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Essays on the Impacts of the Supplemental Nutrition Assistance Program

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ABSTRACT

ESSAYS ON THE IMPACTS OF THE SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM

BY

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This dissertation consists of three chapters, each of which provides causal evidence on the impacts of the Supplemental Nutrition Assistance Program (SNAP) in a distinct, policy-relevant area.

The first chapter provides evidence of SNAP's effects on the food retail industry. I combine data on SNAP participation, industry-specific retailer outcomes, and state SNAP expansions from 1998 to 2016. To address the endogeneity of SNAP participation, I employ a novel simulated eligibility instrumental variables framework exploiting variation in state SNAP eligibility expansions. I find that higher SNAP participation leads firms to operate more stores in industries where benefits are typically accepted, especially smaller general stores – a category dominated by dollar and discount stores.

The second chapter provides evidence of SNAP's effects on Medicaid enrollment and spending. I combine state-level information on SNAP eligibility expansions, SNAP participation, and Medicaid enrollment and spending from 1999 to 2012. I summarize diverse SNAP eligibility expansions through a novel simulated eligibility measure. I find that SNAP expansions increase Medicaid enrollment and decrease Medicaid spending per enrollee, suggesting that SNAP

participation lowers barriers to enrollment in Medicaid for groups who cost lower on average to cover.

The third chapter provides evidence of SNAP's effects on mortality during its introduction in the "War on Poverty" era. SNAP was introduced as the Food Stamp Program (FSP) on a county-by-county basis from 1961 to 1975. I combine county-level information on the timing of FSP introduction with death counts from 1969 to 1978. I estimate the impacts of the FSP's introduction on various mortality rates over time, including the overall mortality rate, population subgroup-specific rates, and cause-specific rates. I find that, among a subsample of high-poverty counties where the program's introduction is likely to have a larger impact, the FSP reduced mortality over time. This effect was largely driven by reductions in the black, male, and age 0-19 mortality rates.

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BY

JORDAN WILLIAM JONES

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ACCEPTANCE

This dissertation was prepared under the direction of Jordan William Jones's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Dedication

To my wife Olivia and my parents Michael and Cheryl, without whom this endeavor would have been maybe-not-impossible-but-definitely-a-lot-more-difficult-and-painful. Thank you so much for your love and encouragement.

And especially to my mom, who didn’t get to see me complete this milestone. I hope you’re somewhere out there, floating around and watching, and if you are, I hope I make you proud.

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I would like to thank everyone who helped me toward this goal, but this section probably shouldn't be the longest one in the dissertation.

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I thank all the great friends I've made at Georgia State inside and outside my cohort, who have helped me survive the stresses of graduate school and who have made my years here some of the best of my life. There's no other group of nerds I'd rather hang out and play board games with.

I thank my parents Michael and Cheryl for their love and immense encouragement in graduate school and beyond. I may not have entered or stayed in economics if not for your steadfast support.

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my mostly willing sounding board, helping me think of research topics, and inspiring me to carry on. I’m glad you stuck around.

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Introduction

The Supplemental Nutrition Assistance Program (SNAP) is the nation's largest nutrition assistance program, annually providing billions of dollars in benefits (or food stamps) to millions of low-income, low-resource households. Given its size and importance to its recipients, SNAP must have far-reaching consequences, both intended and unintended. A growing body of research aims to better understand the program's impacts on recipient and other outcomes using various approaches to causal inference. In this dissertation, I seek to aim to this literature by providing causal evidence on the large-scale impacts of SNAP in three policy-relevant areas.

Chapters I and II are linked by their use of a measure I develop termed the simulated eligibility variable or SEV, which captures variation in state-specific SNAP eligibility expansions since 1996 and represents the generosity of each state's eligibility policy over time. In Chapter I, I provide evidence of SNAP's effects on the food retail industry. SNAP provides monthly benefits redeemable for food at authorized stores to millions of low-income households. Benefits increase food demand, and retailers may respond accordingly. Specifically, I estimate the impacts of SNAP participation on the decisions of food and non-food retailers concerning the number of stores, employment, and average employee earnings. I combine data on SNAP participation and industry-specific retailer outcomes and employ the SEV as an instrument for the SNAP participation rate in an instrumental variables framework. I find that higher SNAP participation leads firms to operate more stores in industries where benefits are typically accepted. This response is primarily attributable to smaller general stores – a category dominated by dollar and discount stores. Additionally, higher SNAP participation increases the annual average earnings of employees of supercenters, warehouse clubs, and general stores.

In Chapter II, I provide evidence of SNAP's effects on Medicaid enrollment and spending. SNAP and Medicaid target largely overlapping low-income populations and therefore may interact with each other. I combine state-level information on SNAP eligibility expansions, SNAP participation, and Medicaid enrollment and spending from 1999 to 2012. I model state Medicaid outcomes as a function of SNAP eligibility expansions as summarized by the SEV. I find that SNAP expansions increase Medicaid enrollment, especially for non-disabled adults and children. Further, I find that these expansions decrease Medicaid spending per enrollee. These findings suggest that SNAP participation lowers barriers to enrollment in Medicaid, but marginal Medicaid enrollees of these kind cost less on average to cover than other enrollees.

In Chapter III, I provide evidence of the mortality impacts of the Food Stamp Program (FSP) – SNAP's original name – during its introduction. The FSP was introduced county-by-county from 1961 to 1975 with the purpose of providing nutrition to low-income households. Access to food stamps – and the income food stamps freed up for other purposes – likely had positive impacts on health, especially for the poorest recipients. I combine county-level information on the timing of FSP introduction and death counts from 1969 to 1978. I estimate the impacts of the FSP's introduction on various mortality rates over time, including the overall mortality rate, population subgroup-specific rates, and cause-specific rates. I find no evidence of the program reducing mortality among the full county sample. However, among a subsample of high-poverty counties where the program's introduction is likely to have a larger impact, I find that the FSP reduced the overall mortality rate over time. This overall effect was largely driven by reductions in the black, male, and age 0-19 mortality rates. I find limited evidence of reductions in deaths from major cardiovascular diseases, suicides, and accidents.

Chapter I: Food Retailer Responses to SNAP

Government social programs annually inject hundreds of billions of dollars into the economy in the form of targeted, in-kind transfers. Relatively little attention in the literature is devoted to the potentially large impacts these transfers have on private industry. The Supplemental Nutrition Assistance Program (SNAP, formerly named the Food Stamp Program) is the largest nutrition assistance program in the United States and a major source of food spending. In 2018, SNAP issued almost $61 billion in benefits (or “food stamps”) to 42 million people, representing about eight percent of all spending on food for consumption at home (USDA FNS 2019c; USDA ERS 2020). Canning and Stacy (2019) estimate the program’s GDP multiplier to be about 1.5 during a slowing economy, indicating far-reaching economic impacts.

The food retail industry in particular is likely to be impacted by SNAP. SNAP receipt has been shown to increase food spending (Fraker et al. 1995; Hoynes and Schanzenbach 2009; Beatty and Tuttle 2015; Hastings and Shapiro 2018; Bruich 2014). All else equal, food retailers will operate more stores in markets with higher demand in order to maximize profits. SNAP may therefore influence retailer decisions like store location, employment, and other factors.

As SNAP is an automatic stabilizer during recessions, understanding how private industry responds to changes in SNAP participation is important for policymakers shaping stimulus or considering program changes. Taking SNAP’s business impacts into account makes for a more complete assessment of the program’s costs and benefits. Further, SNAP impacts recipients directly by increasing their purchasing power, but it may also indirectly affect recipients and nonrecipients alike by altering the food retail environment. Many studies have identified relationships between household access to food stores and outcomes such as food insecurity, diet, and weight.[1] Recognizing how food retailers respond to the program is therefore key to a more complete understanding of its impacts on households.

Though an extensive literature examines SNAP's impacts on recipient outcomes, few studies examine ways in which the program might affect retailers. Several studies estimate mixed effects of the program on food prices over different time horizons (Goldin, Homonoff, and Meckel 2019; Jaravel 2018; Leung and Seo 2019). One study finds that SNAP-eligible households in states with policies leading to greater take-up experienced higher increases in product variety and declines in retailer margins (Jaravel 2018). Another finds that small stores are more likely to become authorized to accept SNAP benefits when local SNAP participation increases in Georgia (Shannon et al. 2016). Another recent study finds that earlier introduction of the Food Stamp Program in a county during its rollout period in the 1960s and 1970s led to the presence of more food stores, more food retail workers, and higher sales at those stores (Beatty, Bitler, and Van der Werf 2020).

The purpose of this study is to estimate the magnitude of the impacts of SNAP on the decisions of firms in the food retail industry concerning store operation, employment, and payroll and to quantify the aggregate magnitudes of these responses. I assemble an annual county-level panel on SNAP participation and retailer establishment counts, employment, and payroll divided by industrial classification. I construct a measure of simulated aggregate eligibility summarizing variation in state policies determining SNAP eligibility and representing the relative generosity of states' SNAP rules. I use this measure to instrument for the actual participation rate to account for the endogeneity of SNAP. I focus on estimating the effects of

[1] E.g., Caspi et al. (2012); Courtemanche and Carden (2011); Courtemanche et al. (2018); Gustafson et al. (2013).

SNAP expansions on the number of stores in different retail industries, and I also estimate their effects on employment and average employee earnings.

