Estimating Local Fiscal Multipliers*

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Abstract

We propose a new source of cross-sectional variation that may identify causal impacts of government spending on the economy. 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 baseline results follow a treatment-effects framework where we estimate the effect of a Census Shock on federal spending, income, and employment growth by re-weighting the data based on an estimated propensity score that depends on lagged economic outcomes and observed economic shocks. Our estimates imply a local income multiplier of government spending between 1.7 and 2, and a cost per job of $30,000 per year. A complementary IV estimation strategy yields similar estimates. We also explore the potential for spillover effects across neighboring counties but we do not find evidence of sizable spillovers. Finally, we test for heterogeneous effects of government spending and find that federal spending has larger impacts in low-growth areas.

Keywords: Government spending, fiscal multiplier, Census Shock.

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The impact of government spending on the economy is 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 bias naïve 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 shock that may be used to estimate causal effects of 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 of Population and Housing (henceforth "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 causal effects of changes 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 a shock that can be used to analyze the impact of government spending on other outcomes as well.

We begin by documenting several desirable properties of the Census Shock that make it an interesting source of variation. We show that, in many cases, the errors in population measurement are large and lead to economically significant changes in federal spending. This variation leads to a strong statistical relationship between federal spending and the Census Shock. This is consistent with the fact that a large number of federal spending programs use local population levels to allocate spending across areas.[2] We also document the fact that it takes two years after the Census is conducted for the Census Shock to affect spending and that it takes several years for different agencies in the federal government to update the population levels used for determining spending. These dynamics generate the testable prediction that spending and economic growth should not respond to the Census Shock until two years after the Census is conducted. In addition, they imply the Census Shock may affect spending growth over several years, even though the Census Shock occurs once every decade. Finally, we also show that the shock is not geographically or serially correlated.

While these properties motivate the Census Shock as a source of identifying variation for govern-

[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.

[2] This dependence operates either through formula-based grants using population as an input or through eligibility thresholds in transfers to individuals and families. 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.

ment spending, a key concern is that the errors in population measurement may be correlated with trends in economic growth that may confound the effect of changes in government spending. We deal with this concern by adopting a treatment-effects framework following in the steps of Angrist and Kuersteiner (2010) and Acemoglu et al. (2014). The identifying assumption behind this approach is one of selection on observables which, in our case, correspond to lagged economic outcomes. This approach amounts to semi-parametrically adjusting the data to recover the treatment effect of a dichotomous version of the Census Shock on spending, income, and employment growth.[3] We implement this approach by estimating a propensity score that relates lagged economic outcomes and observable economic shocks to the likelihood of a Census Shock. This approach is semi-parametric since only the model for the propensity score needs to be specified. One benefit of this approach is that it places no restrictions on the growth dynamics following a Census Shock and thus retains testable implications of the identification strategy. In particular, we test and confirm the predictions that the Census Shock should have no effect on growth in years prior to Census which are not used to generate the propensity score, as well as on years after the Census but before the release of the Census counts.

We use this semi-parametric approach to produce causal estimates of a Census Shock on spending, income, and employment growth over the three years following the release of a Census Shock. We first use the inverse propensity score weights (IPW) of Hirano et al. (2003) to estimate statistically and economically significant effects of a Census Shock that imply a local income multiplier between 1.7 and 2. We find that an additional \$1 million of federal spending increases employment by close to 33 jobs, which implies a cost per job created of close to \$30,000. As in Acemoglu et al. (2014), we also employ a hybrid model that combines IPW with regression adjustment (IPWRA). This estimator has the "doubly-robust" property and results in consistent estimates of treatment effects when either the propensity score or the regression adjustment is properly specified (Wooldridge, 2010, §21.3.4). Our estimates of the reduced-form effects of a Census Shock as well as the implied local fiscal multipliers are robust to using IPW and IPWRA estimators across a range of specifications that control for different levels of fixed effects, lagged economic outcomes, and other observable shocks.

We also explore the dynamics of the Census Shock by estimating event studies for several years before and after a given Census year. These event studies show that a Census Shock is not predictive of past economic growth, but is predictive of future economic growth and has stable predictions across IPW and IPWRA versions of these specifications. We compare our treatment-effects estimates with IV estimates of fiscal multipliers that instrument federal spending with the continuous Census Shock. The IV strategy yields similar estimates to the treatment effects strategy. It is also robust to controlling for lagged economic growth or the propensity score used in our main specification. Both the treatment-effect estimates and the IV estimates imply a return to government spending at the local level that is more than ten times larger than the corresponding OLS estimates. This shows that failing to account for the endogeneity of federal spending leads to a large downward bias due to obvious concerns about endogeneity and reverse causality.[4]

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

[3]For the remaining of the paper we refer to a Census Shock both as an increase in population estimates, in the case of the continuous shock, as well as a positive shock, in the case of the dichotomous shock. See Sections 3.1 for the definition of the continuous shock and Section 3.5 for the case of the binary shock.

[4]For example, some categories of government spending are automatic stabilizers so that spending increases when the local economy experiences a slowdown. An alternative interpretation of this bias could be attenuation due to measurement error in government spending.

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 (2010) 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 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 recipient states or regions, their reduced-form estimates also reflect changes in tax liabilities. Consequently, their multiplier estimate for income growth is lower and around 0.8 at the annual level.[5]

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 analyze a broader set of issues relating to spillover effects and to characterize heterogeneous 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. Nonetheless, it is worth pointing out the striking similarity between our local fiscal multipliers estimates and those found in several of these studies, especially considering the differences in the sources of variation, samples, and estimation models.

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), Barro and Redlick (2011), and Ramey (2011)). This approach has many advantages. Foremost, it allows us to clearly identify the source of 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

[5] Nakamura and Steinsson (2014) and Werning and Farhi (2012) examine how the source of financing, whether federal or local, affects the multiplier.

show that government spending decreases income growth inequality across counties suggesting that automatic stabilizers play an important role in insuring counties from idiosyncratic shocks.

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.[6] 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. In particular, estimates of local fiscal multipliers can inform policy makers on the tradeoffs of using federal transfers to smooth regional business cycles.

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 operates differently in the recipient and neighboring counties, our estimates at the local level could be overestimating the larger regional impact of government spending. While we find negative spillover estimates, these effects are small and we cannot reject the null of no spillovers.[7]

The following section describes institutional details including the statutory link between spending and population estimates, as well as the challenges inherent in measuring population at the local level. Section 2 describes the data used in the study. Section 3 defines the Census Shock, discusses several statistical properties, and introduces a treatment-effects framework for estimating causal effects of a Census Shock. Sections 4 presents semi-parametric treatment effects of a Census Shock, an event-study analysis that explores the dynamics of the Census Shock, and a complementary IV strategy to estimating local fiscal multipliers. Section 5 measures the spillovers of federal spending across neighboring counties while Section 6 analyzes heterogeneity in the impact of government spending. Finally, we conclude in Section 7.

1 Measurement of Population Levels and Federal Spending

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 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 (Long, 1993).

A crucial feature of these estimates is that they are "reset" to Census counts once these data become available after a new Census is conducted. The difference between the two population measures in Census years is called the "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.

[6] See Nakamura and Steinsson (2014) and Werning and Farhi (2012) for detailed discussions.

[7] 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.

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 (U.S. Census Bureau, 2010c). 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, averaging 0.6% across all three Censuses. Census Shocks in urban counties were positive and larger than those experienced by rural counties, due to the fact that rural counties experienced more negative population shocks. For example, Census enumerations consistently found fewer people than the contemporaneous administrative estimates. However, in absolute values, rural counties had larger shocks in every Census. Counties in the South experienced the largest shocks, on average, while those in the Northeast listed the smallest shocks. Midwestern counties' Census Shocks were consistently below the sample average.

Figure 1 illustrates the average county population growth rate across all counties by year. The series shows clear breaks in 1980, 1990 and 2000. Figure 2 presents the full distribution of county population growth rates for 1999 and 2000 separately. The figure demonstrates 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. These figures show that updating population estimates with new Census counts generates a large amount of cross-sectional variation.

The shock 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 a shock to federal spending, it is necessary to determine to 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 Census Shock would identify counties that have grown more than expected in the past decade and are likely to keep growing relatively more in the future. As we argue below, the variation in the Census Shock is likely to come not only from the mismeasurement of population flows, but also from the mismeasurement of population stocks during Census enumerations.

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

1.1 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, and 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. For example, in addition to clerical errors it is believed that linking enumerators' pay to the number of households interviewed may have contributed to duplicated enumerations in 1980 (Lavin, 1996). 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." Despite recommendations by the National Academy of Sciences and the Census Bureau to use statistical techniques to adjust Census counts for known misreporting, the Supreme Court in 1999 sided with the United States House of Representatives against the Department of Commerce to ban their use in calculating the population for purposes of apportionment (Rosenthal, 2000).

Conducting the U.S. Census is a relatively rare, technically challenging, and costly endeavor. Unlike other Anglophone 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 using a comprehensive electronic mapping system in 1990. A continuously updated master address file was only introduced following the Census 2000. Such a master file is 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. Mohammed Shahidullah (2005), Starsinic (1983)), although others have also found mixed evidence (Murdock and 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, home-schooled children and 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, political views of respondents that might make them reluctant to be included in a Census enumeration, etc. (Robinson et al. (2002), Rosenthal (2000), Boscoe and Miller (2004), Judson et al. (2001), 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 (Steffey and Bradburn, 1994).

1.2 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), Louis et al. (2003), Zaslavsky and Schirm (2002), Larcinese et al. (2013)). 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 (U.S. Census Bureau (2010a,b) and U.S. Census Bureau (2001)). 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.[9] 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 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.

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 (e.g., GAO (1999), GAO (2006), GAO (2009), and Louis et al. (2003)). For instance, GAO (2006) finds that relatively small differences (about 0.5%) in the national error of closure in 2000 led 22 states to obtain additional $200 million dollars of funding and 17 states to obtain a deficit of $368 million. Similarly, GAO (2009) simulated changes in population of about 3.2 percent and found that states where population was underestimated would lose $363.2 million, while states with overestimates would gain $377.0 million in federal funding. Other studies have also found similar estimates (Murray

[9] Per capita income depends on population estimates only through the denominator. Zaslavsky and Schirm (2002) explore the role of non-linearities in interactions between population estimates and federal spending. They also note that formulae features, including thresholds or hold-harmless clauses, may amplify the noise of estimated formulae inputs, such as population, and lead to large effects on the allocation of spending.

(1992),GAO (1999)). This issue was also addressed by a National Research Council panel in Louis et al. (2003) that focused on statistical problems in implementing the allocation of various formula programs. The panel of experts concluded that, indeed, the statistical measurement and differential adoption of population estimates across agencies would generate mismatches in the funding across localities. These studies, along with the evidence of population mis-measurement in the previous section, show that errors in population measurement may induce a substantial amount of variation in federal spending.

2 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.[10]

We use contemporaneous county population estimates published by the Census Bureau from 1970 to 2009. These are called postcensal estimates.[11] 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.[12] 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 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 and 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 for multi-year disbursements, we use a three year moving average of total spending in

[10] We exclude Hawaii and Alaska since the county governments play an outsized role, in the case of the former, and since county boundaries are not stable during our sample period, in the case of the latter.

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

[12] 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.

these categories.[13] Panel (a) in Figure 5 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 and employment are taken from the Bureau of Economic Analysis' Regional Economic Information System (REIS). These data are compiled from a variety of administrative sources. Employment and earnings mainly come from the Quarterly Census of Employment and Wages (QCEW) produced by the Bureau of Labor Statistics (BLS). 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, as well as supplements to salaries and wages) uses IRS, Social Security Administration and state unemployment agencies data among other sources.

While these data come mainly from administrative data sources, certain sub-items are allocated at the county level using information from surveys and Census data (Bureau of Economic Analysis (2010)). This could potentially lead to a mechanical correlation between the Census Shock and the dependent variables. To minimize this concern, we focus only on the components of personal income that are the least dependent on these adjustments. Our measure of personal income therefore includes only private non-farm earnings and dividends, interest, and rent.[14] Similarly, for employment, we only consider private non-farm jobs. Across the county-year observations in our sample, we find that farm jobs and income constitute 1% of all private jobs and 4% of all private income. Similarly, public-sector jobs and income represent 10% of all jobs and 15% of total income. We explore the robustness of our results on alternative data sources directly from the QCEW and the IRS Statistics of Income. The employment measure in the QCEW comes from unemployment insurance programs. We use the number of tax filers as a proxy for local employment in the case of the IRS data.

All dollar values are expressed in 2009 dollars using the national Consumer Price Index published by the BLS. Finally, in order to make these data comparable across counties, we normalize income and employment changes by constant population in 1980, the beginning of our sample. Since our source of variation uses changes in population estimates, this normalization ensures that the identifying variation only comes from changes in economic growth.[15] Appendix A contains additional details on data sources and variable definitions.

3 The Census Shock

This section formally defines the Census Shock, explores its statistical properties, documents the lack of a relation between the Census Shock and state spending, and describes a treatment-effects approach to analyzing the variation from the shock.

[13] We discuss the robustness of our results to restricting spending categories to exclude salaries and wages, and procurements and contracts, as well as to different approaches to dealing with outliers in spending data in Section 4.

[14] Our personal income measure also excludes personal transfers, place-of-residence adjustment, and contributions for government social insurance.

[15] An earlier, working paper version of this paper, analyzed outcomes normalized by concurrent population and obtained similar estimates (Suárez Serrato and Wingender, 2014a, Version: March 30).

3.1 Defining the Census Shock

To implement our empirical strategy, we need both Census counts and concurrent population estimates. The Census Bureau 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:

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

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).The components of population change are taken from 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.

This procedure gives us estimated population growth rates from which we can extrapolate population levels in Census years. For the 2000 Census, we calibrate the components of population change identity across counties using population growth during the 1990s. We then use the estimated level of population 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 $\widehat{Pop_{c,\text{Census}}^{PC}}$. We then define the Census Shock as:[17]

$$CS_{c,\text{Census}} = \log(Pop_{c,\text{Census}}^{C}) - \log(\widehat{Pop_{c,\text{Census}}^{PC}}).$$

3.2 Properties of the Census Shock

We now document some statistical properties of the Census Shock that make it an interesting source of variation for measuring the effects of government spending on local economic growth.

First, we note that the Census Shock may lead to large changes in local population estimates. Figure 1 shows that, even at the national level, the error of closure can be substantial. The problem of population counting and updating is exacerbated in smaller geographic areas. While most counties see small revisions, we find that counties in the 25th percentile of the distribution see a downwards revision of 2.5%, while counties in the 75th percentile see an upward revision of 3.3% percent. Similarly, moving a county from a 10th percentile to the 90th percentile implies a change in estimated population of 11.8%.

Second, 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

[16] 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. The Census Bureau did not publish postcensal estimates for 1979 and 1989. The results of the calibration regressions by decade are reported in Table E.1.

[17] Tables E.2-E.4 report the counties with the largest Census Shocks in every decade. Alternative methods of estimating the counterfactual postcensal population estimates, including a raw sum of the components of change (i.e. $\Delta Pop_{c,t}^{PC} = Births_{c,t} - Deaths_{c,t} + Migration_{c,t}$) and using an AR(3) time series model, produce similar estimates and do not alter our main results.

Shock is related to a region-wide shock that might also explain the outcomes of interest. 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 shock appears to be at the county level or below and not driven by region-wide economic shocks.[18]

A third 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 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 Census Shock is serially correlated. Figure 4 presents the scatter plots of the Census Shocks across decades. These plots demonstrate that there is 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 shock. 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.

3.3 Census Shock and State Spending

The statutory formulas described in Section 1.2 motivate the Census Shock as a driver of federal spending. However, a potential concern in analyzing the effects of a Census Shock is that other levels of government spending might also respond to the Census Shock in a way that would confound the effects of changes in federal spending. Unfortunately, analyzing the effect of the Census Shock on state spending is complicated by the lack of state-level spending data that is comparable to the CFFR.

In Appendix B, however, we perform two sets of analyses that explore whether state spending responds to the Census Shock. First, we consider the effects of the Census Shock on government wages for different levels of government as measured by the BEA. We find that, while the Census Shock leads to increases in federal wages, state and local wages are not affected by the Census Shock. In a second indirect test, we use data from the Annual Survey of Governments to analyze whether intergovernmental transfers respond to the Census Shock. We again find that state transfers to local governments are not responsive to the shock. These analyses suggest that our analysis on federal spending is not likely to be confounded by reaction of state spending to the Census Shock.

3.4 A Treatment Effects Strategy to Analyzing the Census Shock

Despite the properties described above, a 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 a large Census Shock and would likely maintain higher-growth rates in the future.

[18] In Section 5 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.

These local shocks could therefore confound our interpretation of the results as the "true" effect of government spending on local growth.

We address this concern by casting the Census Shock in a treatment-effects framework where potential correlations between the Census Shock and lagged economic outcomes are indicative of a problem of selection on observables.[19] We then use variants of the propensity score methods of Rosenbaum and Rubin (1983) to estimate causal effects of the Census Shock. In particular, we use the semi-parametric approach of Angrist and Kuersteiner (2010) and Angrist et al. (2013) to estimate causal effects of the Census Shock on spending, income, and employment growth.

We first cast our setting in the potential outcomes framework of Rubin (1974), following the notation in Acemoglu et al. (2014). Consider a binary version of the Census Shock where $CS_{c,t} = 1$ implies an upward revision in population estimates.[20] For a given value of the Census Shock $d \in \{0, 1\}$ and a given outcome variable $Y_{c,t}$, define the potential outcomes $Y^s_{c,t}(d)$ for year $t + s$ in county c. Similarly, define the potential growth in $Y_{c,t}$ between years $t + s$ and t as:

$$\Delta Y^s_{c,t}(d) = Y^s_{c,t}(d) - Y_{c,t}.$$

The causal effect of a Census Shock on the growth of a given outcome $Y_{c,t}$ is given by

$$\beta^s_Y = \mathbb{E}[\Delta Y^s_{c,t}(1)] - \mathbb{E}[\Delta Y^s_{c,t}(0)].$$

If the Census Shock were a perfectly randomized shock, we may recover estimates of the causal effect by comparing the means of counties with and without a Census Shock.[21]

In practice, however, the Census Shock may not be perfectly randomized, raising the concern that a simple comparison of means will not yield a causal effect due to the potential of selection bias. We address this concern with two complementary approaches. First, we follow the semi-parametric framework of Angrist and Kuersteiner (2010) and Angrist et al. (2013) and estimate a propensity score model where the Census Shock may depend on lagged growth in income and employment. We then weight the data by the inverse of the propensity score (IPW) and estimate treatment effects as the mean difference of the suitably-reweighted data. This strategy has the benefit that the relation between lagged outcomes and the causal effects is left unspecified. This approach shifts the modeling from focusing on outcomes to focusing on the variation in the Census Shock. As a second strategy, we follow Acemoglu et al. (2014) in employing a "doubly-robust" estimator that combines regression adjustment (RA) with inverse-propensity score weighting (IPW) by implementing the estimator described in Wooldridge (2010, IPWRA, §21.3.4). This approach has the benefit that, as long as either the regression adjustment model or the propensity score model are correctly specified, the IPWRA model will deliver consistent estimates of causal treatment effects.

Before presenting the implementation details of each of these models, we discuss the assumptions that are common to both models. As our analysis focuses on three outcomes—Federal Spending

[19] A previous version of this paper (Suárez Serrato and Wingender, 2014a, Version March 30) discusses identification of the Census Shock in a model where the measurement error results in a perfectly randomized shock. We now discuss that model in Appendix C.

[20] We simplify our analysis by analyzing the binary version of the Census Shock. Hirano and Imbens (2004) study continuous treatments and Imbens (2000) and Cattaneo (2010) study multi-valued treatment effects. We follow the methodology in Cattaneo et al. (2013) to explore the potential for spillover effects in Section 5.

[21] This follows since:

$$\mathbb{E}\left[\Delta Y^s_{c,t} | D_{c,t} = 1\right] - \mathbb{E}\left[\Delta Y^s_{c,t} | D_{c,t} = 0\right] = \mathbb{E}\left[\Delta Y^s_{c,t}(1) | D_{c,t} = 1\right] - \mathbb{E}\left[\Delta Y^s_{c,t}(0) | D_{c,t} = 0\right] = \mathbb{E}\left[\Delta Y^s_{c,t}(1) - \Delta Y^s_{c,t}(0)\right].$$

($F_{c,t}$), Employment ($Emp_{c,t}$), and Income ($Inc_{c,t}$)—our selection on observables assumption takes the following form:

Assumption 1 Selection on observables: $\Delta Y^s_{c,t}(d) \perp D_{c,t} | \chi_{c,t}, \mathbb{I}\{\text{State}\}_{c,t}, \mathbb{I}\{\text{Year}\}_{c,t} \ \ \forall s \geq 2$ *and*

- *where* $\chi_{c,t} \subseteq \{\Delta Y^{t+1}_{c,t-1}, \Delta Y_{c,t-3}, \text{Industry Shifter}_{c,t}, \text{Migration Shifter}_{c,t}\}$,
- *for* $Y_{c,t} = F_{c,t}, Emp_{c,t},$ *and* $Inc_{c,t}$,
- *for* $t = 1980, 1990, 2000$, *and* $\forall\ c$.

Our assumption of conditional independence applies to each of our three outcomes and any year $s \geq 2$ following the release of the Census Shock. t is restricted to the three Census years in our sample. The set of observables includes state and year effects, lagged values of our outcomes at two points in time prior to the release of the Census Shock, an observable industry share-shift variable proposed by Bartik (1991), and a migration share-shift variable due to Card (2001).[22] Our preferred specification includes year and state fixed effects but we also present results showing that state-by-year fixed effects result in similar estimates. This assumption allows for the Census Shock to be correlated with past economic growth but presumes that, conditional on the observables $\chi_{c,t}$, the Census Shock is "as good as randomly assigned."

