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The Impacts of Supplemental Nutrition Assistance Program Redemptions on County-Level Employment

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Abstract

This study investigates the impacts of USDA's Supplemental Nutrition Assistance Program (SNAP) redemptions (the value of SNAP benefits redeemed by SNAP-authorized stores) on metro and nonmetro county-level employment from 2001 to 2014. SNAP redemptions had a positive average impact on county-level employment over the entire study period in nonmetro counties (about 0.4 additional job per $10,000 of additional SNAP redemptions) but no measurable impact overall in metro counties. The impacts of SNAP redemptions during and immediately after the Great Recession (2008-10) differed from impacts in nonrecession years in both nonmetro and metro counties. During the recession, an additional $10,000 of SNAP redemptions led to about 1.0 additional job on average in nonmetro counties and about 0.4 additional job in metro counties. Prior to the recession (2001-07), the SNAP impact was positive in nonmetro counties (0.2 job per $10,000) but negative in metro counties (-0.2 job per $10,000). After the recession (2011-14), SNAP redemptions had a statistically insignificant impact in both nonmetro and metro counties. Moreover, during the Great Recession, the impacts per dollar of SNAP redemptions were greater than impacts of other Federal or State government transfer payments combined and greater than the impacts of all Federal Government spending combined. SNAP redemptions also had positive effects on employment in neighboring nonmetro counties but not in neighboring metro counties. These results were robust across several econometric methods.

Keywords: Supplemental Nutrition Assistance Program, SNAP, economic impacts of SNAP, county employment, SNAP employment impacts, rural employment, ARRA, American Recovery and Reinvestment Act

Acknowledgments

The authors would like to thank Leah Williams of Northrup Grumman Corporation and Rebecca Mason of Fitch Ratings (both former USDA, Economic Research Service (ERS) interns) for valuable research assistance and Cynthia Nickerson of ERS for helpful comments and suggestions on this report. They also thank the following internal and external peer reviewers of this report: Brian Stacy of USDA, ERS; Ashley Hungerford of USDA, Office of the Chief Economist; Danielle Deemer of USDA, Food and Nutrition Service; Craig Gundersen of the University of Illinois; and one anonymous technical peer reviewer. Thanks also to John Weber and Andres Guerrero, USDA, ERS, for editorial and design services.

This project was conducted with restricted access to Bureau of Labor Statistics (BLS) and USDA, Food and Nutrition Service (FNS) data. The views expressed here do not necessarily reflect the views of BLS or FNS.

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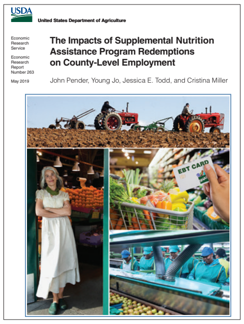


The Impacts of Supplemental Nutrition Assistance Program Redemptions on County-Level Employment

John Pender, Young Jo, Jessica E. Todd, and Cristina Miller

What Is the Issue?

The Supplemental Nutrition Assistance Program (SNAP)—previously called the Food Stamp Program—is the third-largest means-tested Federal program (in terms of outlays) and is the largest USDA program. Inflation-adjusted SNAP payments nearly quadrupled between 2001 and 2013, in part due to changes in policies intended to help stimulate the economy during and after the Great Recession. Increased SNAP benefits may increase economic output and employment by stimulating demand for food and through multiplier effects on other economic sectors, particularly during a recession. A large increase in SNAP benefits authorized by the American Recovery and Reinvestment Act (ARRA) of 2009 was predicted to have the largest impacts on gross domestic product (GDP) per dollar spent of any of the spending authorized by ARRA, mainly because SNAP benefits are targeted to low-income people, who have a high propensity to spend rather than save. Although the stimulus impacts of SNAP payments have been predicted using national economic simulation models, no published studies have investigated the actual impacts of these payments after the fact using statistical methods.

This study fills that gap by using statistical analysis to estimate the impact of SNAP redemptions (the value of SNAP benefits redeemed by SNAP-authorized stores) on county-level employment from 2001 to 2014. The average impact is estimated for this entire period, as well as for three subperiods: prior to the Great Recession (2001-07), a period including the recession and its immediate aftermath (2008-10), and after the recession (2011-14). The use of county-level data enables researchers to determine the extent to which impacts differ between metro and nonmetro counties and the extent to which SNAP redemptions in one county affect employment in neighboring counties ("spillover effects"). The study also compares the impacts of SNAP redemptions to the impacts of other transfer payments and other Federal spending in general on employment.

What Did the Study Find?

SNAP redemptions per capita grew rapidly between 2001 and 2011, more than tripling in inflation-adjusted terms, then declined by about 12 percent between 2011 and 2014. The value of SNAP redemptions per capita varied widely across counties.

Key findings include the following:

- **During the 2001 to 2014 period, SNAP redemptions had a positive average impact on county-level employment in nonmetro counties but a statistically insignificant impact in metro counties**. Employment increased by about 0.4 job per $10,000 of additional SNAP redemptions on average in nonmetro counties, while the estimated impact in metro counties was much smaller (0.05 job per $10,000 of SNAP redemptions) and statistically insignificant.

ERS is a primary source of economic research and analysis from the U.S. Department of Agriculture, providing timely information on economic and policy issues related to agriculture, food, the environment, and rural America.

- **The impact of SNAP redemptions was much greater during the recession period (2008-10) in both nonmetro and metro counties**. SNAP boosted employment by about 1.0 job per $10,000 of additional SNAP redemptions in nonmetro counties and by about 0.4 job per $10,000 of additional SNAP redemptions in metro counties during the recession. Prior to the recession (2001-07), SNAP redemptions increased employment in nonmetro counties by about 0.2 job per $10,000 of SNAP redemptions but reduced employment in metro counties by about 0.2 job per $10,000 of SNAP redemptions. The impacts of SNAP redemptions after the recession (2011-14) were statistically insignificant for both nonmetro and metro counties.

- **During the Great Recession, the impacts of SNAP redemptions per dollar spent were larger than impacts per dollar spent on other Federal or State government transfer payments combined—including Social Security, Medicare, Medicaid, unemployment insurance compensation, veterans' benefits and other government transfer payments to individuals—and were much larger than the impacts of total Federal Government spending per dollar spent.** The estimated average impact of other government transfer payments in all counties during the recession was less than 0.2 job per $10,000, while that for all other Federal Government spending besides SNAP and other transfer payments during the recession was much smaller.

- **In nonmetro counties over the entire study period, SNAP redemptions in neighboring counties had as large an impact on local employment as SNAP redemptions in the same county (0.4 job per $10,000).** In contrast, findings reveal no such spillover effects for all counties on average or in metro counties.

The finding of relatively large impacts of SNAP on economic activity during the recession is consistent with previous studies based on national economic models, but the size of impacts estimated in this study are larger. However, this study's findings are not strictly comparable to those of prior studies that estimated national-level impacts of SNAP, since county-level impacts may represent to some extent shifts in economic activity across counties that could have smaller aggregate effects on national-level employment.

How Was the Study Conducted?

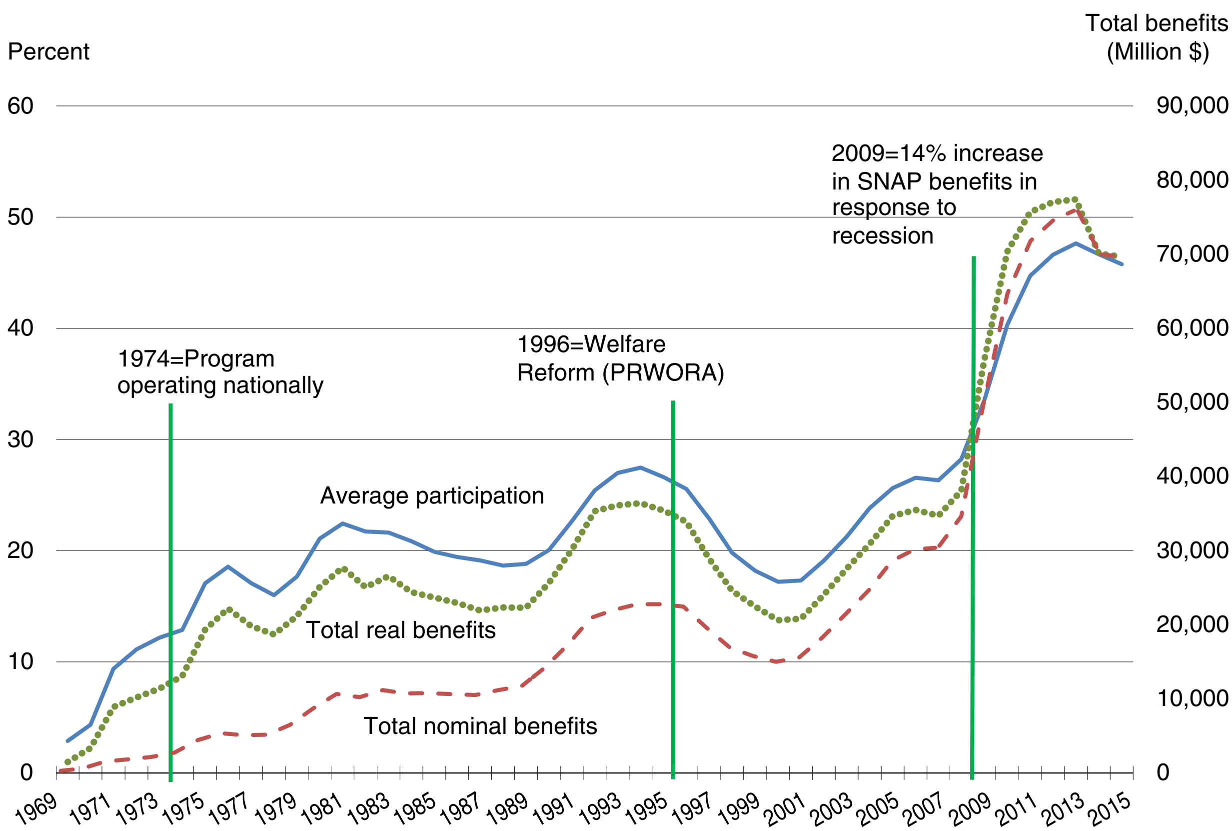
This study used data on employment and transfer payments from the U.S. Bureau of Economic Analysis (BEA), data on SNAP redemptions from USDA's Food and Nutrition Service, and ERS data on SNAP policies. The study also used county-level demographic variables and Federal Government spending data from the U.S. Census Bureau. This study's measure of SNAP spending may be less subject to measurement error than data on other types of government spending, which tends to reduce impact estimates in statistical analysis. The SNAP redemptions data used by ERS are subject to audit and of high quality. "Employment" is defined by the BEA as a "count of jobs, both full-time and part-time," and "includes wage and salary jobs, sole proprietorships, and individual general partners, but not unpaid family workers or volunteers." ERS researchers used a variety of statistical methods to estimate impacts of SNAP redemptions on employment, but the preferred model was the ordinary least squares second difference (OLS-SD) estimator. In alternative analyses, researchers addressed concerns about potential reverse causality (changes in employment may affect as well as be affected by SNAP redemptions) using instrumental variables estimators, which produced estimates that were as large as or larger than the estimates produced by the OLS-SD model.

Introduction

USDA's Supplemental Nutrition Assistance Program (SNAP)—previously called the Food Stamp Program—is the third-largest (in terms of outlays) means-tested Federal program (after Medicaid and the Earned Income Tax Credit) (CBO, 2018) and is the largest USDA program, providing benefits to an average of more than 45 million recipients per month and accounting for 51 percent of USDA's budget outlays in FY 2016.[1] Between 2000 and 2013, average monthly SNAP participation nearly tripled (from 17.2 million to 47.6 million), while the inflation-adjusted value of benefits paid under the program nearly quadrupled (from $19.9 billion to $78.4 billion in 2015 dollars) (fig 1).

Figure 1

Participation and benefits paid in the Food Stamp Program/SNAP



Notes: PRWORA = Personal Responsibility and Work Opportunity Reconciliation Act. SNAP = Supplemental Nutrition Assistance Program. Nominal values deflated by the annual Consumer Price Index for all items, city average. Inflation-adjusted values are in 2015 dollars.

Source: USDA, Economic Research Service using SNAP Participation and Costs, 1969-2015, accessed from USDA, Food and Nutrition Service (FNS) website on July 28, 2016, and USDA-FNS (2014).

[1] This estimate is based on total SNAP costs of $70.9 billion in fiscal year 2016 reported by USDA's Food and Nutrition Service (USDA-FNS, 2018) and total USDA outlays of $138.2 billion in fiscal year 2016 (USDA, 2017).

The growth in the program resulted both from changes in economic conditions, especially increases in unemployment and poverty during and after the Great Recession of 2008-09, and from changes in Federal and State policies promoting participation in the program and increasing benefit rates. A particularly large increase in benefit rates began in April 2009 after enactment of the American Recovery and Reinvestment Act (ARRA), which increased the maximum payments to SNAP participants by 13.6 percent, initially increasing the maximum monthly benefit payment to a family of four by about $80.[2]

The increase in SNAP payments authorized by ARRA was predicted to have a relatively large impact on economic activity. For example, Zandi (2009) estimated that increases in SNAP benefits in ARRA would have the largest national multiplier impact on Gross Domestic Product (GDP) per dollar spent ($1.73 of GDP per $1.00 of SNAP spending) of any of the program increases in the Act considered.[3] The main reason for this large expected impact was the high propensity of SNAP recipients to spend additional SNAP benefits, boosting demand in the economy during a severe recession. Hanson (2010) predicted a similar impact of SNAP on GDP during the recession ($1.79 of GDP per $1.00 of SNAP spending) as well as a large employment impact (an increase of 19,800 full- or part-time jobs per $1 billion of SNAP spending).

The economic impacts of SNAP payments could be larger in nonmetro counties, given that poverty rates are generally higher in these counties—implying that a larger share of the nonmetro population is likely to be eligible for the program—and participation rates among those eligible are higher in those areas.[4] For example, in 2010, the SNAP participation rate was 82 percent in rural areas and 70 percent in urban areas (Gray and Cunnyngham, 2016), and the weighted (by population) average SNAP redemption per capita was $232 in nonmetro counties and $211 in metro counties.[5]

These averages mask wide variation in SNAP redemptions per capita across counties, with annual redemptions reaching more than $500 per capita in many high-poverty rural counties of the South, Appalachia, the Southwest, and Native American reservations and Tribal areas in 2010 (fig. 2). SNAP redemptions at such levels may dwarf the value of economic development grants and loans or commodity program payments flowing into many rural areas. Furthermore, as noted earlier,

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[2] The increase in SNAP benefit rates mandated by ARRA ended on October 31, 2013.

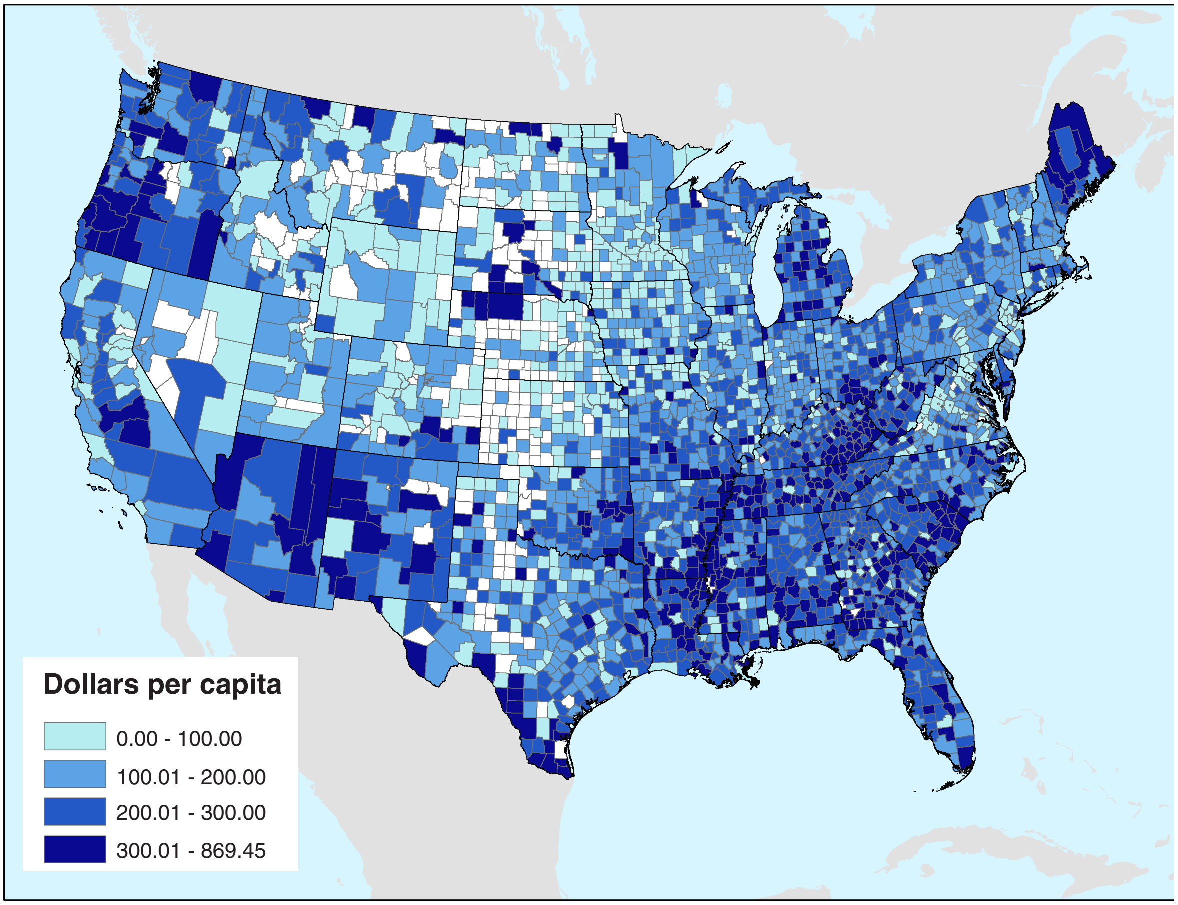
[3] According to Hanson (2010, p. 20), Zandi (2009) and a study by Romer and Bernstein (2009) providing GDP multiplier estimates for Government expenditures and tax cuts "dominated discussions among Congress and the Obama administration about the expected impact of ARRA expenditures."

[4] In this study, we investigate impacts of SNAP redemptions in metropolitan (metro) and nonmetropolitan (nonmetro) counties separately as well as in all counties in aggregate. The classification of counties as metro or nonmetro is based on the 2003 classification by the Office of Management and Budget (OMB). Metro and nonmetro designations are often taken as synonyms for "urban" and "rural" areas, though other classifications of urban and rural areas are also used. For example, the urban and rural classification used by Gray and Cunnyngham (2016) in estimating SNAP participation in urban and rural areas is based on the U.S. Census Bureau's definition of these areas, which uses only population size and density and does not depend on commuting linkages. In this study, we use "metro" and "nonmetro" when referring specifically to the OMB classification and "urban" and "rural" when referring to the U.S. Census Bureau classification or to the more generic concepts of urban and rural. For more information on defining urban versus rural areas, see the Rural Economy & Population topic page on the ERS website.

[5] SNAP redemptions are the value of SNAP benefits redeemed by SNAP-authorized stores. We use SNAP redemptions rather than SNAP benefits in this study because the administrative data on SNAP redemptions are of high quality—being based on electronic benefits transfer (EBT) records and subject to audit—and are associated with the location in which SNAP benefits are spent rather than where they are received. We hypothesize that the local county-level impacts of SNAP funds are likely to be focused more in the county where the benefits are redeemed than in the county where the SNAP recipient resides (as SNAP benefits data are reported), in cases where the county of receipt and redemption of SNAP benefits differ.