This study makes three major contributions. First, this study contributes to the broader empirical literature on the effects of social policy on private industry.[2] Second, this study contributes to the literature on SNAP's effects on the food retail industry. I provide the first evidence in a causal framework of the impacts of modern SNAP expansions on the number of food retail establishments, employment, or average earnings in specific food retail industries, and I measure the size of these impacts. Third, I construct a simulated measure of SNAP eligibility incorporating detailed variation in several kinds of state-level eligibility rules over the twenty years from 1996 to 2015. I employ this measure as a representation of overall state SNAP policy generosity as well as an instrument for the SNAP participation rate. In an instrumental variables framework, this simulated measure individually outperforms other state policy instruments that have been commonly employed in similar studies.

I find evidence that SNAP expansions and the resulting increases in SNAP participation increase the number of retail establishments in operation likely to accept SNAP benefits, which suggests the program acts as stimulus for food retailers. A one percentage point increase in the county SNAP participation rate increases the number of these stores by about 0.6%. This increase is primarily driven by an increase in the number of smaller general stores – primarily dollar stores – of about 1.9%. I also find evidence that SNAP increases average employee earnings in supercenters, warehouse clubs, and general stores despite decreasing average earnings in other retail industries. SNAP expansions appear to primarily improve business for

[2] E.g., Garthwaite (2012); Huang and Perloff (2014); Buchmueller, Miller, and Vujicic (2016); Wagner (2016)

retailers targeting lower-income customers. A full accounting of the welfare impacts of SNAP ought to take into consideration its effects on the retail industry.

1. Background

1.1. SNAP and the role of food retailers

SNAP provides benefits, also known as food stamps, to eligible low-income households which can be redeemed for food for consumption at home.[3] Benefits are federally funded through the Food and Nutrition Service (FNS) of the U.S. Department of Agriculture (USDA), but the program is jointly administered at the federal and state levels. The program has grown substantially over the last few decades: from 1996 to 2016, average participation grew by 73.3% from 25.5 to about 44.2 million, and total annual benefits issued grew by 93.9% from $31.2 to $60.5 billion in 2010 dollars. Several program changes occurred alongside this growth, including state-level expansions to eligibility, the shift to provision of benefits through electronic benefit transfer (EBT), the shift to joint administration with other social programs, a temporary benefit increase as part of the American Recovery and Reinvestment Act (ARRA) from 2009 to 2013, and other federal changes to eligibility and benefit determination.

Benefits are redeemed at authorized food retailers like grocery stores, superstores, convenience stores, and general stores, among others. Redeemed benefits are generally equivalent to other revenue for retailers. To be authorized to accept benefits, a retail location

[3] SNAP benefits generally cannot be spent on foods intended for consumption in-store or hot foods, alcohol, tobacco, or nonfood items. Benefits can be redeemed on food-producing plants and seeds. States can waive the restriction on in-store foods for qualified recipients through the Restaurant Meals Program, but as of 2018, only Arizona and several California counties participate with small pilot programs in place in Rhode Island and Florida.

must meet certain stocking criteria.[4] Authorization is generally not costly for retailers regardless of size.[5] Accepting benefits requires commercially available or specialized point-of-sale equipment and payment of a small monthly or annual flat fee to an EBT processor, though some specialized retailers are eligible for no-cost EBT-only equipment.

Nearly 252,000 stores were authorized to accept benefits at the end of fiscal year 2018 (USDA FNS 2019a). Figure 1 shows a breakdown of authorized stores by type. From most stores authorized to least, these types are convenience stores (with or without gas stations), "combination grocery" and other stores (a category including dollar stores, variety stores, and drug stores), supermarkets and grocery stores, superstores, and other types. Figure 2 shows the portion of benefits redeemed at each of these types of stores. Just over half of benefits in 2018 were spent at superstores, just over one third were spent at supermarkets and grocery stores, and about 5% each went to convenience stores and combination grocery stores.

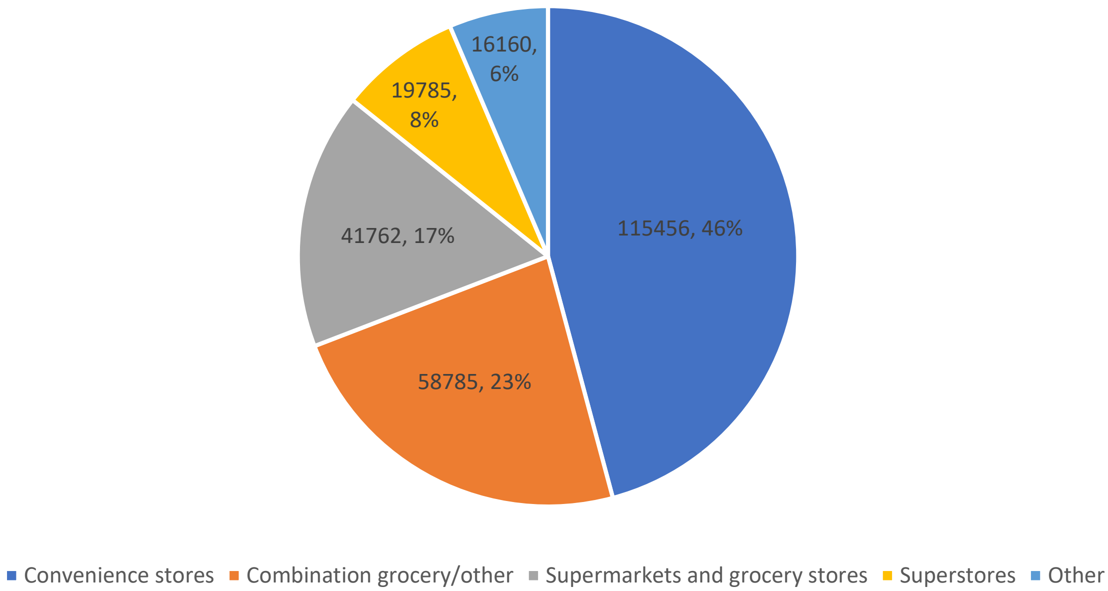
1.2. SNAP and food demand

SNAP provides a monthly in-kind transfer that recipients can only spend at authorized retailers on food for consumption at home. The seminal Southworth model predicts that households who would spend more on SNAP-eligible food than their benefit level in its absence – or "inframarginal" households – would treat their benefit like a cash transfer of equivalent size as it reduces the need for out-of-pocket food spending (Southworth 1945). Most recipients are inframarginal, meaning that SNAP's aggregate effects on consumer demand would theoretically

[4] As of April 2020, locations must meet one of two criteria relating to four USDA-defined categories of staple foods: 1) vegetables or fruits; 2) dairy products; 3) meat, poultry, or fish; 4) breads or cereals. A store must either continuously stock a defined variety and quantity of foods in each category, including some perishable foods, or make more than 50% of its total gross retail sales from the sale of staple foods. Some stores can be authorized without meeting these criteria if they operate in areas with significantly limited food access. Most stores are authorized through the first pathway, while many specialty food stores are authorized through the second (USDA FNS 2016).

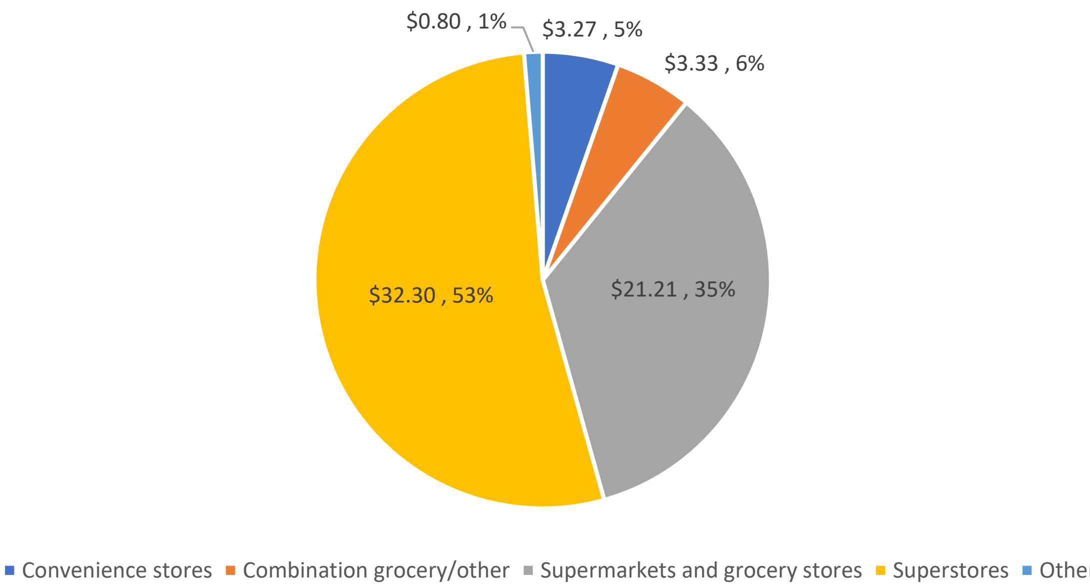
[5] Firms with less than ten locations apply online for free, and firms with ten or more locations work directly with an FNS representative to be authorized and reauthorized.

Figure 1. SNAP-authorized stores by type



Number of stores authorized to accept SNAP benefits in 2018. Source: USDA FNS (2019a).

Figure 2. SNAP benefits redeemed by store type



Total value of SNAP redemptions in 2018 by store type in billions of dollars. Source: USDA FNS (2019a).

be much like those of the effects of an income increase.[6] SNAP is therefore likely to increase demand for both food and non-food goods. Alternatively, inframarginal households may not behave rationally but instead engage in "mental accounting" by distinguishing between SNAP benefits and cash as separate income sources and treating them differently (Thaler 1999; Hastings and Shapiro 2018). If this is the case, SNAP may increase demand for food more than for other goods.