We also make a second assumption that is standard in the analysis of treatment effects:

Assumption 2 Overlap: $0 < \mathbb{P}[d_{c,t} = 1 | \chi_{c,t}] < 1$.

Intuitively, this assumption states that, for any value of $\chi_{c,t}$, there is a non-zero probability that we may observe counties with and without a Census Shock. We discuss the plausibility of this assumption in the next section as we describe the estimated propensity scores.

We now discuss our implementation of the IPW and IPWRA estimators. In a first step, we estimate the probability of having a Census Shock conditional on $\chi_{c,t}$ and year fixed effects, which results in an estimated propensity score $\hat{P}_{c,t}$.[23] As in Acemoglu et al. (2014), we focus on the treatment effect on the treated, and we use $\hat{P}_{c,t}$ to compute the efficient weights of Hirano et al. (2003):[24]

$$\hat{w}_{c,t} = \frac{1}{\widehat{\mathbb{E}}[D_{c,t}]} \left(\mathbb{I}\{D_{c,t} = 1\} - \mathbb{I}\{D_{c,t} = 0\} \frac{\hat{P}_{c,t}}{1 - \hat{P}_{c,t}} \right).$$

Finally, we obtain IPW estimates of the treatment effects of a Census Shock by comparing the means of reweighted data:

$$\hat{\beta}^s_Y = \widehat{\mathbb{E}}[\hat{w}_{c,t} \cdot \Delta Y^s_{c,t}].$$

[22] The industry share-shift 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. The migration share-shift variable has an analogous construction and is meant to capture a specific source of population growth due to a supply shock from immigration. The variable is constructed by 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.

[23] In practice, we use a logit model to estimate the propensity score. Section 3.5 discusses the estimation results and Section 4.1 discusses robustness of our main results to using a probit model for the propensity score.

[24] We focus on estimating the average treatment effect on the treated as it relies on less restrictive assumptions for identification. In addition, the resulting estimates are a more relevant policy guide for counties that are affected by this source of variation. Nonetheless, Section 4 discusses estimates of average treatment effects and shows that we obtain a similar pattern of results.

To implement the IPWRA model, we use $\hat{w}_{c,t}$ to estimate a weighted linear regression of $\Delta Y^s_{c,t}$ on covariates $X_{c,t}$, including year and state fixed effects. We estimate this regression separately by treatment status to recover parameters $(\hat{\alpha}^s_i, \hat{\Gamma}^s_i)$, where $\hat{\alpha}^s_i$ is the weighted mean for treatment group i, and where $\hat{\Gamma}^s_i$ are the coefficients on $X_{c,t}$.[25] The IPWRA estimate of the causal effect of a Census Shock on a given outcome is now:[26]

$$\hat{\beta}^s_Y = \widehat{\mathbb{E}}[(\hat{\alpha}^s_1 + X'_{c,t}\hat{\Gamma}^s_1) - (\hat{\alpha}^s_0 + X'_{c,t}\hat{\Gamma}^s_0)].$$

It follows from this expression that the IPWRA model recovers the IPW estimate in the case where $X_{c,t}$ is empty.

We interpret the causal estimate of a Census Shock on federal spending, β^s_F, as a "first stage," and the effects on employment and income, β^s_{Emp} and β^s_{Inc}, respectively, as reduced-form effects. We also report estimates of the local income multiplier, $\frac{\beta^s_{Inc}}{\beta^s_F}$, and the cost per job created, $\frac{\beta^s_F}{\beta^s_{Emp}}$, in order to normalize the reduced-form effects by a dollar unit. This interpretation belies an assumption that other policies are not directly affected by the Census Shock, which we formalize below.

Assumption 3 Policy Exclusion: *A Census Shock does not directly affect policies other than federal spending.*

Assumption 3 is testable whenever such policies are observable. As discussed in Section 3.3, and in more detail in Appendix B, we find no direct effects of a Census Shock on state spending. We also find this assumption plausible for other policies that may vary at the county or city level. To the best of our knowledge, there has been no evidence of the effects of population mis-measurement on other such policies. In particular, a National Research Council panel reviewed statistical issues related to population mis-measurement and found no such links (Louis et al., 2003). In contrast, as discussed in Section 1.2, errors in population measurement have received considerable attention in debates over federal spending. Note that the interpretation of our results as local fiscal multipliers would not be confounded by changes in state and local policies that respond to changes in federal spending.

3.5 Estimated Propensity Scores and Diagnostic Tests

This section implements the treatment-effects framework described in the previous section, presents evidence of balance with respect to past economic growth, and shows evidence that the overlap assumption is not violated.

We first generate a binary version of the Census Shock in order to implement the treatment-effects framework of Section 3.4. We begin by normalizing the Census Shock by the mean shock for every state in a given decade. This normalization is justified by statutory rules that rely on changes in both state and county-level population estimates (Louis et al., 2003). We then assign treatment status to county-year observations where the Census Shock is in the top 50% of the distribution, and we assign the bottom 50% of the observations to the control group.[27]

[25] That is, $(\hat{\alpha}^s_i, \hat{\Gamma}^s_i) = \arg\min_{\alpha^s_i, \Gamma^s_i} \sum_{c,t}(d_{c,t} - (1 - d_{c,t}))\hat{w}_{c,t}(\Delta Y^s_{c,t} - \alpha^s_i - X'_{c,t}\Gamma^s_i)^2$, for $i = 0, 1$.

[26] See Wooldridge (2010, §21.3.4) for the proof that the estimator following this procedure possesses the "doubly-robust" property. The IPW and IPWRA estimates may be implemented with the command `teffects` in Stata. In practice, we use a custom command to implement these estimators in order to jointly bootstrap the propensity score and the treatment effects of a Census Shock on multiple outcomes in order to perform inference on ratio of treatment effects, which we interpret as multipliers. We confirm that our command produces numerically identical estimates to those computed via `teffects`.

[27] Our results are robust to the choice of discretization, as we discuss below and in Section 4.

We then estimate propensity score models of the binary Census Shock. Tables E.5 and E.6 present results of logit parameters and marginal effects, respectively. We find that the lagged measures of income and employment growth are statistically significant predictors of having a Census Shock, raising the concern of selection bias. Following Angrist and Kuersteiner (2010), Angrist et al. (2013), and Acemoglu et al. (2014), we generate a propensity score, as the probability of having a Census Shock, that depends on these measures of past economic growth. Figure E.13 shows evidence that the overlap assumption (Assumption 2) is likely to hold as the estimated propensity scores have similar distributions and there are no values close to zero or unity.

We now show that the IPW and IPWRA models successfully balance the measures of past growth with respect to the Census Shock. Table 1 presents estimates of a Census Shock on six measures of past growth. Standard errors are obtained via 2000 bootstrap repetitions and allow for arbitrary correlations at the state level.[28] Column (1) presents estimates from a model without IPW that only controls for year and state fixed effects. This column shows that, as mentioned in Section 3.4, the Census Shock is not perfectly randomized with respect to measures of past economic growth. Columns (2)-(5) introduce the IPW and IPWRA estimators with different models of regression adjustment. As can be seen, even the simplest IPW estimator in column (2) results in economically small and statistically insignificant relations between a Census Shock and past measures of economic growth. The fact that the Census Shock is not related to income or employment growth in years $(-1, 1)$ and $(-3, 0)$ is a meaningful diagnostic result, illustrating that the IPW and IPWRA models are able to produce balance with respect to the information used in estimation. Moreover, the fact that Table 1 shows balance of the Census Shock with respect to economic growth starting five years before a Census is conducted (and seven years before the release of the Census Shock), is evidence that selection on observables (Assumption 1) is a valid working assumption, as this variable was not used in the construction of the propensity score or as part of the regression adjustment. We show further evidence that the IPW and IPWRA models balance past growth with respect to the Census Shock in Section 4.2, where we discuss the results of event-study analyses.

4 Estimates of Local Fiscal Multipliers

This section presents our main estimates. We first report treatment effects of a Census Shock on spending, income, and employment growth, and use these effects to construct estimates of fiscal multipliers. We then explore the dynamics of these effects in an event-study framework. Finally, we present a complementary analysis where we use the continuous version of the Census Shock as an instrument for federal spending.

4.1 Semi-parametric Treatment Effects and Implied Local Fiscal Multipliers

Our first set of results reports semi-parametric estimates of the causal effect of a Census Shock on spending, income, and employment growth over a three year period following the release of the Census

[28] We follow the procedure in Andrews and Buchinsky (2000) as implemented by Poi (2004) in selecting the number of bootstrap repetitions. This procedure indicates that 1550 repetitions are sufficient for there to be a 99% chance that the estimated standard errors will be within 5% of the standard errors when the number of repetitions is infinity. At 2000 repetitions, this probability is 99.7% and the chance that the estimated standard errors will be within 2.5% of the standard errors at infinity replications is 85.7%.

Shock.[29] Table 2 presents IPW and IPWRA estimates of these effects. Our preferred specification in column (3) suggests that having a Census Shock increases employment by about $1 (SE = 0.4)$ job per 1000 people. We also find that income per person increases by \$56$(SE = 26)$. When we compare these estimates to the estimated increase in spending of \$30$(SE = 13)$ per person, we find an implied estimate of the income multiplier of $1.86 (SE = 1.12)$ and a cost per job created of \$30,785$(SE = 16,694)$. The estimates of local fiscal multipliers are stable across the specifications in columns (1)-(4) that vary the degree of regression adjustment, including state-by-year fixed effects. We perform inference on the implied multipliers using two complementary approaches. First, we report standard-errors from a delta-method calculation. Second, we report the 90% confidence interval of the bootstrapped samples using the percentile method. We also use the bootstrapped samples to calculate the p-value of a one-sided test that the multipliers is not positive, which we reject at the 5%-level across all specifications. Both inference approaches allow for arbitrary correlation at the state level.

Notice that combining the income multiplier and the cost per job leads to an implied income for the marginal worker that is close to the national median income. In particular, using estimates from column (3), we could posit that a job created would have a total remuneration of $1.86 * \$30,785 \approx \$57,250$, which is slightly above the national median income. This calculation implies that the cost per job created is the share of the total remuneration 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.

We obtain a better grasp of the variation behind these estimates by considering the total spending growth for an average county. Given an average population of 62,183 in the beginning of our sample, the estimate on spending growth implies a total increase of \$5.6 million over a three year period. This suggests that the Census Shock may elicit economically substantial variation in spending that may precisely estimate local fiscal multipliers. Additionally, if we consider that the average county in the control group saw an under-estimate of 2.7% and the average county in the treatment group saw on over-estimate of 3.6%, we find that an additional estimated person results in about \$476 in additional federal spending per year.[30] This calculation is surprisingly close to that of a GAO report that reviewed the 15 largest formula grant programs for fiscal year 1997, and which found that federal spending would increase by \$480 per additional person (GAO (1999)). While the GAO estimate does not encompass all of our estimation period, it is reassuring that our estimates are of the same order of magnitude as this analysis of the largest statutory formulas.

We explore the robustness of these results in several dimensions. First, Table E.7 shows that these results are robust to using a probit model for the propensity score. Second, we explore the robustness to the discretization of the shock. In Table E.8, we present estimates similar to Table 2 but where a Census Shock is defined as being in the top 40% of the distribution of shocks, relative to counties in the bottom 40% of the distribution of shocks. As these shocks represent larger differences between treatment and control groups, we find larger treatment effects on employment, income, and spending growth. However, these effects are stable across specifications and result in very similar implied multipliers. Third, Table E.9 shows that the analysis of average treatment effects results in similar estimates of both treatment effects and implied multipliers. Fourth, we explore the robustness

[29]That is, we estimate $\beta_Y^{5-2}/3$, for a given outcome Y. In the case of employment, we normalize the coefficient to represent the increase in jobs per 1000 people.

[30]This calculation comes from taking ratio of the total dollar increase in spending ($\$29.984 \times 62,183$) to the change in estimated number of people for the average county ($62,183 \times (3.6\% - (-2.7\%))$).

of these results to using different data sources for economic outcomes. Table E.10 shows that using data from tax returns aggregated at the county level results in similar estimates.[31] We also report employment effects and the implied cost per job created using employment measures based on data from unemployment insurance systems as reported in the QCEW series of the BLS in Table E.11, which result in similar estimates of employment effects and the cost per job created.[32] Fifth, we explore the robustness of our estimates with respect to outliers at the state-by-year level. We first conduct a jackknife analysis where we re-estimate Table 2 by iteratively removing counties in each state-year group and analyze which state-year groups have large effects on our estimates. We exclude counties in state-year groups that lead to an average percentage change of more than 10% in the estimation (14 state-year groups in total). Table E.12 reports estimates on this subsample and finds very similar implied multipliers. Finally, we also explore the robustness of these results to various definitions of spending. Table E.13 shows that alternative definitions of the spending variable result in similar, though slightly less precise, estimates. Similarly, Table E.14 shows that most of the "first-stage" effect is driven by increases in grants, while salaries and wages and procurement contracts contribute a relatively small fraction of the effect.

4.2 Dynamic Effects of a Census Shock

We now explore the dynamic effects of a Census Shock and test whether they are 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 should not 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 suggests that the change in population due to the Census Shock should affect spending for several years after the new Census counts are released.

Figure 6 presents the results of two event studies where we plot the cumulative effects of a Census Shock on spending growth and shows that the dynamics effects of the Census Shock align with the expected timing from statutory formulas. Specifically, this figure reports estimates of β_F^s for $s = -6, \cdots, 6$ along with 90% confidence intervals. Panel (a) of Figure 6 plots estimates of an IPW model that does not control for lagged economic outcomes, similar to the estimates of column (1) of Table 2.[33] This plot confirms the prediction that a Census Shock should not affect spending growth prior to the release of the shock. In particular, we see that the estimates prior to year 3 show no trend and are all statistically insignificant. In contrast, we see a marked increase starting in year 3. Panel (b) plots estimates from an IPWRA model that also controls for spending growth between years -2 and 2 and shows a similar pattern of results. The point estimates used to construct Panel (a) are presented in Table 3, column (1) and those for Panel (b) are reported in Table 4, column (1). Figure 6 provides strong evidence in favor of the specific timing of our natural experiment, which supports

[31] These data are only available starting 1989. When using the IRS data, our measure of employment is the number of tax filers. We use our main BEA data for the first decade of our sample and combine the IRS data by analyzing fitted values of a regression of IRS data on income and employment changes on BEA data.

[32] This table also reports estimates of the earnings multiplier, which center around 1(.9). Note that our main measure of income also includes capital income.

[33] Note that data on federal spending are only available starting in 1977. The estimate for years -6 to -4 are estimated from the 1990 and 2000 Censuses, which explains the anomalous pattern in the confidence intervals during these years.

the notion that the effects of a Census Shock on employment and income are a consequence of the increase in federal spending.

Figures 7 and 8 present the results of a similar set of analyses for our two measures of economic growth. In both cases, we see that, prior to the release of the Census Shock, employment and income growth have flat trends that are statistically insignificant. We also see that both income and employment start growing following the release of the Census Shock, which matches the pattern of the dynamic effects on federal spending reported in Figure 6. These results hold for both sets of panels, suggesting that controlling for lagged outcomes does not significantly alter the effects of a Census Shock on economic growth. Columns (2) and (3) in Tables 3 and 4 report the estimates used to produce these figures. We note that, while the figures plot 90% confidence intervals, the cumulative effects on income and employment growth are statistically significant at the 5%-level by year 5.

These event studies imply local fiscal multipliers that are similar to those reported in Section 4.1. Since we only observe an increase in spending after year 3, we divide the cumulative increase in employment and income by the average increase in federal spending in years 2-5.[34] Figure 9 presents estimates of the income multiplier and Figure 10 presents estimates of the number of jobs created per $1 million. We first note that the implied income multiplier and job effect are very close to zero and are statistically insignificant prior to year 2. Starting in year 3, the income multiplier varies for different years and centers around our previous estimate of 2. Similarly, the employment effect hovers around 25-35 jobs per $1 million, which implies a cost per job that is close to our central estimate of $30,000.

We also explore the robustness of these event-studies. We note that the same pattern of results holds for a wider window of years. Figures E.1-E.5 and Tables E.15-E.16 report results of similar analyses where we estimate effects for years $s = -9, \cdots, 9$. These results suggest that our semi-parametric treatment-effect approach yields a balanced Census Shock with respect to lagged spending growth as well as economic outcomes. We also find that there are long-term effects of changes in federal spending, which we analyze in Suárez Serrato and Wingender (2014b). Additionally, Figures E.6-E.10 and Tables E.17-E.18 show that we obtain similar results when we estimate average treatment effects.

4.3 IV Estimates

This section presents a complementary analysis where we use the continuous version of the Census Shock as an instrument for changes in federal spending. This approach is relatively simpler than the treatment-effects framework and results in very similar estimates of local fiscal multipliers. The congruence between these results is reassuring and suggests that the Census Shock may be used in other estimation approaches to analyze spillover and heterogeneous effects of federal spending.

As in Section 4.1, we restrict our analysis to reference years 2 through 5. We estimate linear models of the form:

$$\Delta Y_{c,t} = \alpha_s + \gamma_t + \beta \Delta F_{c,t} + X'_{c,t}\Gamma + \varepsilon_{c,t},$$

where $\Delta Y_{c,t}$ is the average annual growth in income and employment over years 2 to 5 as a function of $\Delta F_{c,t}$, the average growth in federal spending over the same period.

[34] That is, for a given year s, we compute: $\frac{\hat{\beta}^s_Y}{|s-2| \times \hat{\beta}^{5-2}_F/3}$, where the average effect on spending growth is estimated jointly with the dynamic effects on employment and income. Note that since the event-studies on spending, income, and employment growth are not jointly estimated, we do not compare the coefficients across the columns of Tables 3 and 4.

As discussed in Section 3.5, the Census Shock is not perfectly randomized. For this reason, $X_{c,t}$ includes lagged values of income and employment growth, the observed local demand and supply shocks, and the propensity score estimated in Section 3.5. There is a long tradition of controlling for the propensity score (see, e.g., Angrist (1997) and Wooldridge (2010, §21.3.3)) and, as we show below, we obtain very similar estimates when we control for the propensity score or its determinants. We also include state and year fixed effects and explore the robustness of our results to allowing for state-by-year fixed effects. Finally, we allow for arbitrary correlation of the error term $\varepsilon_{c,t}$ at the state level across counties and decades. When Y is personal income, we interpret the coefficient on federal spending β as the local income multiplier. In the case of employment, β is normalized to represent the number of jobs per $1 million. We also report the implied cost per job created.

The exclusion restriction for our IV analysis is:

Assumption 4 IV Exclusion Restriction: $\mathbb{E}[\varepsilon_{c,t}|CS_{c,t}, X_{c,t}, \hat{P}_{c,t}, \mathbb{I}\{\text{State}\}_{c,t}, \mathbb{I}\{\text{Year}\}_{c,t}] = 0 \ \forall \ c$ *and for* $t = 1982, 1992, 2002$.

This assumption is not unlike Assumption 1. Assumption 4 also allows for a correlation between the Census Shock and lagged economic growth but places the restriction that conditional on these measures, the shock should not be related to unobservable factors that may also affect the outcome. However, in contrast to the treatment-effects approach, this strategy has the drawback that it places a linearity restriction on the relation between lagged economic growth and future economic growth.

We now turn to our instrumental variables results. Table 5 shows the first stage results. Column (1) only includes the Census Shock and the fixed effects. The coefficient implies that a 1% Census Shock increases federal spending by $511. 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 et al. (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. The F-statistics are above conventional levels, suggesting that our instrument is not subject to a weak instrument problem.[35] Adding past income growth and the share shift variables has a relatively small impact on both the estimated coefficient and its standard error. We also obtain similar estimates when we control for the propensity score in column (4) or when we include state-by-year fixed effects in the estimation.

Table 6 presents estimates of the local income multiplier. As would be expected from the results in Section 4.1, controlling for lagged outcomes or for the propensity score results in slightly smaller estimates than those without controls. Our preferred estimates in columns (3)-(5) present estimates between 1.9 and 2.2. We present the IV estimates for employment in Table 7. We again focus on the estimates in columns (3)-(5), where we find an employment response of 27-33 new jobs per $1 million, corresponding to a cost per job in the range of $30,000-$36,000. Both of these results are robust to including state-by-year fixed effects.[36] We explore the robustness of these results in Tables E.22 and E.23, where we control for longer lags and include interactions between the propensity score and variables in $X_{c,t}$, as suggested in (Wooldridge, 2010, Eqn. 21.52), and find very similar estimates.

It is reassuring to note that the IV estimates are similar to the implied multipliers from Section 4.1. In contrast, these estimates are more than ten times larger than the corresponding OLS estimates

[35]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 (2010)).

[36]We also report reduced-form effects of the Census Shock on income and employment growth in Tables E.20 and E.21. These tables show that the reduced-form effects are also quite stable across the different specifications.

and are statistically different from them in all cases.[37] The direction of the bias in the OLS estimates suggests that federal spending is directed to counties experiencing low growth.

5 Spillovers

This section explores the degree to which fiscal shocks have externalities across local areas. Depending on the sign of these spillovers, one could under- or over-estimate 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. Such a negative spillover effect might lead to an overestimate of the total impact of federal spending at a regional level.[38]

In order to characterize spillover effects, we analyze pairs of neighboring counties and ask whether a Census Shock leads to similar effects whenever the neighboring county also has a Census Shock. We first conduct this analysis within a treatment-effects framework, by generalizing the approach from Section 3.4 to a multivalued setting where the treatment values correspond to combinations of a county's own treatment status and a given neighbor's treatment status. In a second set of analyses, we generalize the linear IV model from Section 4.3 by including the covariates of a given neighbor. This is equivalent to a spatial average that gives equal weight to neighboring counties and zero weight to other counties.[39]

In both cases, we duplicate our sample to include each pairwise combination between a county and its neighbors and we use two different definitions of neighbors. 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).[40]

We now generalize our treatment-effects framework to the multivalued case. We follow Cattaneo et al. (2013) in the implementation of the estimator in Cattaneo (2010). First, define the multivalued

[37] Tables 6-7 show that the p-values of Hausman tests of equality between the OLS and IV estimates are always below 0.05. Tables E.24-E.25 report the results from the OLS regressions for income and employment growth. The OLS estimates are statistically significant but of small economic magnitude. For instance, they imply that an additional dollar increases income by 15 cents for every additional federal dollar spent in the county. In the case of employment, the OLS estimates imply that an additional job would cost around $400,000 dollars, an implausibly large magnitude.