SNAP redemptions could have a larger economic stimulus impact than many other forms of government spending per dollar spent, especially during a recession, because they are paid directly to low-income people. SNAP benefits are also generally redeemed quickly by recipients,[6] which also may result in greater near-term stimulus impact per dollar spent than for spending that takes longer to occur, such as spending on public infrastructure projects. Finally, because SNAP payments may stimulate spending on food and agricultural commodities more than on other sectors of the economy, the impacts of SNAP per dollar spent may be greater in rural areas than in urban areas, given the greater dependence of rural areas on the food and agriculture sectors.

Figure 2
Per capita SNAP redemptions by county, 2010



Notes: SNAP = Supplemental Nutrition Assistance Program. Redemptions data cannot be disclosed for counties shown in white, which have fewer than four SNAP-authorized stores, to preserve the confidentiality of individual store data.
Source: USDA, Economic Research Service analysis of USDA, Food and Nutrition Service SNAP redemptions data.

[6] For example, USDA-FNS (2011) reported that on average in fiscal year 2009, SNAP households spent more than three-quarters of their benefits by the middle of the month for which the benefits were issued, and that 97 percent of benefits were redeemed by the end of the month.

The wide variation in SNAP redemptions over time and across regions and counties suggests that the impacts of these redemptions could be amenable to statistical analysis. Statistical studies of community-level economic impacts of SNAP appear to be nonexistent. A few studies have simulated national economic impacts of changes in SNAP using predictive economic models (that predict impacts of changes before the fact) rather than statistical models (that investigate impacts of changes after the fact) (see Hanson et al., 2002; Hanson, 2010; Kuhn et al., 1996; Reimer et al., 2015; Zandi, 2009). Only one of those studies investigated the impacts of SNAP or the Food Stamp Program in nonmetro counties (Hanson et al., 2002), based on the predicted employment impacts from a national computable general equilibrium (CGE) model, with the national results disaggregated across industries and applied to local regions based on their industrial employment shares.

The purpose of this study is to investigate multiplier impacts of SNAP redemptions on county employment before, during, and after the Great Recession in nonmetro and metro counties, using statistical (econometric) methods to control for confounding factors and the direction of causality in the relationship between SNAP redemptions and employment. This is the only study to investigate employment multiplier impacts of SNAP redemptions at a county scale and the only study to investigate after-the-fact impacts using statistical methods. The focus on county-level impacts is due to the use of county-level data to conduct the statistical analysis. This approach enables us to investigate whether and how the employment impacts of SNAP redemptions differ between metro and nonmetro counties, and the extent to which SNAP redemptions in one county affect employment in neighboring counties ("spillover effects").[7] The average impact over the period from 2001 to 2014 is estimated, as is the impact for three subperiods: prior to the Great Recession (2001-07), a period including the recession and its immediate aftermath (2008-10), and after the recession (2011-14). We also estimate the impact of SNAP redemptions on employment in nonmetro versus metro counties separately, spillover effects of SNAP redemptions in a county on employment in neighboring counties, and the impacts of other transfer payments and other Federal spending in general on employment.

[7] The estimated spillover effects include spillovers between any adjacent counties, including adjacent metro and nonmetro counties, adjacent nonmetro counties, and adjacent metro counties.

The SNAP Program: Background and Recent History

The Food Stamp Program began as a pilot program in 1961[8] and was made permanent under the Food Stamp Act of 1964. During the late 1960s and early 1970s, the program expanded rapidly as additional counties applied for and became eligible to implement the program. Total participation and benefits paid out track the growth of the program nationally and changes in macroeconomic conditions and program policies (see fig. 1). Legislation has altered eligibility rules and other features of the program since the early 1970s, including income and asset tests for eligibility, work requirements for participants, changes in the eligibility of immigrants, interactions and carryover of eligibility from other assistance programs ("categorical eligibility"), and sanctions for noncompliance with rules (USDA-FNS, 2014).

Policies in the 1980s first focused on reducing costs and limiting participation and then moved to reverse these trends. In the 1990s, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 replaced the Aid to Families with Dependent Children (AFDC) program with the block-grant-funded program Temporary Assistance for Needy Families (TANF). PRWORA also restricted eligibility for SNAP, limiting the time that able-bodied adults without dependents (ABAWDs) could receive benefits when not working and banning legal immigrants who had worked in the United States for less than 10 years (Kuhn et al., 1997). Following PRWORA, SNAP participation fell 35 percent by 2000, and households with children—those most affected by PRWORA—reduced their participation the most (Ganong and Liebman, 2013). Part of the decline in the SNAP caseload following PRWORA was due to improving economic conditions and a reduction in the number of eligible households (Wilde et al., 2000). However, eligible households were also less likely to participate in SNAP following PRWORA (Wilde et al., 2000). Other research found that changes in program rules, such as requiring working families to recertify their eligibility more frequently, reduced participation (Kabbani and Wilde, 2003).

Between 2000 and 2007, annual participation increased over 50 percent to 26.3 million due to increases in program outreach and other policy changes that affected the take-up rate among those eligible, especially working families (Ganong and Liebman, 2013; Klerman and Danielson, 2011). Several SNAP policies implemented by States since the early 2000s encouraged program participation, such as increasing the recertification period (how long before having to provide updated eligibility information), allowing for online applications and recertification, using simplified reporting requirements for changes in beneficiaries' household circumstances, allowing phone interviews rather than in-person interviews to confirm eligibility, allowing all legal noncitizen adults who satisfy other SNAP eligibility requirements such as income and asset limits to be eligible to receive SNAP benefits, providing exemptions to asset tests for vehicles, and using Broad-Based Categorical Eligibility (BBCE) to eliminate the asset test.[9] The increase in participation over this period may also have been the result of a bounceback from the declines following welfare reform. States that had the largest declines in program caseload following PRWORA saw the greatest increases in participation between 2000 and 2007 (Ganong and Liebman, 2013).

[8] The first food stamp program began in the 1930s, but the first pilot of the current program was in 1961 in Paynesville, West Virginia (see history of SNAP on the FNS website). By 1964, when the Food Stamp Act of 1964 was passed, there were 43 pilot programs operating in 40 counties and 3 cities nationwide.

[9] BBCE allows individuals who receive a TANF-funded or Maintenance-of-Effort-funded service to bypass the Federal asset test and use a higher gross income test for SNAP eligibility. Regardless of how eligibility is determined, the final benefit amount is calculated using the standard formula.

In 2008 and 2009, the United States experienced the deepest recession since the Great Depression, and unemployment remained high for several years after the recession's official end in June 2009. SNAP participation rose quickly, reaching a peak of 47.6 million in 2013. Through the ARRA, Congress authorized an increase in SNAP benefits beginning in April 2009 and waived participation time limits for ABAWDs to help ameliorate the effects of the recession (USDA-FNS, 2010).[10] Higher benefits likely increased participation, although this hypothesis has not been empirically tested (Ganong and Liebman, 2013).[11] Policy changes affecting eligibility (ABAWD waivers and BBCE) accounted for less than 20 percent of the increase in SNAP participation between 2007 and 2011, while the remainder of the increase in participation stemmed from increased unemployment and an increase in the participation rate among those eligible for SNAP (Ganong and Liebman, 2013).

The Food, Conservation, and Energy Act (Farm Act) of 2008 made a few changes to Federal asset limit policies in SNAP, excluding retirement and education savings accounts from the assets subject to the asset limit and adjusting the value of the asset limit for inflation (Dean et al., 2008). The Agricultural Act (Farm Act) of 2014 made additional changes to the SNAP program, many of which require rulemaking before they can be implemented by States. Some of the changes that took effect immediately included requiring States to verify wage data from applicants using the National Directory of New Hires through the U.S. Department of Health and Human Services (HHS) and to establish a system to verify immigration status (most States already have such a system). The 2014 reauthorization also banned the use of Federal funds for outreach via television, radio, or billboards and changed how deductions for utility costs are determined when an applicant also receives assistance through the Low-Income Home Energy Assistance Program. Many other changes affected retailers.[12] The Agriculture Improvement Act of 2018 reauthorized the SNAP program with minor changes, maintaining the program's basic eligibility guidelines and work requirements while providing additional funding for enhanced employment and training activities. The 2018 Act also provides for the nationwide expansion of an interstate data match to prevent household receipt of benefits from more than one State and requires States to provide USDA with greater access to SNAP records for inspection and audit.

Rural and Urban Differences in SNAP Participation

Rural and urban areas differ with respect to SNAP participation. Two time series of participation rates estimated by Mathematica Policy Research, Inc., are available: 1994 to 1998 and 2010 to 2014 (fig. 3) (Castner, 2000; Gray and Cunnyngham, 2016).[13] Rates are comparable within the series but not across series. In 1994, the participation rate in urban areas was slightly higher than that in

[10] The benefit increase was set in dollar terms per household size so that households receiving the maximum benefit received an increase of 13.6 percent, while households that were eligible for less than the maximum for their household size received a larger percentage increase in benefits (Nord and Prell, 2011).

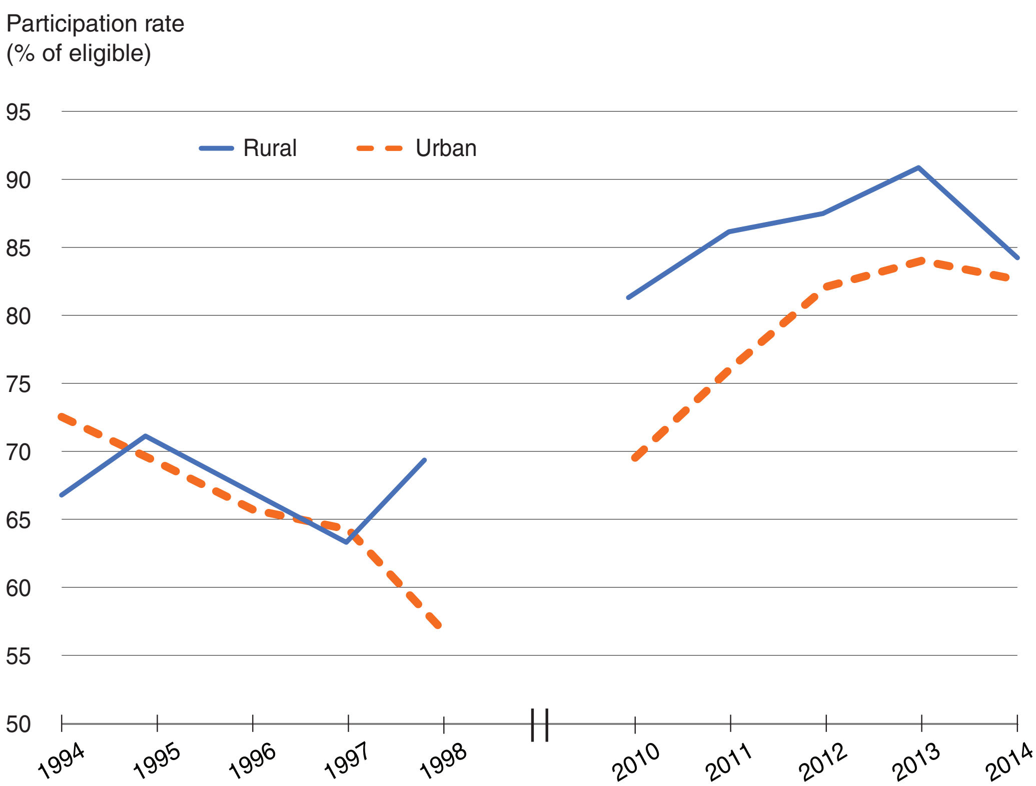
[11] Identification of the impacts of a program or policy change relies on variation across time and space in when the policy or program was implemented. The ARRA benefit increase occurred at the same time for all households and, therefore, its effect on program participation is difficult to estimate empirically.

[12] Memorandums to States that explain how the changes affect the program are posted on the SNAP page on the FNS website.

[13] Mathematica Policy Research, Inc., estimated SNAP participation rates under contract for FNS. Estimates of rural and urban participation rates have been published for 1999-2009; however, the methodology varies nearly every year and is therefore not comparable across time. In these estimates, rural and urban areas are classified by the U.S. Census Bureau's definition of rural and urban.

rural areas, but the rates were similar and declining between 1995 and 1997. In 1998, the end of the first series, the participation rate increased in rural areas but continued to fall in urban areas. In 2010, Mathematica Policy Research, Inc., used a new methodology to estimate eligibility, and data show that the rural participation rate was greater than the urban rate that year and remained higher through 2014.

Figure 3
SNAP eligibility and participation rates in urban and rural areas



Note: SNAP = Supplemental Nutrition Assistance Program. Estimates of rural and urban participation rates have been published for 1999-2009; however, the methodology varies nearly every year and is therefore not comparable across time.

Source: USDA, Economic Research Service compilation of data from Castner (2000) for 1994-98 and Gray and Cunnyngham (2016) for fiscal years 2010-14. Estimates by Castner (2000) are not comparable to those by Gray and Cunnyngham (2016).

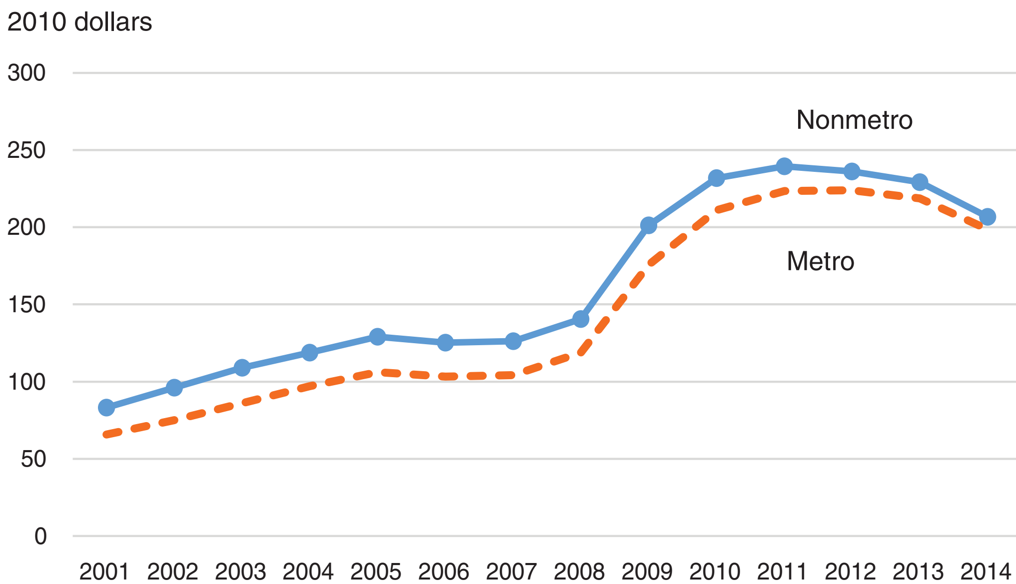
Few studies have explored why participation rates differ between rural and urban areas. McConnell and Ohls (2002) found that in rural areas, although eligibility for SNAP decreased between 1996 and 1998, the participation rate increased. In contrast, both eligibility and participation rates decreased in urban areas over the same period. Based on two sets of qualitative data, the study found that rural participants had a better experience with the program than did urban participants; rural participants were more likely than urban participants to report that their SNAP caseworker treated them respectfully and provided needed services and that they were satisfied overall with the program.

Changes in SNAP Redemptions Between 2001 and 2014

Figure 4 shows the inflation-adjusted value of SNAP redemptions per capita[14] in nonmetro and metro counties between 2001 and 2014 (see box "SNAP Redemptions Data"). Redemptions per capita were greater in nonmetro counties throughout this period, though the difference in rates declined from 28 percent in 2002 to 4 percent in 2014. Between 2001 and 2005, inflation-adjusted redemptions per capita grew steadily in both nonmetro and metro counties, possibly due to changes in State policies encouraging or allowing greater SNAP participation (fig. 5). Redemptions per capita stabilized between 2005 and 2007 in both nonmetro and metro counties and then grew rapidly during and immediately after the Great Recession in 2008 to 2010. The growth in unemployment and poverty during and after the recession contributed substantially to the growth in SNAP participation and redemptions (Ganong and Liebman, 2013), but Federal and State policies, such as the increase in SNAP benefit rates provided in the ARRA, expanded use of BBCE, and the option to use telephone interviews rather than face-to-face interviews during initial certification or recertification, also likely contributed to the increase. From 2011 to 2014, SNAP redemptions per capita stabilized and began to decline.

Figure 4

Inflation-adjusted value of SNAP redemptions per capita per year, nonmetro versus metro



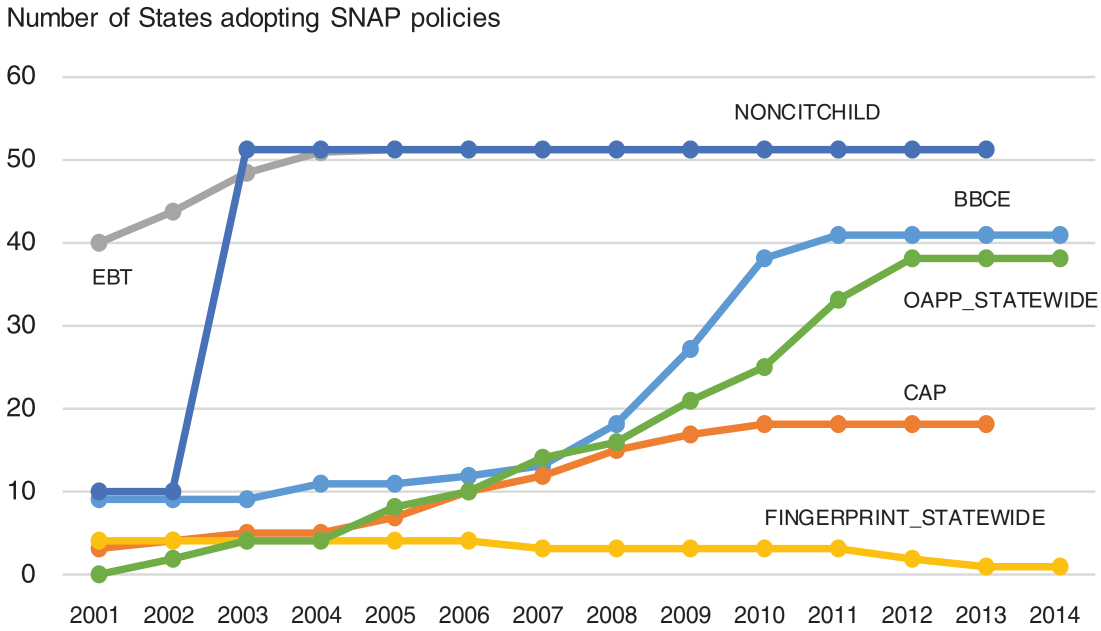
Note: SNAP = Supplemental Nutrition Assistance Program. Nominal values deflated by the annual Consumer Price Index for all items, city average; inflation-adjusted values are in 2010 dollars.

Source: USDA, Economic Research Service analysis of USDA, Food and Nutrition Service SNAP redemptions data.

[14] Here and throughout the remainder of the report, "SNAP redemptions per capita" means the value of SNAP redemptions in a region divided by the total population of the region.

Figure 5

Changes in State-level SNAP policies (number of States adopting each policy by year)



Note: SNAP = Supplemental Nutrition Assistance Program. BBCE refers to Broad-Based Categorical Eligibility; CAP to a Combined Application Project allowing Supplemental Security Income recipients to use a streamlined SNAP application process; EBT to the proportion of SNAP benefits accounted for by Electronic Benefit Transfer; FINGERPRINT_STATEWIDE to a statewide requirement to fingerprint SNAP applicants; NONCITCHILD refers to whether all legal noncitizen children who satisfy other SNAP eligibility requirements such as income and asset limits are eligible for Federal SNAP benefits; and OAPP_STATEWIDE to a statewide policy allowing households to submit online SNAP applications. In the chart, “States” includes the District of Columbia, for a total of 51 States.