A large literature finds that food stamp receipt increases food-at-home spending but provides mixed evidence on the size of the effect relative to that of cash income receipt. Earlier studies typically estimate recipient households' marginal propensity to consume food (MPCF) out of food stamps as significantly higher than their MPCF out of cash income, but these estimates are biased upwards as these studies typically do not account for the endogeneity of household SNAP participation.[7] Experimental and quasi-experimental evidence is mixed. Some studies find that inframarginal households' MPCF out of food stamps is near their MPCF out of cash income, while others estimate a significantly higher MPCF out of food stamps.[8, 9]

1.3. SNAP's effects on food retailers

All else equal, an increase in SNAP participation is likely to increase demand for food for consumption at home as well as for other products, if by a lesser extent. Therefore, retailers may

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[6] Hoynes, McGranahan, and Schanzenbach (2016) estimate that 84% of recipient households spend an amount equal to or above their SNAP benefit level on food for consumption at home.
[7] Fraker (1990) surveys the early literature and places the median estimate of the MPCF out of food stamps at 3.8 times the MPCF out of cash income.
[8] In a series of "cash-out" experiments in which some households received cash instead of food stamps, cash recipients in some areas spent less on food-at-home than standard coupon recipients, while cash recipients in other areas did not alter spending (Fraker et al. 1995). Hoynes and Schanzenbach (2009) estimate a MPCF out of food stamps near the MPCF out of cash income of about 0.1 during the Food Stamp Program rollout in the 1960s and 1970s. Using more recent data, Beatty and Tuttle (2015), Bruich (2014), and Hastings and Shapiro (2018) estimate the MPCF out of food stamp income as about 0.5 to 0.6, 0.3, and 0.5 to 0.6, respectively – significantly higher than their estimates of the MPCF out of cash income.
[9] In addition to overall food spending, several studies examine relationships between SNAP receipt and purchases of specific types of foods (e.g., Anderson and Butcher 2016; Burney 2017; Chang et al. 2015) or specific types of SNAP-ineligible goods (e.g., Burney 2018; Kim 2016).

adjust their operations to sell more food and maximize profits.[10] Retailers may choose to open new stores in areas where more people receive benefits or refrain from closing stores that would otherwise be unprofitable. Accordingly, retailers may also demand more labor to staff additional or current stores, e.g. through extending store hours. Therefore, firms may employ more workers, increase worker hours, or even increase wages to attract more employees. Responses of these types are especially likely for stores targeting lower-income customers who are more likely to be eligible for SNAP. Non-food retailers may respond similarly, but if SNAP tends to increase food spending more than non-food spending, their responses would likely be smaller on average.

Few studies examine SNAP's impacts on retailers like those I describe here. Beatty, Bitler, and Van der Werf (2020) examine closely related questions in a different time period, finding that areas in which the Food Stamp Program was introduced earlier had more food stores relative to those where the program was introduced later. Shannon et al. (2016) find that local SNAP enrollment in Georgia was positively associated with the numbers of SNAP-authorized small stores throughout the state and with large stores outside of the Atlanta area. Similarly, Shannon et al. (2018) find that increasing SNAP enrollment in Atlanta predicted decreased distance to most small SNAP retailers but increased distance to many larger ones. These findings are consistent with work finding that most SNAP recipients – even those in areas of lower food access – tend to travel to redeem benefits at larger stores (Mabli and Worthington 2015; Schwartz et al. 2017; Shannon 2014). Additionally, SNAP benefit increases led to a greater percentage of redemptions at supercenters and supermarkets, suggesting that SNAP may not only affect food demand but also choice of food store (Andrews, Bhatta, and Ver Ploeg 2013). SNAP

[10] It is likely that changes in SNAP participation would similarly impact food producers, but SNAP-induced changes in demand are likely more salient for food retailers than producers due to retailers' direct connection to SNAP. Additionally, food retail is local by nature relative to food production, so retailers are more likely to respond to localized demand changes than producers.

disbursement schedules also matter: low-income households in areas where SNAP benefits are issued near the beginning of the month tend to make purchases at larger stores early in the month and buy more at convenience stores and restaurants later in the month (Damon, King, and Leibtag 2013).[11]

2. Data

I assemble publicly available county-level information over the years 1998 to 2016 on SNAP participation levels, retailer outcomes, and various population characteristics. I exclude from the sample Alaska and Hawaii due to their different benefit formulas, California due to its Supplemental Security Income (SSI) "cash-out" policy, and counties with changing borders or incomplete data.[12]

I use SNAP participation counts aggregated by the U.S. Census Bureau Small Area Estimates Branch (2018).[13] In conjunction with intercensal population estimates, I construct the SNAP participation rate as the number participating in a county over the county population, expressed in percentage points.[14]

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[11] A few studies examine short-term retailer responses to SNAP disbursement schedules. Moran et al. (2018) find significantly higher odds that stores in neighborhoods with high SNAP participation promoted sugar-sweetened beverages during the days benefits were issued. Goldin, Homonoff, and Meckel (2019) find that retailers generally do not vary prices within months to take advantage of fluctuating food expenditures tracking state SNAP issuance.

[12] The maximum allotments in Alaska and Hawaii are higher than the standard federal maximum. California was the only SSI "cash-out" state during the sample period, meaning SSI recipients receive a small payment in lieu of SNAP benefits. This is evident in the data: despite an above-average poverty rate of 14.2%, California's mean SNAP participation rate was 7.3% (three percentage points below the national average), while its mean SSI participation rate was 3.3% (almost one percentage point above the national average). State-level robustness checks similarly exclude Alaska, Hawaii, and California.

[13] This data is primarily gathered from the USDA FNS and supplemented with counts received from state and county SNAP offices when not available from the FNS. County-level numbers reflect the number of program participants in July, except in a few cases in which data is only available for other nearby time periods. In state-level robustness checks, I use data on SNAP participation from the USDA FNS (2019c). State-level participation is the annual average. I also collect data on annual SNAP benefits from the FNS for use in some specifications, though this data is not available for all counties.

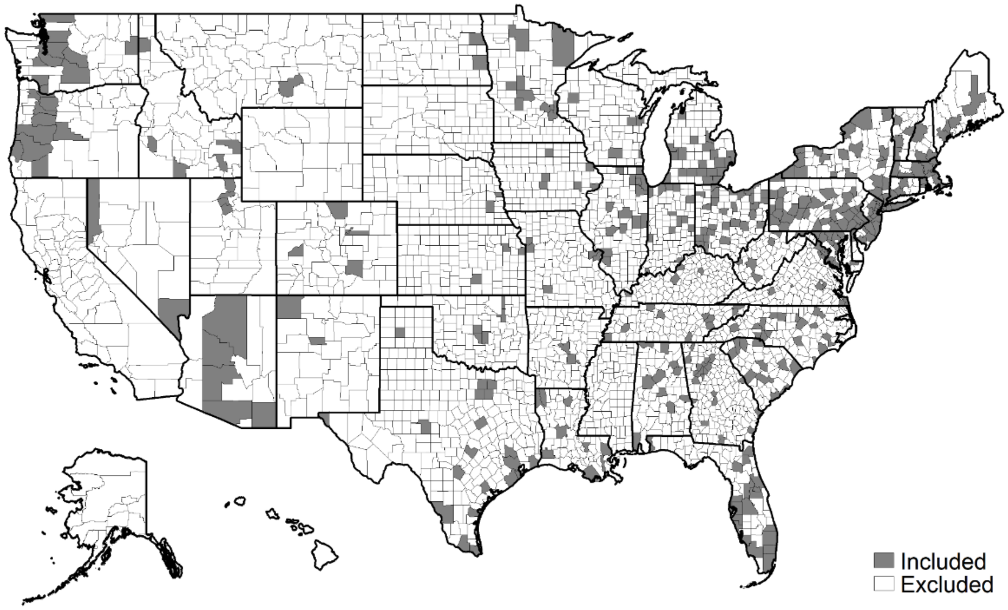
[14] Figure B1 in Appendix B displays variation in the SNAP participation rate between counties over time.

I use annual retail establishment counts from the Census County Business Patterns (CBP) series (U.S. Census Bureau Economy-Wide Statistics Divison 2018). The CBP provides establishment counts broken down by North American Industry Classification System (NAICS) code and/or number of employees, allowing examination of responses by specific types and sizes of retailers.[15] I collect information on establishment counts for industries that primarily retail food, that do not retail food, and that retail a combination of food and non-food goods. Additionally, I use information on annual average retail employment and payroll from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (BLS QCEW) (2019b).[16, 17] The QCEW censors employment and payroll data for counties with relatively few establishments, so I use a subsample of 405 counties with this information available for each year of the sample period in the employment and payroll analyses. These counties are shown in Figure 3. Collectively, these counties contain about 64.0% of the population of all counties in the full sample.

I use the above datasets to construct the outcomes of interest: establishments per 100,000 population, employment as a percentage of population, and average annual earnings per employee – or total payroll over employment. In my primary analyses of impacts on establishment counts, I focus on five NAICS-classified types of food retailers that are likely to be authorized to accept SNAP benefits: supermarkets and grocery stores, convenience stores, gasoline stations with convenience stores, supercenters and warehouse clubs (henceforth referred

[15] The CBP uses the Standard Industrial Classification (SIC) system prior to 1998. I only use CBP data from 1998 and later in order to avoid potential problems from merging different industrial classification systems.
[16] The CBP also contains information on employment and payroll by industry, but I use the QCEW data as it is more suitable to my analyses. Both datasets censor these outcomes in order to avoid disclosing information about individual businesses, but the QCEW censors employment and payroll information for fewer county-years. Additionally, the QCEW provides annual average employment counts, while the CBP provides employment counts only for the week of March 12th. The QCEW also provides information on establishment count, but it censors this information in some county-years, unlike the CBP.
[17] In other analyses, I use equivalent state-level data on businesses from the CBP and QCEW.