[38] 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.

[39] We discuss the connection between the repeated-pairs approach and this spatial average in Appendix D. We present Monte Carlo evidence showing that these approaches result in similar estimates but that the repeated-pairs approach is less likely to be subject to weak instrument concerns. We also present results using a spatial-average approach that weights other counties' shocks by the inverse distance between the counties and that results in similar estimates.

[40] 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 make a degrees-of-freedom correction to the variance matrix and we weight the IV regressions by the inverse of the number of times the observation was duplicated. This allows us to recover the same baseline estimate and standard errors as in the original sample.

treatment indicator:

$$D_{c,t} = \begin{cases} 0 & \text{No Census Shocks} \\ 1 & \text{Only Neighbor has Census Shock} \\ 2 & \text{Only Own Census Shock} \\ 3 & \text{Both Census Shocks} \end{cases}.$$

As in Section 3.4, for a treatment group $d \in 0, 1, 2, 3$, the potential outcome for a given outcome is $Y^s_{c,t}(d)$ for year $t + s$, and the potential growth is $\Delta^s Y_{c,t}(d)$. Define as P_j the generalized propensity score of Imbens (2000) for a given treatment level j. Intuitively, given an estimate $\hat{P}_j$, we may estimate the mean potential outcome for treatment level status j, $\mu^s_{Y,j}$, by IPW:[41]

$$\hat{\mu}^s_{Y,j} = \widehat{\mathbb{E}}\left[\frac{\mathbb{I}\{D_{c,t} = j\}\Delta^s Y_{c,t}}{\hat{P}_j}\right].$$

We may then estimate the effect of treatment level j relative to 0, $\hat{\mu}^s_{Y,i} - \hat{\mu}^s_{Y,0}$, and compare these effects across treatment levels. In particular, we estimate the causal effect of treatment levels 2 and 3, relative to the baseline level 0 of no Census Shocks, and ask whether these two effects are different. In the case of positive spillovers, we would expect to see a larger treatment effect of level 3 but in the case of negative spillovers, we would expect to see a larger effect for level 2.

We first estimate the generalized propensity scores using a multinomial logit model and report the estimates and marginal effects in Tables E.26 and E.27 for the case of the nearest 10 neighbors, and in Tables E.28 and E.29 for counties in the same MSA. Figures E.14 and E.15 plot the estimated propensity scores and provide evidence in favor of the overlap assumption.

Table 8 reports estimates of treatment effects of level j relative to level 0.[42] Comparing the effects on income and employment growth, we see that, while the closest 10 specification finds larger effects in treatment level 3, the MSA specification finds larger effects in treatment level 2. Table 9 reports implied multipliers and cost per job created. We find slightly larger income multipliers and jobs per \$1 million in treatment level 3 for the case of the closest 10 counties and almost identical effects for the case of the MSA specification. While this evidence is suggestive of negative spillovers in the case of the closest 10 counties, we note that the difference between these two estimates is economically small, and not statistically significant.

Consider now the IV approach where we estimate the following spillover equation via 2SLS:

$$\Delta Y_{\tilde{c},t} = \alpha_s + \gamma_t + \beta \Delta F_{\tilde{c},t} + \beta^n \Delta F^n_{\tilde{c},t} + \Gamma X_{\tilde{c},t} + \Gamma^n X^n_{\tilde{c},t} + \varepsilon_{\tilde{c},t},$$

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. The first two columns show 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. In the

[41]For brevity, we only define the estimator below and refer the reader to Cattaneo et al. (2013) for the generalizations of Assumptions 1 and 2. In practice, we use an efficient influence function (EIF) estimator that also possesses the "doubly-robust" property (Cattaneo et al., 2013).

[42]We report the estimated means of potential outcomes in Table E.30.

last two columns, 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. Once again, the Census Shocks do not explain spending changes across neighboring counties.

We present the IV estimates for income growth in Table 11. 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 small and statistically insignificant effect. Combining both effects, we find a sum of multipliers around 1.8, which is within the range of our previous estimates. Table 12 presents the estimates for employment. We once again find a similar although slightly smaller own employment multiplier to the one estimated in our baseline IV regressions. The impact of federal spending changes in neighboring counties on employment is also negative and statistically insignificant. The cost per job created (calculated as the inverse of the sum of the two local employment multipliers) is now slightly larger.

We explore the robustness of these regressions to using a spatial-average approach that weights spending in other counties by the inverse of the distance between the counties. We report these results in Tables D.2 and D.3, which also present small and statistically insignificant spillovers. While we consistently find evidence of negative spillover effects, these effects are small and statistically insignificant. These results inform the mechanism behind the local effects of government spending, as we do not find evidence that local fiscal multipliers are a consequence of shifting of economic activity across neighboring counties.

6 Heterogeneity

Our main estimates from Section 4 show that government spending has large impacts on the conditional means of income and employment growth across counties. 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. We employ the continuous version of the Census Shock to estimate heterogeneous impacts of government spending using the instrumental variable quantile regression (IVQR) approach of Chernozhukov and Hansen (2008). This approach can reveal whether government spending is more impactful in faster- or slower-growing counties. Additionally, it may answer the question of whether such spending may reduce inequality in economic outcomes across counties.[43]

The IVQR we implement acknowledges the endogeneity of government spending and leverages the Census Shock to provide consistent estimates of the β^q's that are not subject to endogeneity bias. For a given quantile q of the outcome distribution of $\Delta Y_{c,t}$, consider the quantile function

$$Q_q(\Delta Y_{c,t}) = \alpha_t^q + \beta^q \Delta F_{c,t} + \gamma^q CS_{c,t} + \Gamma^q X_{c,t},$$

with α_t^q decade fixed effects, $\Delta F_{c,t}$ the per capita change in federal spending and county covariates $X_{c,t}$. As in Section 4.3, we focus on growth between years 2 and 5. We do not include state fixed effects as we are interested in comparing counties relative to the national distribution. Including state fixed

[43] We also consider an alternative quantile treatment-effects approach with the discretized Census Shock. These results show that a Census Shock has the largest effects on employment and income growth in lower growth counties. However, in contrast to the IVQR approach, it is not possible to compute income and job multipliers by quantile.

effects would change the interpretation of the results by limiting the comparison to counties within the same state.

If $\Delta F_{c,t}$ varied exogenously, we could estimate β^q with a standard quantile regression by setting $\gamma^q = 0$. The IVQR framework uses the insight that, at the true value of the structural parameter β^q, the Census Shock will not influence the 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 the test that $\gamma^q = 0$.[44]

Figure 11 presents the results of these estimations for income and employment growth and for seven values of q. These graphs show that counties with lower income and employment growth are more impacted by changes in government spending. 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 11 show that increasing government spending not only raises income but also decreases inequality of economic growth across counties. Figure E.16 shows that this result is robust to including state fixed effects and to controlling for the propensity score of the binary Census Shock. By way of comparison, Figure E.17 presents quantile regression estimates that do not account for the endogeneity of government spending and finds much smaller effects. These estimates also have a reversed pattern with larger effects on the higher quantiles. However, the difference in patterns is swamped by the level of the effects.[45]

7 Conclusion

Now several years into a slow recovery from the Great Recession, whether government spending stimulated the economy is one of the most important policy questions we face. The federal government spent vast amounts of money with the intention of stimulating the economy, but many economists

[44] 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

$$Q_q(\Delta Y_{c,t}) = \alpha_t^q + \tilde{\beta}^q \Delta F_{c,t} + \gamma^q CS_{c,t} + \Gamma^q X_{c,t},$$

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. We also implemented the dual inference approach of Chernozhukov and Hansen (2008), which yielded similar conclusions.

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 only if the model is correctly specified. Alternative methods that are robust to model misspecification have been proposed by Chen and Pouzo (2009).

[45] As an alternative approach, we also estimate quantile treatment effects of the binary Census Shock following Cattaneo et al. (2013). Table E.31 estimates quantiles of the potential outcomes distribution for counties with and without a Census Shock and Table E.32 presents quantile treatment effects, where we make the assumption of rank-preservation. These estimates show that a Census Shock has the largest effect on income and employment growth in low quantiles. This result is consistent with the conclusion of the IVQR analysis, however, this approach is unable to deliver estimates of local multipliers.

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 local fiscal multipliers. We rely on cross-sectional instead of time-series variation and propose a new shock to identify the causal impact of federal spending. This new approach is a powerful yet transparent way to measure several important parameters including 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. We do not find large spillover effects and we find that government spending provides higher returns in depressed areas, which has contributed to reducing inequality in income and employment growth 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, 2012; Christiano et al., 2011; Woodford, 2010). The Census Shock is also relevant for the field of urban and regional economics. The 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, 2011). We address some of these questions in a follow-up paper on the incidence of federal government spending (Suárez Serrato and Wingender, 2014b).

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Table 1: Semi-parametric Estimates of the Effect of a Census Shock on Past Growth

	(1)	(2)	(3)	(4)	(5)
Income Growth (-1,1)	54.671**	17.832	19.698		
	(23.188)	(21.131)	(20.291)		
Employment Growth (-1,1)	1.347***	0.326	0.366		
	(0.336)	(0.230)	(0.227)		
Income Growth (-3,0)	140.511***	-11.116	-10.568	-20.622	-1.773
	(24.477)	(32.405)	(32.136)	(31.212)	(26.118)
Employment Growth (-3,0)	2.332***	-0.083	-0.086	-0.247	-0.047
	(0.370)	(0.418)	(0.421)	(0.402)	(0.346)
Income Growth (-5,0)	127.341***	0.084	0.219	-6.743	10.578
	(21.293)	(29.687)	(29.251)	(26.900)	(23.101)
Employment Growth (-5,0)	2.238***	0.276	0.281	0.158	0.371
	(0.301)	(0.399)	(0.400)	(0.365)	(0.333)
Observations	9,173	9,173	9,173	9,173	9,173
IPW		Y	Y	Y	Y
RA			Y	Y	Y
RA Controls			Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	
State-Year Fixed Effects					Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on past economic growth. See Section 3.5 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 2: Semi-parametric Estimates of the Effect of a Census Shock:
Reduced-form Effects and Implied Multipliers

	(1)	(2)	(3)	(4)
Employment Growth	1.064***	1.100***	0.974***	0.936**
	(0.395)	(0.412)	(0.371)	(0.406)
Income Growth	61.610**	63.298**	55.681**	51.252*
	(27.251)	(28.605)	(26.322)	(28.429)
Federal Spending Growth	31.319**	31.374**	29.984**	30.148**
	(13.211)	(12.497)	(13.156)	(12.760)
Implied Multipliers				
Income Multiplier	1.967*	2.018*	1.857*	1.7
	(1.124)	(1.16)	(1.119)	(1.104)
90% CI (percentile)	[.42,5.96]	[.38,6.14]	[.35,5.7]	[.13,5.49]
Bootstrap p-value	.025	.029	.024	.039
Cost per Job	29442*	28534*	30785*	32202*
	(15230)	(14563)	(16694)	(18284)
90% CI (percentile)	[7601,73169]	[8873,74722]	[9004,77600]	[8513,91924]
Bootstrap p-value	.015	.011	.012	.015
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. See Section 4.1 for details and Section A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Event Study of Census Shock and Implied Multipliers

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per $1M
Census Shock$_{t+6}$	9.474	6.925	0.500	-0.046	2.686
	(112.050)	(139.289)	(1.333)	(0.624)	(5.883)
Census Shock$_{t+5}$	9.203	-9.118	-0.133	-0.127	0.022
	(116.956)	(119.703)	(1.145)	(0.622)	(5.513)
Census Shock$_{t+4}$	24.965	7.259	-0.077	-0.059	0.343
	(146.093)	(106.550)	(0.990)	(0.650)	(5.803)
Census Shock$_{t+3}$	-23.570	44.191	0.284	0.169	2.844
	(16.300)	(85.274)	(0.744)	(0.660)	(6.076)
Census Shock$_{t+2}$	-11.898	25.727	0.111	0.062	2.097
	(9.485)	(44.030)	(0.391)	(0.481)	(5.161)
Census Shock$_{t+1}$	1.028	13.145	-0.146	-0.054	-0.091
	(5.848)	(12.531)	(0.209)	(0.331)	(4.638)
Census Shock$_{t}$	0.000	0.000	0.000	-0.295	2.323
	(0.000)	(0.000)	(0.000)	(0.572)	(8.847)
Census Shock$_{t-1}$	6.129	11.581	-0.166	-0.213	-0.954
	(6.683)	(13.920)	(0.250)	(1.052)	(13.657)
Census Shock$_{t-2}$	-8.224	18.151	-0.138	0.000	0.000
	(20.646)	(34.912)	(0.503)	(.)	(.)
Census Shock$_{t-3}$	42.938	70.533	0.974	1.702*	37.451**
	(41.028)	(49.821)	(0.788)	(1.000)	(17.417)
Census Shock$_{t-4}$	71.907	159.985***	1.783*	2.304**	32.333*
	(47.799)	(56.791)	(1.036)	(1.155)	(17.393)
Census Shock$_{t-5}$	113.739**	199.890**	2.357*	1.968*	27.995*
	(48.155)	(85.154)	(1.261)	(1.121)	(16.741)
Census Shock$_{t-6}$	115.030***	234.418**	2.535*	1.756	22.502
	(44.137)	(113.934)	(1.500)	(1.099)	(14.804)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. This table reports average treatment effects on the treated. See Table E.17 for estimates of average treatment effects. See Section 4.2 for details and Appendix A for data sources. Columns (1)-(3) report treatment effects on spending, income, and employment growth. The models in columns (2) and (3) also estimate treatment effects on average spending growth between years 2 and 5, which we use to compute the implied multipliers in columns (4) and (5). That is, columns (4) and (5) report $\frac{\hat{\beta}_Y^s}{|s-2|\times\hat{\beta}_F^{5-2}/3}$ for a given year s. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. The base specification uses the propensity score model in column (4) of Table E.5 and includes RA for variables in column (4) of Table 2. The propensity score models in columns (1)-(3) also control for the corresponding $\Delta Y_{c,t-1}$ and the model in column (3) also controls for $\Delta Emp_{c,t-6}$. Table E.19 shows that not including $\Delta Emp_{c,t-6}$ in column (3) results in substantively similar estimates. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Event Study of Census Shock and Implied Multipliers
Controlling for Lagged Outcomes

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per $1M
Census Shock$_{t+6}$	39.064	-38.238	0.017	-0.153	0.071
	(77.870)	(59.482)	(0.740)	(0.254)	(3.042)
Census Shock$_{t+5}$	39.288	-50.329	-0.568	-0.231	-2.676
	(72.111)	(53.507)	(0.707)	(0.281)	(3.961)
Census Shock$_{t+4}$	41.513	-29.258	-0.411	-0.156	-2.258
	(63.327)	(36.510)	(0.556)	(0.212)	(3.529)
Census Shock$_{t+3}$	-13.047	11.279	0.028	0.072	0.185
	(8.665)	(20.881)	(0.313)	(0.137)	(2.033)
Census Shock$_{t+2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t+1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-3}$	60.534**	51.712*	1.187***	1.659*	39.143**
	(29.316)	(26.901)	(0.455)	(0.971)	(18.106)
Census Shock$_{t-4}$	100.183**	143.231**	2.063**	2.297*	34.017*
	(41.213)	(57.122)	(0.859)	(1.227)	(18.170)
Census Shock$_{t-5}$	143.042***	187.252**	2.691**	2.002*	29.573*
	(47.118)	(84.666)	(1.165)	(1.172)	(17.486)
Census Shock$_{t-6}$	147.356***	222.785**	2.927**	1.787	24.126
	(43.804)	(109.815)	(1.417)	(1.131)	(15.627)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. This table reports average treatment effects on the treated. See Table E.18 for estimates of average treatment effects. See Section 4.2 for details and Appendix A for data sources. This table expands the models in Table 3 with RA for the following lagged outcomes in columns (1)-(3): $\Delta Y^{s}_{c,t}$ for $s = -2, \cdots, 2$. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: First Stage Effect of Census Shock on Federal Spending

	(1)	(2)	(3)	(4)	(5)	(6)
Census Shock	511.426***	547.821***	460.406***	486.245***	514.855***	589.600***
	(114.675)	(119.653)	(114.660)	(120.504)	(119.830)	(104.781)
Migration Shifter		-0.318		-0.263		23.310***
		(0.564)		(0.556)		(4.430)
Industry Shifter		7.044		2.065		-138.640***
		(11.041)		(11.395)		(28.010)
Employment Growth (-1,1)			1.536*	2.157***		65.215***
			(0.783)	(0.653)		(11.770)
Income Growth (-1,1)			0.009	0.001		-0.211***
			(0.017)	(0.017)		(0.048)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
First-Stage F Stat	19.890	20.962	16.123	16.282	18.460	31.663
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from OLS regressions of federal spending growth between years 2 and 5. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: IV Estimates of Income Multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending Growth	2.663***	2.570***	2.092**	1.981**	1.893**	2.165***
	(0.901)	(0.870)	(0.832)	(0.802)	(0.792)	(0.663)
Migration Shifter		-2.303		-2.096		-59.712***
		(2.150)		(1.988)		(18.735)
Industry Shifter		-25.212		-55.623***		317.481***
		(19.369)		(14.898)		(110.514)
Employment Growth (-1,1)			7.119***	6.645***		-152.856***
			(2.426)	(2.332)		(50.133)
Income Growth (-1,1)			0.097	0.125**		0.687***
			(0.064)	(0.059)		(0.188)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
First-Stage F Stat	19.890	20.962	16.123	16.282	18.460	31.663
P-Value Hausman Test	0.003	0.003	0.015	0.017	0.025	0.002
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from IV regressions of income growth between years 2 and 5. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: IV Estimates of Jobs per \$1M and Cost per Job

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending Growth	41.275***	39.529***	30.103**	28.301**	27.460**	32.731***
	(14.304)	(13.517)	(13.207)	(12.475)	(12.044)	(9.510)
Migration Shifter		-0.056		-0.054*		-0.924***
		(0.035)		(0.031)		(0.255)
Industry Shifter		-0.314		-0.724***		4.811***
		(0.279)		(0.204)		(1.514)
Employment Growth (-1,1)			0.221***	0.220***		-2.172***
			(0.037)	(0.037)		(0.680)
Income Growth (-1,1)			-0.000	-0.000		0.008***
			(0.001)	(0.001)		(0.002)
Cost per Job	24227***	25298***	33219**	35335**	36417**	30552***
	(8396)	(8651)	(14575)	(15575)	(15972)	(8877)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
First-Stage F Stat	19.890	20.962	16.123	16.282	18.460	31.663
P-Value Hausman Test	0.003	0.003	0.027	0.030	0.036	0.001
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from IV regressions of income growth between years 2 and 5. The coefficient on Federal Spending is multiplied by 1000 to represent the number of jobs from an additional \$1M in spending. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Semi-parametric Estimates of Treatment Effects
of a Census Shock with Spillovers

	(1) Closest 10 Counties			(2) MSA		
Treatment Level	Federal Spending	Employment	Income	Federal Spending	Employment	Income
1- Only Neighbor has Census Shock	7.800	0.256	12.346	-2.937	-0.283	-13.484
	(5.336)	(0.134)	(9.027)	(2.105)	(0.053)	(3.133)
2- Only Own Census Shock	32.235**	1.373***	83.358***	27.672*	1.365***	68.849***
	(5.062)	(0.136)	(9.091)	(1.901)	(0.050)	(3.082)
3- Both Census Shocks	34.424**	1.685***	90.005***	23.585	1.181***	58.587**
	(4.935)	(0.128)	(8.571)	(1.907)	(0.050)	(3.066)
N	91428			547986		

Notes: This table reports treatment effects from a multivalued treatment effects approach to analyzing spillover effects. Each row reports the estimate of a given treatment level relative to a 0-level treatment of No Census Shocks. All variables report growth between years 2 and 5. Table E.30 reports the means of potential outcomes. See Section 5 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: Implied Multipliers of Treatment Effects Estimates with Spillovers

	(1) Closest 10 Counties	(2) MSA
Income Multiplier		
2- Only Own Census Shock	2.586^{*}	2.488^{*}
	(1.475)	(1.503)
3- Both Census Shocks	2.615^{*}	2.484
	(1.334)	(1.746)
Jobs per \$1 M		
2- Only Own Census Shock	43^{*}	49^{*}
	(24)	(28)
3- Both Census Shocks	49^{**}	50
	(24)	(34)
Cost-per-job		
2- Only Own Census Shock	23473^{*}	20271^{*}
	(13.181)	(11689)
3- Both Census Shocks	20435^{**}	19966
	(9942)	(13512)
Observations	91,428	547,986

Notes: This table reports implied multipliers from a multivalued treatment effects approach to analyzing spillover effects. Table E.30 reports the means of potential outcomes and Table 8 presents treatment effects for treatments 1-3 relative to treatment 0. This table reports ratios of these coefficients. See Section 5 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 10: First Stage Spillover Estimates

	(1) 10 Closest Counties		(2) Same MSA	
	Own	Neighbors	Own	Neighbors
Census Shock				
Own	444.539***	112.031	456.861***	-26.929
	(107.263)	(79.211)	(111.779)	(69.347)
Neighbors	82.365	350.653***	-44.673	480.150***
	(81.751)	(116.840)	(67.422)	(168.559)
Observations	91,428	91,428	547,986	547,986
R-squared	0.04	0.04	0.04	0.04
Angrist-Pischke F-Stat	17.59	8.86	16.30	8.01

Notes: The table reports the results of OLS regressions of growth in federal spending between years 2 and 5. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. All regressions include state and year fixed effects and all the covariates included in Table 5 for both own and neighboring counties. See Section 5 and Appendix A for data sources. 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	1.986**	1.986**
	(0.880)	(0.833)
Neighbors	-0.214	-0.183
	(0.700)	(0.425)
Sum of Multipliers	1.772**	1.803*
	(0.886)	(1.049)
Observations	91,428	547,986

Notes: This table reports the estimated coefficients of IV regressions. The dependent variable is the average annual growth in local personal income between years 2 and 5. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. All regressions include state and year fixed effects and all the covariates included in Table 6 for both own and neighboring counties. See Section 5 and Appendix A for data sources. 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 12: Spillover Estimates for Employment

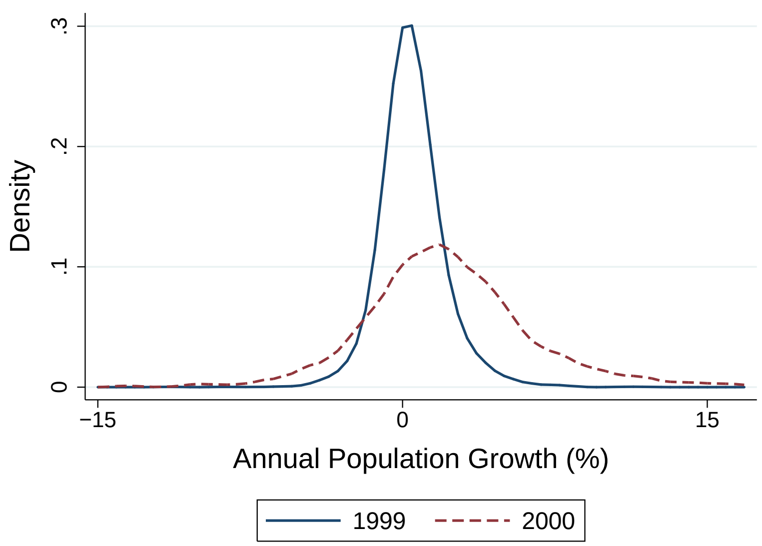
	(1) 10 Closest Counties	(2) Same MSA
Federal Spending		
Own	28.652*	28.861*
	(13.312)	(13.104)
Neighbors	-3.551	-3.994
	(9.810)	(5.587)
Cost per Job	39,839	40,214
	(22,535)	(26,120)
Observations	91,428	547,986

Notes: This table reports the estimated coefficients of IV regressions. The dependent variable is the average annual growth in local employment between years 2 and 5. The coefficient on federal spending is multiplied by 1000 in order to interpret it as the number of job per additional $1M of spending. Column (1) defines neighbors as the 10 geographically closest counties. Column (2) uses all other counties in own MSA. All regressions include state and year fixed effects and all the covariates included in Table 7 for both own and neighboring counties. See Section 5 and Appendix A for data sources. Standard errors clustered at the state level with degrees-of-freedom adjustment to account for duplicated observations in parentheses.