Source: USDA, Economic Research Service, SNAP Policy Data Set (through 2011); ERS Food Economics Division (after 2011).

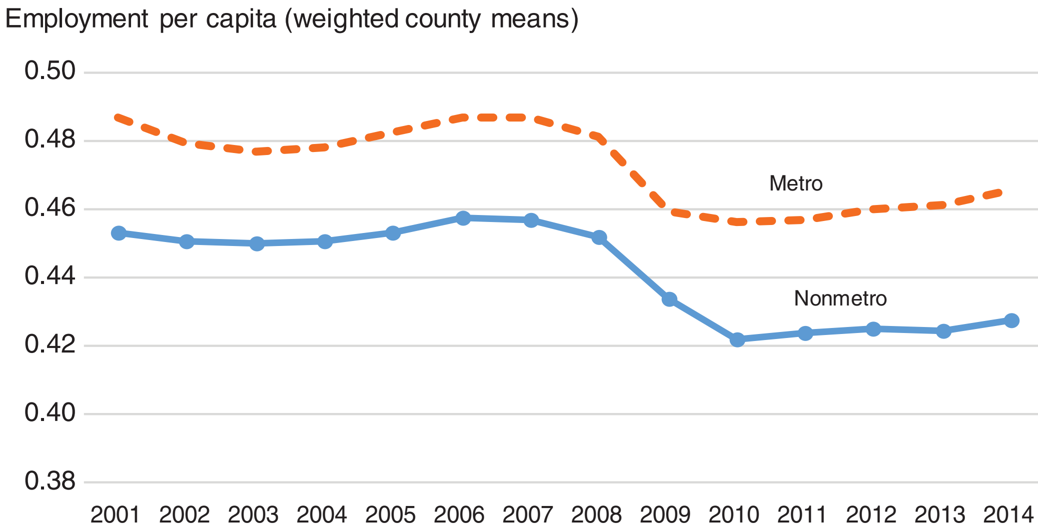
SNAP Redemptions Data

For the analysis in this study, ERS used confidential administrative data provided by USDA's Food and Nutrition Service (FNS) on Supplemental Nutrition Assistance Program (SNAP) redemptions as the indicator of SNAP payments, rather than county-level estimates of SNAP benefits per capita, as reported by the U.S. Bureau of Economic Analysis (BEA) or in the ERS SNAP Data System. The SNAP redemptions data have several advantages over the SNAP benefits data for the purpose of this study. SNAP redemptions are reported at the level of the store that redeems SNAP benefits, which is the location at which local economic multiplier impacts (if they occur) are expected to begin. By contrast, SNAP benefits data are reported by county of residence of SNAP recipients, which may differ from the county where the benefits are redeemed. The SNAP redemptions data are based on actual financial payments to retailers by FNS and are subject to regular audits, so the quality of the data is likely to be very good. Prior to the development of Electronic Benefit Transfer (EBT) systems, the redemptions data were based on redemptions through the banking system. Since EBT systems became universal (throughout the United States by 2004), SNAP redemptions data have been based on electronic records resulting from EBT transactions, through the Anti-Fraud Locator for EBT Redemption Transaction (ALERT) System. Store-level SNAP redemptions were aggregated to the county level for this analysis. By contrast to the SNAP redemptions data, which are accurate at the county level, some of the BEA and ERS data on SNAP benefit payments by county are imputed.

County-Level Employment Trends[15]

Employment per capita was lower in nonmetro counties than in metro counties throughout the entire study period, reflecting that nonmetro counties have an older and less educated population on average than metro counties (fig. 6). The trends in employment per capita were similar in metro and nonmetro counties, staying relatively flat between 2001 and 2007, declining during 2008 to 2010, and then growing slowly after 2010 but with more rapid growth in metro counties.

Figure 6
Employment per capita, nonmetro versus metro counties



Source: USDA, Economic Research Service analysis of U.S. Department of Labor, Bureau of Labor Statistics employment data.

These comparisons and trends are generally opposite to the comparisons and trends in SNAP redemptions, which indicate greater SNAP redemptions per capita in nonmetro counties in almost all years and a pattern of rising SNAP redemptions per capita during 2008 to 2010 and falling SNAP redemptions per capita after 2010 in both metro and nonmetro counties (see fig. 4). That is not surprising, since SNAP eligibility and payments are based on household poverty, which tends to rise when employment is falling. This emphasizes the challenge of measuring the impacts of SNAP redemptions on employment, since the level of local employment and unemployment can affect SNAP redemptions as well as be affected by SNAP redemptions. The econometric methods used to address this challenge are discussed briefly in the next section and in more detail in appendix A.

[15] In this study, we use data on county employment from the BEA Regional Economic Information System, which defines employment as "A count of jobs, both full-time and part-time. It includes wage and salary jobs, sole proprietorships, and individual general partners, but not unpaid family workers nor volunteers."

Impacts of SNAP Redemptions on County-Level Employment

As noted earlier, SNAP redemptions grew substantially between 2001 and 2011, especially during and immediately after the Great Recession. In this section, we present estimates of the impacts of SNAP payments on county-level employment, controlling statistically for other factors, in nonmetro versus metro counties during 2001 to 2014. We focus on the period 2001 to 2014 because this provides a contrast of pre-recession, recession, and post-recession periods. We hypothesize that the economic impacts of SNAP payments differ between such periods, and between nonmetro and metro counties.

Expected Impacts of SNAP Payments on County Economies

SNAP payments may affect regional economies in many ways. These payments may increase demand for goods and services in a region as a result of an effective increase in income of SNAP recipients. The receipt of SNAP benefits can stimulate purchases of nonfood goods and services as well as food by SNAP recipients, to the extent that SNAP recipients already spend some of their cash income on food and decide to purchase some food items with SNAP benefits rather than cash, freeing up some of their cash income to spend on nonfood items or for savings. The short-term net impacts on the economy depend on how much SNAP recipients decide to spend versus save the additional resources and how much of any additional spending will be on food. Because SNAP benefits are targeted to low-income people, who have a relatively high propensity to spend rather than save additional income, SNAP benefits can have a relatively large economic impact, especially during a recession when lack of demand can lead to unemployment or underemployment in the economy (Zandi, 2009; Hanson, 2010). The impacts of SNAP may also be felt relatively quickly, compared to other forms of government spending, because SNAP beneficiaries spend these benefits quickly. Furthermore, since SNAP benefits have been shown to increase food consumption by more than a comparable increase in cash income (Fraker, 1990; Wilde and Ranney, 1996; Wilde et al., 2009; Beatty and Tuttle, 2014; Smith et al., 2016; Tuttle, 2016), the impacts of SNAP may be particularly strong in nonmetro regions, where agricultural production and food processing and distribution account for a larger portion of the economy than in metro regions (Hanson et al., 2002).

The impacts of an increase in SNAP benefits ripple through a regional economy through what are called multiplier effects, which include direct, indirect, and induced demand effects (Hanson, 2010). The direct effects are primarily effects on the retail, wholesale, and transportation sectors of providing additional food and nonfood goods and services resulting from increased spending by SNAP recipients. To be able to produce and provide these goods and services, the directly affected sectors increase their demand for the outputs of other sectors that are inputs to their production, such as agricultural commodities, food processing, fuel, and other industrial outputs. These effects are called the indirect or interindustry effects of the initial increase in demand. Then, to the extent that the direct and indirect effects lead to increased employment and earnings in the regional economy, this can lead to increased income of consumers in the economy, both through increased labor income (wages, salaries, and other earnings) and through increased returns to capital and land owned by people in the regional economy (dividends, interest, and rent). The effects of increased income are called the induced effects of the initial increase in demand.

The impacts of these effects on a regional economy depend on the extent to which the goods and services demanded are produced/provided locally or "imported" from other regions (not necessarily from other countries) and on the ownership of the resources used to meet these demands. In small, open regional economies such as counties, which are the focus of this study, a substantial share of demand may be met by imports from outside the region, and a substantial share of the resources may be owned by nonresidents of the region. Both of these considerations tend to reduce the multiplier effects of an increase in demand.

The impacts of increased demand in a regional economy also depend on the ability and costs of the region to supply the goods and services demanded (which affects how much of the increased demand will be met by local production versus imports). If supply constraints are not binding or local costs of production and prices do not increase significantly as a result of increased demand—as may be more likely during a recession when excess capital and labor are available—the multiplier impacts of increased demand may be relatively unaffected by supply constraints and largely determined by the demand effects. Such a scenario is more likely to occur in small open economies in which additional supplies of labor, capital, and land are either locally available or can be readily brought in (in the case of labor and capital).

The preceding discussion suggests that the impacts of an increase in local demand, such as stimulated by an increase in SNAP benefits, on production and employment in a regional economy will likely be greater during a recession when labor or capital are unemployed or underused.[16] It also suggests that the impacts per dollar of additional benefits may be either smaller or larger in a smaller and more rural county. The impacts per dollar of SNAP benefits will tend to be smaller in a smaller county because smaller counties tend to be less economically diversified than larger counties and, hence, tend to import a larger share of the goods and services consumed from outside the region (Low and Isserman, 2009).[17] But more remote rural counties may depend more on their own production rather than imports due to high transportation costs, tending to reduce imports and increasing potential local multiplier effects. On the other hand, smaller and more accessible rural regions may be able to attract sufficient flows of labor and capital from outside regions and have sufficient land available so that regional output and employment can be relatively more responsive in such regions than in that of larger and more densely populated urban economies, especially if they are located close to metro regions (the opposite may be the case for more remote rural regions). The impacts of SNAP redemptions in rural versus urban economies also depend on the extent of these economies' dependence on agricultural and food production, which are more directly affected by SNAP spending and relatively more important to rural economies. Considering all of these effects, the relative impacts of increased SNAP benefits in rural versus urban economies is conceptually ambiguous.

The impacts of an increase in government spending may also depend on how and by whom the increase is financed. If the increase is financed by an increase in taxes within the region, there may be no net increase (or a decrease) in local income or demand as a result of such spending, though the spending can change the composition of local demand and thus lead to shifts in the industrial composition of employment and output. This is one of the main lessons of national-level studies of

[16] Numerous empirical studies, discussed later in this study, support this hypothesis for the effects of other types of government spending.

[17] In the analysis reported in this study, we control for the size of the county economy by including population size as a control. This is expected to reduce the tendency to find larger multiplier impacts in metro settings.

the impacts of SNAP spending that use CGE models and assume that increases in SNAP spending are budget neutral (Hanson et al., 2002; Reimer et al., 2015). But regarding the effects of SNAP spending on county economies, such spending is financed by Federal taxes or deficits and does not have a direct linkage to the taxes paid by local county residents, though forward-looking residents might anticipate increases in future Federal taxes as a result of an increase in SNAP spending. Such spending may therefore be regarded as “windfall-financed” from the local perspective (Clemens and Miran, 2012), so that the impacts of financing requirements on local demand and economic activity are muted.[18] Thus, we do not expect a large effect of financing considerations on our estimates of county-level impacts of SNAP spending.

Although SNAP payments can increase income and employment in regional economies through income transfer and multiplier effects, especially during a recession and if they are not offset by tax increases, they may have negative impacts if they reduce the supply of labor. Since the benefits offered under the program decline as income increases, the program rules act as an effective tax on increased earnings. Although the “marginal tax rate” (the effective tax on an additional dollar of earnings, considering effects of additional earnings on program eligibility and benefit rates) resulting from the SNAP program can be quite high for particular individuals (especially those close to the income threshold for program eligibility) (Kotlikoff and Rapson, 2006), marginal tax rates averaged across entire populations for social safety net programs as a whole were fairly modest in the 2000 to 2010 period (Moffitt, 2015).[19] This evidence on marginal tax rates, and earlier studies of the labor-supply impacts of the Food Stamp Program (e.g., Fraker and Moffitt, 1988; Hagstrom, 1996; Currie, 2003; Hoynes and Schanzenbach, 2012), suggest that the SNAP program has had a minimal impact on labor supply overall, although impacts on particular groups such as single mothers can be significant. For some groups such as able-bodied adults without dependents (ABAWDs), SNAP work requirements may limit the ability of SNAP recipients to work less or may cause some to increase employment. Thus, the impacts of SNAP benefits on employment via labor-supply effects may be muted.

Data

This study uses store-level data on SNAP redemptions from FNS; county-level data on employment (total and by industry) from the U.S. Bureau of Economic Analysis (BEA) and the Bureau of Labor Statistics (BLS); transfer payments data from BEA; total Federal Government spending data (until 2010) from the U.S. Census Bureau’s Consolidated Federal Funds Report (as cleaned and archived on ERS’s website); unemployment rate and inflation rate data from BLS; poverty, population, and other demographic data from the U.S. Census Bureau; and State-level SNAP policy indicators from the ERS SNAP Policy Database (with updates for selected policies through 2013). Except for the SNAP redemptions data, the BEA/BLS employment data by industry, and the State-level SNAP policy data for 2012 and 2013, all of the data used are publicly available (see appendix B for more details on the data and sources and descriptive statistics of the data).

[18] Furthermore, econometric studies that control for national and State-level factors using year or State-by-year fixed effects in the regressions (as we do) implicitly control for the effects of macroeconomic policies or how Federal Government spending is financed, so the resulting estimates of local fiscal multipliers are not affected by such higher level policies (Clemens and Miran, 2012; Nakamura and Steinsson, 2014). We discuss this issue further later in this report.

[19] Moffitt (2015) estimated that the effective marginal tax rate (MTR) from all transfer programs facing households with income less than 50 percent of the poverty line increased from 14 percent in 2000 to 18 percent in 2010, while the MTR increased from 3 to 7 percent over the same period for households with incomes from 50 to 100 percent of the poverty line, and from 5 to 15 percent in 2010 for households with incomes from 100 to 150 percent of the poverty line.

Empirical Approach

We estimated the impacts of SNAP redemptions per capita on county employment per capita in metro versus nonmetro counties using several econometric methods to assess the validity and robustness of the conclusions (see appendix A for details of the models). Our preferred model is an ordinary least squares (OLS) second difference (SD) regression model, which subtracts out the effects of any unobserved county-level fixed factors or linear time trends. We also include State-by-year fixed effects in the model to account for any national or State and year-specific factors (such as national or State policies or economic conditions) that may affect employment per capita.[20] However, our estimates could still be biased by problems of measurement errors, "omitted variable bias" (effects on employment outcomes of factors not accounted for in the regressions that are correlated with SNAP redemptions), "reverse causality" (the fact that SNAP redemptions could be affected by changes in employment per capita as well as affecting employment per capita), or more generally by the "endogeneity" of the SNAP redemptions variable and the control variables.[21] Robustness checks including additional control variables and using other more advanced econometric methods were also estimated and tested to address these issues; however, our preferred method remained the second difference OLS regression. See appendixes A and C for explanations of the alternative methods, their results, and the reasons we prefer the second difference OLS model. The analysis implicitly assumed that the magnitude of impacts of an increase in SNAP redemptions is the same (but in a different direction) as the magnitude of impacts of a decrease in SNAP redemptions; we did not test that assumption or attempt to estimate differences in impacts of increases versus decreases in SNAP redemptions.

Estimated Impacts of SNAP Redemptions on Employment

The estimated average impacts of SNAP redemptions on county-level employment during 2001-14, controlling statistically for other factors, are reported in table 1.[22] Over the entire study period and across all counties, an increase in SNAP redemptions of $10,000 (in 2010 inflation-adjusted dollars) is associated with an increase in employment of 0.456 job (with a margin of error of +/- 0.143).[23] Put differently, the expected cost of creating an additional job through an increase in SNAP redemptions, over the entire study period and all counties, is about $22,000 ($10,000/0.456).

[20] "Fixed effects" refer to factors that are assumed to shift the mean level of the dependent variable. For example, "State-by-year fixed effects" allow for a different mean level of employment per capita in each State-by-year combination, after accounting for the effects of other observed variables in the regression model.

[21] Endogenous explanatory variables are variables that may respond to factors affecting the outcome variable and, hence, are possibly correlated with the error term in the regression. Reverse causality is one reason that a variable may be endogenous but is not the only reason.

[22] Since the variable of interest (SNAP redemptions per capita) and the outcome variable (employment per capita) are both in per capita terms, and since the relationship between these variables is assumed to be linear, the impacts presented in table 1 can be interpreted either as the amount by which county employment per capita changes for a $10,000 increase in SNAP redemptions per capita, or as the amount by which total county employment changes for a $10,000 increase in total SNAP redemptions in a county. We adopt the latter interpretation, since a $10,000 increase in SNAP redemptions per capita is far outside the range of the data for SNAP redemptions per capita.

[23] For a 95-percent confidence interval, the margin of error = 1.96 x the standard error of the estimate, which is reported in parentheses in table 1. For example, the estimate of 0.456 for all counties and the entire period in table 1 has a standard error of 0.073; multiplying this by 1.96 yields the margin of error estimate of 0.143.

Table 1

Impacts of SNAP redemptions on county employment (jobs/$10,000 of SNAP redemptions)

Time period	All counties	Nonmetro counties	Metro counties
Entire period, 2001-14	0.456*** (0.073)	0.437*** (0.091)	0.052 (0.082)
Pre-Great Recession, 2001-07	0.173** (0.086)	0.250*** (0.089)	-0.218** (0.086)
During recession, 2008-10	1.247*** (0.182)	1.043*** (0.234)	0.414** (0.178)
Post-recession, 2011-14	0.178* (0.092)	0.152 (0.130)	0.123 (0.081)

Note: Robust clustered standard errors in parentheses – clustered by county. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. SNAP = Supplemental Nutrition Assistance Program. Each impact reported in the table is the coefficient of the SNAP redemptions variable in a separate regression model representing different time periods (entire period or one of three subperiods) and groups of counties (all counties, nonmetro counties, or metro counties). The same control variables were included in all regression models represented in the table, including controls for county economic conditions (unemployment rate and poverty rate), demographic factors (population level, shares of population by age group, shares of population by race and ethnicity), and economic structure (shares of employment by major industry categories). See appendix A for an explanation of the model, appendix B for an explanation of the variables and data sources, and appendix C for more complete regression results.

Source: USDA, Economic Research Service analysis of determinants of changes in county employment, using an Ordinary Least Squares Second Difference (OLS-SD) model and including State-by-year fixed effects.

Table 1 also reports our estimates of the impacts of SNAP redemptions on employment in nonmetro versus metro counties and during three periods: pre-Great Recession (2001-07), during and immediately after the recession (2008-10), and post-recession (2011-14). As expected, we find larger impacts of SNAP redemptions during the Great Recession than either before or after the recession. For all counties, an increase of $10,000 in SNAP redemptions is associated with a statistically significant increase of 1.247 jobs during the Great Recession, compared to increases of 0.173 job before the recession and 0.178 job after the recession. This supports our hypothesis that the multiplier impacts of SNAP payments on local economies is greater during a recession.

We also find that SNAP redemptions have a larger positive impact in nonmetro counties than in metro counties. Over the entire period, the estimated impact per $10,000 of SNAP redemptions is 0.437 job in nonmetro counties but only 0.052 job in metro counties (and the estimated impact for metro counties is "statistically insignificant"—i.e., it cannot be statistically distinguished from zero with 95 percent confidence). We find larger positive impacts of SNAP redemptions in nonmetro counties than in metro counties before the recession and during the recession. After the recession, the impacts of SNAP redemptions in both nonmetro and metro counties are statistically insignificant.

