Job* from the Level Estimation is taken from Table 9.

A.7 Additional Figures

Figure A.1: Reduced Form Estimates on Income Per Capita

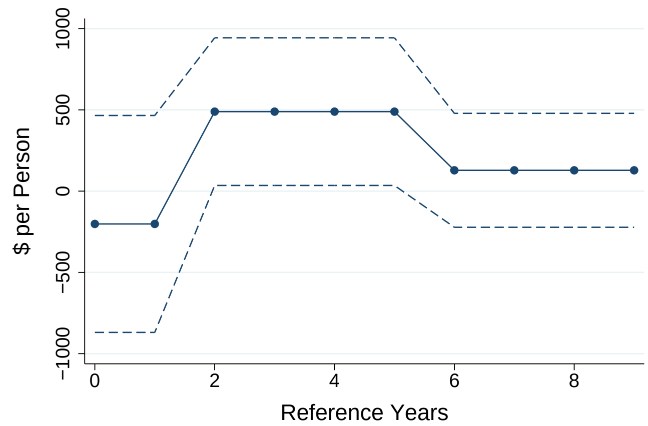


Figure A.2: Reduced Form Estimates on Earnings Per Capita

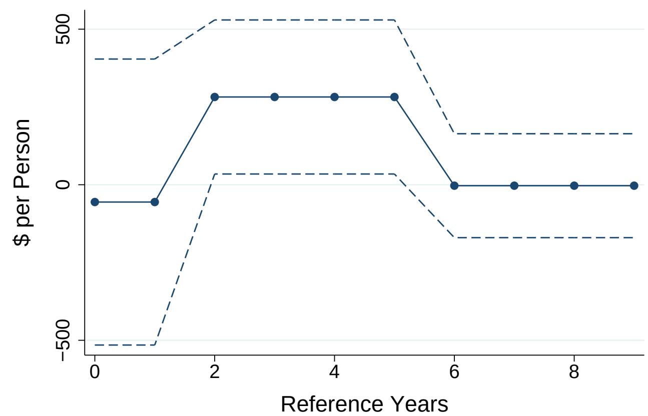
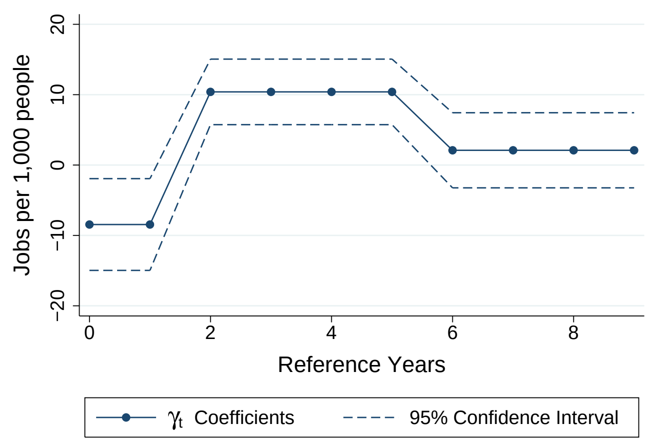
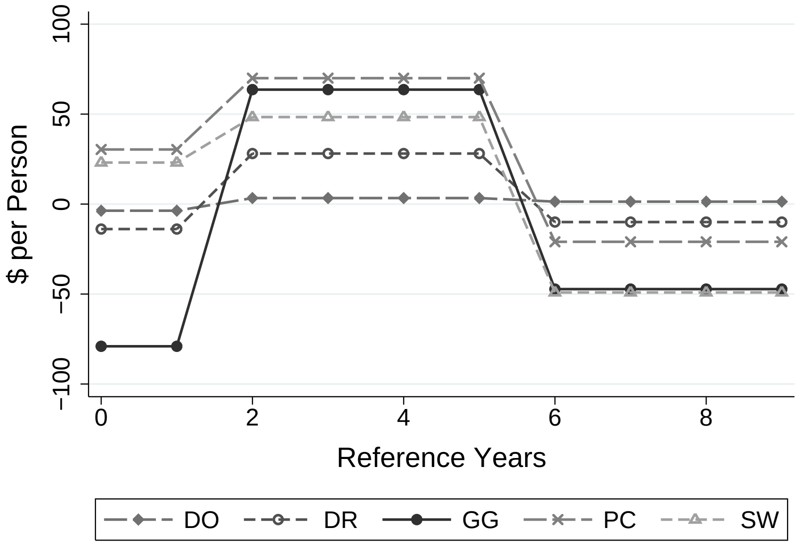


Figure A.3: Reduced Form Estimates on Employment Per Capita



Notes: The figures plots the same estimated coefficients from the reduced form equations by year as in Figure 10 along with their 95% confidence intervals. The coefficients are estimated and reported jointly for years 0 and 1, 2 to 5 and 6 to 9.

Figure A.4: First Stage Estimate by Spending Category



Notes: The figure plots the estimated first stage coefficients from Equation (4) separately by spending category along with their 95% confidence interval. The point estimates are estimated and reported jointly for years 0 and 1, 2 to 5 and 6 to 9. DO is Direct Payments to Individuals, DR is Retirement Payments, GG is grants, PC is Procurement and Contracts and SW is Salaries and Wages.