Figure 1: Average County Population Growth Rate by Year

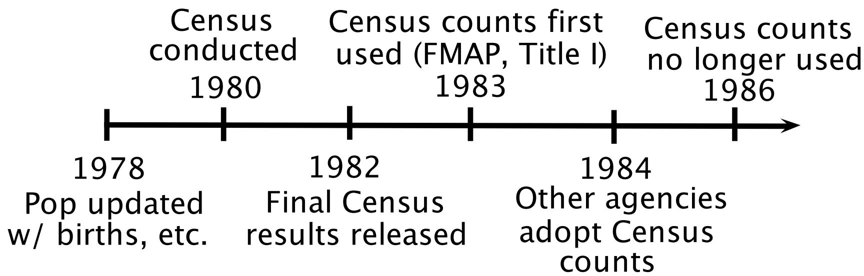
Notes: This figure plots the unweighted average population growth in percent across all counties by year using postcensal population estimates. See Appendix A for data sources.

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 Appendix A for data sources.

Figure 3: Timeline

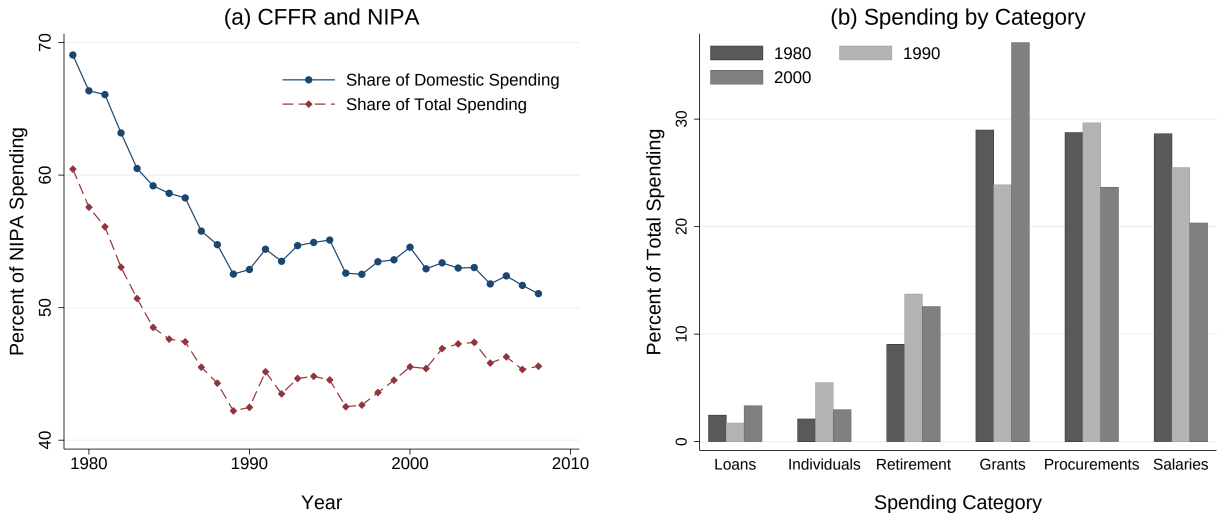


Notes: This figure plots a stylized timeline of events surrounding a Census enumeration. See Section 1.2 for details and Appendix A for data sources.

Figure 4: Serial Correlation of the Census Shock

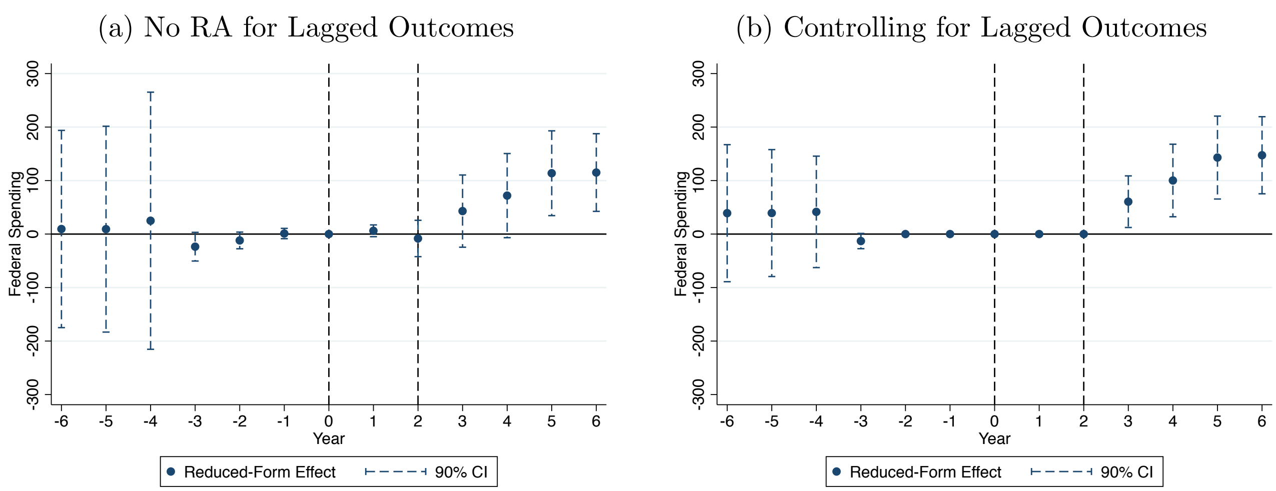
Notes: The figure shows the scatter plots and estimated linear relation between each county's Census Shocks across two consecutive Censuses after controlling for state fixed effects. See Section 3 for details and Appendix A for data sources..

Figure 5: 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 are from the Bureau of Economic Analysis. CFFR data is from the U.S. Census Bureau (2010d).

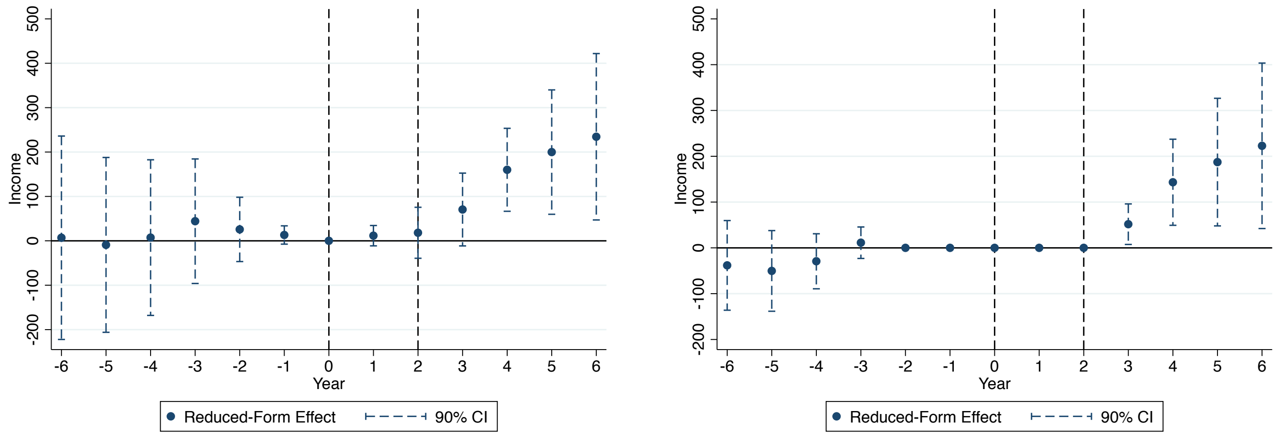
Figure 6: Semi-Parametric Reduced-Form Effects on Federal Spending from Doubly-Robust Estimation



Notes: These figures plot estimated reduced-form effects of a Census Shock on federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (1) of Tables 3 and 4. Note that, since federal spending data is available starting in 1977, the the estimates for years -6 to -4 have a smaller estimation sample, which explains the change in the size of the confidence interval. See Section 4.2 for more details and Appendix A for data sources.

Figure 7: Semi-Parametric Reduced-Form Effects on Income from Doubly-Robust Estimation

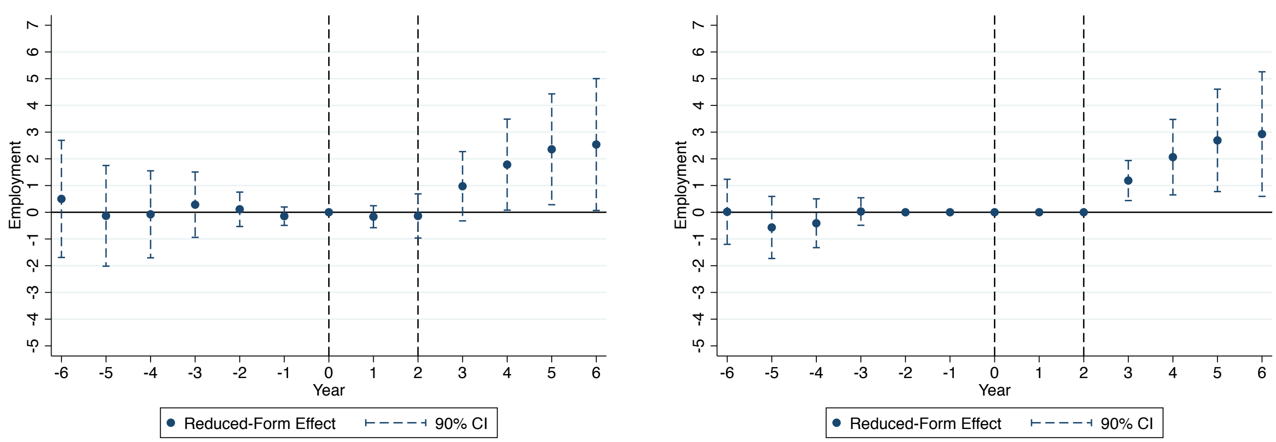
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated reduced-form effects of a Census Shock on income with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (2) of Tables 3 and 4. See Section 4.2 for more details and Appendix A for data sources.

Figure 8: Semi-Parametric Reduced-Form Effects on Employment from Doubly-Robust Estimation

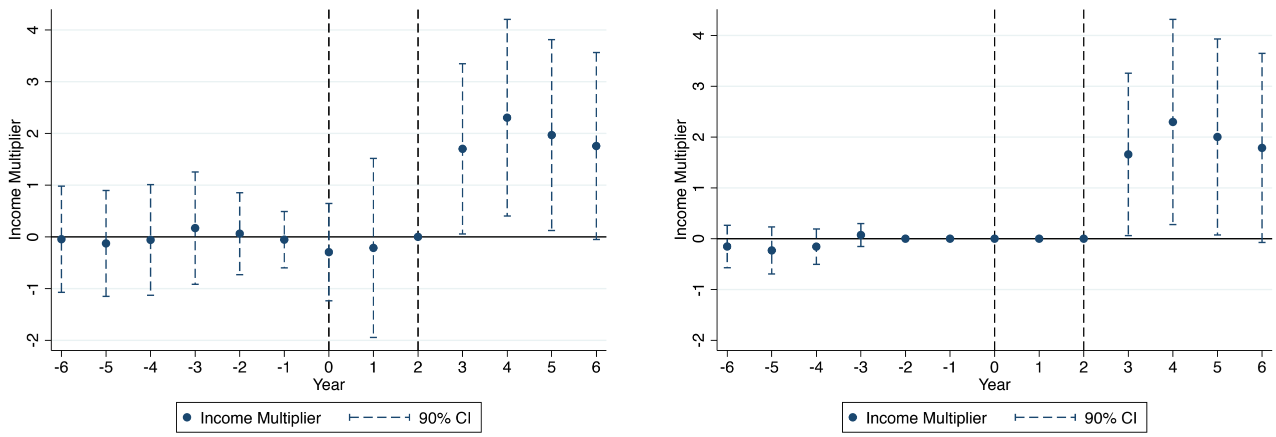
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated reduced-form effects of a Census Shock on employment with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (3) of Tables 3 and 4. See Section 4.2 for more details and Appendix A for data sources.

Figure 9: Semi-parametric Estimates of the Income Multiplier from Doubly-robust Estimation

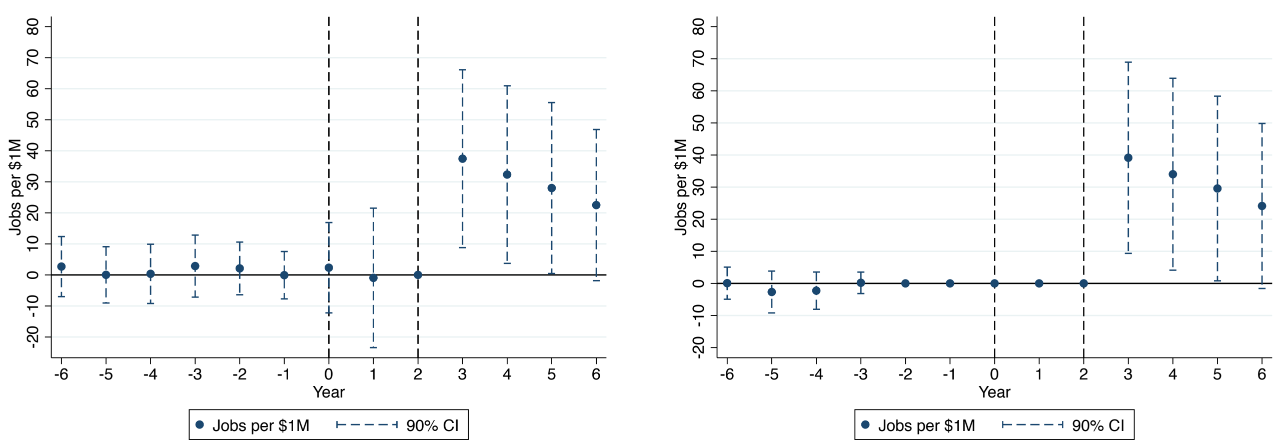
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated local income multiplier of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (4) of Tables 3 and 4. See Section 4.2 for more details and Appendix A for data sources.

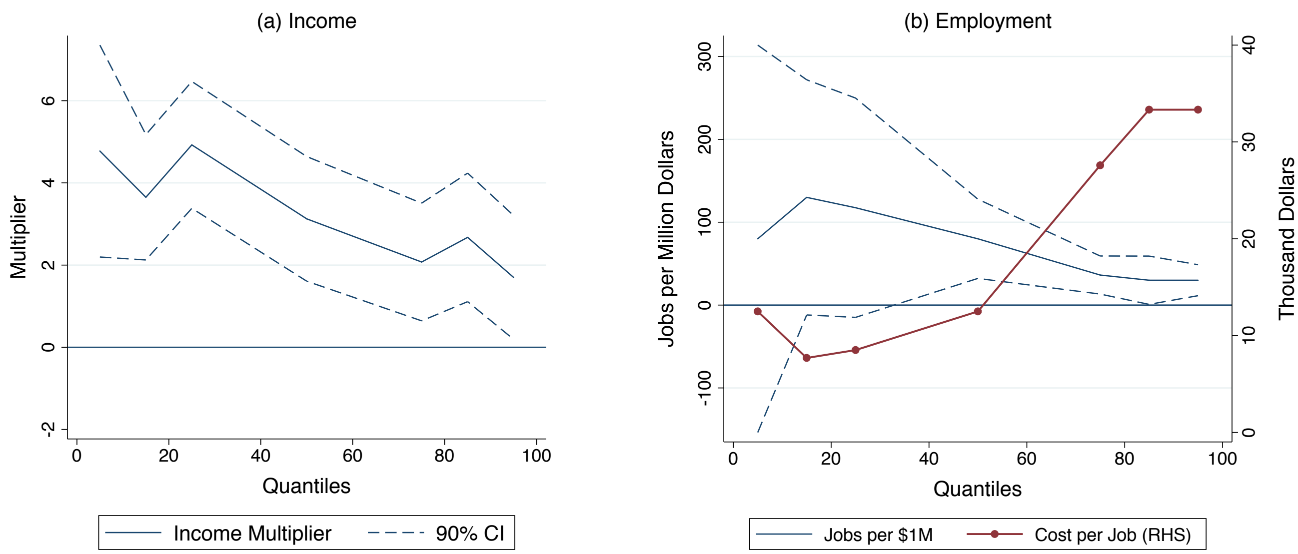
Figure 10: Semi-parametric Estimates of Jobs per $1M from Doubly-robust Estimation

(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated employment effects per an additional million dollars of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (5) of Tables 3 and 4. See Section 4.2 for more details and Appendix A for data sources.

Figure 11: IVQR Estimates of Spending Multipliers



Notes: The figure plots the estimated multipliers from the IVQR analysis along with a 90% confidence interval for 7 quantiles of the distribution of the dependent variable. See Section 6 for details and Appendix A for data sources. Panel (a) uses the average annual growth in local personal income from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Panel (b) uses the average annual growth in employment. Panel (b) also reports the cost per job created in dollars of 2009 on the right hand side axis at the corresponding quantiles. Standard errors are computed via bootstrap. See Figure E.17 for the corresponding quantile regression estimates.

Online Appendix Not For Publication

A 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. Note that CFFR data cover the federal government fiscal year, which starts in October 1st of the previous calendar year. 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).

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	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. We also exclude Medicare and Social Security transfers. See main text for details. All variables are normalized by constant population level from 1980.
Personal Income	Total personal income minus place-of-residence adjustment (BEA Table CA5 line 42), personal current transfer receipts (line 47), farm earnings (line 81) and government employee earnings (line 2000) plus contributions for government social insurance (line 36). Alternatively, this can be calculated as the sum of gross private nonfarm labor earnings (line 90) and dividends, interests and rental income (line 46).
Employment	Total private nonfarm employment (line 90). Total employment as reported by the BEA is the sum of full-time and part-time employment for both employees and sole proprietors.
SOI Measures of Income and Employment	We use earnings from tax returns aggregated at the county-year level from the IRS Statistics of Income Division. We also use the number of tax filers as an alternative measure of employment.
QCEW Measures of Employment and Earnings	We use earnings and employment from all private industries as alternative measures of earnings and employment.

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 national changes in population 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.

B 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 omitting it could potentially 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.[46]

The main reason we limit the analysis to federal spending is that, to our knowledge, there does not exist 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 B.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. Column (1) presents 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.

Table B.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 a regression on the Census Shock 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

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

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

state transfers to local governments vary in response to the Census Shock. Column (1) in Table B.2 reports the coefficient on the Census Shock using data from the CFFR as the dependent variable. In columns (2) and (3), we use respectively 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 therefore should not play a large role in identifying the total marginal impact of government spending changes in our estimation framework.

Table B.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 a regression on the Census Shock 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.

C A Measurement Error Model for the Census Shock

In this section, we present a simple framework that formalizes the source of variation in the Census Shock. This model relates the shock to specific factors that could potentially challenge the exclusion restriction underlying an IV approach, namely, that the Census Shock only affects local 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.