Figure 3. Counties included in subsample



to collectively as “supercenters”), and non-supercenter general stores (henceforth referred to as “general stores”). Supercenters and general stores are the first and second fastest growing industries I consider from 1998 to 2016, with their numbers increasing by about 213% and 68%, respectively.[18] I also construct two aggregations of NAICS-classified retail industries: those retail stores that may plausibly be authorized to accept SNAP benefits (“SNAP-plausible”) and those that are unlikely to be authorized (“SNAP-implausible”).[19] Table 1 details these breakdowns. In subsequent analyses using the uncensored county subsample, I focus on four higher-level NAICS-classified retailer groups: all (food and non-food) retailers; grocery stores, supermarkets, and convenience stores; gas stations (with or without convenience stores); and supercenters, warehouse clubs, and other general stores.[20]

[18] General stores increased from 24,424 stores in 1998 to 41,008 in 2016, and supercenters from 1,788 to 5,601.
[19] Figures B2 and B3 in Appendix B display variation in the number of SNAP-plausible and SNAP-implausible establishments per 100,000 population over time, respectively. Figure B4 illustrates trends over time in the national average number of SNAP-plausible and SNAP-implausible establishments per 100,000 population.
[20] I focus on higher-level NAICS groups in these analyses to minimize the loss of sample size from censoring, which is more prevalent for finer NAICS codes.

Table 1. Primary NAICS codes and aggregations

NAICS code or aggregation	Description	Larger category	Share of larger category
445110	**Grocery stores & supermarkets:** establishments primarily engaged in retailing a general line of food	445 Food and Beverage Stores	44.9% *
445120	**Convenience stores:** establishments primarily engaged in retailing a limited line of goods that generally includes milk, bread, soda, and snacks	445 Food and Beverage Stores	19.2% *
447110	**Gasoline stations with convenience stores:** establishments engaged in retailing automotive fuels in combination with convenience store items	447 Gasoline Stations	77.8%
452910	**Supercenters (& warehouse clubs):** establishments primarily engaged in retailing a general line of groceries in combination with general lines of new merchandise	452 General Merchandise Stores	8.2%
452990	**(Other) General stores:** generally smaller establishments than supercenters or warehouse clubs primarily engaged in retailing a general line of new merchandise including groceries, with none of the lines predominating; examples include dollar stores, general stores, trading posts, and variety stores	452 General Merchandise Stores	70.4%
"**SNAP-plausible**" (SP)	Sum of establishment counts listed above plus 4452 Specialty Food Stores	44-45 Retail Trade	22.4%
"**SNAP-implausible**" (SI)	44-45 Retail Trade minus "SNAP-plausible" establishments	44-45 Retail Trade	77.6%

* The other contributors in the "Food and Beverage Stores" category are 4452 Specialty Food Stores and 445310 Beer, Wine, and Liquor stores at 18.0% and 17.9%, respectively.
** I focus on the codes and aggregations listed above but examine effects on other retailer types not listed here.

I assemble information on a variety of state-level policies expanding SNAP eligibility in various ways over the sample period. I use this information to construct a state-level measure I term the "simulated eligibility variable" or SEV, which represents the collective generosity of these policies in a state and year. I discuss these policies and the SEV in depth in Section 3.

I use information on other county characteristics in various contexts. I use demographic information from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System in conjunction with population data to construct estimates of the percentage of the population in each year that is living in a rural area, black, Hispanic, age 17 or younger, age 60 or older, married, foreign-born, or educated with a bachelor's degree or higher (Manson et al. 2019).[21] In some specifications, I use information about the percentage of the population in each county that had income between 125% and 199% of the federal poverty level (FPL) in 1990 (Manson et al. 2019). Other robustness checks use annual county-level and/or state-level information from a variety of sources.[22]

The dataset consists of 3,030 counties and 57,570 county-year observations. Table 2 reports the population-weighted summary statistics of the key variables I use in my primary analyses for the full county sample and the uncensored subsample used in analyses of employment and payroll.[23] The average SNAP participation rate is about 11%, and the average SEV is about 17%. The average county has 86 establishments in SNAP-plausible industries per 100,000 residents and 288 establishments in SNAP-implausible industries. Counties in the

[21] Where this information is only available decennially, estimates are constructed by linear interpolation.
[22] These data include unemployment rates from the BLS Local Area Unemployment Statistics (2019a); poverty rates from the U.S. Census Small Area Income and Poverty Estimates (SAIPE) program (2019); personal income and government transfers from the Bureau of Economic Analysis' Regional Economic Accounts (2019); economic, state government, and other social program participation information from the University of Kentucky Center for Poverty Research (2019).
[23] Table B1 in Appendix B presents summary statistics of all variables I use in the primary analyses and robustness checks.

Table 2. Selected summary statistics

	Full county sample		Uncensored county sample	
	Mean	Std. dev.	Mean	Std. dev.
SNAP variables				
Participation rate (%)	11.12	(6.643)	10.89	(6.372)
Simulated eligibility variable (SEV) (%)	17.38	(4.227)	17.67	(4.344)
Establishment counts per 100,000 population				
Grocery stores & supermarkets	22.09	(12.18)	22.35	(12.20)
Convenience stores	9.923	(6.423)	10.67	(5.430)
Gas stations w/ conv. stores	32.84	(17.77)	26.01	(12.31)
Supercenters & warehouse clubs	1.332	(1.160)	1.239	(0.832)
General stores	11.81	(7.339)	9.580	(4.278)
SNAP-plausible stores	86.38	(26.32)	79.37	(17.55)
SNAP-implausible stores	288.0	(88.10)	291.4	(68.78)
Population (unweighted)	85360.4	(234710.7)	408407.9	(524664.6)
Demographic characteristics as % of population				
Rural	21.92	(25.74)	8.747	(11.61)
Black	13.28	(13.28)	15.07	(12.02)
Hispanic	12.31	(14.57)	15.01	(15.51)
Age 0-17	24.22	(3.069)	24.21	(2.867)
Age 60+	18.52	(4.665)	17.77	(4.186)
Married	53.16	(6.726)	51.65	(5.943)
Have bachelor's degree	26.95	(10.43)	30.29	(9.183)
Foreign-born	10.17	(9.735)	13.34	(10.43)
% with income of 125-199% of FPL in 1990	13.60	(4.083)	12.08	(3.293)
Counties	3,030		405	
Years	19		19	
Observations	57,570		7,695	

Statistics are weighted by county population, excluding population itself. The full sample excludes AK, HI, CA, and counties that either change borders during the sample or for which data is not available for the entire period. The uncensored county subsample includes the 405 counties with uncensored information about retail employment and payroll available for selected NAICS codes over the entire sample period. The sample period is 1998-2016, although simulated eligibility and benefits instruments are not available for 2016. Full summary statistics are displayed in Table B1 in Appendix B.

subsample used for employment and payroll analyses do not differ greatly from those in the full sample in terms of average SNAP participation, establishment counts, or SEV, though they are proportionally more urban, black, Hispanic, highly educated, and foreign-born.

3. Methodology

3.1. Endogeneity of SNAP

The goal of this study is to estimate the effects of SNAP on businesses' decisions concerning the operation of retail establishments, employment, and employee compensation. One approach to estimating these effects would be to estimate the fixed effects model

$$B_{ct} = \beta_0 + \beta_1 SNAP_{ct} + \boldsymbol{\beta_2 X_{ct}} + \boldsymbol{\beta_3 CFE_c} + \boldsymbol{\beta_4 YFE_t} + \boldsymbol{\beta_5 TT_{ct}} + \epsilon_{ct} \quad (1)$$

Here, B_{ct} represents a county-level business outcome in a given county c and year t such as the number of stores. $SNAP_{ct}$ represents the SNAP participation rate. $\boldsymbol{X_{ct}}$ represents a vector of covariates, while $\boldsymbol{CFE_c}$, $\boldsymbol{YFE_t}$, and $\boldsymbol{TT_{ct}}$ represent county fixed effects, year fixed effects, and county-specific time trends.

Any study of the causal effects of SNAP must address the potential endogeneity of SNAP participation.[24] First, unobservable economic factors may influence retail businesses as well as households' SNAP participation, income, and subsequent benefit size. Second, firm decisions concerning employment and pay could affect households' decision to participate in SNAP. Third, food retailer expansion would likely increase households' access to food and therefore increase the benefits to households of participating in SNAP. The first two possibilities would introduce downward bias to the estimate of β_1 in model (1), while the third would introduce upward bias. The ideal study would make use of randomized variation in $SNAP_{ct}$ to study

[24] SNAP selection issues are well-documented in the literature, e.g., in the context of determining the effect of SNAP on food security (Gregory, Rabbitt, and Ribar 2016).

impacts on businesses, but variation of this kind is not available. To address these issues, I focus instead on variation in state-level policies governing SNAP eligibility.