A somewhat surprising result is the negative and statistically significant impact of SNAP redemptions in metro counties before the Great Recession (-0.218 job per $10,000). This suggests the possibility that any negative impacts of SNAP payments on employment via labor-supply disincentives, fiscal effects, or other effects may be greater in metro counties and greater during an expansionary economy.[24] We did not have strong theoretical reasons to expect this result specific to metro coun-

[24] This result also could be indicative of reverse causality (i.e., reduced employment may lead to greater poverty and greater SNAP redemptions). However, it is not clear why reverse causality would cause this association to be negative while the association between SNAP redemptions and employment is positive in nonmetro counties during multiple time periods and in metro counties during the recession.

ties and only one time period, and the result is not robust across the models estimated;[25] so we take this as a result worthy of further research. The main findings in table 1—that SNAP redemptions have a positive and statistically significant impact on county-level employment, that these impacts were larger during the Great Recession than before or after it, and that the impacts were larger in nonmetro than metro counties—are robust across the models estimated.[26]

Spillover Effects of SNAP Redemptions in Neighboring Counties

The results in table 1 focus on the local effects of SNAP redemptions in the county where SNAP redemptions occur. These results could be incomplete if the impacts of SNAP redemptions "spill over" into other counties. To address this issue, we investigated the impacts of SNAP redemptions in neighboring counties using a spatial econometrics model.[27] Table 2 reports the estimated impacts of SNAP redemptions over the entire study period (2001-14), considering both within county effects and effects of SNAP redemptions in neighboring counties.

Table 2

Impacts of SNAP redemptions on county employment, accounting for effects of SNAP redemptions in neighboring counties – 2001-14 (jobs/$10,000 of SNAP redemptions)

	All counties	Nonmetro counties	Metro counties
Within-county effects of SNAP redemptions	0.388*** (0.054)	0.388*** (0.062)	-0.119 (0.085)
Effects of SNAP in neighboring counties	0.070 (0.093)	0.384*** (0.106)	-0.044 (0.155)

Note: Robust standard errors in parentheses. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. SNAP = Supplemental Nutrition Assistance Program. Each impact reported in a column in the table is the coefficient of the SNAP redemptions variable in the same county and in neighboring counties in a regression model for all sample years, based on a Spatial Durbin Error Model (SDEM) with common coefficients for all counties and an SDEM allowing different coefficients for nonmetro and metro counties. The same control variables were included in both regression models represented in the table, including controls for own-county and neighboring-county economic conditions (unemployment rate and poverty rate), demographic factors (population level, shares of population by age group, shares of population by race and ethnicity), and economic structure (shares of employment by major industry categories). See appendix A for an explanation of the model, appendix B for an explanation of the variables and data sources, and appendix C for more complete regression results.

Source: USDA, Economic Research Service analysis of determinants of changes in county employment, using an SDEM and including State-by-year fixed effects.

For all counties combined, the estimated within-county impact is 0.388 job per $10,000 of SNAP redemptions, while the estimated impact of SNAP redemptions in neighboring counties is much smaller (0.070 job per $10,000) and statistically indistinguishable from no effect. For nonmetro counties, the within-county effect is identical to the estimate for all counties (0.388 job per $10,000), but the spillover effect from neighboring counties is much larger than the estimate for all counties

[25] The coefficient of SNAP redemptions per capita was also negative and statistically significant in the ordinary least squares first difference model with State-by-year fixed effects but was statistically insignificant in the ordinary least squares first and second difference models with year-fixed effects (see appendix A for an explanation of these models). We did not estimate the model for nonmetro and metro counties separately by subperiods using the other estimators discussed in appendix A (instrumental variables or dynamic panel estimators) because of the sensitivity of variations in the State-level SNAP policy instruments used in the instrumental variables model to the subperiod estimated and the use of many lags of each variable in the dynamic panel model.

[26] All regression results available upon request.

[27] See appendix A for an explanation of the spatial econometrics model and appendix C for more complete results of that model.

and statistically significant (0.384 job per $10,000). By contrast, the within-county and spillover effects for metro counties are much smaller and statistically insignificant.

These results suggest that SNAP redemptions had a larger impact in nonmetro counties than in metro counties over the entire study period (consistent with the results in table 1), and that part of the reason for larger impacts in nonmetro counties is larger spillovers from neighboring counties. This may be due to large spillover effects of SNAP redemptions in metro counties on adjacent nonmetro counties. We do not investigate that hypothesis here but leave that as a useful question for future research on this issue.

Effects of Other Government Programs

Although the analyses reported in table 1 control statistically for many factors that could affect employment outcomes and that may be correlated with SNAP redemptions, other omitted factors still may affect our results. One such factor to consider is government spending on other programs besides SNAP. Especially following enactment of the ARRA, Federal Government spending increased across many types of programs, and this spending may have been correlated with SNAP redemptions and also may have affected employment outcomes. To address this issue, we incorporated data on government transfer payments to individuals (from BEA) and total Federal Government expenditures in a county (from the U.S. Census Bureau's Consolidated Federal Funds Report (CFFR)).[28] In our analysis based on these additional data, we focus only on the 2008 to 2010 period, in part because the estimated impacts of SNAP during the Great Recession may have been particularly affected by other government spending during this period, and in part because the CFFR data are available only until 2010.[29]

The estimated impacts of SNAP redemptions and other government spending on employment during 2008 to 2010 are reported in table 3. For all counties as a whole and for nonmetro counties, the estimated impacts of SNAP redemptions are similar to those reported in table 1—1.100 jobs per $10,000 of SNAP redemptions for all counties and 0.992 job per $10,000 of SNAP redemptions for nonmetro counties. The impact of SNAP redemptions is smaller for metro counties when other forms of government spending are included in the analysis—0.234 job per $10,000 rather than 0.414 job per $10,000 as in table 1—and not statistically significant.

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[28] The BEA data on transfer receipts from governments include data on retirement and disability benefits (such as Social Security benefits), medical benefits (such as Medicare and Medicaid benefits), income maintenance benefits (such as Supplemental Security Income, Earned Income Tax Credit, and SNAP), unemployment insurance compensation, veterans benefits, education and training assistance, and other transfer receipts from governments, including several programs and tax credits authorized by ARRA. The CFFR data include data on Federal grants, direct and guaranteed loans, salaries and wages, procurement contracts, direct payments, and insurance. There is overlap in these data, since Federal transfer payments are included in both data sources. The regression model estimates the effect of each type of spending, controlling for the other type. Thus, the coefficient of total Federal spending in table 3 nets out the effect of Federal transfer payments.

[29] We also analyzed impacts of SNAP and other government spending during the pre-recession period from 2004 to 2007. Our results for the pre-recession period are qualitatively similar to those reported in table 1 (results available upon request).

Table 3

Impacts of SNAP redemptions and other government spending on county employment during the Great Recession

Time period	Variable	All counties	Nonmetro counties	Metro counties
During recession, 2008-10	SNAP redemptions ($10,000)	1.100*** (0.184)	0.992*** (0.236)	0.234 (0.177)
	Other transfer payments ($10,000)	0.132*** (0.015)	0.088*** (0.018)	0.150*** (0.021)
	Federal expenditures ($1 million)	0.063** (0.030)	0.056 (0.038)	0.003 (0.030)

Note: Robust clustered standard errors in parentheses – clustered by county. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. SNAP = Supplemental Nutrition Assistance Program. Each column of impacts reported in the table are the coefficient of the SNAP redemptions variable, other transfer payments, and Federal grants and loans from separate regressions representing different groups of counties (all counties, nonmetro counties, or metro counties). The same control variables were included in all regression models represented in the table, including controls for county economic conditions (unemployment rate and poverty rate), demographic factors (population level, shares of population by age group, shares of population by race and ethnicity), and economic structure (shares of employment by major industry categories). See appendix A for an explanation of the model and appendix B for an explanation of the variables and data sources.

Source: USDA, Economic Research Service analysis of determinants of changes in county employment, using an Ordinary Least Squares Second Difference (OLS-SD) model and including State-by-year fixed effects.

The estimated impacts of SNAP redemptions in all counties and in nonmetro counties during the recession are roughly 10 times the magnitude of the estimated impacts of other transfer payments on employment and several orders of magnitude larger than the impact of total Federal spending (note that the impact of Federal expenditures in all counties is 0.063 job per $1 million of total Federal expenditures). These results support the hypothesis that SNAP spending had larger employment multiplier impacts than many other forms of government expenditures during the Great Recession.

Addressing Concerns About Reverse Causality and Measurement Error

The associations reported in tables 1, 2, and 3 may not reflect a causal relationship (i.e., the impact of an increase in SNAP redemptions on county employment). One concern is the possibility that these relationships are affected by reverse causality (i.e., SNAP redemptions could be affected by employment per capita rather than causality only going from SNAP redemptions to employment). Generally, we would expect reverse causality to bias our estimates downward, since we would expect lower employment per capita to contribute to higher SNAP redemptions. We address this issue using an instrumental variables estimator. Essentially, this estimator predicts the growth in SNAP redemptions per capita after 2008 in each county using SNAP redemptions per capita in 2007 in that county and then uses the predicted value of SNAP redemptions rather than actual redemptions in the regression model. We expected (and found) that SNAP redemptions per capita in a county in 2007 are a strong predictor of the growth in SNAP redemptions per capita during the period in which ARRA increased SNAP benefit rates (April 2009 through October 2013), since one of the main effects of that ARRA provision was to increase the value of SNAP payments to households that were already receiving SNAP benefits before the recession.[30] This approach eliminates the dependence of the estimated SNAP redemptions on local economic conditions during the period considered, thus addressing the reverse causality concern.

Another concern is measurement error in the SNAP redemptions variable and in other variables used in the analysis. As discussed earlier, the administrative data on SNAP redemptions are of high quality, especially since EBT cards have been used, ensuring that all records of SNAP redemptions are electronically recorded at the point of sale. Nevertheless, measurement errors in this and other variables could bias our results. A standard approach to addressing this issue is to use "long differencing" (i.e., analyzing impacts of changes over longer periods of time). This approach helps to reduce the influence of measurement errors by increasing the magnitudes of changes relative to the size of errors in the data. Thus, we use instrumental variables estimation with long differencing over multiple years. The period that we focus on for this analysis is from the beginning of the Great Recession in 2008 to 2013. This is the subperiod of our study period when the ARRA increase in SNAP benefit rates was in effect.

The results of these estimations are presented in table 4, considering alternative end years for the period studied. As in table 3, the analysis includes other transfer payments per capita, which are instrumented using the level of other transfer payments per capita in 2007.[31] In all of the regressions, the estimated impacts of SNAP redemptions on employment are positive and statistically significant and range from 1.0 job to 1.8 jobs per $10,000 of SNAP redemptions. The estimated impacts of SNAP tend to be larger the longer the time period considered. In all regressions, the impacts of SNAP are substantially larger than the impacts of other transfer payments, the effects of which are not statistically significant at the 95-percent confidence level in any regression. These results strengthen our confidence that SNAP redemptions have a positive causal impact on local employment in both nonmetro and metro counties, and that the employment impacts of SNAP are larger than those of other transfer programs, taken as a whole.

[30] SNAP benefits paid during the ARRA period were also affected by changes in participation in the program, which this instrumental variable does not reflect. The instrumental variables estimator thus reflects the impacts of changes in SNAP payments predicted by the allocation of benefits in 2007 (which is not subject to reverse causality concerns) but not the impacts of changes in SNAP participation (which are subject to such concerns). A similar instrumental variables approach was used by Chodorow-Reich et al. (2012) to estimate the employment impacts of increased Medicaid payments authorized by ARRA and by Wilson (2012) to estimate the employment impacts of increased Federal Government spending more generally as a result of ARRA.

[31] We excluded the variable for total Federal payments that was included in the analyses in table 3 from the analyses in table 4 because of the very small coefficients for that variable found in table 3 and because the Federal funds data are not available from the U.S. Census Bureau after 2010.

Table 4
Impacts of SNAP redemptions and other transfer payments on county employment, using long differences and instrumental variable estimation

Time period	Variable	All counties	Nonmetro counties	Metro counties
2008-10	SNAP redemptions ($10,000)	1.159*** (0.307)	1.025*** (0.370)	1.197*** (0.458)
	Other transfer payments ($10,000)	0.175* (0.106)	0.206 (0.153)	-0.058 (0.159)
2008-11	SNAP redemptions ($10,000)	1.320*** (0.322)	1.219*** (0.381)	1.221** (0.577)
	Other transfer payments ($10,000)	0.179 (0.159)	0.157 (0.215)	0.064 (0.276)
2008-12	SNAP redemptions ($10,000)	1.608*** (0.375)	1.520*** (0.455)	1.122** (0.568)
	Other transfer payments ($10,000)	0.213 (0.153)	0.234 (0.194)	0.184 (0.304)
2008-13	SNAP redemptions ($10,000)	1.811*** (0.466)	1.756*** (0.579)	1.402** (0.702)
	Other transfer payments ($10,000)	0.140 (0.132)	0.146 (0.178)	0.044 (0.237)

Note: Robust clustered standard errors in parentheses – clustered by county. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. SNAP = Supplemental Nutrition Assistance Program. Each impact reported in the table is the coefficient of the SNAP redemptions variable or other transfer payments variable in a regression model representing different time periods and groups of counties (all counties, nonmetro counties, or metro counties). The same control variables were included in all regression models represented in the table, including controls for county economic conditions (unemployment rate and poverty rate), demographic factors (population level, shares of population by age group, shares of population by race and ethnicity), and economic structure (shares of employment by major industry categories). The instrumental variables used to predict the change in SNAP redemptions per capita and change in other transfer payments per capita in each county are the level of SNAP redemptions per capita and the level of other transfer payments per capita in 2007. See appendix A for an explanation of the model, appendix B for an explanation of the variables and data sources, and appendix C for more complete regression results.

Source: USDA, Economic Research Service analysis of determinants of changes in county employment, using an Instrumental Variable Long Difference (IV-LD) model and including State fixed effects.

Alternative instrumental variables approaches were also used to address concerns about reverse causality and, more generally, endogeneity of SNAP redemptions and other explanatory variables in the regressions (these approaches are discussed in appendix A and the results are presented in appendix C). These approaches yielded results that are qualitatively consistent with the results presented in table 1—supporting a finding of a positive and statistically significant causal impact of SNAP redemptions on employment—but suggest that the impact could be even larger than that estimated in table 1. Across the instrumental variables approaches used to estimate impacts for all counties and years, estimated impacts per $10,000 of SNAP redemptions range from 0.476 job added to 0.761 job added, larger than the comparable estimate of 0.456 job per $10,000 reported in table 1.

Other Robustness Checks

As mentioned earlier, we estimated the impacts of SNAP redemptions using several alternative OLS models. Some of the key results of these alternative models are presented in appendix C. In general, the qualitative conclusions of the base model presented in table 1 are very similar in these alternative models, with $10,000 of SNAP redemptions associated (with statistical significance at the 99-percent confidence level) with more jobs in all of the models estimated for all counties and years. The smallest impact found across these models was 0.337 job per $10,000 of SNAP redemptions.

We also investigated the robustness of our results to an alternative specification of the SNAP and employment variables—measuring these per the working-age population (ages 15 to 64) rather than per the entire population. The results were little different from the results shown in table 1 (results available upon request).

Comparison to Results in the Literature

To the best of our knowledge, no other studies have investigated the impacts of SNAP redemptions on county-level employment using an econometric approach. Several studies have investigated impacts of SNAP (or the Food Stamp Program (FSP)) on the national economy using economic simulation models (Hanson et al., 2002; Hanson, 2010; Reimer et al., 2015). Both Hanson et al. (2002) and Reimer et al. (2015) used a CGE model of the national economy to estimate the impacts of changes in the size of the SNAP program. Hanson et al. (2002) estimated the impacts if there had been a $5 billion cut in the FSP in 1996 and (among other results) estimated that 22,100 net jobs would have been added as a result—though 7,500 jobs would have been lost in the farm and food processing sectors while gains were predicted for all other sectors—implying a negative impact of the FSP on employment overall. Reimer et al. (2015) estimated a small net loss in the U.S. economy of 5,641 jobs if SNAP had been completely eliminated in 2010. Considering the full costs of SNAP in 2010 ($68.3 billion), that translates to a jobs multiplier of only 0.0008 job per $10,000 (in 2010 dollars).

One of the main reasons that these national CGE models predict such small and, in some cases, negative employment impacts of SNAP spending is that they assume that any change in SNAP spending is budget neutral (i.e., a reduction or increase in SNAP spending is immediately offset by a reduction or increase in Federal taxes to keep the budget deficit unchanged). The offsetting effects on taxes result in the SNAP program having mostly distributional impacts, with little predicted impact on total employment. Furthermore, the baseline scenarios considered in these studies assume either a full employment economy (Reimer et al., 2015) or a labor supply that is relatively unresponsive (or "inelastic") to changes in wages (Hanson et al. 2002). These assumptions allow for little overall national employment response from a change in SNAP, although changes in SNAP are predicted to shift employment among sectors.[32]

In our estimation approach, by contrast, there is no assumption of budget neutrality, full employment, or inelastic labor supply. Any effects of SNAP policy changes on Federal or State tax rates in our regression analysis will be absorbed in the State-by-year fixed effects included in the regressions. Our estimates of local multiplier effects reflect what are sometimes called "windfall-financed" government spending (i.e., the effects of additional government spending in a region holding constant tax rates or other revenue-generating policies that might be affected by changes in government spending and deficits) (Clemens and Miran, 2012; Wilson 2012).

In theory, local multipliers can be smaller or larger than national multipliers (Nakamura and Steinsson, 2014). They may be smaller if a larger share of consumption is imported from outside the region (not necessarily from outside the country) in a small region such as a county than in the national economy, which tends to reduce the multiplier effect of increased demand. But they may be larger because the supply of labor or capital may be relatively more responsive to increased demand in a county than in the national economy, due to the relative openness of counties. To the extent that more labor and capital flow toward counties where SNAP spending has increased more and away from other counties where SNAP spending has increased less, the net impact of increased SNAP

[32] Reimer et al. (2015) considered alternative scenarios with elastic labor supply response and possible unemployment but found that these also predicted small impacts of SNAP elimination on total employment. The main reason for the small employment impact appears to be the study's budget neutrality assumption.

spending on the national economy may be relatively small, and the local impacts may represent mainly a redistribution of economic activity across locations.[33]

More comparable estimates of the employment effects of SNAP result from the modeling approach used by Hanson (2010), who sought to estimate the stimulus effects of an increase in SNAP spending in the context of the Great Recession and the then-recent enactment of the ARRA. Using the Food Assistance National Input-Output Multiplier (FANIOM) model—which did not assume budget neutrality and did assume that output and employment are determined by demand and are not constrained by a limited supply of labor or other factors of production—Hanson (2010) estimated three types of national SNAP employment "multipliers" (the impact on employment of a $1 billion increase in SNAP spending). The "Type I multiplier"—accounting for the "direct" employment impacts (e.g., employment in the food retail, wholesale, and transportation sectors to provide additional food and other products to SNAP beneficiaries) and the "indirect" employment impacts (e.g., additional employment in farming, food processing, food manufacturing, and other sectors needed to provide additional food and other products to SNAP beneficiaries)—was estimated to be 9,800 additional jobs (full- or part-time jobs or self-employed jobs) per $1 billion of SNAP spending (in 2008 dollars). The "Type II multiplier"—which accounts for all of the impacts included in the Type I multiplier, plus the induced effects of increased labor income resulting from the Type I effects on demand for goods and services (thus inducing increased demand for labor)—was estimated to be 15,900 jobs per $1 billion of SNAP spending. The "Type III multiplier"—which accounts for all of the impacts included in the Type II multiplier, plus the induced effects of increased capital income (dividends, interest, and rent) on demand for goods and services—was estimated to be 19,800 jobs per $1 billion of SNAP spending.