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

where $\Delta\alpha = \alpha^C - \alpha^{PC}$ and $\Delta u_c = u^C_{c,t} - u^{PC}_{c,t}$.[47] 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. The third line assumes $\Delta\alpha$ is constant. The fourth line uses 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 (C.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.

[47] 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.

D Spillover Analysis

Section 5 presents estimates of spillovers by analyzing repeated pairs of counties with each of their neighboring counties. This approach is similar to a spatial average that gives equal weight to neighboring counties and zero weight to other counties. This spatial average, however, may result in a weak first-stage relationship whenever the Census Shock is not geographically correlated. This appendix presents the results of two analyses. First, we show using a Monte Carlo experiment, that the repeated-pairs and the spatial-average approaches result in similar estimates but that the spatial-average approach is less robust to weak instrument concerns. Second, we show that an alternative spatial average approach that weights spending in other counties but the inverse of the distance between each county results in similar estimates of spillover effects.

Monte Carlo Experiment of Repeated-Pairs and Spatial-Average Estimates

Consider the following data generating process:

$$\Delta y = \alpha + \beta_1 \Delta F + \beta_2 \sum_n \Delta F_n + \gamma_1 W + \gamma_2 \sum_n W_n + u,$$

where Δy is the outcome of interest, ΔF is the change in federal spending, and ΔF_n is the change in federal spending for a neighboring county. W and W_n are shocks unobservable to the econometrician such that

$$corr(\Delta F, W) = corr(\Delta F_n, W_n) = \delta \geq 0.$$

Assume that an instrument exists, labeled CS, such that

$$corr(\Delta F, CS) = corr(\Delta F_n, CS_n) = \rho > 0,$$

and $E[CS|W, W_n, u] = 0$. Importantly, assume that CS is not geographically correlated. This implies that an instrumental variables estimation of all pairs of the form:

$$\Delta y = \alpha + \beta_1 \Delta F + \beta_2 \Delta F_n + u,$$

that is instrumented by CS and CS_n, and where we weight each county the inverse of the number of total pairs, will result in consistent estimates of β_1 and β_2.

An alternative approach would be to construct a spatial average of the neighboring counties CS_n and ΔF_n for each county and run the following spatial-average regression:

$$\Delta y = \alpha + \beta_1 \Delta F + \beta_2 \bar{\Delta} F_n + u,$$

where we now instrument the average change in spending for the neighbors $\bar{\Delta} F_n$ with the average instrument $\bar{C} S_n$. In cases where this first-stage relationship is statistically strong, this will also provide consistent estimates of β_1 and β_2. However, since the instrument is not geographically correlated and has mean zero, this strategy may reduce the power of the instrument in the first stage and invite concerns of weak instruments.

We illustrate this possibility by generating 1000 datasets of 1000 counties each with 10 neighboring counties and where we have set $\beta_1 = 2$ and $\beta_2 = -.1$. Consider first the case where $\delta = 0$ and ρ is large. This case implies that there is no omitted variable and the instrument is strong; such that both OLS and IV approaches will result in consistent estimates. Table D.1 presents the results from the case where $\delta = 0$ and $\rho = 0.5$. Note that the OLS and IV estimators perform well in both approaches. The standard errors of the estimated coefficients in the spatial-average approach have a similar order of magnitude as in the paired approach. However, the F-stats from the first stage are smaller in the

Table D.1: Results of Monte Carlo Simulation

			Spatial Average		Repeated Pairs	
Case	Estimate	Statistic	OLS	IV	OLS	IV
1: $\delta = 0$, $\rho = 0.5$	Own Federal Spending	Mean	1.995	1.992	1.995	1.991
		Std. Dev.	.119	.222	.118	.222
	Neighbor Federal Spending	Mean	-.1	-.102	-.1	-.102
		Std. Dev.	.038	.07	.038	.069
	First-Stage F-Stat (Own)	Mean		224.616		2264.351
	First-Stage F-Stat (Neighbor)	Mean		224.645		2228.958
2: $\delta = 0.5$, $\rho = 0.5$	Own Federal Spending	Mean	1.538	2.001	1.536	2
		Std. Dev.	.11	.215	.125	.222
	Neighbor Federal Spending	Mean	-.562	-.097	-.56	-.098
		Std. Dev.	.036	.071	.043	.072
	First-Stage F-Stat (Own)	Mean		223.784		2252.239
	First-Stage F-Stat (Neighbor)	Mean		223.515		2230.26
3: $\delta = 0.5$, $\rho = 0.1$	Own Federal Spending	Mean	1.502	2.017	1.504	2.096
		Std. Dev.	.097	3.877	.107	1.63
	Neighbor Federal Spending	Mean	-.598	.01	-.597	-.084
		Std. Dev.	.031	1.07	.039	.4
	First-Stage F-Stat (Own)	Mean		5.999		56.121
	First-Stage F-Stat (Neighbor)	Mean		6.035		52.261

Notes: This table reports results from a Monte Carlo experiment. For each model and each case, we estimate the relevant model 1000 times and report the means and standard deviations of selected statistics.

spatial-average approach.

Now consider the case where $\delta > 0$ and ρ is large. In this case, OLS will not be unbiased but both IV approaches might still yield consistent estimates. Case 2 in Table D.1 presents this case where we have set $\delta = \rho = 0.5$. As expected, both OLS approaches result in biased estimates. While the F-stats in the spatial-average approach are smaller than for the case of repeated pairs, both IV approaches have a strong first stage and result in unbiased estimates of β_1 and β_2.

Finally, consider the case where $\delta > 0$ and ρ is small. Case 3 in Table D.1 presents this case where we have set $\delta = 0.5, \rho = 0.1$. As in the previous case, OLS will not be unbiased. In this case we see that the IV spatial-average approach results in significantly larger standard errors, smaller First-Stage F-stats, and an estimate of β_2 that is not close to the true value. In contrast, the repeated pairs case provides consistent estimates and robust First-Stage F-stats.

Spatial Average Estimates of Spillover Effects

We now implement the spatial average estimation discussed in the previous section. We explore three sets of spatial averages. The first two give constant weight to neighbors and zero weight to other counties. The third approach weights all counties by the inverse of the distance between the counties. Table D.2 estimates of income multipliers and Table D.3 reports estimates of employment effects and cost per job created. The first-stage F-stats are smaller than those in our repeated-pairs estimates in Table 10. This is particularly true for the MSA specification. The inverse distance specification, however, has first-stage F-stats that are slightly larger. From these estimates, columns (2) and (6) provide similar results to those discussed in Section 5.

Table D.2: Spillover Estimates for Local Income Multipliers (Spatial Average)

	(1)	(2)	(3)	(4)	(5)	(6)
	10 Closest Counties		Same MSA		Distance	
Federal Spending						
Own	2.025*	2.084*	1.659	2.063*	2.085*	2.105*
	(1.052)	(1.084)	(5.244)	(1.245)	(1.144)	(1.160)
Neighbors	0.167	-0.261	13.350	3.736	0.014	-0.283
	(1.710)	(2.075)	(113.656)	(10.526)	(1.389)	(1.503)
Income Growth (-1,1)						
Own	0.097	0.089	0.157	0.132	0.097	0.102
	(0.064)	(0.068)	(0.578)	(0.172)	(0.065)	(0.071)
Neighbors		-0.025		0.032		-0.117
		(0.141)		(0.254)		(0.161)
Employment Growth (-1,1)						
Own	0.097	0.089	0.157	0.132	0.097	0.102
	(0.064)	(0.068)	(0.578)	(0.172)	(0.065)	(0.071)
Neighbors		-0.025		0.032		-0.117
		(0.141)		(0.254)		(0.161)
Sum of Multipliers	2.193	1.823	15.008	5.798	2.098**	1.823*
	(1.355)	(1.643)	(108.866)	(9.858)	(1.023)	(1.094)
Observations	9,177	9,177	9,177	9,177	9,177	9,177
Angrist-Pishke F-Stat (Own)	11.42	10.93	6.50	13.52	9.33	9.04
Angrist-Pishke F-Stat (Neighbor)	6.94	4.05	0.01	0.21	13.53	10.21

Notes: The table reports the estimated coefficients for own and neighbor's Federal Spending on average income growth between years 2 and 5. Columns (1) and (2) give uniform weights to the 10 closest counties, columns (3) and (4) give uniform weights to the counties in the same MSA, and columns (5) and (6) give an inverse-distance weight to all other counties. All regressions include state and decade fixed effects. Standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table D.3: Spillover Estimates for Local Employment Multipliers (Spatial Average)

	(1)	(2)	(3)	(4)	(5)	(6)
	10 Closest Counties		Same MSA		Distance	
Federal Spending						
Own	28.630*	29.823*	23.405	28.983	30.844*	31.265*
	(15.142)	(15.435)	(81.899)	(20.426)	(16.730)	(17.040)
Neighbors	3.699	-6.500	206.402	65.779	-1.443	-7.703
	(24.691)	(28.374)	(1749.972)	(175.929)	(18.602)	(19.896)
Income Growth (-1,1)						
Own	-0.000	-0.000	0.001	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.009)	(0.003)	(0.001)	(0.001)
Neighbors		-0.002		-0.001		-0.003
		(0.002)		(0.004)		(0.002)
Employment Growth (-1,1)						
Own	0.097	0.089	0.157	0.132	0.097	0.102
	(0.064)	(0.068)	(0.578)	(0.172)	(0.065)	(0.071)
Neighbors		-0.025		0.032		-0.117
		(0.141)		(0.254)		(0.161)
Cost per Job	30932	42878	4351	10553	34013*	42442
	(20608)	(44387)	(31710)	(18182)	(18036)	(28249)
Observations	9,177	9,177	9,177	9,177	9,177	9,177
Angrist-Pishke F-Stat (Own)	11.42	10.93	6.50	13.52	9.33	9.04
Angrist-Pishke F-Stat (Neighbor)	6.94	4.05	0.01	0.21	13.53	10.21

Notes: The table reports the estimated coefficients for own and neighbor's Federal Spending on average income growth between years 2 and 5. The coefficients on spending are multiplied by 1000 to represent jobs per $1M. 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. Columns (1) and (2) give uniform weights to the 10 closest counties, columns (3) and (4) give uniform weights to the counties in the same MSA, and columns (5) and (6) give an inverse-distance weight to all other counties. All regressions include state and decade fixed effects. Standard errors clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

E Additional Tables and Graphs

Table E.1: 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. See Section 3.1 for details and Appendix A for data sources. 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table E.2: 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

Notes: The table reports the top and bottom 25 Census Shocks for 1980. See Section 3.1 for details and Appendix A for data sources.

Table E.3: 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

Notes: The table reports the top and bottom 25 Census Shocks for 1990. See Section 3.1 for details and Appendix A for data sources.

Table E.4: 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

Notes: The table reports the top and bottom 25 Census Shocks for 2000. See Section 3.1 for details and Appendix A for data sources.

Table E.5: Estimates of Logit Model of Propensity Score

	(1)	(2)	(3)	(4)
Income Growth (-1,1)	-0.021	-0.089**		-0.088**
	(0.031)	(0.037)		(0.041)
Employment Growth (-1,1)	5.281***	1.734		2.053
	(1.531)	(1.521)		(1.587)
Federal Spending Growth (-1,1)	0.024	-0.028		-0.040
	(0.046)	(0.053)		(0.056)
Income Growth (-3,0)		0.085*		0.082*
		(0.047)		(0.046)
Employment Growth (-3,0)		9.269***		9.356***
		(3.349)		(3.224)
Federal Spending Growth (-3,0)		0.098		0.108
		(0.069)		(0.075)
Migration Share Shifter			2.299	2.086
			(2.063)	(1.794)
Industry Share Shifter			-2.162	-8.146
			(18.477)	(19.084)
FE for 1990	-42.401	-46.647	-65.378	-80.680
	(43.184)	(45.791)	(62.290)	(69.673)
FE for 2000	-133.667***	-168.752***	-132.922*	-166.314**
	(42.809)	(48.723)	(68.058)	(67.064)
Constant	38.860	-30.083	29.018	-46.707
	(27.267)	(30.645)	(36.975)	(39.798)
Observations	9,173	9,173	9,099	9,099

Notes: The table reports estimates from propensity score models of the Census Shock. All coefficients are multiplied by 1000. See Section 3.5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$
‘

Table E.6: Estimates of Marginal Effects in Logit Model of Propensity Score

	(1)	(2)	(3)	(4)
High Census Shock				
Income Growth (-1,1)	-0.021	-0.089**		-0.088**
	(0.031)	(0.037)		(0.041)
Employment Growth (-1,1)	5.281***	1.734		2.053
	(1.531)	(1.521)		(1.587)
Federal Spending Growth (-1,1)	0.024	-0.028		-0.040
	(0.046)	(0.053)		(0.056)
Income Growth (-3,0)		0.085*		0.082*
		(0.047)		(0.046)
Employment Growth (-3,0)		9.269***		9.356***
		(3.349)		(3.224)
Federal Spending Growth (-3,0)		0.098		0.108
		(0.069)		(0.075)
Migration Share Shifter			2.299	2.086
			(2.063)	(1.794)
Industry Share Shifter			-2.162	-8.146
			(18.477)	(19.084)
FE for 1990	-42.401	-46.647	-65.378	-80.680
	(43.184)	(45.791)	(62.290)	(69.673)
FE for 2000	-133.667***	-168.752***	-132.922*	-166.314**
	(42.809)	(48.723)	(68.058)	(67.064)
Constant	38.860	-30.083	29.018	-46.707
	(27.267)	(30.645)	(36.975)	(39.798)
Observations	9,173	9,173	9,099	9,099

Notes: The table reports marginal effects from propensity score models of the Census Shock. All coefficients are multiplied by 1000. See Section 3.5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.7: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers using Probit Estimates of Propensity Score

	(1)	(2)	(3)	(4)
Employment Growth	1.099***	1.135***	0.988***	0.950**
	(0.398)	(0.414)	(0.372)	(0.407)
Income Growth	63.986**	65.695**	56.634**	52.141*
	(27.431)	(28.725)	(26.364)	(28.461)
Federal Spending Growth	31.342**	31.389**	29.813**	30.036**
	(13.221)	(12.497)	(13.160)	(12.748)
Implied Multipliers				
Income Multiplier	2.042*	2.093*	1.9*	1.736
	(1.141)	(1.176)	(1.135)	(1.117)
90% CI (percentile)	[.46,6.15]	[.45,6.29]	[.37,5.79]	[.17,5.59]
Bootstrap p-value	.024	.028	.024	.037
Cost per Job	28525**	27663**	30166*	31626*
	(14527)	(13900)	(16332)	(17885)
90% CI (percentile)	[7647,70989]	[8593,72090]	[8456,76202]	[8487,89747]
Bootstrap p-value	.014	.011	.012	.015
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. This table reports estimates using a probit for the propensity score. See Section 4.1 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. The controls for the propensity score model corresponds to those in column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.8: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers using Alternative Discretization of Census Shock

	(1)	(2)	(3)	(4)
Employment Growth	1.345***	1.409***	1.284***	1.172**
	(0.500)	(0.525)	(0.463)	(0.504)
Income Growth	85.029***	88.457***	80.076**	70.695**
	(32.452)	(34.106)	(31.189)	(34.183)
Federal Spending Growth	41.054**	41.538**	39.979**	44.285***
	(17.181)	(16.409)	(17.158)	(17.074)
Implied Multipliers				
Income Multiplier	2.071*	2.13*	2.003*	1.596*
	(1.148)	(1.161)	(1.136)	(.961)
90% CI (percentile)	[.48,6.48]	[.54,6.19]	[.59,6.06]	[.34,4.78]
Bootstrap p-value	.024	.023	.017	.022
Cost per Job	30521*	29472*	31127*	37799*
	(16432)	(15441)	(17071)	(21259)
90% CI (percentile)	[7366,86018]	[8035,82994]	[8393,78577]	[10622,107715]
Bootstrap p-value	.018	.016	.014	.014
Observations	7,337	7,337	7,337	7,337
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. This table explores an alternative discretization of the Census Shock where counties with a continuous Census Shock in the top 40% of the distribution is placed in the treatment group and counties in the bottom 40% are placed in the control group. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.9: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers (ATE)

	(1)	(2)	(3)	(4)
Employment Growth	1.129***	1.146***	1.043***	1.072***
	(0.347)	(0.356)	(0.327)	(0.343)
Income Growth	65.025***	65.852**	59.280**	60.478**
	(24.539)	(25.633)	(23.769)	(24.425)
Federal Spending Growth	32.612**	32.582***	31.986**	32.419***
	(13.020)	(12.555)	(12.934)	(12.230)
Implied Multipliers				
Income Multiplier	1.994*	2.021*	1.853*	1.866*
	(1.035)	(1.062)	(.995)	(.972)
90% CI (percentile)	[.61,5.63]	[.58,5.79]	[.54,4.81]	[.59,4.84]
Bootstrap p-value	.013	.013	.014	.013
Cost per Job	28887**	28431**	30676**	30240**
	(13200)	(12914)	(14234)	(13777)
90% CI (percentile)	[10131,61502]	[9983,65826]	[11350,66340]	[11433,66741]
Bootstrap p-value	.009	.005	.007	.009
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. This table reports the average treatment effect. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.10: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers using IRS Data Tax Return Data

	(1)	(2)	(3)	(4)
Employment Growth	1.249***	1.261***	1.248***	1.105***
	(0.225)	(0.219)	(0.219)	(0.242)
Income Growth	80.972***	82.102***	75.760***	64.310***
	(21.699)	(21.511)	(21.203)	(23.286)
Federal Spending Growth	39.005***	39.030***	39.017***	38.215***
	(13.579)	(12.859)	(13.529)	(13.347)
Implied Multipliers				
Income Multiplier	2.076**	2.104**	1.942**	1.683**
	(.893)	(.897)	(.852)	(.855)
90% CI (percentile)	[1,5.12]	[1.01,5.11]	[.87,4.77]	[.59,4.44]
Bootstrap p-value	.004	.002	.003	.008
Cost per Job	31222***	30946***	31255***	34595**
	(11407)	(11010)	(11406)	(13506)
90% CI (percentile)	[12723,52608]	[12947,51785]	[13229,52817]	[13550,61580]
Bootstrap p-value	.004	.002	.003	.005
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. Outcome data come from IRS Statistics Of Income and from BEA. The employment measure is tax files, when available. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.11: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers using QCEW Data

	(1)	(2)	(3)	(4)
Employment Growth	1.143**	1.148**	1.151***	1.094**
	(0.453)	(0.465)	(0.422)	(0.439)
Earnings Growth	39.848	40.426	40.623	36.734
	(33.350)	(34.333)	(31.105)	(32.471)
Federal Spending Growth	36.956***	36.945***	36.112***	36.496***
	(13.180)	(12.390)	(12.898)	(12.846)
Implied Multipliers				
Earnings Multiplier	1.078	1.094	1.125	1.007
	(.942)	(.957)	(.907)	(.916)
90% CI (percentile)	[-.73,3.33]	[-.79,3.2]	[-.43,3.48]	[-.62,3.48]
Bootstrap p-value	.147	.148	.103	.134
Cost per Job	32321**	32180**	31366**	33370**
	(15551)	(15151)	(14372)	(15643)
90% CI (percentile)	[10546,97770]	[12662,96213]	[12615,74259]	[12061,83573]
Bootstrap p-value	.018	.013	.008	.012
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. Outcome data come from QCEW. Note that this table only reports earning and not total income. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table E.12: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers Eliminating Outliers Identivied via Jackknife

	(1)	(2)	(3)	(4)
Employment Growth	1.456***	1.480***	1.385***	1.469***
	(0.459)	(0.460)	(0.449)	(0.492)
Income Growth	75.979**	76.984**	72.833**	76.616**
	(29.629)	(30.257)	(29.910)	(33.573)
Federal Spending Growth	35.133***	35.520***	36.254***	37.255***
	(11.869)	(11.829)	(11.944)	(11.032)
Implied Multipliers				
Income Multiplier	2.163**	2.167**	2.009**	2.057**
	(.952)	(.979)	(.907)	(.897)
90% CI (percentile)	[.75,4.34]	[.72,4.45]	[.66,4.03]	[.63,3.93]
Bootstrap p-value	.009	.008	.008	.01
Cost per Job	24123***	24008***	26177***	25362***
	(8228)	(8306)	(8887)	(8352)
90% CI (percentile)	[13251,47417]	[12842,46274]	[14475,49180]	[14560,49596]
Bootstrap p-value	.001	.002	.002	.001
Observations	8,184	8,184	8,184	8,184
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. This table excludes outlier observations. We first run a jackknife procedure at the State-Year level and identify the State-Years that have large effects on the main estimate. This table excludes counties in State-Years that lead to an average percentage change of more than 10% in the estimation. In total, 14 State-Years are omitted from the estimation. * $p < .1$, ** $p < .05$, *** $p < .01$ * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.13: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: Reduced-form Effects and Implied Multipliers using Alternate Definition of Spending

	(1)	(2)	(3)	(4)
Employment Growth	1.059***	1.092***	0.970***	0.938**
	(0.391)	(0.406)	(0.369)	(0.409)
Income Growth	59.815**	61.394**	54.176**	50.151*
	(26.710)	(27.743)	(26.068)	(28.464)
Federal Spending Growth	22.560***	22.600***	22.033***	24.061***
	(6.405)	(6.281)	(6.387)	(6.447)
Implied Multipliers				
Income Multiplier	2.651*	2.717*	2.459*	2.084
	(1.38)	(1.443)	(1.384)	(1.282)
90% CI (percentile)	[.62,5.59]	[.54,5.84]	[.56,5.78]	[.27,5.33]
Bootstrap p-value	.019	.025	.019	.032
Cost per Job	21307**	20698**	22708**	25648**
	(9277)	(9267)	(10387)	(12646)
90% CI (percentile)	[10250,53629]	[10076,56119]	[10275,56068]	[10760,72100]
Bootstrap p-value	.003	.004	.003	.004
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on outcomes from year 2 to year 5. The definition of spending in this tables does not use a 3-year moving average but instead winsorizes the top 1% of changes in federal spending. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Standard errors for multipliers are calculated using delta method. We also report a bootstrapped confidence interval using the percentile method and the p-value of a one-sided test that the multiplier is negative. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.14: Robustness of Semi-parametric Estimates of the Effect of a Census Shock: First Stage Results and Types of Spending