3.2. SNAP expansions

Under the federal SNAP rules, households are eligible if they have gross income under 130% of the FPL, net income under 100% of the FPL, and countable resources under the asset limit (USDA FNS 2019d).[25] Alternately, households are categorically eligible for SNAP if all household members receive Temporary Assistance for Needy Families (TANF), SSI, and/or General Assistance in some states. The benefit formula is also determined at the federal level. Each household's monthly benefit is equal to a maximum monthly allotment, which increases with household size, minus 30% of net income.

Since the enactment of welfare reform in 1996 and subsequent federal guidance, states have been given the flexibility to expand SNAP eligibility beyond the federal limits (Aussenberg and Falk 2019).[26] One option available is to alter the asset test by aligning SNAP vehicle policy with other social programs. States can increase the standard deduction applied to each vehicle's fair market value, exclude extra vehicles from the test, or eliminate vehicles from consideration. Every state has altered vehicle treatment in some way as of 2007.[27] Another option is to implement a standard medical expense deduction (SMED) that effectively reduces the net

[25] Households with elderly (age 60 or older) or disabled members are exempt from the gross income test. Monthly net income is equal to gross income minus 20% of earned income, a standard deduction varying over time, dependent care expenses (capped in earlier years of the time period), child support expenses, out-of-pocket medical costs over $35 for elderly and disabled members, and an excess shelter deduction equal to shelter costs over half of adjusted income but no more than the upper limit. The asset limit varies by year and is higher for households with elderly or disabled members. As of 2019, the asset limit is $2,250 for households without elderly or disabled members and $3,500 for households with such members. Included in countable resources is the fair market value of owned vehicles minus a $4650 deduction per driver in the household.

[26] States are not permitted to restrict eligibility to households that are eligible under the federal rules, only expand it to those households that are ineligible under the federal rules.

[27] Figure A1 shows how states altered the treatment of vehicles over time for households without elderly or disabled members. Many states adopted less restrictive vehicle policies in the early 2000s, and most eventually moved to exclude all vehicles from the asset test.

income of households with elderly or disabled members with out-of-pocket medical expenses below the deduction level.[28] 16 states have implemented SMEDs as of 2015.[29]

States are also able to implement "broad-based categorical eligibility" (BBCE) expansions in which they extend SNAP eligibility to households receiving certain non-cash benefits provided using TANF or maintenance-of-effort funds. States typically extend BBCE through the provision of simple benefits like brochures or referrals to telephone hotlines, making these expansions relatively inexpensive (Aussenberg and Falk 2019). Aligning SNAP eligibility to eligibility for these benefits effectively loosens or eliminates one or more of the gross income, net income, or asset tests for all or some subset of households.[30,31] From 1996 until 2015, the most common outcome of BBCE expansions was the elimination or alteration of the asset test, and the second most common outcome was a higher gross income limit. In 2015, 28 jurisdictions had expanded the gross income limit for some households without elderly or disabled members through BBCE expansions, 36 had eliminated or altered the asset test for at least some households, and 40 jurisdictions in total had implemented expansions of some type.[32]

[28] Federal SNAP rules define a person aged 60 years or more as elderly and a person receiving specific federal or state disability benefits as disabled.
[29] Figure A2 shows the 16 states that have implemented SMEDs as of 2015. Most states that implemented SMEDs did so in the late 2000s or early 2010s.
[30] BBCE expansions sometimes alter the income or asset tests only for households of a certain type or alter these tests differently for households of different types, e.g. households with any elderly and/or disabled members or households with children.
[31] Despite the extension of eligibility in these ways, it is important to note that some households that are made technically eligible for SNAP cannot receive a positive benefit due to their calculated benefit, which depends on net income and household size, being at or below zero. Larger households with net incomes higher than about 100% of the federal poverty level are ineligible for a positive benefit even if they pass their state's altered gross income, net income, and asset tests. In some years of the sample period, this threshold is as high as 115%. However, smaller households of one to two members passing these tests are always eligible for a small minimum monthly benefit ranging between $10 and $16 from 1996 to 2015.
[32] Figure A3 shows the least restrictive non-elderly gross income test that may be applied to households without elderly or disabled members that are made eligible through BBCE policies. Relative to changes in vehicle asset and SMED policies over the period from 2000 to 2015, changes to the gross income test are less concentrated in timing. Adoption of more flexible gross income tests are concentrated in states in the Northeast, Upper Midwest, Southwest, and Pacific regions, with many states in the Midwest and Southeast not expanding in this way.

States and their social services agencies may have several reasons to adopt the SNAP policies described here. These expansions are relatively inexpensive to states as the federal government funds SNAP benefits. They typically simplify administration, allow greater coordination between assistance programs, reduce the potential for errors in determining eligibility, and generally ease entry into SNAP for eligible households (Aussenberg and Falk 2019). It is possible states would expand SNAP in response to increasing need during economic downturns or for political reasons, but I test for these possibilities in Section 5 and find little evidence that they are driving factors.[33]

3.3 Simulated SNAP eligibility variable

States expand SNAP in the ways described above in greatly differing ways. Vehicle alterations, SMEDs, and BBCE expansions can take on very different "strengths." For instance, states could use BBCE to only increase the countable resource limit or to do away entirely with the asset test and net income test and raise the gross income test from 130% to 200% of the FPL. States frequently implement more than one type of expansion at once such that they interact with each other to determine household eligibility criteria. Some states' expansions impose different criteria for different subpopulations, e.g., households with elderly or disabled members or households with children. Binary indicators for whether certain types of expansions exist fail to capture the full extent of the variation in these policies.

One approach originally used to overcome issues of endogeneity between Medicaid participation and other outcomes is the construction of simulated measures of eligibility (Currie

[33] Most states do not expand SNAP to the maximum extent possible. SNAP expansions rely on alignment to other program eligibility criteria. Though the benefits these programs provide may be cheap as in the case of BBCE expansions, states must still bear the costs of providing them. Though administrative costs per case may decrease, overall administrative costs may increase if expansions greatly increase SNAP participation. Inertia or a hostile political environment may prevent some states from expanding SNAP. Further, states may expect expansions to SNAP to increase caseloads in other social programs, which would increase both financial and administrative costs.

and Gruber 1996; Cutler and Gruber 1996). A simulated eligibility variable (SEV) is typically constructed as the portion of a fixed sample of people or households eligible for a program under the changing rules in place in each of several areas at different times. The sample is fixed in that it always includes the same individuals or households with the same characteristics. The only variable factors are the changing eligibility criteria, often at the state-year-level. The use of a shared, fixed sample means that variation in the SEV derives only from changes in rules or policies, not endogenous changes in state-specific demographic or economic characteristics. Similarly, movement between states due to policy changes does not factor into the SEV's construction. The SEV therefore represents a measure of relative policy generosity that can be used to compare states over time; if it is higher in one state-year than another, that state extends eligibility to a larger portion of the common sample in that year than the other state-year.

The simulated eligibility approach is a convenient way to summarize the state rule changes I describe in a single measure. Other studies have employed simulated eligibility and benefit measures to study the effects of SNAP in various contexts (Han 2016, 2019; Leung and Seo 2019). I construct a simulated eligibility measure for use in the area-level context of this study that incorporates detailed variation in several types of state policies that affect SNAP eligibility and only counts households as eligible for SNAP if they qualify for a non-zero benefit.[34] Further, I employ this measure in an instrumental variables (IV) framework in order to estimate the impacts of greater SNAP participation tied to variation in the SEV.

[34] Han's (2016, 2019) simulated eligibility measure captures variation in BBCE policies. My measure also uses variation in SMED policies and non-BBCE vehicle policies relevant to determining the eligibility of households living in states without BBCE or who are not eligible for SNAP through their state's BBCE policy. It also uses policy variation covering a longer time period. Han (2019) considers a simulated eligibility measure excluding zero-benefit households but purposefully includes these households in the baseline measure as their "technical eligibility" is relevant to their eligibility for other programs.

I gather information from the USDA Economic Research Service's SNAP Policy Database on how states alter their BBCE and vehicle asset policies over time (2018). I gather additional details of these and other policies I require using reports from additional sources.[35] These include information such as which types of households are affected by BBCE expansions, how many vehicles are exempted from the asset test, the size of SMEDs, and the size of allotments and standard deductions varying by household size and year. I verify these policy details and the timing of their implementation using specific state SNAP policy manuals and reports or contacting state program administrators. Specific information on these rules and their changes over time is included in Appendix Tables A1 and A2.