The Type III multiplier may be the most comparable to the local SNAP impacts that we estimate econometrically because our impact estimates can reflect the effects of any of the direct or induced effects included in a Type III multiplier.[34] Hanson's (2010) estimate of this multiplier (equivalent to 0.198 job per $10,000 of SNAP spending) is comparable to the jobs multiplier that we estimate for all counties for the pre-recession and post-recession periods (0.17 to 0.18 job per $10,000 of SNAP redemptions) but is less than one-sixth the size of the multiplier that we estimate for the Great Recession period (1.2 jobs per $10,000), using the results reported in table 1.[35]

One possible reason for our larger multiplier estimates is the previously discussed difference between a local and a national employment multiplier, with local multipliers possibly overstating impacts at a larger scale. On the other hand, if there are positive spillover effects of increased SNAP spending in a county on employment in neighboring counties, the local employment impacts may

[33] Ramey (2011, p. 681) makes this point in the context of local output multiplier effects of Government spending more generally, arguing that "if the government transfers $1 to Mississippi and finances it by increasing lump-sum taxes across all States, the true aggregate multiplier is 0, since the taxes and transfers cancel out in the aggregate. However, if we run a panel regression with time fixed effects (which net out the economywide rise in tax liabilities), we will estimate a multiplier of mpc/(1-mpc), where mpc is the marginal propensity to consume. If the marginal propensity to consume were 0.6, then we would estimate a multiplier of 1.5 at the State level, even though the aggregate multiplier for this experiment is 0."

[34] In addition, the data source that we used to estimate employment—the BEA county-level employment data—reports the number of full or part time jobs or self-employed, comparable to the measure of employment used for the estimates cited above from Hanson (2010). Hanson also reports employment multipliers in full-time equivalent (FTE) jobs plus self-employed, which are somewhat smaller than his estimated multipliers for full- or part-time jobs and are not comparable to our estimates.

[35] Recall that our estimates for the Great Recession and soon after, incorporating other government spending and using instrumental variables approaches, were of a similar magnitude (see tables 3 and 4).

underestimate the broader economic impacts of SNAP spending. As shown in table 2, we do find positive spillover effects of SNAP spending in neighboring counties on employment in nonmetro counties, though we find no statistically significant spillover effects from neighboring counties for metro counties or for all counties as a whole. So spillover effects to neighboring counties do not appear to explain why our estimated local multipliers are so much larger than Hanson's national jobs multiplier estimates. Negative spillover impacts of SNAP spending on non-neighboring counties may account for some of the difference, or Hanson's national multiplier estimates may be too small.[36] We leave this as a topic for further research.

A rapidly growing literature has estimated local multipliers for government spending of various types in the United States, especially focusing on the impacts of ARRA spending (though none of these studies estimated the impacts of SNAP spending specifically). The approaches and key findings of 10 recent studies are summarized in table 5 and discussed in appendix D. The local employment multipliers estimated across these studies range from a low value of 0.05 job per $10,000 to a high value of over 1.0 job per $10,000. The wide range of estimated local fiscal employment multipliers in this literature is not surprising, given the range of time periods (pre- to post-ARRA enactment in most studies but including much longer time periods in a few studies), geographic focus (States in most studies, counties or multicounty regions in some), types of government spending investigated (e.g., Federal ARRA spending, defense spending, total State spending, spending by Federal department), and econometric approaches and specifications used.

Table 5

Key findings of related studies of local multiplier effects of government spending

Study	Econometric approach	Geography	Period	Key results	Jobs/$10,000 of spending
Feyrer and Sacerdote (2011)	Cross-sectional OLS and IV long difference (LD) and panel fixed effects (FE) regressions. OLS panel regressions of monthly employment on State-level monthly spending to investigate timing of impacts of ARRA spending overall and by Federal department.	States and counties	Feb. 2009 to Oct. 2010	$100,000 increase in ARRA spending increased employment by 1.1 jobs in IV-LD State-level regression; 2.3 jobs in IV-FE State-level regression; 3.3 jobs in IV-LD county-level regression. Largest effects of USDA (mainly SNAP) and HUD ARRA spending—60 jobs per $100,000 increase in monthly USDA spending (5 jobs per $100,000 annual increase).	0.11 (IV-LD State level) 0.23 (IV-FE State level) 0.33 (cross-sectional county first difference IV) 0.50 for USDA (mainly SNAP) spending (monthly State-level OLS panel).

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[36] A recent meta-analysis of 98 empirical studies of fiscal GDP multipliers (i.e., the increase in GDP per additional dollar of government spending of different kinds) found that during a recession, the estimated multiplier was largest for government transfer payments—with a mean estimated value of more than 2.5 (Gechert and Rannenberg, 2018). Hanson (2010) estimated a GDP multiplier for SNAP spending during the Great Recession of 1.79, substantially smaller than the mean estimated value implied by Gechert and Rannenberg's empirical results. If Hanson's GDP multiplier was an underestimate, it suggests that his employment multiplier estimates, which were based on the GDP multiplier, also could be underestimates.

Table 5

Key findings of related studies of local multiplier effects of government spending—continued

Study	Econometric approach	Geography	Period	Key results	Jobs/$10,000 of spending
Chodorow-Reich et al. (2012)	OLS-LD and IV-LD regressions.	States	Dec. 2008 to July 2009	$100,000 increase in Medicaid outlays increases employment by 3.8 job-years.	0.38 (IV)
Wilson (2012)	OLS-LD and IV-LD regressions.	States	Feb. 2009 to Feb. 2010	$100,000 increase in ARRA spending increases employment by 2.2 jobs (for outlays); by 1.1 jobs (for obligations); by 0.8 job (for announcements).	0.22 (for outlays); 0.11 (for obligations); 0.08 (for announcements).
Shoag (2013)	IV first difference (IV-FD) regressions.	States	2009 and 2010	$100,000 increase in State spending increases employment by 4.5 jobs and personal income by $143,000 (income multiplier = 1.43)	0.45
Conley and Dupor (2013)	Cross-sectional IV regressions.	States	April 2009 to March 2011	Cost of creating 1 job-year using ARRA spending was $202,000.	0.05 in preferred specification (0.08 if relax assumption of fungibility of ARRA funds and State revenue losses).
Nakamura and Steinsson (2014)	IV – State or region military procurement spending predicted by national procurement spending.	States and 10 multi-State regions.	1966 to 2006	A 1 percent of GDP increase in military procurement spending in a State increases State GDP over 2 years by 1.43 percent (GDP multiplier = 1.43) and State employment by 1.28 percent. Regional multipliers are larger. Larger GDP multiplier in periods and States with more excess capacity.	0.16 (= 1.28 x average employment/GDP ratio during study period).
Dube et al. (2015) (unpublished)	OLS-FE regressions.	Counties and aggregation of nearby counties.	First quarter of 2008 to third quarter of 2011	$100,000 increase in ARRA spending increases employment within county by 0.76 job-year over 2 years; increases employment in counties within 120-mile radius by 3.28 job-years; larger effects in counties with more excess capacity.	0.08 for within county; 0.33 for aggregations of counties with centroids within 120-mile radius of county centroid.

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Table 5

Key findings of related studies of local multiplier effects of government spending—continued

Study	Econometric approach	Geography	Period	Key results	Jobs/$10,000 of spending
Suarez-Serrato and Wingender (2016) (unpublished)	Treatment effects and regular and quantile IV estimation based on effects of "census shocks" in population estimates on flow of Federal funds.	Counties	1970 to 2009	Average Federal cost per job of $31,000 and personal income multiplier of 1.86 from treatment effects model. Similar results from IV regressions and a wide range of robustness checks. Spillover effects from neighboring counties small and statistically insignificant but reduce multiplier estimates slightly. Quantile IV regressions show larger multiplier effects in slowest growing counties.	0.32 in baseline specification; larger than 1.0 for slowest growing counties in quantile IV regressions.
Dupor and Guerrero (2017)	IV panel regressions (with and without State and year fixed effects in State-level analysis).	States and national	1951 to 2014	A 1 percent of personal income increase in defense spending increases 2-year growth in national inflation-adjusted personal income by 0.33 percent (income multiplier = 0.33) and 2-year employment growth by 0.39 percent; State-level 2-year income growth multiplier = 0.22 and employment growth multiplier = 0.27 (with State fixed effects); spillovers with primary State trading partner increase 2-year multipliers to 0.25 for income growth and 0.31 for employment growth.	0.05 (= 0.27 x average ratio of employment to inflation-adjusted personal income during study period).

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Table 5

Key findings of related studies of local multiplier effects of government spending—continued

Study	Econometric approach	Geography	Period	Key results	Jobs/$10,000 of spending
Dupor and McCrory (2017)	OLS and IV cross-sectional regressions.	Large counties and satellite labor market areas (LMAs).	Fourth quarter of 2008 to fourth quarter of 2010.	$1 million in ARRA spending in one part of a LMA increases employment growth there by 10.3 people and employment growth in the rest of the LMA by 8.5 people. The wage multiplier is 0.64 for spending in the same subregion and 0.50 for spending in the adjacent subregion. Spillover effects are confined to the services sector. Total wage and employment impacts are larger from funds spent in the satellite subregion than from funds spent in the largest county of a LMA; e.g., employment multiplier of 30.8 job-years per $1 million spent in satellite subregion versus 12.6 job-years per $1 million spent in largest county. Multiplier impacts not statistically significantly larger with larger LMA aggregations.	0.10 for spending within subregions of an LMA, 0.09 for spending in adjacent subregion, 0.19 total for spending in an LMA. 0.31 for spending in satellite subregions and 0.13 for spending in largest county of a LMA.

Notes: LMA=labor market areas. OLS=ordinary least squares. LD=long difference. FE=fixed effects. ARRA= American Recovery and Reinvestment Act. SNAP=Supplemental Nutrition Assistance Program. HUD=U.S. Department of Housing and Urban Development. FD=first difference. GDP=Gross Domestic Product.

Source: USDA, Economic Research Service analysis of studies noted.

With only one exception (Dupor and McCrory 2017), the studies that reported estimates using both OLS and instrumental variables (IV) regression methods found larger impacts of government spending using the IV approach, suggesting that OLS estimates are biased downward due to problems of reverse causality and/or measurement error in the government spending variable. This is consistent with our results, which also find larger impacts using IV estimators. The study that comes closest to estimating local multiplier impacts of SNAP is Feyrer and Sacerdote (2011), which (among other analyses) investigated the impacts of ARRA spending by Federal department on State-level employment using OLS regressions. They found that ARRA spending by most Federal departments had small and statistically insignificant effects on employment, but ARRA spending by USDA (89 percent of which was through the SNAP program through September 2010) was associated with an average increase of about 0.50 job per $10,000 of annualized spending within the first 8 months after ARRA was enacted.[37] Given that this result was from an OLS regression, this estimate may be biased downward as noted earlier.

[37] Feyrer and Sacerdote (2011) estimated even larger impacts of ARRA spending by the U.S. Department of Housing and Urban Development, but these impacts were very imprecisely estimated.

Another reason that Feyrer and Sacerdote's (and other studies') impact estimates could be biased downward is measurement error in the data that they used for government spending. Especially at the local level, such data may be subject to substantial errors due to missing data, possible double-counting of payments (e.g., payments to primary contractors and payments to subcontractors from the same flow of funds), and misclassification of the location of the recipient of funds (e.g., Federal payments to State governments that are passed on to local governments but counted in Federal funds data as payments to the county of the State capital; payments to an organization with an address in one county that actually uses the funds in a different county). Few of these measurement problems are present with the SNAP redemptions data that we used. These funds are electronically reported, subject to audits, and tied to specific grocery stores in specific locations. Since measurement error for an explanatory variable in a regression tends to bias the estimated coefficient of that variable towards zero (Greene, 1990), the impacts of government spending in some of the studies reviewed may be underestimated for that reason.[38]

No other study reviewed besides Feyrer and Sacerdote (2011) investigated impacts of different types of Federal government spending within the same study. Chodorow-Reich et al. (2012) estimated the employment impacts of increased Medicaid payments to States under ARRA, finding an employment multiplier of 0.38 job-year per $10,000 spent by July 2009. Although Medicaid is targeted to a similar low-income population as SNAP, the short-term impacts of Medicaid payments to States may be smaller than the impacts of SNAP benefits, since SNAP benefits are paid directly to low-income people who spend those benefits almost immediately. By contrast, Medicaid payments to States are not provided directly to low-income people, may not be spent as quickly and may substitute for State spending on Medicaid or other programs. Indeed, as Chodorow-Reich et al. (2012) pointed out, the ARRA legislative text says that the first purpose of the increase in the Federal Medicaid Assistance Percentages was to "provide fiscal relief to States in a period of economic downturn." Given the intended use of increased Medicaid reimbursement rates to provide fiscal relief to States, the mechanism of impact of the payments studied by Chodorow-Reich et al. (2012) was likely much different than the mechanism of impact of increased SNAP benefit rates, leading plausibly to smaller short-term impacts than the impacts of SNAP redemptions.

Two other studies also focused on the effects of State spending as affected by ARRA spending (Wilson, 2012; Conley and Dupor, 2013). Wilson (2012) used factors determining formula grants by the Federal Highway Administration, the school-aged share of the population, and prior Medicaid allocations to a State to predict ARRA funding to States and using those predicted values in an IV regression framework found that ARRA outlays had an employment multiplier of 0.22 job per $10,000.[39] Since Wilson's estimates reflect variations in ARRA spending driven by variations in transportation, education, and Medicaid spending, all of which are filtered through State governments, the impacts that he estimates may plausibly be smaller than the impacts of SNAP redemptions that we estimate, for the same reasons discussed earlier.

Conley and Dupor (2013) estimated an even smaller employment multiplier of State spending (0.05 job-year per $10,000), using Federal highway funding and the share of total State revenues derived from relatively "inelastic" revenue sources (such as property taxes, selective sales taxes, liquor store

[38] Use of IV regressions in many fiscal impact studies may eliminate the bias caused by measurement error (Greene, 1990), but only if there are not measurement errors in the instrumental variables used to predict government spending.

[39] Wilson also estimated employment multiplier effects of announcements of spending and obligations under ARRA and found smaller effects.

revenue, and others) to predict State spending.[40] In their baseline regression model, Conley and Dupor (2013) assumed that Federal funds are fungible, so that States could use ARRA funds to offset State revenue losses rather than increasing spending. When they relaxed that assumption, they estimated a somewhat larger employment multiplier (0.08 job-year per $10,000), though still well below that estimated by most other studies and our own estimates of SNAP impacts.

Two studies investigated the impacts of defense spending at the State level over a long span of time (Nakamura and Steinsson, 2014; Dupor and Guerrero, 2017). Both of those studies estimated substantially smaller employment impacts of defense spending than our estimated impact of SNAP redemptions. This is perhaps not surprising, given that SNAP benefits are used directly by low-income individuals with a high propensity to spend those benefits, compared to payments for defense contracts that may not be spent in a local or even State economy and that are not targeted to low-income people.

Several studies investigated spillover effects of government spending in one location on neighboring locations, as this study does. Some of these studies found positive spillover effects across States (Nakamura and Steinsson, 2014; Dupor and Guerrero, 2017) and some found positive spillover effects across counties (Dube et al., 2015; Dupor and McCrory, 2017), but significant spillover effects were not found in all studies (Suarez-Serrato and Wingender, 2016).

Dupor and McCrory (2017) also investigated how local fiscal multipliers vary between the largest county and the satellite counties in a subregion, and found larger total employment effects from spending in satellite subregions (0.31 job-year per $10,000 spent in a satellite region) than in the largest county of a labor market area (0.13 job-year per $10,000 spent in the largest county). These results suggest that ARRA spending in exurban and rural counties surrounding urban areas generated more employment than an equal amount of spending in central urban areas and are consistent with our finding of larger employment multiplier impacts in nonmetro than in metro counties.

Some studies have also investigated the heterogeneity of impacts of government spending depending on the economic conditions in a region. Nakamura and Steinsson (2014) found larger GDP multiplier effects of defense spending during periods and in States when there was more excess capacity (based on differences in unemployment rates). Dube et al. (2015) found much larger multiplier effects of ARRA spending in counties and regions with greater excess capacity (measured by differences in employment to population ratio). Suarez-Serrato and Wingender (2016) investigated heterogeneous impacts of Federal spending across quantiles of the distribution of county-level growth and found employment multipliers greater than 1 job per $10,000 for slower growing counties. These findings are consistent with our finding of larger employment multipliers during the recession than prior to or after the recession and with our finding of higher multipliers in nonmetro than metro counties (since nonmetro counties tend to grow more slowly than metro counties).[41]

[40] Whether the inelastic share of State revenue is actually "exogenous to" (unaffected by) local economic conditions appears questionable. If this assumption is not true, it may have biased the estimates of Conley and Dupor (2013). They did not address this issue in their study.

[41] Findings of larger employment multipliers during a recession from our study and other studies are also qualitatively consistent with the findings of the meta-analysis of Gechert and Rannenberg (2018), who found much larger GDP multipliers for government spending in general and for transfer payments in particular during a recession. Gechert and Rannenberg (2018) did not investigate employment multipliers of government spending, so their results are not directly comparable to ours.

Overall, our results are broadly consistent with the literature on local employment growth multipliers of government spending, although the level of impact that we estimate for SNAP redemptions is larger than found in previous studies for other types of government spending or for government spending in general. We believe, based on our own analysis and review of the approaches and results in the literature, that these differences are evidence of the relative effectiveness of SNAP as a form of economic stimulus, especially during and soon after the Great Recession, and are not the result of biases caused by reverse causality, measurement error, or other econometric problems. It appears that as a program targeted directly to low-income consumers with a high propensity to quickly spend additional benefits, SNAP has among the largest employment multiplier impacts of any Federal program thus far studied.

Conclusions

Participation in the SNAP program grew rapidly from 2001 to 2013, with greater participation and average redemptions per capita in rural than in urban areas. The growth in participation and redemptions was particularly rapid during the Great Recession and immediately after, due to rising unemployment and poverty and to changes in SNAP policies. One of the arguments cited to support an increase in SNAP benefits in the 2009 American Recovery and Reinvestment Act was the relatively large impact SNAP benefits were predicted to have on economic activity and employment per dollar spent (Zandi, 2009; Hanson, 2010). Although the impacts of SNAP payments on economic activity have long been predicted using national-level economic simulation models, such impacts have not been statistically tested and confirmed after the fact.[42]

This study is the first of its kind to attempt to estimate such impacts of SNAP payments on county-level employment. The use of county-level data enabled estimation of the extent to which impacts differ between metro and nonmetro counties and the extent to which SNAP redemptions in one county affect employment in neighboring counties. The study also compared the impacts of SNAP redemptions to the impacts of other transfer payments and other Federal spending in general on employment.

We find that SNAP redemptions (the value of SNAP benefits redeemed by SNAP-authorized stores) had a positive average impact on county-level employment throughout the 2001 to 2014 period, with an increase of between 0.4 and 0.5 job per $10,000 of additional SNAP redemptions across all counties over the entire period. However, considering impacts in nonmetro versus metro counties, the impacts were positive and statistically significant over the entire study period only in nonmetro counties. As expected, the impact of SNAP redemptions on employment was greater during the recession period (2008 to 2010) than either the pre-recession period (2001 to 2007) or the post-recession period (2011 to 2014) in both nonmetro and metro counties. In the recession period, SNAP redemptions contributed to about 1.0 job per $10,000 of additional SNAP redemptions in nonmetro counties and about 0.4 job per $10,000 of additional SNAP redemptions in metro counties. During the pre-recession period, SNAP redemptions had a smaller positive impact in nonmetro counties (about 0.2 job per $10,000 of SNAP redemptions) but a negative impact in metro counties (about -0.2 job per $10,000 of SNAP redemptions). In the post-recession period, SNAP redemptions had a statistically insignificant impact on employment in both nonmetro and metro counties.

The finding of positive impacts of SNAP on employment during the recession is qualitatively similar to that of a national-level study predicting the impacts of SNAP during the Great Recession, but our estimated local employment multiplier impacts are larger (Hanson, 2010). We also found that the SNAP local employment multiplier is substantially larger than the average employment multipliers of government transfer payments to individuals or Federal spending as a whole and is larger than that estimated for government spending in other econometric studies. Possible reasons for larger local employment multipliers in the present study include:

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[42] As noted in our review of related literature above and in appendix D, no other published studies examined the impacts of SNAP on county-level employment using econometric methods. The only study to come close to that focus is Feyrer and Sacerdote (2011), which investigated impacts of additional spending by different Federal departments (including USDA) under ARRA. Their findings concerning the impacts of additional USDA spending under ARRA (which was dominated by the increase in SNAP payments) support our qualitative conclusion that SNAP benefits had a relatively (compared to other types of Government spending) large impact on employment during the recession. The findings of Feyrer and Sacerdote (2011) and other relevant studies, and their methodological approaches and limitations, are discussed in more detail in appendix D.