	(1)	(2)	(3)	(4)
Growth in Federal Spending	34.487***	34.307***	34.738***	36.440***
	(13.225)	(12.673)	(13.007)	(12.553)
Growth in Federal Spending Less Salaries and Wages	33.710***	33.592***	34.053***	35.229***
	(12.955)	(12.390)	(12.767)	(12.386)
Growth in Federal Spending Less Procurement Contracts	34.547***	34.476***	34.941***	35.550***
	(11.209)	(10.794)	(11.076)	(10.750)
Growth in Procurement Contracts	-0.060	-0.169	-0.203	0.890
	(5.103)	(4.854)	(5.017)	(5.135)
Growth in Salaries and Wages	0.777	0.715	0.685	1.210
	(1.666)	(1.676)	(1.667)	(1.620)
Observations	9,173	9,173	9,173	9,173
IPW	Y	Y	Y	Y
RA		Y	Y	Y
RA Controls		Shocks	Shocks, Lagged Outcomes	Shocks, Lagged Outcomes
State Fixed Effects	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	
State-Year Fixed Effects				Y

Notes: This table presents semi-parametric estimates of the effect of a Census Shock on spending growth from year 2 to year 5. See Section 4.1 for details and Appendix A for data sources. The propensity score model corresponds to column (4) of Table E.5. The shocks used as RA controls include the Migration and Industry Share Shifters. The lagged outcomes used as RA controls include, income, employment, and spending growth between years (-1,1). Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.15: Event Study of Census Shock and Implied Multipliers

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per $1M
Census Shock$_{t+9}$	55.709	27.838	0.636	0.018	2.368
	(142.291)	(129.253)	(1.767)	(0.418)	(6.449)
Census Shock$_{t+8}$	0.781	0.100	0.176	-0.069	1.056
	(122.793)	(122.944)	(1.614)	(0.439)	(6.461)
Census Shock$_{t+7}$	-31.506	-20.664	0.436	-0.151	2.148
	(105.674)	(110.748)	(1.406)	(0.445)	(6.508)
Census Shock$_{t+6}$	9.474	-18.219	0.500	-0.160	2.686
	(96.247)	(95.717)	(1.217)	(0.445)	(6.589)
Census Shock$_{t+5}$	9.203	-33.751	-0.133	-0.254	0.022
	(110.255)	(80.635)	(1.074)	(0.452)	(6.674)
Census Shock$_{t+4}$	24.965	-13.388	-0.077	-0.187	0.343
	(139.647)	(64.751)	(0.868)	(0.438)	(6.769)
Census Shock$_{t+3}$	-23.570*	28.222	0.284	0.043	2.844
	(12.473)	(51.106)	(0.687)	(0.438)	(7.220)
Census Shock$_{t+2}$	-11.898*	23.086	0.111	0.012	2.097
	(7.197)	(34.314)	(0.316)	(0.368)	(5.873)
Census Shock$_{t+1}$	1.028	11.697	-0.146	-0.106	-0.091
	(6.395)	(15.007)	(0.206)	(0.394)	(5.542)
Census Shock$_{t}$	0.000	0.000	0.000	-0.346	2.323
	(0.000)	(0.000)	(0.000)	(0.698)	(9.621)
Census Shock$_{t-1}$	6.129	9.625	-0.166	-0.383	-0.954
	(6.948)	(16.538)	(0.217)	(1.212)	(17.397)
Census Shock$_{t-2}$	-8.224	21.547	-0.138	0.000	0.000
	(17.320)	(42.144)	(0.563)	(.)	(.)
Census Shock$_{t-3}$	42.938	77.628	0.974	1.803	37.451*
	(30.587)	(55.831)	(0.834)	(1.125)	(20.277)
Census Shock$_{t-4}$	71.907*	162.277**	1.783*	2.262**	32.333*
	(38.416)	(63.690)	(0.995)	(1.130)	(16.857)
Census Shock$_{t-5}$	113.739***	204.833***	2.357*	1.964**	27.995*
	(42.031)	(78.616)	(1.254)	(0.993)	(15.135)
Census Shock$_{t-6}$	115.030**	248.589**	2.535	1.825*	22.502*
	(46.651)	(100.386)	(1.583)	(0.939)	(13.459)
Census Shock$_{t-7}$	134.454***	274.359**	2.984	1.626*	21.022
	(49.805)	(133.261)	(1.948)	(0.924)	(13.286)
Census Shock$_{t-8}$	153.243***	400.697***	3.743*	2.032**	21.776*
	(51.084)	(153.466)	(2.235)	(1.032)	(13.158)
Census Shock$_{t-9}$	171.330***	452.853***	5.269**	1.981**	26.007*
	(54.163)	(160.142)	(2.388)	(0.979)	(13.664)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. This table reports average treatment effects on the treated. See Section 4.2 for details and Appendix A for data sources. This table reports similar models to Table 3 for years $s = -9, \cdots, 9$. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.16: Event Study of Census Shock and Implied Multipliers Controlling for Lagged Outcomes

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per $1M
Census Shock$_{t+9}$	-21.296	-1.441	0.097	-0.004	0.290
	(109.551)	(85.444)	(1.066)	(0.249)	(3.206)
Census Shock$_{t+8}$	-39.547	-28.231	-0.362	-0.091	-1.194
	(89.622)	(81.245)	(0.940)	(0.260)	(3.097)
Census Shock$_{t+7}$	-31.568	-46.148	-0.073	-0.164	-0.266
	(78.220)	(70.485)	(0.761)	(0.255)	(2.781)
Census Shock$_{t+6}$	44.932	-38.238	0.017	-0.153	0.071
	(69.721)	(59.589)	(0.629)	(0.243)	(2.596)
Census Shock$_{t+5}$	-5.861	-50.329	-0.568	-0.231	-2.676
	(66.768)	(49.086)	(0.596)	(0.237)	(2.969)
Census Shock$_{t+4}$	-26.956	-29.258	-0.411	-0.156	-2.258
	(58.864)	(34.757)	(0.466)	(0.191)	(2.684)
Census Shock$_{t+3}$	-14.365	11.279	0.028	0.072	0.185
	(9.339)	(23.014)	(0.309)	(0.153)	(2.045)
Census Shock$_{t+2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t+1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-3}$	55.443**	51.712*	1.187***	1.659	39.143*
	(22.075)	(28.972)	(0.423)	(1.088)	(20.085)
Census Shock$_{t-4}$	90.760***	143.231***	2.063***	2.297*	34.017*
	(32.143)	(53.872)	(0.732)	(1.188)	(17.497)
Census Shock$_{t-5}$	129.807***	187.252***	2.691***	2.002*	29.573*
	(41.161)	(71.565)	(0.991)	(1.030)	(15.483)
Census Shock$_{t-6}$	131.143***	222.785**	2.927**	1.787*	24.126*
	(47.495)	(88.551)	(1.228)	(0.928)	(13.362)
Census Shock$_{t-7}$	152.989***	236.794**	3.452**	1.519*	22.762*
	(50.520)	(109.941)	(1.500)	(0.851)	(12.795)
Census Shock$_{t-8}$	168.426***	355.208***	4.260**	1.899**	23.410*
	(51.755)	(129.870)	(1.724)	(0.953)	(12.513)
Census Shock$_{t-9}$	184.535***	406.611***	5.779***	1.863**	27.221**
	(55.533)	(134.508)	(1.886)	(0.903)	(13.110)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. This table reports average treatment effects on the treated. See Section 4.2 for details and Appendix A for data sources. This table reports similar models to Table 4 for years $s = -9, \cdots, 9$. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.17: Event Study of Census Shock and Implied Multipliers: Average Treatment Effects

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per $1M
Census Shock$_{t+6}$	-24.348	-84.377	-0.666	-0.414	-3.194
	(115.182)	(84.534)	(0.807)	(0.388)	(3.542)
Census Shock$_{t+5}$	-83.213	-87.498	-1.100	-0.486	-5.594
	(134.167)	(73.256)	(0.717)	(0.407)	(4.192)
Census Shock$_{t+4}$	-150.144	-61.343	-0.849	-0.434	-5.215
	(168.319)	(64.527)	(0.646)	(0.390)	(4.230)
Census Shock$_{t+3}$	-20.713	-13.110	-0.267	-0.226	-2.607
	(14.382)	(46.725)	(0.423)	(0.336)	(3.668)
Census Shock$_{t+2}$	-11.823	-3.912	-0.146	-0.212	-2.307
	(7.816)	(23.721)	(0.211)	(0.266)	(3.670)
Census Shock$_{t+1}$	1.584	11.286	-0.133	-0.128	-2.938
	(5.703)	(10.652)	(0.190)	(0.279)	(4.359)
Census Shock$_{t}$	0.000	0.000	0.000	-0.364	-2.325
	(0.000)	(0.000)	(0.000)	(0.444)	(7.320)
Census Shock$_{t-1}$	6.063	13.449	-0.110	-0.317	-8.097
	(6.805)	(11.454)	(0.222)	(0.807)	(11.115)
Census Shock$_{t-2}$	-7.775	23.811	0.148	0.000	0.000
	(18.689)	(27.574)	(0.475)	(.)	(.)
Census Shock$_{t-3}$	43.008	73.456*	1.354**	1.519*	37.846***
	(38.877)	(38.184)	(0.688)	(0.805)	(13.924)
Census Shock$_{t-4}$	73.344	154.544***	2.103**	1.999**	30.679**
	(45.673)	(49.413)	(0.901)	(0.935)	(13.593)
Census Shock$_{t-5}$	113.263**	206.393***	2.831***	1.862*	28.068**
	(46.464)	(75.568)	(1.092)	(0.980)	(13.692)
Census Shock$_{t-6}$	115.333***	253.692**	3.135**	1.758*	23.434*
	(41.391)	(98.985)	(1.295)	(0.973)	(12.274)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. See Section 4.2 for details and Appendix A for data sources. This table reports average treatment effects using the same models as in Table 3. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.18: Event Study of Census Shock and Implied Multipliers Controlling for Lagged Outcomes: Average Treatment Effects

	(1) Federal Spending Growth	(2) Income Growth	(3) Employment Growth	(4) Income Multiplier	(5) Jobs per \$1M
Census Shock$_{t+6}$	11.365	-38.238	0.017	-0.153	0.071
	(69.941)	(59.482)	(0.740)	(0.254)	(3.042)
Census Shock$_{t+5}$	-10.431	-50.329	-0.568	-0.231	-2.676
	(76.124)	(53.507)	(0.707)	(0.281)	(3.961)
Census Shock$_{t+4}$	-20.323	-29.258	-0.411	-0.156	-2.258
	(71.140)	(36.510)	(0.556)	(0.212)	(3.529)
Census Shock$_{t+3}$	-8.734	11.279	0.028	0.072	0.185
	(5.667)	(20.881)	(0.313)	(0.137)	(2.033)
Census Shock$_{t+2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t+1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-1}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-2}$	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(.)	(.)
Census Shock$_{t-3}$	58.745**	51.712*	1.187***	1.659*	39.143**
	(23.942)	(26.901)	(0.455)	(0.971)	(18.106)
Census Shock$_{t-4}$	96.986***	143.231**	2.063**	2.297*	34.017*
	(34.419)	(57.122)	(0.859)	(1.227)	(18.170)
Census Shock$_{t-5}$	136.058***	187.252**	2.691**	2.002*	29.573*
	(40.503)	(84.666)	(1.165)	(1.172)	(17.486)
Census Shock$_{t-6}$	139.100***	222.785**	2.927**	1.787	24.126
	(37.172)	(109.815)	(1.417)	(1.131)	(15.627)
Observations	9,173	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. See Section 4.2 for details and Appendix A for data sources. This table reports average treatment effects using the same models as in Table 4. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.19: Event Study of Census Shock on Employment: Alternative Propensity Score Model

	(1) Employment Growth	(2) Jobs per $1M	(3) Employment Growth	(4) Jobs per $1M
Census Shock$_{t+6}$	-1.836	-7.775	-0.584	-2.446
	(1.572)	(6.994)	(1.434)	(6.330)
Census Shock$_{t+5}$	-1.808	-8.761	-0.686	-3.284
	(1.292)	(7.011)	(1.156)	(6.174)
Census Shock$_{t+4}$	-0.927	-5.499	0.083	0.462
	(1.031)	(5.926)	(0.849)	(4.680)
Census Shock$_{t+3}$	0.067	-0.205	0.885	5.930
	(0.728)	(5.163)	(0.559)	(3.907)
Census Shock$_{t+2}$	0.033	-0.527	0.000	0.000
	(0.409)	(4.352)	(0.000)	(.)
Census Shock$_{t+1}$	-0.111	-2.253	0.000	0.000
	(0.217)	(4.169)	(0.000)	(.)
Census Shock$_{t}$	0.000	-1.591	0.000	0.000
	(0.000)	(7.581)	(0.000)	(.)
Census Shock$_{t-1}$	-0.110	-6.722	0.000	0.000
	(0.253)	(12.000)	(0.000)	(.)
Census Shock$_{t-2}$	0.099	0.000	0.000	0.000
	(0.484)	(.)	(0.000)	(.)
Census Shock$_{t-3}$	1.428*	42.725**	1.212**	40.601**
	(0.759)	(17.769)	(0.473)	(19.764)
Census Shock$_{t-4}$	2.398**	36.956**	2.181**	36.551*
	(1.039)	(17.669)	(0.899)	(19.872)
Census Shock$_{t-5}$	3.161**	32.815*	2.806**	31.342*
	(1.279)	(17.116)	(1.226)	(18.823)
Census Shock$_{t-6}$	3.582**	27.997*	2.925*	24.508
	(1.557)	(15.667)	(1.518)	(16.523)
Observations	9,173	9,173	9,173	9,173

Notes: This table presents semi-parametric estimates of dynamic effect of a Census Shock. See Section 4.2 for details and Appendix A for data sources. Columns (1) and (3) of this table correspond to columns (3) in Tables 3 and 4, respectively, except that the propensity score model does not include $\Delta Emp_{c,t-6}$. Columns (2) and (4) report the corresponding estimates of jobs per $1M. Bootstrap statistics based on 2000 bootstrap samples. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.20: OLS Reduced Form Effects of Census Shock on Income Growth

	(1)	(2)	(3)	(4)	(5)	(6)
Census Shock	1362.077***	1407.829***	963.102***	963.292**	974.625**	1276.660***
	(404.962)	(416.797)	(355.326)	(362.794)	(388.625)	(361.855)
Migration Shifter		-3.121		-2.618		-9.239**
		(2.374)		(2.140)		(3.780)
Industry Shifter		-7.110		-51.533**		17.285
		(19.931)		(19.977)		(28.333)
Employment Growth (-1,1)			10.333***	10.917***		-11.646
			(1.980)	(2.094)		(9.099)
Income Growth (-1,1)			0.115*	0.127*		0.230***
			(0.068)	(0.066)		(0.084)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from OLS regressions of income growth between years 2 and 5. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.21: OLS Reduced Form Effects of Census Shock on Employment Growth

	(1)	(2)	(3)	(4)	(5)	(6)
Census Shock	21.109***	21.655***	13.860**	13.761**	14.138**	19.298***
	(6.342)	(6.545)	(5.629)	(5.720)	(6.006)	(5.347)
Migration Shifter		-0.069*		-0.062*		-0.161***
		(0.039)		(0.034)		(0.056)
Industry Shifter		-0.036		-0.666**		0.273
		(0.303)		(0.304)		(0.343)
Employment Growth (-1,1)			0.267***	0.281***		-0.037
			(0.032)	(0.033)		(0.118)
Income Growth (-1,1)			-0.000	0.000		0.001
			(0.001)	(0.001)		(0.001)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from OLS regressions of employment growth between years 2 and 5. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.22: IV Estimates of Income Multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending	1.973**	2.054**	2.377***	1.719***	2.106***	2.086***
	(0.856)	(0.876)	(0.851)	(0.618)	(0.709)	(0.683)
Migration Shifter		-4.286			84.600	59.387
		(38.774)			(59.183)	(51.638)
Industry Shifter		-72.669			-401.884	-506.146
		(483.908)			(412.874)	(351.981)
Employment Growth $(-1, 1)$			-96.199**	-101.613***	-191.087	-170.852
			(39.659)	(35.209)	(127.023)	(121.517)
Income Growth $(-1, 1)$			1.606**	0.537	1.160	0.746
			(0.802)	(0.710)	(0.905)	(0.799)
Employment Growth $(-3, 0)$				2.904	29.950	20.680
				(51.190)	(55.016)	(47.242)
Income Growth $(-3, 0)$				2.643***	2.403**	2.540***
				(1.025)	(0.997)	(0.947)
Observations	9,103	9,103	9,103	9,103	9,103	9,103
First-Stage F Stat	16.019	14.466	17.819	14.757	14.718	16.210
P-Value Hausman Test	0.029	0.021	0.003	0.012	0.002	0.001
P-score Control	Y	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from IV regressions of income growth between years 2 and 5. The coefficient on Federal Spending is multiplied by 1000 to represent the number of jobs from an additional $1M in spending. All regressions include propensity score and interactions with explanatory variables as in Wooldridge (2010, Eqn. 21.52). See Section 4.3 for details and Section A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.23: IV Estimates of Jobs per $1M and Cost per Job

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending	28.071**	28.298**	33.171***	22.961**	27.980***	29.018***
	(13.092)	(13.413)	(12.667)	(9.072)	(10.421)	(8.712)
Migration Shifter		-0.033			1.170	1.128*
		(0.686)			(0.762)	(0.626)
Industry Shifter		-0.738			-3.447	-4.589
		(7.248)			(6.215)	(5.540)
Employment Growth $(-1, 1)$			-0.466	-0.776	-2.337	-1.860
			(0.545)	(0.493)	(1.933)	(1.826)
Income Growth $(-1, 1)$			0.010	-0.001	0.010	0.006
			(0.011)	(0.009)	(0.013)	(0.012)
Employment Growth $(-3, 0)$				0.925	1.273*	1.110*
				(0.711)	(0.720)	(0.633)
Income Growth $(-3, 0)$				0.022*	0.018	0.021**
				(0.012)	(0.012)	(0.010)
Cost per Job	35.625**	35.339**	30.147***	43.553**	35.74***	34.461***
	(16.615)	(16.75)	(11.512)	(17.209)	(13.311)	(10.345)
Observations	9,103	9,103	9,103	9,103	9,103	9,103
First-Stage F Stat	16.019	14.466	17.819	14.757	14.718	16.210
P-Value Hausman Test	0.045	0.044	0.006	0.022	0.005	0.001
P-score Control	Y	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from IV regressions of employment growth between years 2 and 5. The coefficient on Federal Spending is multiplied by 1000 to represent the number of jobs from an additional $1M in spending. All regressions include propensity score and interactions with explanatory variables as in Wooldridge (2010, Eqn. 21.52). See Section 4.3 for details and Section A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.24: OLS Estimates of Income Multiplier

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending Growth	0.188***	0.187***	0.158***	0.153***	0.172***	0.159***
	(0.038)	(0.038)	(0.032)	(0.031)	(0.035)	(0.033)
Migration Shifter		-2.958		-2.502		-13.133***
		(2.334)		(2.108)		(4.045)
Industry Shifter		-8.963		-52.702***		39.170
		(18.300)		(18.589)		(28.557)
Employment Growth (-1,1)			10.420***	10.907***		-22.125**
			(2.030)	(2.133)		(10.042)
Income Growth (-1,1)			0.112*	0.125*		0.264***
			(0.067)	(0.065)		(0.086)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from OLS regressions of income growth between years 2 and 5. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.25: OLS Estimates of Jobs per $1M and Cost per Job

	(1)	(2)	(3)	(4)	(5)	(6)
Federal Spending Growth	2.836***	2.820***	2.350***	2.255***	2.572***	2.362***
	(0.544)	(0.540)	(0.500)	(0.464)	(0.503)	(0.470)
Migration Shifter		-0.066*		-0.060*		-0.219***
		(0.039)		(0.034)		(0.059)
Industry Shifter		-0.064		-0.682**		0.599*
		(0.277)		(0.282)		(0.341)
Employment Growth (-1,1)			0.268***	0.281***		-0.193
			(0.033)	(0.034)		(0.128)
Income Growth (-1,1)			-0.000	-0.000		0.002**
			(0.001)	(0.001)		(0.001)
Cost per Job	352605***	354604***	425569***	443547***	388760***	423298***
	(67665)	(67924)	(90546)	(91202)	(75965)	(84282)
Observations	9,177	9,103	9,177	9,103	9,103	9,103
P-score Control					Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	
Year Fixed Effects	Y	Y	Y	Y	Y	
State-Year Fixed Effects						Y

Notes: This table reports results from OLS regressions of employment growth between years 2 and 5. The coefficient on Federal Spending is multiplied by 1000 to represent the number of jobs from an additional $1M in spending. See Section 4.3 for details and Appendix A for data sources. Standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.26: Multinomial Logit for Propensity Scores with Spillovers: Closest 10 Neighbors