To construct the SEV, I use a sample of households from the Survey of Income and Program Participation (SIPP) (2019) from every state and most years from 1996 to 2013.[36] The SIPP contains detailed information on household assets, income, expenses, and other characteristics necessary to determine household SNAP eligibility and benefit size. The inclusion of households from every state and many years ensures that the sample is widely representative of the United States on a national level during the sample period. To construct the SEV for a given state-year, I first adjust each household's finances for inflation to the relevant year. Then, I apply the federal and state rules in place in the given state and year to determine each households' SNAP eligibility. Since some "technically eligible" households have net income high enough to disqualify them for a positive benefit, I also calculate each eligible household's benefit according to the benefit formula in place in the relevant year. I consider only those that

[35] These sources are detailed in Table A3 in Appendix A.
[36] The SIPP includes information on about 343,000 household-year observations composed of about 877,000 individual-year observations and covers every year from 1996 to 2013 except 2000, 2006-2008, and 2012. Specific information on the SIPP sample, sample exclusions, and more is included in Appendix A.

are also eligible for a positive benefit to be “practically eligible.” I then construct SEV_{st} for the state s and year t as

$$SEV_{st} = \frac{\text{\# SIPP individuals in practically eligible households}_{\text{st}}}{\text{Total \# SIPP individuals}} \tag{2}$$

I repeat this process for each state and Washington, D.C. from 1996 to 2015. I represent SEV_{st} in percentage points, and it ranges from 12.3 to 27.0 percentage points. Appendix A contains an in-depth discussion on the SEV, its construction, and the policies contributing to its variation.[37]

Most of the variation in the SEV derives from BBCE expansions, especially those doing away with asset tests and/or increasing the gross income limit. Table 6, which is discussed in full in Section 4, shows how several typical expansions affect the SEV. Figure 4 illustrates interstate variation in the SEV over time. The SEV tends to increase or stay constant over time as most states only expand SNAP eligibility during the sample period, although a few states reverse expansions or change their policies such that the SEV falls. There is also a slight decline in the SEV in many states between 2010 and 2015 because the ARRA temporary benefit increase – which made some higher-income SIPP households temporarily eligible – expired in 2013. Figure 5 illustrates variation in the national average of the SEV, the average simulated federal eligibility rate – the portion of the SIPP sample that would be eligible for a positive benefit if no states expanded eligibility beyond the federal minimum – and the actual participation rate. Increases in the average value of the SEV above and beyond the simulated federal eligibility rate represent aggregate increases in SNAP policy generosity. Expansions occurred largely in two waves: vehicle test alterations and some BBCE expansions in the early 2000s and more BBCE

[37] I also construct and consider a “simulated potential benefit variable” (SPBV) representing the average monthly SNAP benefit received by households in the same common SIPP sample used to construct the SEV if every eligible household participated and received their maximum benefit. Further details are included in Appendix A.

Figure 4. Simulated SNAP eligibility variable (SEV) by state

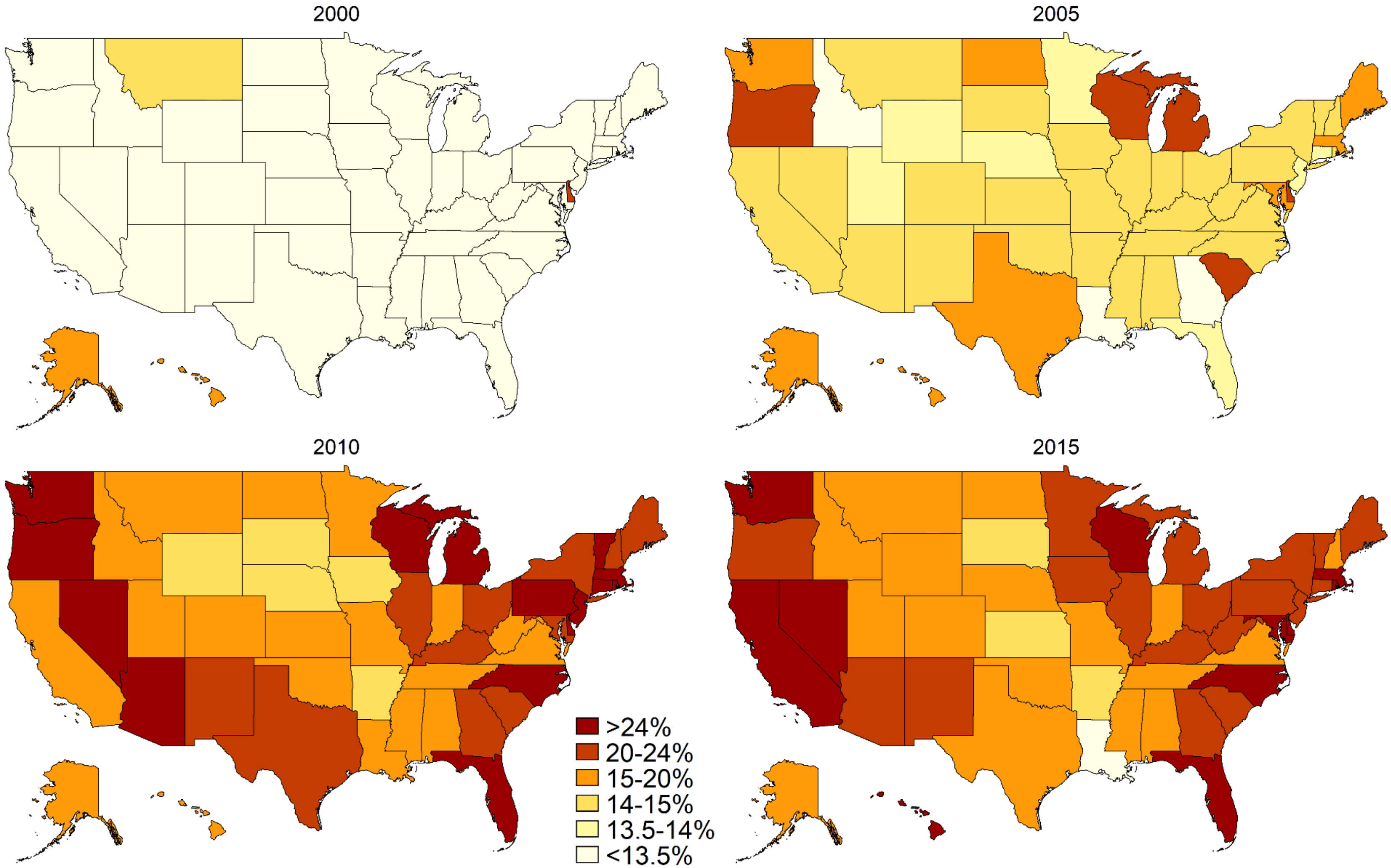
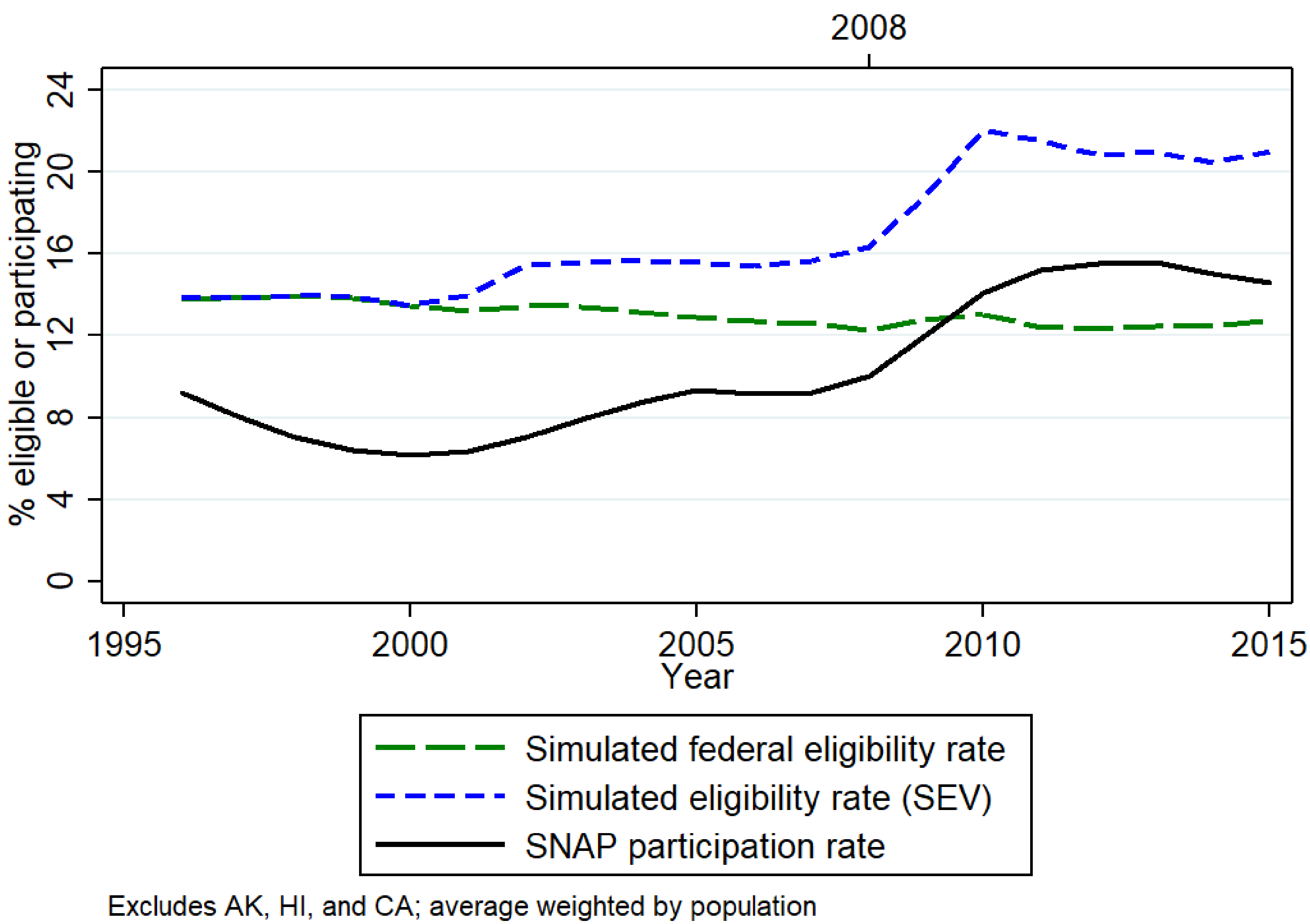


Figure 5. National simulated eligibility, simulated federal eligibility, and SNAP participation



expansions in and around the late 2000s during the Great Recession. Figure 5 also suggests a strong positive relationship between the SEV and the participation rate.