- Our measure of government spending—administrative data on SNAP redemptions—is less subject to measurement errors than many other sources of data on government spending.
- Some of the other studies used econometric methods that may have produced downwardly biased multiplier estimates.[43]
- The fact that SNAP provides direct payments to low-income households rather than grants to States or private contractors may result in larger employment impacts than many other types of government spending, particularly during a major recession.

Our results comparing employment impacts of SNAP during the Great Recession to impacts pre- and post-recession are qualitatively consistent with several studies that have shown that local employment impacts of government spending are greater in periods or locations when or where there are more unused resources, such as unemployed labor or unused capacity of capital, in the economy. Our findings of larger employment impacts of SNAP in nonmetro counties and of larger positive spillover impacts in nonmetro counties are also consistent with several studies, as discussed in the previous section.

As with all econometric studies, our estimates are subject to limitations, including concerns about omitted factors and reverse causality. We sought to address problems of omitted factors by statistically controlling for local economic conditions (unemployment and poverty), the economic and demographic structure of counties, and other government spending, and by using second difference least square regressions to control for county-level unobserved fixed factors and linear employment trends. Including these controls increased the size of the positive estimated impacts. We addressed the possible reverse causality between SNAP redemptions and employment outcomes using a variety of advanced econometric methods. These methods estimated even larger positive impacts of SNAP redemptions than the OLS models. The robustness of our results to a variety of estimation approaches, and the general consistency of our findings with the literature despite the larger impacts that we estimate for SNAP, strengthens our confidence that they represent the true causal impact of SNAP redemptions on employment.

Even assuming our estimates represent true impacts at the county level, they may not aggregate to impacts per dollar of redemptions as large at the national level. If increases in employment occurring in one county due to increased SNAP redemptions occur at the expense of employment in other counties that had a smaller increase in SNAP redemptions, the employment impacts of SNAP redemptions at a larger geographic scale could be smaller than those occurring at the county scale. Although our spatial econometric model did not find evidence of such negative spillover impacts on neighboring counties, such displacement effects need not only affect neighboring counties. Investigating this issue is a topic worthy of future research.

Given the large possible impacts of SNAP payments that we have found in this analysis, their importance for policy, and the lack of comparable published work, the need for additional research on this topic appears to be great. Further econometric research could seek to identify the impacts of SNAP

[43] For example, Feyrer and Sacerdote (2011) and Dube et al. (2015) used OLS regression methods that may have yielded downwardly biased estimates of impacts of government spending, due to reverse causality, as discussed previously. Our own OLS regression results could also be downwardly biased for the same reason, and our instrumental variables (IV) results shown in table 4 and appendix C suggest that may be the case. Thus, our estimates of the impacts of SNAP redemptions on county-level employment may be conservative, even though they are larger than impacts of other government spending found in other studies.

payments on regional economies using other outcome metrics (e.g., impacts on earnings, personal income, or local GDP), other units of analysis (e.g., aggregations of counties such as commuting zones), and other identification strategies. In addition, further simulation model-based research could investigate whether and under what conditions the magnitudes of local impacts measured in this study are consistent with model predictions.

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Appendix A. Econometric Approach

We estimated the impacts of SNAP benefits per capita on county employment per capita in metro versus nonmetro counties using several econometric methods to assess the validity and robustness of the conclusions.

Ordinary Least Squares First Difference (OLS-FD) Regression Model

The OLS-FD models are based on the regression equation:

$$(1)\quad Y_{ct} = \alpha_c + \alpha_t + \beta S_{ct} + \gamma X_{ct} + \varepsilon_{ct}$$

Y_{ct} represents employment per capita in county c during year t; S_{ct} represents inflation-adjusted SNAP redemptions per capita; X_{ct} is a set of observed control variables that may be correlated with SNAP redemptions per capita and with employment per capita (economic conditions; demographic characteristics of the population, and economic structure of the county); α_c are the effects of fixed county-level factors such as location and climate ("county fixed effects"); α_t are the effects of common (across counties) factors in a given year such as national macroeconomic conditions ("year-fixed effects"); ε_{ct} is an idiosyncratic error term assumed to be uncorrelated with S_{ct} and X_{ct}; and β is the parameter of interest, reflecting the effect of SNAP redemptions per capita on employment per capita at the county level.[44]

Taking first differences of equation (1) to eliminate the county fixed effects (α_c), and renaming $\Delta\alpha_t = \alpha_t'$, we have:

$$(2)\quad \Delta Y_{ct} = \alpha_t' + \beta \Delta S_{ct} + \gamma \Delta X_{ct} + \Delta\varepsilon_{ct}$$

where $\Delta Y_{ct} = Y_{ct} - Y_{ct-1,}$ and similarly for other variables in equation (2).

In one version of the OLS-FD model, we account for all time-varying State-level factors (such as State policies and State-level economic trends) by including State-by-year fixed effects (α_{st}) instead of year-fixed effects:

$$(2')\quad \Delta Y_{ct} = \alpha_{st}' + \beta \Delta S_{ct} + \gamma \Delta X_{ct} + \Delta\varepsilon_{ct}$$

Ordinary Least Squares Second Difference (OLS-SD) Regression Model

If the true regression model includes linear time trends in each county (and the other assumptions of equation (1) hold), a second difference regression model with county and year-fixed effects will produce unbiased estimates of the true parameters (while the first difference regression model may yield biased estimates):

$$(3)\quad Y_{ct} = \alpha_{c0} + \alpha_c t + \alpha_t + \beta S_{ct} + \gamma X_{ct} + \varepsilon_{ct}$$

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[44] Since both the numerator and denominator of β are in per capita terms, it is equivalent to interpret β as the number of jobs produced per $ of SNAP redemptions. We use this interpretation in our presentation and discussion of results.

Taking first differences of equation (3) (and renaming $\Delta\alpha_t = \alpha_t'$), we have:

$$(4)\ \Delta Y_{ct} = \alpha_c + \alpha_t' + \beta\Delta S_{ct} + \gamma\Delta X_{ct} + \Delta\varepsilon_{ct}$$ [45]

Taking first differences of equation (4) (second differences of equation (3)), and renaming $\Delta\alpha_t' = \alpha_t''$, we have:

$$(5)\ \Delta^2 Y_{ct} = \alpha_t'' + \beta\Delta^2 S_{ct} + \gamma\Delta^2 X_{ct} + \Delta^2 \varepsilon_{ct}$$

where $\Delta^2 Y_{ct} = \Delta Y_{ct} - \Delta Y_{ct-1,}$ and similarly for other variables in equation (5).

Equation (5) is the equation estimated by the OLS-SD estimator, which allows for both county-level fixed effects and county-level linear time trends. Since equation (3) is a more general form of equation (1), estimating the parameters β and using equation (5) yields more robust estimates of the parameters, accounting for potential biases resulting from different linear employment trends across counties. Including State-by-year fixed effects (α_{st}'') in equation (5) instead of year-fixed effects also makes the parameter estimates robust to differences in State-level policies and economic conditions:

$$(5')\ \Delta^2 Y_{ct} = \alpha_{st}'' + \beta\Delta^2 S_{ct} + \gamma\Delta^2 X_{ct} + \Delta^2 \varepsilon_{ct}$$

Because of its robustness to numerous omitted factors, equation (5') is our preferred specification among the OLS models estimated. However, it may not be robust to endogeneity of either S_{ct} or X_{ct}. We use instrumental variables methods to address this concern.

Instrumental Variables Fixed Effects (IV-FE) and Long Difference (IV-LD) Models

The IV-FE model addresses the potential endogeneity of S_{ct} in equation (1) by using instrumental variables (Z_{ct}) to predict S_{ct}:

$$(6)\ S_{ct} = \delta_c + \delta_t + \theta Z_{ct} + \lambda X_{ct} + \nu_{ct}$$

We use State-level SNAP policies (P_{st}) as the instrumental variables (Z_{ct}) to estimate equation (6). State-level SNAP policies have varied substantially over time and across States, are arguably exogenous to county-level economic outcomes, arguably affect economic outcomes only through their effects on SNAP participation and redemptions, and have been shown in several studies to significantly predict differences in SNAP participation. Equation (6) is combined with equation (1) to estimate the IV-FE model. [46] We test the strength and validity of the instrumental variables using tests for weak identification and overidentification.

An alternative IV estimator is to estimate equation (6) in "long differences" (IV-LD); i.e., taking differences of more than 1 year for all variables in equation (6):

$$(6')\ \Delta S_{c,2008\rightarrow t} = \Delta\delta_t + \theta Z_{c,2007} + \lambda\Delta X_{c,2008\rightarrow t} + \Delta\nu_{c,2008\rightarrow t}$$

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[45] If a first difference estimator were used to estimate equation (4), excluding the county-level fixed effects (α_c), the resulting estimate of β could be biased if the fixed effects (α_c) are correlated with the change in SNAP redemptions (ΔS_{ct}), since the fixed effects would be included in the error term.

[46] Equation (2') cannot be estimated using instrumental variables estimation because the instrumental variables used are State-level SNAP policies, which are perfectly correlated with the State-by-year fixed effects in equation (2').

For the IV-LD estimation, we use the level of SNAP redemptions per capita in 2007 ($S_{c,2007}$) as an instrumental variable ($Z_{c,2007}$) to predict changes in SNAP redemptions per capita between 2008 and later years. Because of the increase in SNAP benefit rates authorized by ARRA, we expect SNAP redemptions per capita in 2007 to be a good predictor of the change in benefits during the period of ARRA implementation. We also expect SNAP redemptions per capita in 2007 to be exogenous and valid to exclude from the second-stage regression.

Dynamic Panel Generalized Method of Moments (DP-GMM) Regression Model

The IV models may fail to resolve problems caused by endogeneity of S_{ct}. If the instrumental variables do not pass the weak instruments or overidentification tests, the estimated IV-FE or IV-LD results may be more biased than the OLS results (Bound et al., 1995). Furthermore, endogeneity of the control variables (X_{ct}) could also lead to biases in the estimated coefficients using either OLS (first or second difference) or simple IV methods that instrument only for SNAP redemptions.

To address these issues, we use the estimation method of Arellano and Bond (1991), which applies generalized method of moments (GMM) estimation methods to dynamic panel data. The equation estimated is a slightly generalized version of equation (2'), possibly including lagged values of the dependent variable as explanatory variables:[47]

$$(7)\ \Delta Y_{ct} = [\sum_{i=1}^{L} \delta_i\, \Delta Y_{c,t-i}] + \alpha_{st}{}' + \beta \Delta S_{ct} + \gamma \Delta X_{ct} + \Delta\varepsilon_{ct}$$

The instrumental variables used in the GMM estimation are lagged values of, Y_{ct}, S_{ct} and X_{ct}. Under the assumption that ε_{ct} is not serially correlated, the values of these variables lagged at least two periods are uncorrelated with $\Delta\varepsilon_{ct}$, even if these variables are endogenous (i.e., ε_{ct} correlated with current or future values of these variables).[48] For example, these assumptions imply:[49]

$$(8)\ E\,(X_{c,t-2}\Delta\varepsilon_{ct}) = E\,(X_{c,t-2}\varepsilon_{ct}) - E\,(X_{c,t-2}\varepsilon_{c,t-1}) = 0$$

The DP-GMM estimator uses the sample analog of moment conditions of the form of equation (8) to estimate the parameters of equation (7).[50] A large number of moment conditions and instruments can be generated using this method, leading to potential problems of overfitting and weak instruments (Roodman, 2009).[51] We seek to minimize these problems by using only one lag period for each explanatory variable to construct instruments. The lag period chosen was based on a test

[47] We also estimated DP-GMM versions of the second difference model in equation (5'), which relies on similar moment conditions as the first difference model (but uses variables lagged at least three periods as instrumental variables). We do not report the full results of these models but comment below when discussing results on how the parameter of interest (coefficient of SNAP redemptions per capita) varies between the first difference and second difference models. Generally, the second difference DP-GMM models estimate a somewhat larger impact of SNAP redemptions on county employment than the first difference DP-GMM models.

[48] This assumes that ε_{ct} is uncorrelated with past values of, Y_{ct} , S_{ct} and X_{ct}.

[49] Note that $E\,(X_{c,t-1}\,\Delta\varepsilon_{ct})$ is not 0 because $E\,(X_{c,t-1}\,\varepsilon_{c,t-1})$ is not zero if X_{ct} is endogenous. This is why at least two lags are necessary for the GMM instruments.

[50] Since equation (8) holds separately for each variable in $X_{c,t-2}$ and each t, a large number of moment conditions of the form of equation (8) holds if the no serial correlation assumption is true. Similar moment conditions also hold for each $Y_{c,t-2}$, S_{ct-2}, and t, and for longer time lags of $X_{c,t-i}$, $Y_{c,t-i}$, and S_{ct-i}, for values of i greater than 2.

[51] To our knowledge, no test for weak instruments in the dynamic panel GMM model is available.

of serial correlation in the first difference error term developed by Arellano and Bond (1991) and implemented in Stata, along with the DP-GMM estimator, by Roodman (2009). If the assumption of no serial correlation in ε_{ct} holds, there should be zero second order and higher order serial correlation in $\Delta\varepsilon_{ct}$.[52] In this case, $X_{c,t-2}$ are valid instrumental variables. In the more general case of no (n+1)th or higher order serial correlation in ε_{ct} (e.g., if ε_{ct} follows a moving average process of order MA(n)), there is no (n+2)th or higher order serial correlation in $\Delta\varepsilon_{ct}$, and $X_{c,t-n-2}$ are valid instrumental variables (Arellano and Bond 1991). Hence, we select the lag length (n+2) (with $n \geq 0$) of the instrumental variables to correspond to the lag length in which a serial correlation test fails to reject the null hypothesis of no serial correlation of order n+2 in the residuals in estimating equation (7).

Unfortunately, in all cases that we estimated using the DP-GMM model, the overidentification test indicates a strong rejection of the validity of the instrument set. Since the DP-GMM model uses separate moment conditions for each t, rejection of an overidentification test could result from heterogeneity in the coefficients of the endogenous explanatory variables over time; e.g., if the impacts of SNAP benefits on employment were different in 2010 than in 2005 (controlling for other factors).[53] The model thus could fail an overidentification test even if it correctly identifies the average (over years) impact of each explanatory variable on the dependent variable.

Spatial Durbin Error Model (SDEM)

To address the possibility of spillovers of impacts of SNAP redemptions from neighboring counties, we estimate a SDEM version of equation (5'), which assumes that the outcome variable can be affected by SNAP redemptions per capita and the control variables in the same county (S_{ct} and X_{ct}) and by the average values of SNAP redemptions and the control variables in neighboring counties (S_{nt} and X_{nt}):[54]

$$(9)\ \Delta^2 Y_{ct} = \alpha_{st}'' + \beta_o \Delta^2 S_{ct} + \beta_n \Delta^2 S_{nt} + \gamma_o \Delta^2 X_{ct} + \gamma_n \Delta^2 X_{nt} + \Delta^2 u_{ct}$$

The coefficient β_o is the estimate of impact of SNAP redemptions in the own county, and β_n is the estimated impact of the average of SNAP redemptions in neighboring counties. The SDEM allows for spatial autocorrelation in the error term ($\Delta^2 u_{ct}$). The model estimates the average correlation between the error terms in neighboring counties and takes the autocorrelation into account in estimating standard errors. The SDEM is estimated by maximum likelihood, assuming normality of the error term.

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[52] First order serial correlation in $\Delta\varepsilon_{ct}$ is present even if ε_{ct} is not serially correlated: $E(\Delta\varepsilon_{ct}\Delta\varepsilon_{ct-1}) = E((\varepsilon_{ct}-\varepsilon_{ct-1})(\varepsilon_{ct-1}-\varepsilon_{ct-2})) = -E(\varepsilon_{ct-1}\varepsilon_{ct-1}) < 0$.

[53] Our estimates using OLS models for different time periods (reported in table 1) indicate that there is heterogeneity over time in the impacts of SNAP redemptions on employment.

[54] Equation (9) is a simplified exposition of the SDEM model using our notation. The model is formally specified as:

$$Y = \alpha + X\beta + WX\theta + u$$

$$u = \lambda W u + \varepsilon$$

Y is the dependent variable, X is the vector of explanatory variables (including SNAP redemptions and the control variables), W is a spatial weights matrix used to determine which neighbors are included in the weighted averages of variables for the neighbors (and may assign different weights depending on distance criteria), u is a spatially autocorrelated error term, ε is a spatially uncorrelated error term assumed to be normally distributed for the maximum likelihood estimation, β is the vector of coefficients of own-county impacts, θ is the vector of coefficients of neighboring-county effects, and λ is the spatial autocorrelation of the error term (u) (LeSage and Pace, 2009).

Appendix B. Variables and Data

The dependent variable in the analysis (Y_{ct}) is county-level employment per capita. The data for employment per capita are from the Regional Economic Information System of the U.S. Bureau of Economic Analysis (BEA).

The key explanatory variable is the county-level inflation-adjusted value (in 2010 $) of SNAP redemptions per capita (S_{ct}). The values of county-level SNAP redemptions were estimated by ERS using confidential store-level administrative data on SNAP redemptions provided to ERS by USDA, Food and Nutrition Service. County SNAP redemptions were divided by population estimates from the U.S. Census Bureau County Intercensal Estimates to estimate SNAP redemptions per capita and converted to inflation-adjusted 2010 values using the Consumer Price Index (CPI-U), from the Bureau of Labor Statistics (BLS).

The other explanatory control variables (X_{ct}) include variables representing county-level economic stress, economic structure, demographic structure, and other government spending. The economic stress variables include the county-level unemployment rate and poverty rate. The data for the county-level unemployment rate are from the BLS's Local Area Unemployment Statistics. The data for county-level poverty are from the U.S. Census Bureau's Small Area Income and Poverty Estimates.

The economic structure variables include the share of county employment by major industry type (using two-digit industry type codes from the North American Industrial Classification System (NAICS) to represent major industry types). Data on employment by industry type are from the Regional Economic Information System of the BEA, which uses data from the BLS Quarterly Census of Employment and Wages. For some industries in some counties and years, these data are not publicly available and were accessed through a memorandum of understanding between ERS and BLS.

The demographic structure variables include the population of the county, the child (under age 15) share of the population, the elderly (over age 64) share of the population, the Black or African American-only share of the population, the American Indian-only share of the population, the Asian or Pacific Islander-only share of the population, and the Hispanic share of the population. The source of these data was the U.S. Census Bureau County Intercensal Estimates.

The variables for other government funding include other transfer payments besides SNAP payments and total Federal funds spent in a county. Data on transfer payments are from the BEA's Regional Economic Information System. Data on total Federal funds spent in a county are from the U.S. Census Bureau's Consolidated Federal Funds Report (CFFR), which was discontinued after 2010.