	(1)		
	Treatment Level		
	1	2	3
Own Lagged Income Growth	-0.059***	-0.029*	-0.038**
	(0.017)	(0.017)	(0.016)
Own Lagged Employment Growth	4.512***	6.450***	7.488***
	(0.980)	(0.972)	(0.945)
Own Industry Shifter	-57.044***	-0.684	-42.814***
	(7.545)	(7.352)	(7.073)
Own Migration Shifter	-4.568***	3.460***	0.896
	(0.695)	(0.690)	(0.660)
Other Lagged Income Growth	-0.015	-0.058***	-0.050***
	(0.017)	(0.017)	(0.016)
Other Lagged Employment Growth	5.839***	3.437***	8.298***
	(0.981)	(0.999)	(0.955)
Other Industry Shifter	1.341	-44.866***	-31.883***
	(7.652)	(7.818)	(7.329)
Other Migration Shifter	4.191***	-3.595***	0.755
	(0.703)	(0.703)	(0.671)
Observations	91,428		

Notes: This table reports results from a multinomial logit model of treatment effect status relative to treatment level 0. See Section 5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.27: Marginal Effects of Multinomial Logit for Propensity Scores with Spillovers: Closest 10 Neighbors

	(1)		
	Treatment Level		
	1	2	3
Own Lagged Income Growth	-0.059***	-0.029*	-0.038**
	(0.017)	(0.017)	(0.016)
Own Lagged Employment Growth	4.512***	6.450***	7.488***
	(0.980)	(0.972)	(0.945)
Own Industry Shifter	-57.044***	-0.684	-42.814***
	(7.545)	(7.352)	(7.073)
Own Migration Shifter	-4.568***	3.460***	0.896
	(0.695)	(0.690)	(0.660)
Other Lagged Income Growth	-0.015	-0.058***	-0.050***
	(0.017)	(0.017)	(0.016)
Other Lagged Employment Growth	5.839***	3.437***	8.298***
	(0.981)	(0.999)	(0.955)
Other Industry Shifter	1.341	-44.866***	-31.883***
	(7.652)	(7.818)	(7.329)
Other Migration Shifter	4.191***	-3.595***	0.755
	(0.703)	(0.703)	(0.671)
Observations	91,428		

Notes: This table reports marginal effects from a multinomial logit model of treatment effect status relative to treatment level 0. See Section 5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.28: Multinomial Logit for Propensity Scores with Spillovers: MSA

	(1) Treatment Level		
	1	2	3
Own Lagged Income Growth	-0.007	-0.084***	-0.095***
	(0.007)	(0.007)	(0.007)
Own Lagged Employment Growth	-0.563	10.451***	10.248***
	(0.408)	(0.398)	(0.400)
Own Industry Shifter	9.501***	-34.830***	-23.854***
	(2.362)	(2.422)	(2.383)
Own Migration Shifter	0.006	1.725***	1.568***
	(0.241)	(0.242)	(0.241)
Other Lagged Income Growth	-0.083***	-0.007	-0.094***
	(0.007)	(0.007)	(0.007)
Other Lagged Employment Growth	10.387***	-0.570	10.133***
	(0.398)	(0.408)	(0.400)
Other Industry Shifter	-34.894***	9.401***	-24.263***
	(2.421)	(2.362)	(2.383)
Other Migration Shifter	1.677***	-0.056	1.417***
	(0.242)	(0.241)	(0.241)
Observations	547,986		

Notes: This table reports results from a multinomial logit model of treatment effect status relative to treatment level 0. See Section 5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.29: Marginal Effects of Multinomial Logit for Propensity Scores with Spillovers: MSA

	(1)		
	Treatment Level		
	1	2	3
Own Lagged Income Growth	-0.007	-0.084***	-0.095***
	(0.007)	(0.007)	(0.007)
Own Lagged Employment Growth	-0.563	10.451***	10.248***
	(0.408)	(0.398)	(0.400)
Own Industry Shifter	9.501***	-34.830***	-23.854***
	(2.362)	(2.422)	(2.383)
Own Migration Shifter	0.006	1.725***	1.568***
	(0.241)	(0.242)	(0.241)
Other Lagged Income Growth	-0.083***	-0.007	-0.094***
	(0.007)	(0.007)	(0.007)
Other Lagged Employment Growth	10.387***	-0.570	10.133***
	(0.398)	(0.408)	(0.400)
Other Industry Shifter	-34.894***	9.401***	-24.263***
	(2.421)	(2.362)	(2.383)
Other Migration Shifter	1.677***	-0.056	1.417***
	(0.242)	(0.241)	(0.241)
Observations	547,986		

Notes: This table reports marginal effects from a multinomial logit model of treatment effect status relative to treatment level 0. See Section 5 for details and Appendix A for data sources. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.30: Treatment Effects Estimates of Potential Outcomes with Spillovers

	(1) Closest 10 Counties			(2) MSA		
Treatment Level	Federal Spending	Employment	Income	Federal Spending	Employment	Income
0 - No Census Shocks	14.510	9.162***	466.431***	-13.496	7.206***	324.199***
	(3.664)	(0.095)	(6.424)	(1.492)	(0.038)	(2.256)
1- Only Neighbor has Census Shock	22.311*	9.418***	478.777***	-16.433	6.922***	310.715***
	(3.782)	(0.100)	(6.582)	(1.449)	(0.037)	(2.169)
2- Only Own Census Shock	46.746***	10.535***	549.789***	14.176	8.571***	393.048***
	(3.652)	(0.100)	(6.465)	(1.196)	(0.035)	(2.159)
3- Both Census Shocks	48.934***	10.847***	556.436***	10.089	8.387***	382.786***
	(3.296)	(0.089)	(5.822)	(1.200)	(0.035)	(2.125)
N	91428			547986		

Notes: This table reports means of potential outcomes from a multivalued treatment effects approach to analyzing spillover effects. All variables report growth between years 2 and 5. See Section 5 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.31: Estimated Quantiles of the Distributions of Potential Outcomes

Quantile	(1) Federal Spending	(2) Income	(3) Employment
5			
High Census Shock=0	-459.235***	-489.979***	-7.573***
	(21.053)	(23.521)	(0.378)
High Census Shock=1	-395.089***	-429.908***	-5.867***
	(15.998)	(19.933)	(0.389)
15			
High Census Shock=0	-169.599***	-151.747***	-1.513***
	(7.851)	(12.402)	(0.204)
High Census Shock=1	-130.248***	-97.082***	-0.498***
	(5.495)	(11.969)	(0.186)
25			
High Census Shock=0	-72.327***	29.708***	1.549***
	(4.010)	(7.342)	(0.166)
High Census Shock=1	-51.938***	66.938***	2.496***
	(3.343)	(10.120)	(0.180)
50			
High Census Shock=0	29.818***	349.956***	7.416***
	(2.915)	(9.466)	(0.148)
High Census Shock=1	40.162***	394.911***	8.373***
	(2.009)	(9.240)	(0.164)
75			
High Census Shock=0	126.179***	772.176***	14.628***
	(3.788)	(15.613)	(0.269)
High Census Shock=1	126.064***	799.071***	15.491***
	(3.459)	(13.323)	(0.252)
85			
High Census Shock=0	205.431***	1078.108***	20.230***
	(6.526)	(21.273)	(0.359)
High Census Shock=1	204.841***	1117.224***	20.607***
	(6.234)	(19.110)	(0.315)
95			
High Census Shock=0	515.393***	2000.694***	34.605***
	(29.301)	(62.756)	(1.146)
High Census Shock=1	488.247***	1941.322***	33.605***
	(28.628)	(44.658)	(0.956)
Observations	9,099	9,099	9,099

Notes: This table reports quantiles of the distribution of potential outcomes. All variables report growth between years 2 and 5. See Section 6 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table E.32: Quantile Treatment Effects of a Census Shock

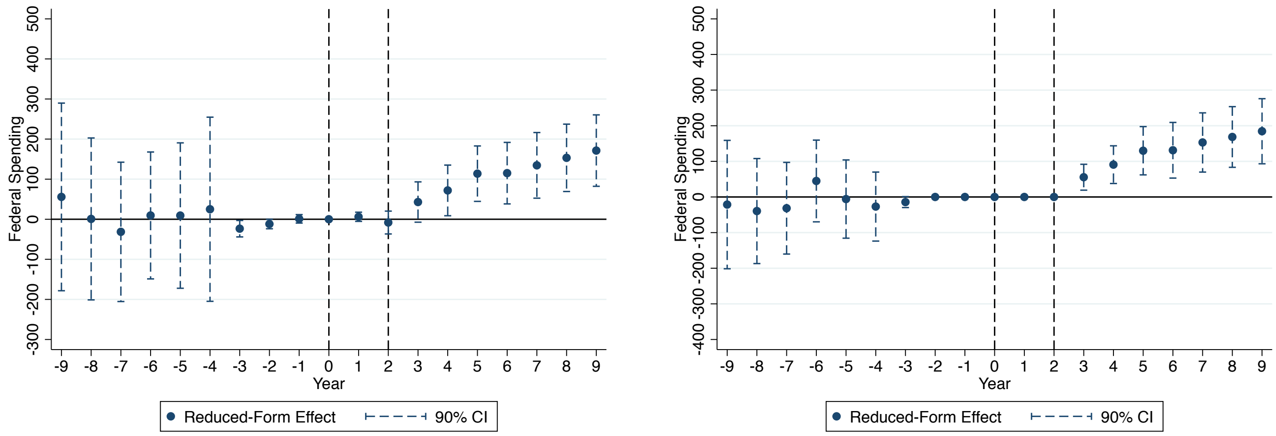
Effect at Quantile	(1) Federal Spending	(2) Income	(3) Employment
5	64.146**	60.071**	1.706***
	(26.150)	(30.561)	(0.535)
15	39.350***	54.665***	1.015***
	(9.664)	(17.227)	(0.278)
25	20.390***	37.230***	0.947***
	(5.272)	(12.433)	(0.247)
50	10.344***	44.955***	0.957***
	(3.575)	(12.902)	(0.223)
75	-0.114	26.895	0.862**
	(5.123)	(20.624)	(0.368)
85	-0.589	39.117	0.377
	(9.186)	(28.502)	(0.468)
95	-27.146	-59.372	-1.000
	(41.701)	(75.688)	(1.461)
Observations	9,099	9,099	9,099

Notes: This table reports differences in the quantiles of the distribution of potential outcomes. All variables report growth between years 2 and 5. See Section 6 for details and Appendix A for data sources. Bootstrapped standard errors that allow for arbitrary correlation at the state level are reported in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Figure E.1: Semi-parametric Reduced-form Effects on Federal Spending from Doubly-robust Estimation

(a) No RA for Lagged Outcomes

(b) Controlling for Lagged Outcomes

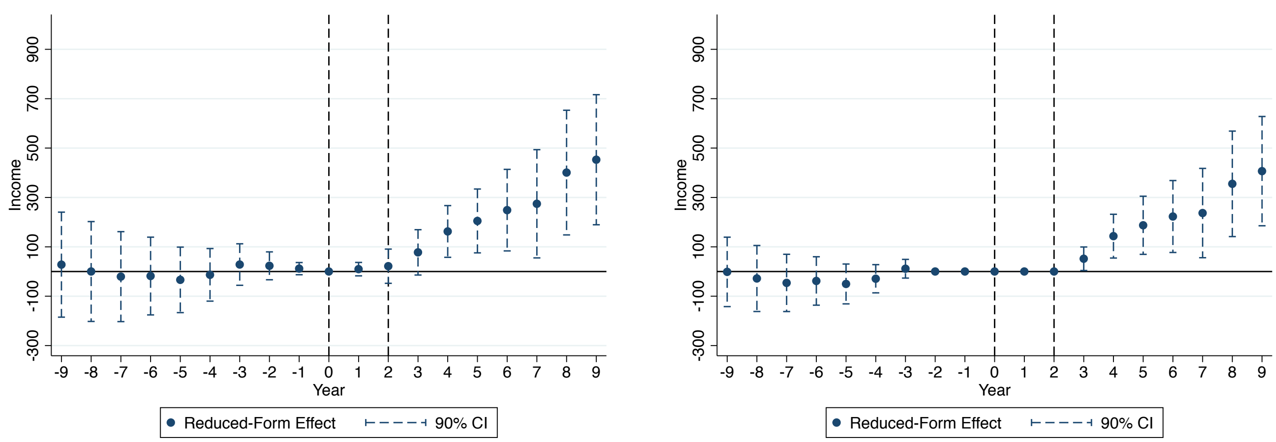


Notes: These figures plot reduced-form estimates of a Census Shock on federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (3) of Tables E.15 and E.16. See Section 4.2 for more details and Appendix A for data sources.

Figure E.2: Semi-parametric Reduced-form Effects on Income from Doubly-robust Estimation

(a) No RA for Lagged Outcomes

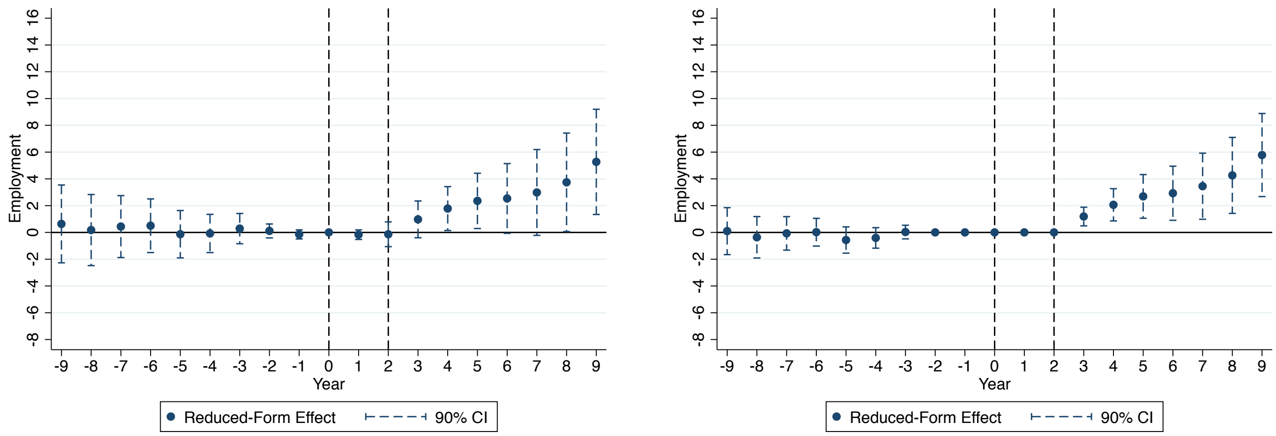
(b) Controlling for Lagged Outcomes



Notes: These figures plot reduced-form estimates of a Census Shock on income with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (2) of Tables E.15 and E.16. See Section 4.2 for more details and Appendix A for data sources.

Figure E.3: Semi-parametric Reduced-form Effects on Employment from Doubly-robust Estimation

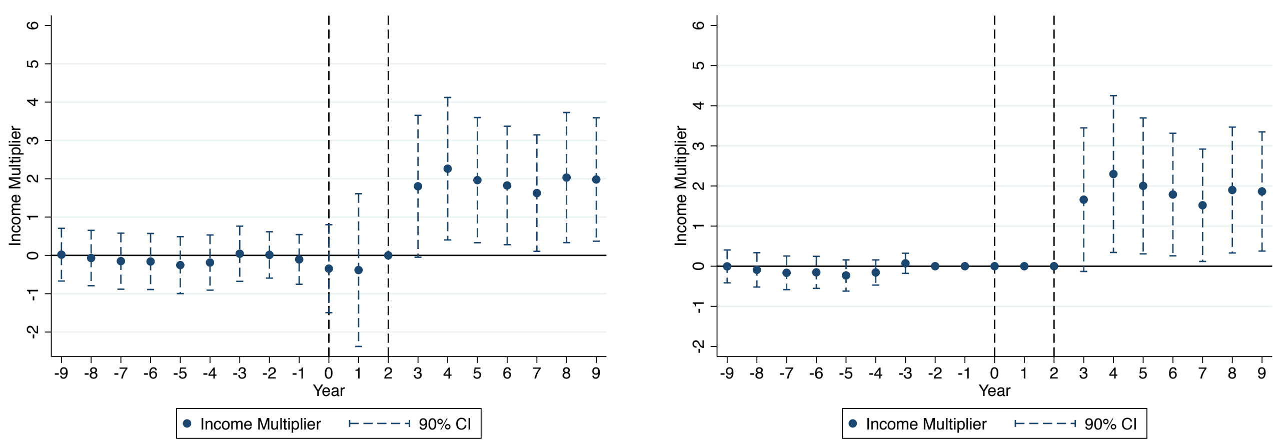
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot reduced-form estimates of a Census Shock on employment with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (3) of Tables E.15 and E.16. See Section 4.2 for more details and Appendix A for data sources.

Figure E.4: Semi-parametric Estimates of Income Multiplier from Doubly-robust Estimation

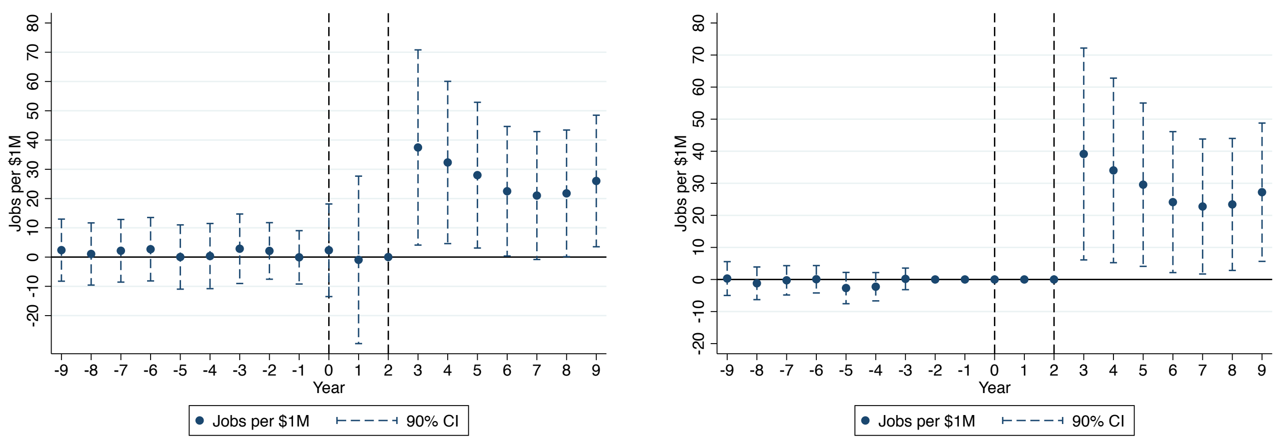
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated income multipliers of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (4) of Tables E.15 and E.16. See Section 4.2 for more details and Appendix A for data sources.

Figure E.5: Semi-parametric Estimates of Jobs per \$1M from Doubly-robust Estimation

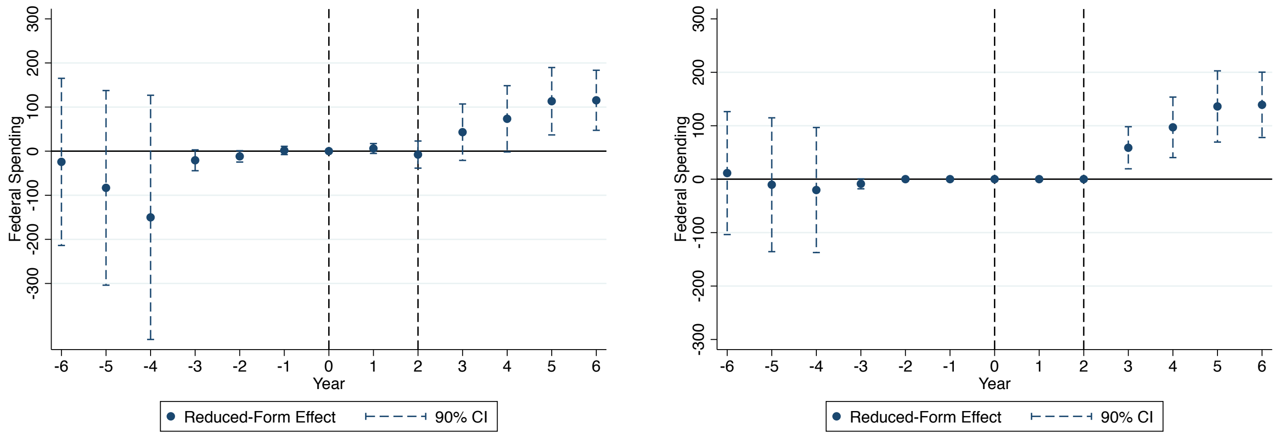
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated employment effects per an additional million dollars of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (5) of Tables E.15 and E.16. See Section 4.2 for more details and Appendix A for data sources.

Figure E.6: Semi-parametric Reduced-form Effects on Federal Spending from Doubly-robust Estimation: Average Treatment Effects

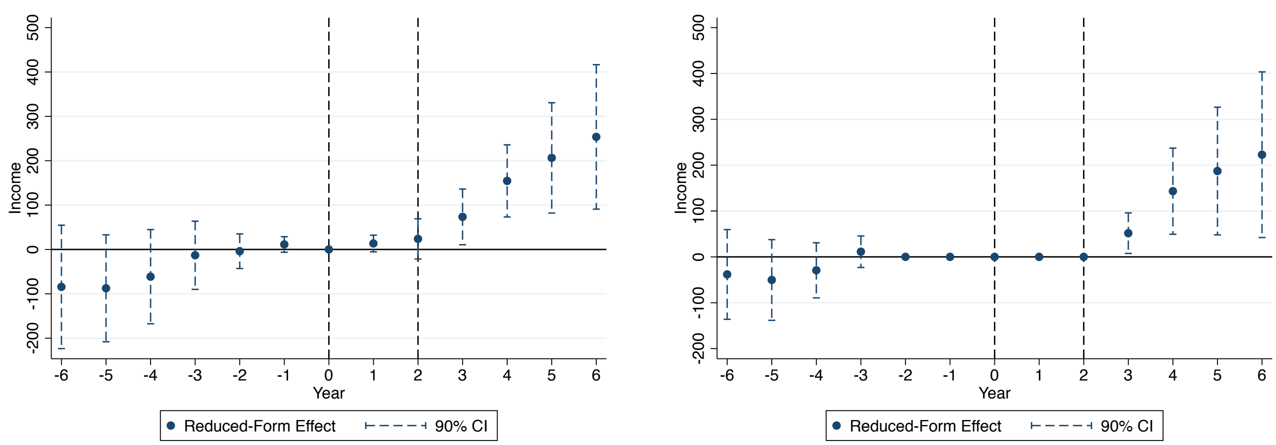
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot reduced-form estimates of a Census Shock on federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (3) of Tables E.17 and E.18. See Section 4.2 for more details and Appendix A for data sources.