3.4 Reduced form models

To examine the business impacts of SNAP eligibility expansions, I estimate fixed effects models of the form:

$$B_{ct} = \beta_0 + \beta_1 SEV_{s,t-1} + \boldsymbol{\beta_2 X_{ct}} + \boldsymbol{\beta_3 CFE_c} + \boldsymbol{\beta_4 YFE_t} + \boldsymbol{\beta_5 TT_{st}} + \epsilon_{ct} \tag{3}$$

Baseline models include B_{ct} as one of several variables representing the number of establishments, employment, or average earnings for retail businesses in each industry. I lag SEV_{st} by one year in order to allow time for businesses to respond to or be affected by eligibility changes.[38] $\boldsymbol{X_{ct}}$ represents a vector of demographic covariates.[39] I exclude variables representing economic conditions from $\boldsymbol{X_{ct}}$ in the baseline models since they depend in part on B_{ct}. $\boldsymbol{CFE_c}$ and $\boldsymbol{YFE_t}$ are county and year fixed effects, which account for time-invariant county characteristics and nationwide trends over time. $\boldsymbol{TT_{st}}$ represents state-specific time trends, which I include due to the length of the sample period. Robust standard errors are clustered by state as the SEV only varies at the state level s.[40]

In order to examine potential heterogeneity in the estimates of β_1, I estimate models in which SEV_{st} is interacted with estimates of the percentage of each county population with incomes between 125% and 199% of the FPL in the pre-sample period year 1990.[41] Households

[38] Establishment count is particularly likely to require time to adjust in response to changes in SNAP eligibility and participation resulting from those eligibility changes, and employment and payroll would also require time as they vary with the number of stores in operation. I consider alternative timings in other regressions.
[39] Baseline models include in $\boldsymbol{X_{ct}}$ the percentages of the population living in rural areas, black, Hispanic, age 0-17, age 60+, married, educated with a bachelor's degree or higher, or foreign-born.
[40] I also consider state-level models as well as models using county-specific in place of state-specific time trends.
[41] Rates from 1990 are chosen because they represent conditions occurring prior to the sample period and are therefore not potentially impacted by the eligibility expansions, which begin in 1996. The cutoff values of 125% and 199% are chosen because they are available and approximate the range between 130% and 200%.

with income between 130% and 200% of the FPL range are particularly likely to gain eligibility from BBCE expansions, which account for most of the variation in the SEV. Therefore, eligibility expansions may have stronger impacts on businesses in counties where more of these households are concentrated.

These models take the form:

$$B_{ct} = \beta_0 + \beta_1 SEV_{s,t-1} + \beta_2 SEV_{s,t-1} * FPL_c + \boldsymbol{\beta_3 X_{ct}} + \boldsymbol{\beta_4 CFE_c} + \boldsymbol{\beta_5 YFE_t} + \boldsymbol{\beta_6 TT_{st}} + \epsilon_{ct} \quad (4)$$

FPL_c represents the percentage of households with incomes between 125% and 199% of the FPL in 1990. These models have the same structure and controls as model (3) other than the addition of the interaction term on the right-hand side.[42] If it is the case that eligibility expansions have a greater impact on businesses in counties with more of the households described above – presumably through households of that type – then the estimate of β_2 should be positive. The predicted impact of changes in the SEV on businesses can be determined through the magnitudes of the estimates of β_1 and β_2 and the values of SEV_{st} and FPL_c for the county in question.

Results from baseline regressions are presented in Section 4. I describe and test the identification assumptions I make in detail and perform various robustness checks in Section 5.

3.5 Instrumental variables model

Reduced form estimates of β_1 from model (3) show whether eligibility expansions impact various business outcomes, but it is difficult to interpret the magnitudes of these estimates. The SEV is a measure I construct solely to compare the collective generosity of SNAP policy between states and is not directly analogous to an actual eligibility rate because the SIPP oversamples low-income and low-resource households. Therefore, I estimate IV models in which the SEV instruments for the actual SNAP participation rate.

[42] Model (4) cannot include FPL_c on its own due to the inclusion of county fixed effects.

I estimate the first-stage model

$$SNAP_{c,t-1} = \alpha_0 + \alpha_1 SEV_{s,t-1} + \boldsymbol{\alpha_2 X_{ct}} + \boldsymbol{\alpha_3 CFE_c} + \boldsymbol{\alpha_4 YFE_t} + \boldsymbol{\beta_5 TT_{st}} + \varepsilon_{ct} \tag{5}$$

to obtain $\widehat{SNAP}_{ct}$, the predicted values of the participation rate $SNAP_{ct}$. SEV_{st} and $SNAP_{ct}$ are lagged by one year. The baseline model is otherwise structured and includes the same controls as model (3). Using $\widehat{SNAP}_{ct}$, I then estimate second-stage models of the form:

$$B_{ct} = \beta_0 + \beta_1 \widehat{SNAP}_{c,t-1} + \boldsymbol{\beta_2 X_{ct}} + \boldsymbol{\beta_3 CFE_c} + \boldsymbol{\beta_4 YFE_t} + \boldsymbol{\beta_5 TT_{st}} + \epsilon_{ct} \tag{6}$$

These models are structured the same and include the same controls as model (3) but with $\widehat{SNAP}_{ct}$ in place of SEV_{st}.

Results from baseline regressions are presented in Section 4. I describe and test the identification assumptions I make in detail and consider alternative models as robustness checks in Section 5.

4. Results

4.1. Hypotheses

I hypothesize that SNAP eligibility expansions – as represented by an increase in the SEV – would increase SNAP participation and subsequently increase demand for the goods of retail firms. Most evidence using modern data suggests that SNAP receipt increases demand for food more than equivalent cash transfers (Beatty and Tuttle 2015; Hastings and Shapiro 2018; Bruich 2014). I therefore expect that demand would increase more for the goods of "SNAP-plausible" retailers selling primarily food or a combination of food and non-food goods than it would for the goods of "SNAP-implausible" retailers.

I hypothesize that retail firms would generally respond to the increases in demand accompanying expansions of SNAP eligibility by operating more stores. This might happen through firms being more likely to open new stores or refraining from closing existing stores in areas where SNAP is expanded, though I cannot test which mechanism predominates in the framework of this study. I also expect that firms would demand more labor in response to expansions. This may take the form of hiring more employees to staff new or existing stores, increasing employee hours, and/or increasing wages. I do not observe hours or wages in my dataset directly, but I do observe total employment and total payroll, which I use to construct retail industry employment as a percentage of the population and average earnings per employee. I expect SNAP expansions to increase both employment and average employee earnings, as an increase in hours or wages would increase this measure. I refrain from hypothesizing whether the IV estimates of SNAP participation's impacts on businesses are larger or smaller than the naïve estimates due to the competing sources of downward and upward bias I identify in Section 3.[43]

While many types of retailers may respond to changes in SNAP-induced demand, I hypothesize that certain types will respond more strongly. I divide all analyses by industry or industry group in order to examine differential responses. I hypothesize that retailers targeting low-income customers are more likely to respond to increases in SNAP participation as those increases disproportionately impact their customers' demand for goods. The industry groups I consider are too broad to specifically estimate impacts on these types of stores, but two industry groups stand out as likely to respond for this reason: convenience stores and general stores, which are most frequently dollar and discount stores.[44] Supercenters may also respond for this reason, though these stores are somewhat more diverse in terms of customer base.

[43] Unobservable economic factors may influence retail businesses as well as the SNAP participation rate. Retail businesses also have some influence on the participation rate through employment and compensation decisions.
[44] In 2016, the two largest firms Dollar General and Dollar Tree operated more than two-thirds of the stores in this group alone (Dollar Tree Stores Inc. 2017; Dollar General Corporation 2017).

I also hypothesize that small stores are more responsive to SNAP expansions than large stores. Firms tend to plan the opening of large food stores like supercenters years in advance, so it is less likely that SNAP-induced changes in demand strongly influence decisions concerning their operation. In contrast, smaller stores are less costly to build and operate as well as quicker to open, so marginal changes in demand are more likely to influence the number of these stores in operation.

Finally, I hypothesize that retailers respond most strongly to SNAP expansions in areas with more households with income and/or resources just above the federal cutoffs. More people stand to gain eligibility when SNAP is expanded in these areas, so the size of the corresponding increase in demand for food and other goods in these areas is likely also larger.

4.2. Primary results

Table 3 reports the main results from regressions of retail establishment counts per 100,000 population on the SEV for each of the seven retail industries and industry aggregations I consider. The SEV is expressed in percentage points and can range from 0 to 100. I find evidence that SNAP eligibility expansions increase the number of general stores. This industry includes dollar stores, general stores, trading posts, and variety stores, among others. Specifically, a one percentage point increase in the SEV – roughly 5.8% of the mean value of 17.4% – increases the number of general stores operated by 0.038 establishments per 100,000 population, or about 0.33% of the mean. I also estimate positive average effects on the numbers of supercenters and convenience stores. A one percentage point increase in the SEV increases their numbers by 0.29% and 0.23% of their respective mean establishment counts. However, neither of these estimates are statistically significant, indicating that these effects are less

Table 3. Establishment count reduced form regression results

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
SNAP SEV	-0.00933	0.0224	0.0336	0.00384	0.0384*	0.0919*	-0.0483
	(0.0547)	(0.0224)	(0.0366)	(0.00382)	(0.0200)	(0.0527)	(0.125)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean est. count per 100,000 population	22.09	9.923	32.84	1.332	11.81	86.38	288.0
Mean SEV	17.38	17.38	17.38	17.38	17.38	17.38	17.38
R^2	0.350	0.190	0.104	0.369	0.242	0.186	0.700
Observations	57,570	57,570	57,570	57,570	57,570	57,570	57,570

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points and indicates the percentage of the SIPP sample belonging to an eligible household when each state-year's SNAP eligibility rules are applied. SEV is lagged one year. Establishment counts are expressed as the number per 100,000 population.

consistent across county-years.[45] Overall, I estimate that a one percentage point increase in the SEV increases the number of "SNAP-plausible" stores by 0.092 per 100,000 population, or about 0.11% of the mean. This increase is primarily driven by effects on the number of general stores (41.8% of the effect), gas stations with convenience stores (36.6%), and convenience stores (24.4%). I find no evidence of a corresponding increase in the number of "SNAP-implausible" stores. Though these estimates seem small, the SEV varies from a minimum of 12.3% to a maximum of 27.0%, so SNAP eligibility expansions can and do increase the SEV by a much larger amount than one percentage point.