The State-level SNAP policy variables used as instrumental variables to predict SNAP redemptions per capita (P_{st}) are from ERS's SNAP Policy Database for the years 2001-11. ERS has assembled some of the State-level SNAP policy variables for additional years beyond 2011, but these have not yet been publicly released. For this study, we used six SNAP policy variables that are available through 2013 from ERS, including (i) use of broad-based categorical eligibility to eliminate the SNAP asset test (BBCE); (ii) whether the State operated a Combined Application Project (CAP) for recipients of Supplemental Security Income (SSI) so that SSI recipients are able to use a streamlined SNAP application process; (iii) the proportion of the dollar value of all SNAP benefits that are accounted for by electronic benefit transfer (EBT); (iv) whether the State required fingerprinting of

SNAP applicants statewide (FINGERPRINT); (v) whether all legal noncitizen children who satisfy other SNAP eligibility requirements such as income and asset limits are eligible for Federal SNAP benefits (NONCITCHILD); and (vi) whether the State allows online applications statewide (OAPP). Only three of these variable were available through 2014: BBCE, FINGERPRINT, and OAPP. Other variables that are available in the SNAP Policy Database are either not available beyond 2011 or 2012, are not strictly policy variables, or are similar to variables that were used as instrumental variables.[55]

Many of these control variables and State SNAP policy variables have been used in previous studies of the determinants of SNAP participation (Kabbani and Wilde, 2003; Hanratty, 2006; Ratcliffe et al, 2008; Klerman and Danielson, 2011; Danielson and Klerman, 2013; and Ganong and Liebman, 2013). The control variables included are expected to affect both SNAP program participation and county-level employment; hence, we include them to address omitted variable bias concerns. Unlike previous studies of determinants of SNAP participation, however, we do not include State-level policy variables (such as TANF policies, minimum wage policies, or tax policies) as control variables in the regression models since we are able to control completely for such variables in the versions of the models that include State-by-year fixed effects (i.e., equations (1'), (5'), and (7)).[56]

Descriptive statistics of the variables used in the analysis are shown in table B-1.

Table B-1
Descriptive statistics of variables used in the analysis

	All counties		Nonmetro counties		Metro counties	
Variable	Unweighted mean	Std. dev.	Unweighted mean	Std. dev.	Unweighted mean	Std. dev.
Employment/capita	0.4538	0.0681	0.4471	0.0742	0.4663	0.0522
SNAP redemptions/cap. (2010 $)	135.2	105.0	137.8	112.1	130.4	90.0
Unemployment rate	0.0661	0.0279	0.0672	0.0293	0.0639	0.0247
Poverty rate	0.1495	0.0586	0.1602	0.0603	0.1292	0.0493
Population	98,095	312,696	25,036	24,676	236,625	502,718
Share of population:						
- Under age 15	0.1952	0.0288	0.1920	0.0295	0.2013	0.0264
- Over age 64	0.1570	0.0427	0.1693	0.0417	0.1337	0.0339
- Black only	0.0899	0.1448	0.0795	0.1480	0.1096	0.1363
- American Indian only	0.0204	0.0732	0.0263	0.0886	0.0093	0.0214

—continued

[55] State policy variables that are not available beyond 2011 or 2012 include whether the State operates call centers to serve SNAP recipients, whether the State disqualifies SNAP applicants who fail to comply with requirements of other means-tested programs such as the Temporary Assistance for Needy Families (TANF) program, whether the State has been granted a waiver to use a telephone interview in lieu of a face-to-face interview at initial certification or recertification, whether the State uses simplified reporting requirements for households with earnings to report changes, and whether the State excludes any or all vehicles from the asset test. Variables that are not strictly policy variables include the proportions of different SNAP recipient populations having recertification periods of particular ranges of months; these proportions depend on the distribution of the SNAP recipient population as well as on State policies and procedures. Variables similar to ones used as instrumental variables include using broad-based categorical eligibility to increase rather than eliminate the asset test, indicators of partial rather than full statewide implementation of a policy (e.g., call centers operated in part of the state rather than statewide), and various indicators related to the eligibility of particular legal noncitizen groups (children, adults, elderly) for SNAP benefits. Use of additional instrumental variables based on such indicators does not greatly affect the results of the analysis, and the failure of the model to pass the overidentification test is a problem for all such expanded models.

[56] It is not possible to estimate the IV-FE model, which uses State-level SNAP policies as instrumental variables, including State-by-year fixed effects.

Table B-1

Descriptive statistics of variables used in the analysis—continued

- Asian only	0.0114	0.0249	0.0064	0.0168	0.0208	0.0335
- Hispanic	0.0781	0.1292	0.0746	0.1335	0.0847	0.1203
Share of employment in:						
- Farming	0.0753	0.0710	0.0946	0.0733	0.0387	0.0485
- Forestry and fishing	0.0155	0.0186	0.0191	0.0196	0.0085	0.0139
- Mining	0.0179	0.0428	0.0227	0.0495	0.0088	0.0233
- Utilities	0.0056	0.0121	0.0057	0.0112	0.0052	0.0137
- Construction	0.0631	0.0272	0.0599	0.0265	0.0691	0.0275
- Manufacturing	0.0964	0.0768	0.0980	0.0823	0.0933	0.0652
- Wholesale trade	0.0277	0.0172	0.0266	0.0183	0.0297	0.0147
- Retail trade	0.1063	0.0260	0.1040	0.0266	0.1106	0.0241
- Transportation	0.0329	0.0208	0.0333	0.0210	0.0321	0.0205
- Information services	0.0110	0.0080	0.0100	0.0075	0.0130	0.0086
- Finance and insurance	0.0356	0.0169	0.0332	0.0143	0.0401	0.0202
- Real estate	0.0312	0.0182	0.0281	0.0177	0.0371	0.0175
- Professional services	0.0344	0.0265	0.0278	0.0246	0.0469	0.0255
- Management services	0.0047	0.0086	0.0034	0.0077	0.0072	0.0096
- Administration & waste mgmt.	0.0387	0.0219	0.0326	0.0202	0.0502	0.0202
- Educational services	0.0123	0.0149	0.0104	0.0134	0.0161	0.0169
- Health & social services	0.0880	0.0380	0.0848	0.0386	0.0940	0.0360
- Arts, entertainment, & recreation	0.0159	0.0161	0.0141	0.0152	0.0193	0.0172
- Accommodations & food services	0.0604	0.0313	0.0582	0.0317	0.0645	0.0301
- Other services	0.0601	0.0138	0.0595	0.0141	0.0614	0.0133
- Government	0.1671	0.0715	0.1739	0.0725	0.1542	0.0676
Transfer payments/cap. (besides SNAP) (2010 $)	7,289	1,532	7,671	1,481	6,562	1,354
Total Federal spending/cap. (2010 $)	14,687	11,941	15,380	11,881	13,367	11,944

Source: USDA, Economic Research Service.

Appendix C. Selected Econometric Results

Table C-1 presents results for the OLS-FD and OLS-SD models for all counties and all years, with year-fixed effects versus State-by-year fixed effects models. SNAP benefits have a positive and statistically significant impact on employment in all of these models, with the estimated coefficients ranging from 0.337 job to 0.586 job per $10,000 of SNAP benefits. Including State-by-year fixed effects results in similar estimated SNAP impacts on employment in the OLS-FD model but smaller impacts in the OLS-SD model. Allowing for county-specific linear time trends in employment (which the OLS-SD model allows for) results in larger estimated SNAP impacts than not allowing for such time trends (as in the OLS-FD model), whether year-fixed effects or State-by-year fixed effects are included in the model. Among all of the OLS models, the OLS-SD model with State-by-year fixed effects is the preferred model, since it is the least restrictive model among those estimated, accounting for more potential omitted factors than the others.

Table C-2 presents results of the instrumental variables models (IV-LD, IV-FE, and DP-GMM). The IV-LD results are presented for all counties for the period 2008-10. The estimated impact of SNAP redemptions for this period (1.16 jobs per $10,000 of SNAP redemptions) is very similar to the estimated impact of SNAP redemptions for all counties during this period using the OLS-SD model (1.25 jobs per $10,000); strengthening our confidence in the OLS-SD results as reflecting the true causal impact of SNAP redemptions. Nevertheless, a C test of the exogeneity of the SNAP redemptions variable in the IV-LD regression strongly rejects that assumption.[57]

The IV-FE model is presented for all counties for the period 2001-13, using three State SNAP policies as instrumental variables: CAP, EBT, and NONCITCHILD.[58] This was the only IV-FE model with at least three instrumental variables which jointly passed a weak instruments test (Kleibergen-Paap rank F statistic = 146.08) and an overidentification test (Hansen's J = 0.77, p = 0.6791).[59,60] The IV-FE model estimated a larger point estimate of the impact of inflation-adjusted SNAP redemptions for all counties over the entire study period than any of the OLS models estimated—0.761 job per $10,000 of SNAP redemptions. Although the coefficient estimate for inflation-adjusted SNAP redemptions per capita in this IV-FE model is much larger than the coefficient estimate in any of the OLS models, an endogeneity test only weakly rejects the exogeneity of inflation-adjusted SNAP redemptions per capita (p = 0.0683), reflecting the relatively large standard error of the IV-FE estimate of this coefficient.

Three versions of the DP-GMM model are reported in table C-2: one without any lag of the dependent variable (as in all of the preceding models), one including the first lag of the dependent variable, and one including the first and second lags of the dependent variable. All of the DP-GMM models reported used five lags of the dependent variable and all explanatory variables in each period

[57] The coefficient of SNAP redemptions per capita in the OLS version of the long difference model shown in table C-2 is 0.361 job per $10,000 (standard error = 0.195), indicating that the impacts of SNAP redemptions are substantially underestimated in this model relative to the IV-LD estimate.

[58] Data were available for six State-level policies for the period 2001-13: broad-based categorical eligibility to eliminate asset test (BBCE), combined application project (CAP), issuance of electronic benefit transfer cards (EBT), fingerprints required statewide (FPRINT), allowing noncitizen children who otherwise meet eligibility requirements to receive SNAP benefits (NONCITCHILD), and online applications allowed statewide (OAPP).

[59] A few combinations of two policies also passed these tests.

[60] The IV-FE model using all six of the State-level SNAP policies with data available for 2001-13 variables passed the weak instruments test (Kleibergen-Paap rank F statistic = 95.69, much larger than the Stock-Yogo weak identification critical values) but failed the overidentification test (Hansen's J = 68.87, p = 0.000), indicating a failure of the validity of the instruments or differences in implications of different instruments due to heterogeneous impacts and the fact that different instruments identify different local average effects.

as instrumental variables. Five lags was the minimum lag length to ensure that the serial correlation tests did not indicate the presence of serial correlation of order equal to or greater than the lag length used for the instruments.[61] All of the DP-GMM models estimate a positive and statistically significant coefficient of SNAP redemptions per capita that is larger than the same coefficient in the OLS models reported in table C-1, ranging from 0.597 job per $10,000 of SNAP redemptions in the DP-GMM model without a lagged dependent variable to 0.476 job per $10,000 of SNAP redemptions in the model with two lags of the dependent variable.[62]

All of the IV and GMM model results presented in table C-2 yielded larger coefficients of SNAP redemptions than comparable OLS-SD models. Hence, the OLS-SD results in tables 1 and 3 appear to be conservative estimates of the true impacts of SNAP redemptions.

The SDEM results are presented in table C-3. The first model restricts all coefficients to be the same for metro and nonmetro counties and the second allows the coefficients to be different. Both models show strong evidence of spatial autocorrelation. Spatial lags are more evident in the unrestricted model. SNAP redemptions in neighboring counties have a statistically insignificant impact in the restricted model and for metro counties in the unrestricted model. SNAP redemptions in neighboring counties have a positive and statistically significant impact for nonmetro counties in the unrestricted model of about the same magnitude as own-county SNAP redemptions (about 0.4 job per $10,000).

[61] Models with shorter lag lengths for the instrumental variables did not pass the serial correlation tests. The coefficient of real SNAP redemptions per capita estimated by these models were similar or larger in all cases.

[62] In the DP-GMM model using second differences, the coefficient of SNAP redemptions per capita ranged from 0.725 job per $10,000 of SNAP redemptions in the model with no lag of the dependent variable to 0.746 job per $10,000 of SNAP redemptions in the model with two lags of the dependent variable (coefficient statistically significant at the $p = 0.001$ level in all of these models).

Table C-1

Results of OLS regression models—all counties

Dependent variable: county employment per capita

Explanatory variable	OLS-FD, 2001-14		OLS-SD, 2001-14	
	With year-fixed effects	With State x year fixed effects	With year-fixed effects	With State x year fixed effects
Inflation-adjusted SNAP redemptions per capita ($10,000)	0.337*** (0.052)	0.342*** (0.057)	0.586*** (0.065)	0.456*** (0.073)
Unemployment rate	-0.779*** (0.023)	-0.989*** (0.032)	-0.850*** (0.030)	-1.071*** (0.037)
Share of population in poverty	0.0110** (0.0056)	0.0153*** (0.0052)	0.0150*** (0.0058)	0.0162*** (0.0052)
Population (million)	0.0099 (0.0146)	0.0219 (0.0148)	-0.361*** (0.095)	-0.372*** (0.094)
Share of population under age 15	-0.197*** (0.044)	-0.161*** (0.045)	-0.082 (0.060)	-0.089 (0.058)
Share of population over age 64	-0.187*** (0.046)	-0.151*** (0.043)	0.064 (0.073)	0.003 (0.066)
Share of population Black only	-0.279*** (0.064)	-0.211*** (0.059)	-0.336*** (0.126)	-0.219* (0.119)
Share of population American Indian only	-0.115 (0.128)	-0.102 (0.120)	0.119 (0.150)	0.056 (0.144)
Share of population Asian only	-0.411** (0.175)	-0.489*** (0.185)	-1.074*** (0.397)	-0.997** (0.399)
Share of population Hispanic	0.019 (0.050)	-0.060 (0.051)	0.102 (0.110)	0.016 (0.100)
Employment shares by major industry	X	X	X	X
Year-fixed effects	X		X	
State-by-year fixed effects		X		X
Number of observations	39,871	39,871	36,804	36,804
Number of counties	3,067	3,067	3,067	3,067
R^2 (overall)	0.2971	0.4175	0.2595	0.3837

Note: Robust clustered standard errors in parentheses—clustered by county. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. OLS = Ordinary Least Squares.

Source: USDA, Economic Research Service.

Table C-2

Results of IV and GMM regression models—all counties

Dependent variable: county employment per capita

	IV-LD 2008-10	IV-FE 2001-13	DP-GMM, 2001-14		
Explanatory variable	2007 Level of SNAP redemptions per capita as IV	Selected SNAP policies as IVs[a]	With no lags of dependent var.[b]	With one lag of dependent var.[b]	With two lags of dependent var.[b]
Lag of county employment per capita				0.459*** (0.025)	0.483*** (0.024)
Second lag of county employment per capita					-0.036** (0.016)
Inflation-adjusted SNAP redemptions per capita ($10,000 – 2010 $)	1.159*** (0.307)	0.761*** (0.293)	0.597* (0.358)	0.488** (0.229)	0.476** (0.226)
Inflation-adjusted other transfer payments per capita ($10,000 – 2010 $)	0.175* (0.106)				
Unemployment rate	-1.894*** (0.108)	-0.945*** (0.023)	-1.186*** (0.116)	-0.930*** (0.080)	-0.920*** (0.080)
Share of population in poverty	0.114*** (0.036)	-0.0127 (0.0150)	0.2663** (0.1251)	0.1944*** (0.0752)	0.1959*** (0.0746)
Population (million)	0.302 (0.086)	0.0177** (0.0072)	-0.0240 (0.0639)	-0.0139 (0.0397)	-0.0143 (0.0399)
Share of population under age 15	-0.896*** (0.267)	-0.528*** (0.033)	-2.252*** (0.351)	-1.098*** (0.210)	-1.126*** (0.214)
Share of population over age 64	-0.549* (0.307)	-0.475*** (0.030)	-2.297*** (0.331)	-1.082*** (0.162)	-1.096*** (0.163)
Share of population Black only	-0.625*** (0.241)	-0.204*** (0.022)	-2.242*** (0.492)	-1.851*** (0.297)	-1.853*** (0.297)
Share of population American Indian only	0.255 (0.561)	-0.265** (0.125)	-0.804 (0.523)	-0.409 (0.340)	-0.472 (0.346)
Share of population Asian only	-1.548* (0.884)	0.313*** (0.084)	0.437 (0.639)	0.249 (0.469)	0.230 (0.473)
Share of population Hispanic	-0.402 0.255)	-0.141*** (0.019)	0.632* (0.333)	-0.153 (0.187)	-0.160 (0.187)
Employment shares by major industry	X	X	X	X	X
County fixed effects		X	X	X	X
Year-fixed effects		X			
State-by-year fixed effects[c]	X		X	X	X
Number of observations	3,066	39,871	39,871	36,804	33,737
Number of counties	3,066	3,067	3,067	3,067	3067
R^2 (within counties)	0.3591	0.4340	NA	NA	NA
Weak identification test (Kleibergen-Paap rk F stat.)	96.37	146.08	NA	NA	NA

—continued

Table C-2

Results of IV and GMM regression models—all counties—continued

Dependent variable: county employment per capita

	IV-LD 2008-10	IV-FE 2001-13	DP-GMM, 2001-14		
Explanatory variable	2007 Level of SNAP redemptions per capita as IV	Selected SNAP policies as IVs[a]	With no lags of dependent var.[b]	With one lag of dependent var.[b]	With two lags of dependent var.[b]
Stock-Yogo weak ID test critical value (10% maximal IV size)	7.03	22.30	NA	NA	NA
Overidentification test (Hansen's J, number of instruments, & p value)	NA	0.77 (3 IVs) (p=0.6791)	515.61 (249 IVs) (p = 0.0000)	720.07 (147 IVs) (p=0.0000)	714.96 (95 IVs) (p=0.0000)
Endogeneity test of inflation-adjusted SNAP redemptions/cap	19.106 (p=0.0000)	3.325 (p=0.0683)			
Arellano-Bond tests for serial correlation in first differences (z and p-level)					
AR(2)			z = 0.40 (p = 0.686)	z = 1.00 (p = 0.317)	z = 2.02 (p = 0.043)
AR(3)			z = -0.94 (p = 0.350)	z = -1.01 (p = 0.315)	z = -1.32 (p = 0.186)
AR(4)			z = -0.94 (p = 0.350)	z = -1.64 (p = 0.102)	z = -2.06 (p = 0.039)
AR(5)			z = -1.19 (p = 0.234)	z = 1.35 (p = 0.177)	z = 1.06 (p = 0.289)
AR(6)			z = -0.06 (p = 0.952)	z = 0.32 (p = 0.746)	z = 0.51 (p = 0.608)

Note: Robust standard errors in parentheses. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. IV = Instrumental Variable. GMM = Generalized Method of Moments.

[a] Instrumental variables include combined application project (CAP), issuance of electronic benefit transfer cards (EBT), and allowing noncitizen children who otherwise meet eligibility requirements to receive SNAP benefits (NONCITCHILD).

[b] Fifth lag of dependent variable and all explanatory variables in each time period used as instrumental variables. One-step GMM estimator used (Arellano and Bond, 1991). Results are qualitatively similar if use two-step estimator.

[c] State fixed effects used for IV-LD regression.

Source: USDA, Economic Research Service.