Figure E.7: Semi-parametric Reduced-form Effects on Income from Doubly-robust Estimation: Average Treatment Effects

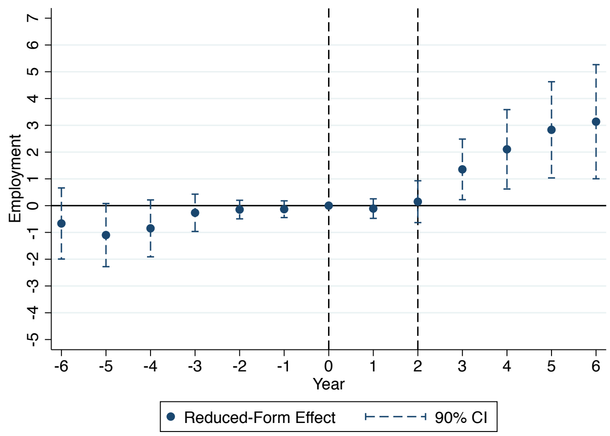
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



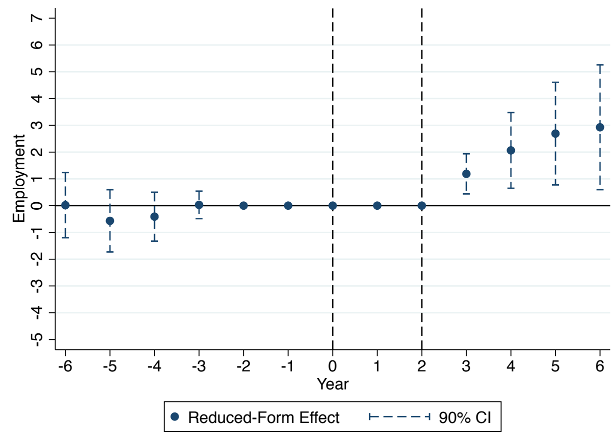
Notes: These figures plot reduced-form estimates of a Census Shock on income with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (2) of Tables E.17 and E.18. See Section 4.2 for more details and Appendix A for data sources.

Figure E.8: Semi-parametric Reduced-form Effects on Employment from Doubly-robust Estimation: Average Treatment Effects

(a) No RA for Lagged Outcomes



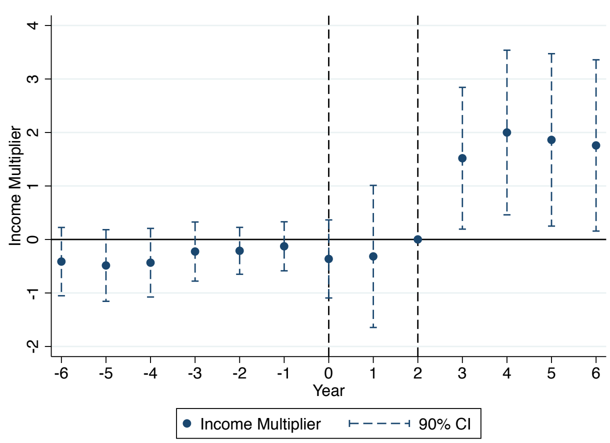
(b) Controlling for Lagged Outcomes



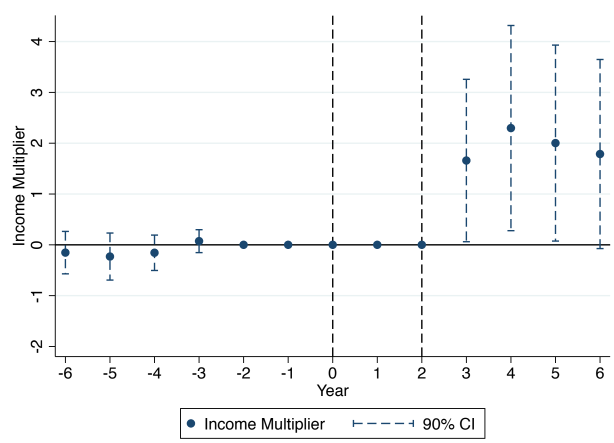
Notes: These figures plot reduced-form estimates of a Census Shock on employment with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (3) of Tables E.17 and E.18. See Section 4.2 for more details and Appendix A for data sources.

Figure E.9: Semi-parametric Estimates of Income Multiplier from Doubly-robust Estimation: Average Treatment Effects

(a) No RA for Lagged Outcomes



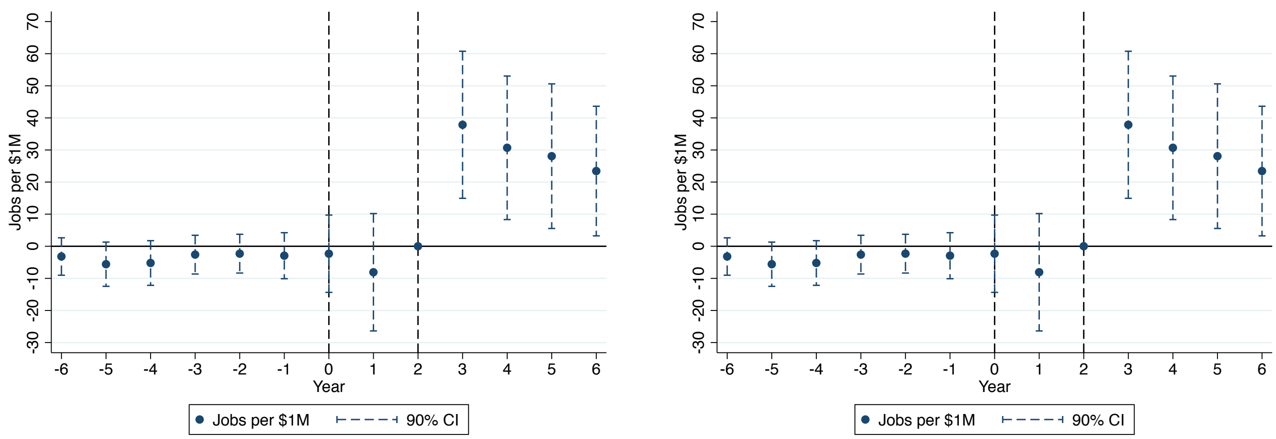
(b) Controlling for Lagged Outcomes



Notes: These figures plot estimated income multipliers of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (4) of Tables E.17 and E.18. See Section 4.2 for more details and Appendix A for data sources.

Figure E.10: Semi-parametric Estimates of Jobs per $1M from Doubly-robust Estimation: Average Treatment Effects

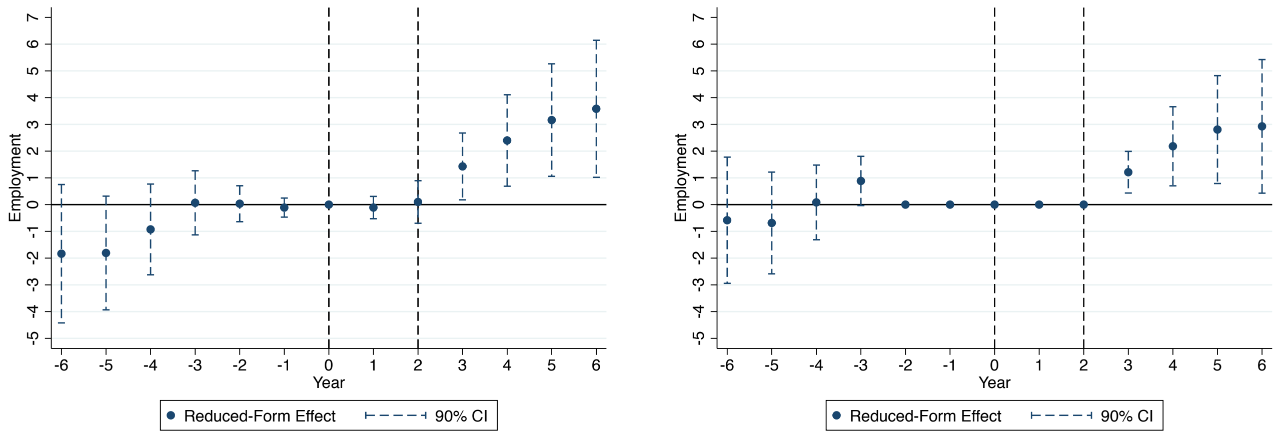
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot estimated employment effects per an additional million dollars of federal spending with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in column (5) of Tables E.17 and E.18. See Section 4.2 for more details and Appendix A for data sources.

Figure E.11: Semi-parametric Reduced-form Effects on Employment from Doubly-robust Estimation: Alternative Propensity Score

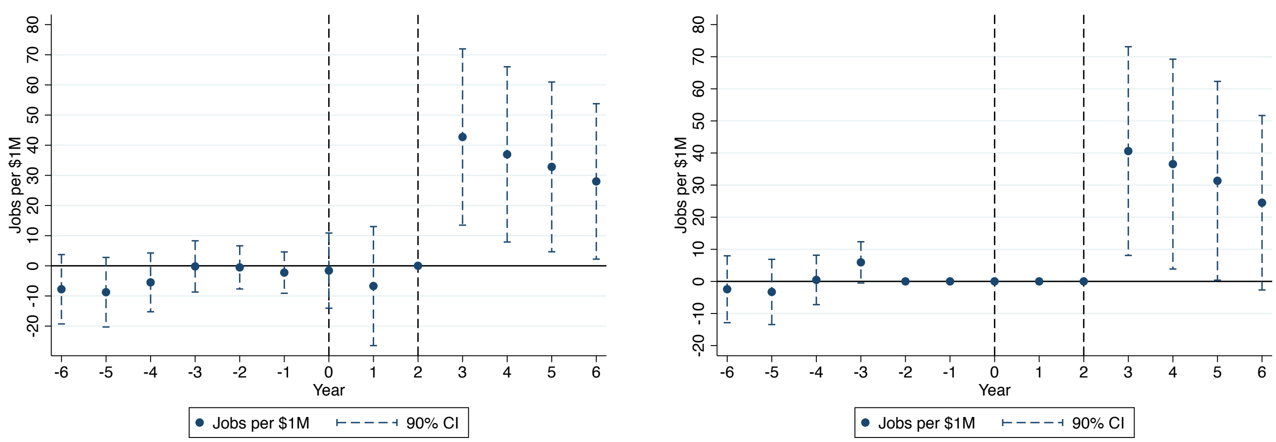
(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



Notes: These figures plot reduced-form estimates of a Census Shock on employment with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in Table E.19. See Section 4.2 for more details and Appendix A for data sources.

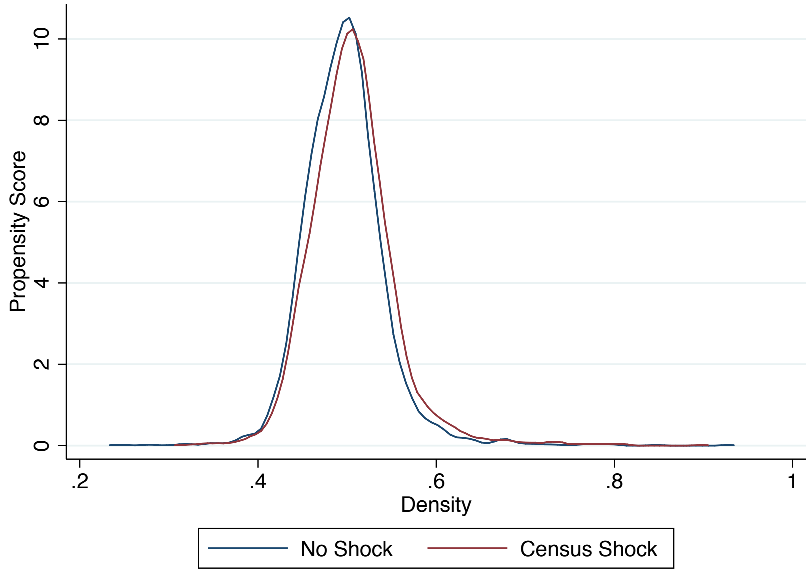
Figure E.12: Semi-parametric Estimates of Jobs per $1M from Doubly-robust Estimation: Alternative Propensity Score

(a) No RA for Lagged Outcomes (b) Controlling for Lagged Outcomes



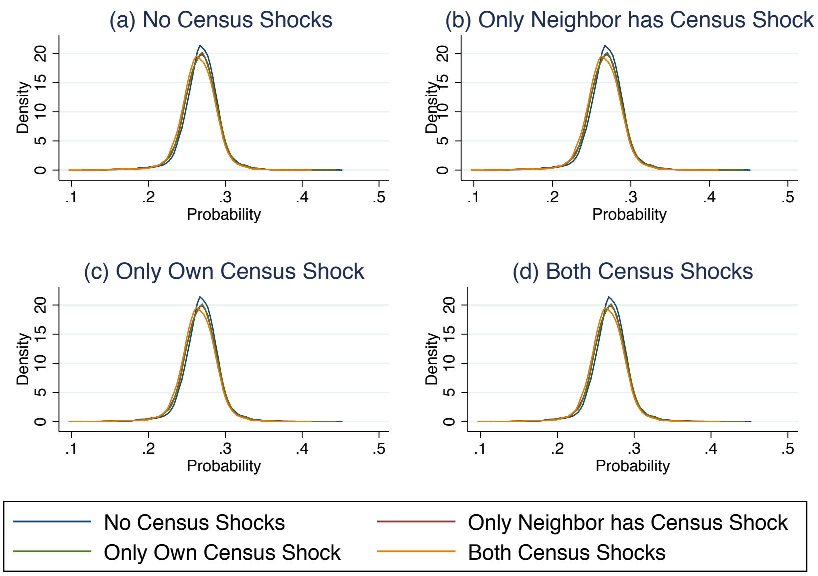
Notes: These figures plot reduced-form estimates of a Census Shock on employment with a 90% confidence interval. Panel (a) reports estimates that do not control for lagged outcomes. Panel (b) reports estimates that control for outcomes in years -2 to 2. Standard errors are bootstrapped and allow for arbitrary correlation at the state level. The plots are based on estimates reported in Table E.19. See Section 4.2 for more details and Appendix A for data sources.

Figure E.13: Smoothed Density for the Estimated Propensity Score of a Census Shock



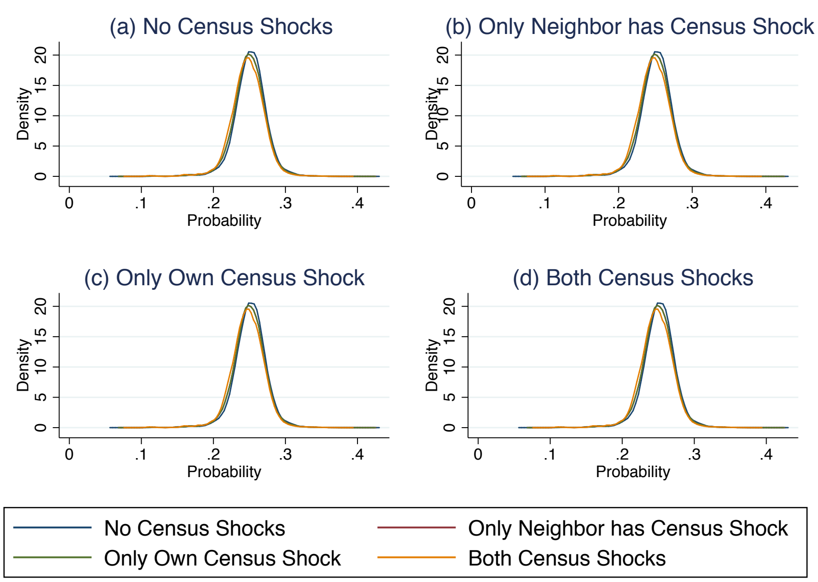
Notes: This figure plots the smoothed density of the estimated propensity score of Census Shock using a standard Epanechnikov kernel. See Section 3.5 for details and Appendix A for data sources.

Figure E.14: Smoothed Densities for the Estimated Propensity Score in Spillover Analysis: Closest 10 Neighbors



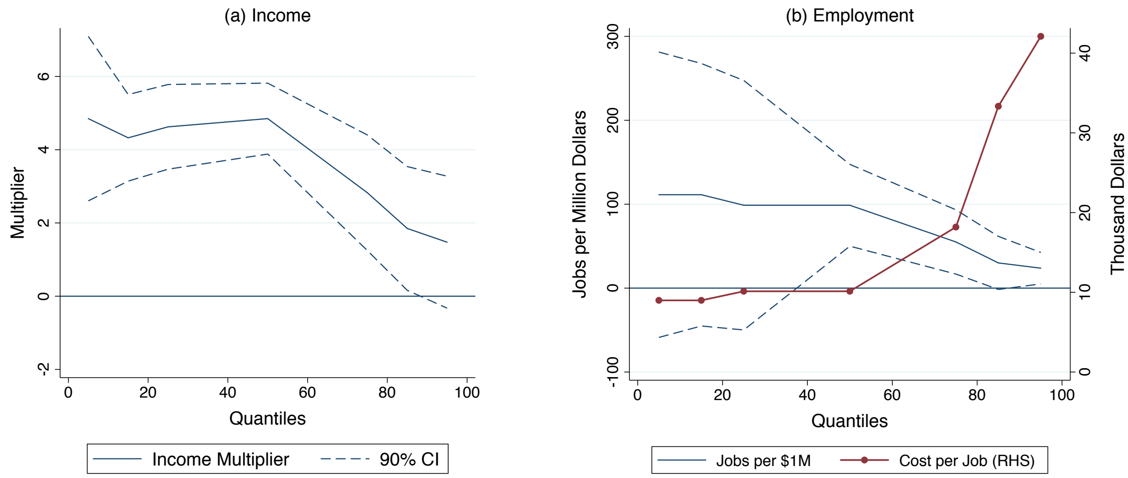
Notes: These figures plot the smoothed density of the estimated generalized propensity score for the spillover analysis using a standard Epanechnikov kernel. See Section 5 for details and Appendix A for data sources.

Figure E.15: Smoothed Densities for the Estimated Propensity Score in Spillover Analysis: MSA Level



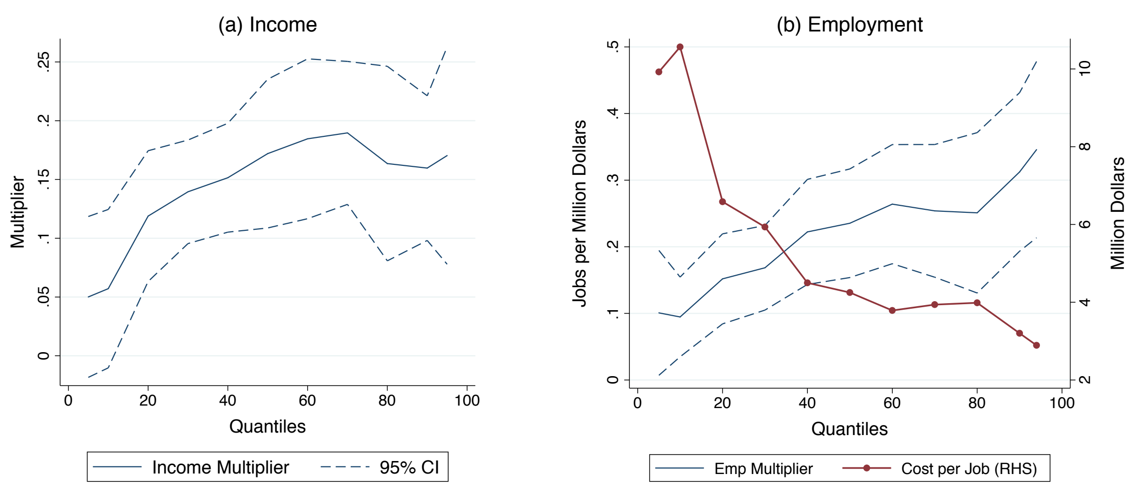
Notes: These figures plot the smoothed density of the estimated generalized propensity score for the spillover analysis using a standard Epanechnikov kernel. See Section 5 for details and Appendix A for data sources.

Figure E.16: IVQR Estimates of Spending Multipliers with State Fixed Effects



Notes: The figure plots the estimated multipliers from the IVQR analysis where we control for state fixed effects and estimated propensity scores along with a 90% confidence interval for 7 quantiles of the distribution of the dependent variable. See Section 6 for details and Appendix A for data sources. Panel (a) uses the average annual growth in local personal income from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Panel (b) uses the average annual growth in employment. Panel (b) also reports the cost per job created in dollars of 2009 on the right hand side axis at the corresponding quantiles. Standard errors are computed via bootstrap. See Figure E.17 for the corresponding quantile regression estimates.

Figure E.17: Quantile Regression Estimates of Spending Multipliers



Notes: The figure plots the estimated multipliers from the IVQR analysis along with a 90% confidence interval for 7 quantiles of the distribution of the dependent variable. See Section 6 for details and Appendix A for data sources. Panel (a) uses the average annual growth in local personal income from 1982 to 1985, 1992 to 1995 and 2002 to 2005. Panel (b) uses the average annual growth in employment. Panel (b) also reports the cost per job created in dollars of 2009 on the right hand side axis at the corresponding quantiles. Standard errors are computed via bootstrap. See Figure 11 for the corresponding IVQR estimates.