Table C-3

Results of SDEM models—all counties and metro/nonmetro

Dependent variable: county employment per capita

Explanatory variable	Own counties/Neighboring counties	All counties	Nonmetro counties	Metro counties
Inflation-adjusted SNAP redemptions per capita ($10,000)	Own	0.3881*** (0.0545)	0.3885*** (0.0619)	-0.1188 (0.0852)
	Neighboring	0.0696 (0.0929)	0.3838*** (0.1063)	-0.0441 (0.1553)
Unemployment rate	Own	-1.098*** (0.012)	-1.035*** (0.012)	-0.998*** (0.019)
	Neighboring	0.0209 (0.0182)	0.4588*** (0.0194)	0.5239*** (0.0306)
Share of population in poverty	Own	0.0169*** (0.0038)	0.0129*** (0.0044)	0.0207** (0.0082)
	Neighboring	-0.0131* (0.0073)	0.0074 (0.0089)	-0.0530*** (0.0171)
Population (million)	Own	-0.3625*** (0.0694)	-7.4225*** (0.4571)	-0.2176*** (0.0735)
	Neighboring	-0.2464* (0.1316)	0.9240 (0.9852)	-0.0103 (0.1584)
Share of population under age 15	Own	-0.1027*** (0.0254)	-0.1105*** (0.0290)	0.1839** (0.0783)
	Neighboring	-0.0813 (0.0508)	0.0305 (0.0620)	0.2963* (0.1631)
Share of population over age 64	Own	-0.0060 (0.0301)	0.0578* (0.0334)	-0.0837 (0.0886)
	Neighboring	-0.0992* (0.0599)	0.2607*** (0.0691)	0.2079 (0.1730)
Share of population Black only	Own	-0.1909*** (0.0369)	-0.1615*** (0.0426)	-0.1677* (0.0932)
	Neighboring	-0.0309 (0.0717)	-0.3451*** (0.0863)	-0.3949** (0.1862)
Share of population American Indian only	Own	0.1019*** (0.0347)	0.1193*** (0.0372)	-0.2267 (0.2574)
	Neighboring	0.0969 (0.0671)	-0.0901 (0.0758)	-0.3362 (0.5489)
Share of population Asian only	Own	-0.9493*** (0.0879)	-1.3497*** (0.0970)	0.1425 (0.2956)
	Neighboring	-0.2345 (0.1706)	-1.5113*** (0.2067)	-0.7000 (0.6171)
Share of population Hispanic	Own	0.0121 (0.0292)	0.1754*** (0.0321)	-0.1601 (0.1140)
	Neighboring	0.0005 (0.0591)	0.2948*** (0.0651)	0.1971 (0.2330)
Employment shares by major industry		X	X	X

—continued

Table C-3

Results of SDEM models—all counties and metro/nonmetro—continued

Dependent variable: county employment per capita

Explanatory variable	Own counties/Neighboring counties	All counties	Nonmetro counties	Metro counties
State-by-year fixed effects		X	X	X
Number of observations		39,871	39,871	
Number of counties		3,067	3,067	
Spatial error correlation		0.2064*** (0.0072)	0.3789*** (0.0062)	

Note: Robust standard errors in parentheses. *, **, and *** mean the coefficient is statistically significantly different from 0 at the 10-percent, 5-percent, and 1-percent level, respectively. SDEM = Spatial Durbin Error Model.

Source: USDA, Economic Research Service.

Appendix D. Recent Econometric Studies of Local Fiscal Multipliers

In this appendix we briefly review the approaches and key findings of several recent econometric studies investigating the local multiplier impacts of government spending in the United States. The studies reviewed include those found that most closely related to the current study. Most of these studies focused on the impacts of the American Recovery and Reinvestment Act (ARRA), and most use State-level data and analysis, while a few used data on counties or multi-county regions. None of these studies estimated the impacts of SNAP specifically.

Feyrer and Sacerdote (2011) investigated impacts of ARRA spending at the State and county level using multiple data sources and methods. Using State-level monthly data on spending in total and by Federal agency and monthly data on employment, they analyzed changes in employment per capita between February 2009 and October 2010 using long difference (between ending and beginning month) ordinary least squares (OLS-LD) and instrumental variables (IV-LD) regressions.[63] In the OLS-LD regression, they estimated that a \$100,000 increase in ARRA funding over the 20-month period was associated with an increase of 0.5 job. Using IV-LD, they estimated a larger employment multiplier of 1.1 jobs per \$100,000. Using instead fixed effects (FE) regressions with the State monthly panel, Feyrer and Sacerdote (2011) estimated even larger employment impacts—1.4 jobs per \$100,000 of ARRA spending in the OLS-FE regression and 2.3 jobs per \$100,000 in the IV-FE regression. Using county-level cross-sectional long difference regressions (investigating changes from the first quarter of 2009 to the first quarter of 2010), they estimated a multiplier of only 0.2 job per \$100,000 using OLS-LD but 3.3 jobs per \$100,000 using IV-LD. The larger impacts estimated using IV suggest that their OLS estimates are biased downward, possibly due to reverse causality (more ARRA spending going to places with worse employment outcomes) and/or measurement error. As shown below, almost all other studies that used both OLS and IV methods found a similar result.

Feyrer and Sacerdote (2011) also used OLS regressions with the State monthly panel of data and incorporating leads and lags of ARRA spending by Federal department (up to 8 months after ARRA was enacted) to investigate the timing of impacts and how impacts differed across types of spending.[64] In all cases, they found no evidence of impacts before ARRA was enacted, supporting the validity of their results and suggesting that anticipation effects were limited. They estimated small or negative and statistically insignificant effects of spending by most Federal departments on employment over the period studied. The exceptions were for funding by the U.S. Department of Housing and Urban Development (HUD) and the U.S. Department of Agriculture (USDA), whose ARRA funding to States had large positive and statistically significant impacts on growth in employment. A \$100,000 monthly increase in USDA ARRA funding was associated with an increase of 60 jobs over the 8-month post-enactment period studied, which translates to 5 additional jobs per \$100,000 increase in annual USDA funding. The estimated impact was about three times as large for HUD funding but with a much larger error margin. Overall, Feyrer and Sacerdote's (2011) results imply

[63] Feyrer and Sacerdote (2011) used the mean seniority rank of a State's delegation in the U.S. House of Representatives as an instrumental variable to predict ARRA spending.

[64] Feyrer and Sacerdote (2011) did not use IV regressions to estimate the timing of impacts overall or by Federal department; presumably because their instrumental variable strategy was not sufficient to identify these impacts. As with the other OLS estimates in their analysis, these estimates may be biased downward.

average employment impacts of ARRA spending in the range of 0.11 job to 0.33 job per $10,000 of ARRA annual spending but as large as 0.50 job per $10,000 of USDA ARRA spending (mostly for SNAP) and possibly larger for HUD ARRA spending.[65]

Chodorow-Reich et al. (2012) investigated the employment impacts of aid provided to State governments under ARRA through the Medicaid Reimbursement process, using monthly data at the State level for December 2009 to July 2010 and OLS and IV LD regressions.[66] Their preferred IV-LD model estimated that $100,000 of additional Medicaid funding under ARRA resulted in 2.8 additional jobs over the 8-month period studied. Their OLS-LD model, by contrast, estimated a much smaller and statistically insignificant impact. Their IV-LD results were robust to a range of alternative model specifications and use of different data sources for employment data. Considering impacts estimated over several alternative end dates of the IV-LD regressions, they estimated a State's receipt of an additional $100,000 in Medicaid outlays resulted in an additional 3.8 job-years.

Wilson (2012) investigated the impacts of ARRA spending using monthly data on State-level allocations of Federal stimulus funds from February 2009 to February 2010 and OLS-LD and IV-LD regressions. [67] He estimated impacts of three measures of ARRA spending—announcements of funding, obligations, and outlays. In the preferred IV-LD regressions, he found that $100,000 of announced ARRA funding led to an additional 0.8 job, while the impacts of obligations and outlays were larger—1.0 job per $100,000 for obligations and 2.2 jobs per $100,000 for outlays. The impacts estimated by OLS-LD were substantially smaller. His results were robust to numerous alternative specifications.

Shoag (2013) investigated the impacts of the growth in State-level spending on changes in employment and personal income in 2009 and 2010, using annual data on State-level spending, employment, and personal income and a first difference instrumental variables (IV-FD) estimator.[68] He estimated that $100,000 of additional spending adds 4.5 jobs and $143,000 to personal income (income multiplier of 1.43).

Conley and Dupor (2013) investigated the impacts of ARRA spending on employment growth from April 2009 to March 2011 using State-level monthly data on employment and ARRA spending with OLS and IV cross-sectional regressions for average employment growth rate.[69] Based on their baseline IV model results, they estimated that the cost in ARRA spending of creating a job lasting 1 year

[65] According to ARRA spending figures provided by Feyrer and Sacerdote (2011), SNAP spending accounted for 89 percent of total USDA ARRA spending through September, 2010.

[66] Chodorow-Reich et al. (2012) used an instrumental variable approach similar to our instrumental variable approach to estimate the impacts shown in table 4. Their instrumental variable for the increase in Medicaid spending following ARRA was the amount of Medicaid outlays in a State prior to the recession divided by the population age 16 and older.

[67] Wilson (2012) used as instrumental variables (i) the U.S. Department of Transportation's predicted ARRA obligations in 2009 based on total lane miles of Federal-aid highways in 2006, total vehicle miles traveled on Federal-aid highways in 2006, the estimated tax payments attributable to highway users paid into the Federal Highway Trust Fund in 2006, and Federal Highway Administration obligation limitations in 2008; (ii) the school-aged share of the State's population (to predict the Department of Education's ARRA spending under the State Fiscal Stabilization Fund); and (iii) pre-ARRA Medicaid expenditures in each State (to predict the increase in Medicaid spending under ARRA).

[68] Shoag (2013) used excess (unanticipated) returns to State pension funds in a year as an instrumental variable to predict State spending in the following year.

[69] Conley and Dupor (2013) used two instrumental variables in their baseline specification to predict ARRA spending in a State: U.S. Department of Transportation highway funding obligated under ARRA through FY 2010, which was based mainly on funding formulas, and the ratio of a State's "relatively inelastic" sources of revenue (including property taxes, selective sales taxes, charges and miscellaneous revenue, utility revenue, liquor store revenue, and intergovernmental transfers) to its total revenue. Whether the inelastic revenue share is truly exogenous and valid to exclude from the employment growth regressions is questionable.

was $202,000 (implying an employment multiplier of 0.05 job per $10,000). As in other studies, they found smaller employment impacts in OLS than in IV regressions. They found similar ranges of employment impacts in alternative specifications of their IV model. In their baseline model, Conley and Dupor (2013) assumed that ARRA funds were fungible so that States could use ARRA funds to offset lost revenue. In alternative estimations, they relaxed that assumption, by estimating separate impacts of ARRA funds and losses in State revenue. The latter estimates imply a point estimate of total employment impacts of ARRA funds that is about 50 percent larger than their baseline estimate (implying an employment multiplier of about 0.08 job/$10,000), but this estimate was not statistically significant at the 90-percent confidence level.

Nakamura and Steinsson (2014) investigated the impacts of military procurement spending on GDP and employment at the state and multistate region level using data from 1996 to 2006.[70] They estimated that a 1-percent of GDP increase in military procurement spending in a State increases State GDP over 2 years by 1.43 percent (i.e., the State GDP multiplier was 1.43) and increased State employment by 1.28 percent, which we estimate implies a jobs multiplier of 0.16 job per $10,000 (2009 dollars) of military procurement spending.[71] For 10 multistate regions, they estimated larger multipliers—1.85 for regional GDP and 1.76 for regional employment. They also investigated differences in multipliers for periods and States with greater excess capacity in the economy (based on differences in unemployment rates) and found a larger GDP multiplier of military spending in periods or States with more excess capacity but a smaller and statistically insignificant effect of excess capacity on the employment multiplier.

Dube et al. (2015) investigated the impacts of ARRA spending on employment and earnings at the county level and for aggregations of counties with centroids up to 120 miles from the focal county centroid for the first 10 quarters after enactment of ARRA using OLS fixed effects regressions (OLS-FE). They estimated that a $100,000 increase in ARRA spending was associated with an increase in employment within a county of 0.76 job-year based on quarterly impacts aggregated over eight quarters (employment multiplier of 0.08 job-year per $10,000), and increased earnings of $41,000 based on quarterly impacts aggregated over four and six quarters (earnings multiplier of 0.41). For aggregations of counties within 120 miles, Dube et al. (2015) estimated much larger impacts, with an employment multiplier of 0.33 job-year per $10,000 and an earnings multiplier of 1.82. Dube et al. (2015) also found much larger impacts of ARRA spending in counties and regions with greater excess capacity (measured by the relative decline in employment to population ratio during and following the Great Recession), with employment and income multipliers roughly twice as large as the national average for the half of counties with greater excess capacity and small and statistically insignificant multipliers for the half of counties with less excess capacity.

Suarez-Serrato and Wingender (2016) investigated the impacts of Federal Government spending on employment and personal income using county-level data from 1970 to 2009 and a treatment

[70] Nakamura and Steinsson (2014) used as an instrumental variable for military procurement spending in a State the predicted military spending based on a regression of State-level military spending on national-level military spending interacted with State dummy variables.

[71] Nakamura and Steinsson (2014) did not estimate the jobs multiplier per dollar spent, but that can be estimated by multiplying their estimated impact of military spending on employment (1.28 percent increase in employment per 1 percent of GDP increase in military spending) by the employment to GDP ratio. Using data for national annual average nonfarm employment from the Bureau of Labor Statistics Current Employment Series and real national GDP (in 2009 dollars) from the U.S. Bureau of Economic Analysis, we estimate the average employment to GDP ratio during 1996 to 2006 to be 0.126 job per $10,000 (2009 dollars). Multiplying this by 1.28 yields a jobs multiplier estimate of 0.16 job per $10,000 of military procurement spending.

effects framework. The "treatment" that they used to identify impacts of Federal Government spending was the "census shock" to Federal spending that occurs following each decennial census, when population estimates based on the preceding census and other information are replaced by population counts from the census. Because many Federal grants and other forms of Federal spending can be affected by the official population count, these adjustments to estimated population can have substantial real impacts on Federal spending at the county level. In their primary econometric model, Suarez-Serrato and Wingender (2016) classified the census shocks (in each of 1980, 1990, and 2000) as a binary variable and used a treatment effects estimator with propensity score reweighting (with and without regression adjustment) to estimate impacts of the census shocks on subsequent Federal spending, employment, and personal income. The employment or income multipliers were then estimated as the ratio of coefficients of employment or income impacts of the shock to the impact of the shock on Federal spending. Their baseline results were that the average Federal cost per job was about $31,000 (implying a jobs multiplier of 0.32 job per $10,000 of Federal spending) and the income multiplier was 1.86. These results were robust to a wide range of alternative specifications, including using an IV estimator (using a continuous rather than binary measure of the census shock), alternative control variables, inclusion of spillover effects from neighboring counties, and others. Spillover effects from neighboring counties were found to be small and statistically insignificant, but reduced the point estimators of job and income multipliers slightly. As in other studies, the IV results yielded larger multiplier estimates than comparable OLS models. The authors conducted "event studies" investigating the timing of impacts and found as expected that the effects of census shocks on Federal spending, jobs, or income were not present until at least 2 years after the census year (when the census results are published) and continue for several more years. They used a quantile IV model to investigate how the multiplier impacts vary across the quantiles of the distribution of the outcome variables and found that both job and income impacts of Federal spending were greater for slower growing counties. The point estimates from the quantile regression models indicate that jobs impacts greater than one job per $10,000 of Federal spending and income multipliers as large as four are evident in the slowest growing 40 percent of counties.

Dupor and Guerrero (2017) investigated the impacts of national defense spending on growth over 2 years and 4 years in State and national personal income and employment using IV regressions.[72] In their baseline models, they estimated an income multiplier of defense spending of 0.33 and an employment multiplier of 0.39 (in terms of percent growth in employment per 1 percent of personal income increase in defense spending) at the national level for 2 years of cumulative impacts, and smaller and statistically insignificant national income and employment multipliers for 4 years of cumulative impact. In their State-level analysis of 2-year cumulative multipliers, they estimated an income growth multiplier of 0.22-0.23 and an employment growth multiplier of 0.27-0.30 (with versus without State-fixed effects) when they excluded year effects, but much smaller and statistically insignificant income and employment growth multipliers when they included year effects as well as State fixed effects. They interpreted the large effect of including year-fixed effects as the difference due to aggregate effects of defense spending shocks (i.e., including year effects removes any effect of national-level factors on the multiplier estimates). We estimate that Dupor and Guerrero's baseline employment growth multiplier (0.27) implies an average impact of 0.05 job per $10,000 (in 2009 dollars) of defense spending during their study period.[73]

[72] For their national-level analysis, Dupor and Guerrero (2017) used the 1-year change in national defense spending per GDP as the instrumental variable for future growth in defense spending. For the State-level analysis, they used the ratio of a State's share of national military spending to the State's share of national income in the prior 2 years, multiplied by the 1-year change in national defense spending per GDP.

[73] The average ratio of nonfarm employment to real personal income (in 2009 dollars) during Dupor and Guerrero's (2017) study period (1951-2014) was 0.167 job per $10,000. Multiplying this ratio by their employment growth multiplier (0.27) yields the estimate of 0.05 job per $10,000.

Dupor and Guerrero (2017) also investigated the effects of spillovers between each State and its major trading partner State and found evidence of positive spillovers and slightly larger aggregate (for pairs of States) multipliers—0.25 for 2-year cumulative income growth and 0.31 for 2-year employment growth. Their 4-year employment growth multiplier accounting for spillovers was smaller—0.20—while their 4-year income growth multiplier with spillovers was small and statistically insignificant. The State-level multipliers estimated by Dupor and Guerrero (2017) are much smaller than those estimated by Nakamura and Steinsson (2014), though they used a similar econometric approach and data. Dupor and Guerrero (2017) showed that the inclusion of additional years of data (especially for the Korean War period) in their analysis was the main reason for the difference between their income multiplier estimate and Nakamura and Steinsson's (2014) larger GDP multiplier estimate. The comparison of the findings of these two studies highlights the potential importance of the temporal (as well as the spatial) context in determining the size of estimated multipliers.

Dupor and McCrory (2017) investigated the impacts of ARRA spending on employment and wage and salary earnings ("wage bill") growth from the fourth quarter of 2008 to the fourth quarter of 2010 in 1,293 labor market areas (LMAs), including spillover effects between the largest county and the satellite region of each LMA. They used OLS and IV cross sectional regressions.[74] In their preferred IV model, they estimated a wage bill multiplier of 0.64 from ARRA spending in the same subregion and 0.50 from spending in the adjacent subregion within the same LMA, or a total wage impact within an LMA of $1.14 per $1 of ARRA spending in the LMA. Their OLS model yielded a similar estimated total wage impact within an LMA ($1.06 per $1 of ARRA spending) but with more of the estimated impact within the same subregion of an LMA where the spending occurs (0.90 multiplier from within subregion, 0.16 from adjacent subregion). Their preferred IV model for the employment growth impact of ARRA spending resulted in estimated multipliers of 10.3 job-years per $1 million of ARRA spending within the same subregion and 8.5 job-years per $1 million in the adjacent region, or a total multiplier within an LMA of 18.8 job-years per $1 million (0.19 job per $10,000) of ARRA spending. They found that the spillover effects are confined to the services sector, with small and statistically insignificant spillover effects in the goods-producing sector.

Dupor and McCrory (2017) also investigated total and spillover multiplier effects when relaxing the symmetry assumption in their baseline model that spillover impacts from the large county to the satellite region are the same as the spillover impacts from the satellite region to the large county. They found that the total wage and employment multiplier impacts within an LMA are larger for ARRA spending in the satellite subregion of an LMA than for spending in the largest county in an LMA—the total wage multiplier is 1.27 and the employment multiplier is 30.8 job-years per $1 million spent in the satellite subregion, compared with a total wage multiplier of 1.18 and employment multiplier of 12.6 job-years per $1 million spent in the large county of an LMA. These results suggest that Federal spending in exurban or rural areas surrounding an urban center can have a larger multiplier impact on the local labor market than an equivalent amount of Federal spending in the urban part of a local labor market.

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[74] Dupor and McCrory (2017) used as an instrumental variable the sum of Federal spending in a region from ARRA programs that did not allocate funds to economically distressed or economically strong areas, including selected programs of the U.S. Environmental Protection Agency, the U.S. General Services Administration, the U.S. Department of Education, the U.S. Department of Energy, the U.S. Department of Justice, the Federal Transit Administration, and the U.S. Army Corps of Engineers. The total spending authorized by ARRA for these selected programs was about $56 billion.

Dupor and McCrory investigated the sensitivity of their results to using less or more aggregated labor market areas and found smaller multipliers for a classification with smaller and more numerous LMAs and larger but less precise wage multipliers for a classification with larger and less numerous LMAs. Their point estimate of the total employment multiplier within an LMA increases to a maximum of about 25 job-years per $1 million of ARRA spending with 750 rather than 1,293 LMAs, but this estimate is not statistically distinguishable from their estimate with 1,293 LMAs (about 19 job-years per $1 million). Thus they conclude that spillover effects do not have a large impact on multipliers beyond the level of aggregation in their baseline model.