The higher responsiveness of SNAP-plausible stores to SNAP expansions is consistent with these expansions increasing SNAP participation, which subsequently increases aggregate demand for retail goods. The lack of evidence for SNAP-implausible stores being similarly responsive suggests that these expansions primarily increase demand for goods at stores where benefits are more frequently accepted. These estimates do not provide evidence that demand for food for consumption at home specifically drives the increase in the number of SNAP-plausible stores. The industries that primarily retail food-at-home goods – grocery stores, supermarkets, and conveniences stores – are not the largest responders to SNAP expansions. The most responsive retail industries are smaller general stores followed by supercenters. Both industries retail a mix of food-at-home and non-food goods and have experienced huge growth during the sample period. It could be that increasing SNAP participation increases demand for these retailer's food and non-food goods because stores in these industries tend to target lower-income

[45] The point estimate of the coefficient on the count of supercenters is similar in relative size to that on general stores. Both industries retail several lines of merchandise including food and changes in SNAP-induced demand may affect both similarly. One reason the estimate on the count of supercenters may be less precise than that on general stores is their relative infrequency: on average, there are only 1.3 relative to the 11.8 general stores per 100,000 population.

consumers. General stores tend to be small and may therefore be quicker to adjust to demand.[46] Most SNAP benefits are spent at supercenters, which could explain their relatively greater responsiveness to SNAP expansions (USDA FNS 2019a).

Table 4 reports abbreviated results for the first-stage regressions using the full county sample and the county subsample used for employment and payroll analyses. The SEV is strongly positively correlated with the SNAP participation rate in all samples. Like the SEV, the SNAP participation rate is expressed in percentage points, hypothetically ranging from 0 to 100. A one percentage point increase in the SEV increases the SNAP participation rate in either sample by 0.17 percentage points on average, or by about 1.5% of the mean participation rate of 11.1%. The first-stage F-statistics of 17.4 and 17.6 indicate that the SEV is adequately powered to instrument for SNAP participation in either sample.[47] I compare the results of first-stage regressions using the SEV and other policy instruments in Appendix B and Table B2. The SEV I construct outperforms a variety of SNAP policy instruments used in other IV studies.

Table 5 reports the second-stage IV results of regressions of retail establishment counts per 100,000 population on the predicted SNAP participation rate for each of the seven retail industries and industry aggregations I consider. As the IV model is just-identified, these estimates are proportional in magnitude to those reported in Table 3. Taken with those results, the IV results in Table 5 provide context for how changes in the SEV affect retail establishment counts through SNAP eligibility expansions' effects on the participation rate. Table 5 also shows

[46] Table B3 in Appendix B reports reduced form and IV estimates from regressions of the number of SNAP-plausible or SNAP-implausible establishments divided by the number of employees at each establishment on the SEV or predicted SNAP participation rate to examine heterogeneity in retailer response by retailer size as opposed to specific NAICS classification. Though few estimates are statistically significant, smaller SNAP-plausible stores increase in number relatively more than larger stores, providing suggestive evidence for the hypothesis that smaller retailers are nimbler and therefore more responsive to SNAP expansions than larger ones.

[47] All subsample F-statistics exceed the critical value of 16.4 defined by Stock and Yogo (2005) to limit the maximum Wald test size distortion to 0.10 at the 5% significance level.

Table 4. First-stage regression results

	SNAP participation rate Full county sample	SNAP participation rate Uncensored county subsample
SNAP SEV	0.167***	0.171***
	(0.0400)	(0.0407)
Demographic controls	Yes	Yes
County and year FE	Yes	Yes
State-specific time trends	Yes	Yes
Mean SNAP part. rate	11.12	10.89
Mean SEV	17.38	17.67
First-stage F-statistic	17.44	17.60
R^2	0.881	0.911
Observations	54,540	7,290

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The SNAP participation rate and the simulated SNAP eligibility variable (SEV) are expressed in percentage points. The participation rate indicates the actual percentage of the relevant county population belonging to a household that receives SNAP benefits, and the SEV indicates the percentage of the SIPP sample belonging to an eligible household when each state-year's SNAP eligibility rules are applied. Results shown are for current-period participation rates and simulated eligibility rates. Lagged values of these variables are used in baseline IV regressions, and first-stage regressions using these and other timings produce similar results.

Table 5. Establishment count second-stage and naïve regression results

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
Instrumental variables second stage							
Predicted SNAP part. rate	-0.0547	0.131	0.197	0.0225	0.225**	0.538*	-0.283
	(0.316)	(0.129)	(0.196)	(0.0238)	(0.0966)	(0.289)	(0.722)
R^2	0.346	0.191	0.107	0.369	0.248	0.202	0.699
Naive fixed effects							
SNAP part. rate	0.286	0.0954**	0.193***	0.0101**	0.175***	0.792***	0.624*
	(0.189)	(0.0417)	(0.0705)	(0.00469)	(0.0295)	(0.197)	(0.354)
Mean est. count per 100,000 population	22.09	9.923	32.84	1.332	11.81	86.38	288.0
Mean SNAP part. rate	11.12	11.12	11.12	11.12	11.12	11.12	11.12
R^2	0.357	0.192	0.107	0.370	0.249	0.204	0.701
Observations	57,570	57,570	57,570	57,570	57,570	57,570	57,570

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The predicted SNAP participation rate from the first stage and the actual SNAP participation rate are expressed in percentage points. Establishment counts are expressed as the number per 100,000 population. Participation rate and SEV are lagged one year.

results from the naïve fixed effects regressions of establishment count per 100,000 on the SNAP participation rate.

A one percentage point increase in the SNAP participation rate – roughly 9.0% of the mean rate of 11.1% – increases the number of general stores by 0.23 establishments per 100,000 population, or about 1.9% of the mean. Similarly, a one percentage point increase in the participation rate increases the numbers of supercenters and convenience stores by 1.7% and 1.3% of their respective mean establishment counts, though neither of these estimates are statistically significant. Overall, a one percentage point increase in the participation rate increases the number of "SNAP-plausible" stores by 0.54 per 100,000 population, or about 0.6% of the mean. The estimates from the naïve regressions are more precise, but they understate the effects of SNAP participation on the numbers of general stores, supercenters, and convenience stores. This is likely due to the IV strategy avoiding the sources of downward bias I describe in Section 3.[48]

4.3. Other results

Table 6 outlines how several common state SNAP expansions increase the value of the SEV, increase the estimated SNAP participation rate, and increase the number of SNAP-plausible stores. I present one of the most generous state expansions possible as an example: eliminating the asset and net income tests and raising the gross income test to 200% FPL. The estimated impacts of an expansion like this are of reasonable size. The expansion increases the value of the SEV by about 10.8 percentage points, which I estimate would increase the SNAP

[48]The IV strategy produces estimates of the effects of changes in SNAP participation tied to eligibility expansions on business outcomes, while the naïve fixed effects strategy produces estimates of the relationship between the actual SNAP participation rate and business outcomes. Households participating due to gaining eligibility from expansions are a subgroup of all households participating in SNAP, and they likely differ in observable and unobservable characteristics alike. Therefore, differences between the IV and naïve results may also represent the consequences of aggregate differences in the responses of these types of households to participating in SNAP. I discuss observable differences between households by eligibility status in Section 5.

Table 6. Predicted impacts of common state SNAP eligibility expansions

Policy	Mean SEV (%)	Increase over baseline (% points)	Est. increase in SNAP part. rate (% points)	Est. increase in SNAP-plausible stores (# per 100,000)
Baseline: no state rule change (federal minimum eligibility)	13.04	-	-	-
BBCE: Eliminate asset and net income tests; gross income test of:				
130% FPL	17.08	4.04	0.67	0.37
165% FPL	21.24	8.20	1.37	0.75
185% FPL	22.77	9.73	1.62	0.89
200% FPL	23.82	10.78	1.80	0.99
BBCE: Eliminate asset test; net income test of 100% FPL; gross income test of:				
130% FPL	16.65	3.61	0.60	0.33
165% FPL	18.31	5.27	0.88	0.48
185% FPL	18.55	5.51	0.92	0.51
200% FPL	18.63	5.59	0.93	0.51
BBCE: Eliminate asset and net income tests; gross income test of:				
200% FPL for households with children	18.05	5.01	0.84	0.46
200% FPL for households with elderly or disabled members; 130% FPL for others	20.74	7.70	1.29	0.71
200% FPL for households with elderly or disabled members	18.56	5.52	0.92	0.51
SMED of:				
$100	13.11	0.07	0.01	0.01
$200	13.25	0.21	0.04	0.02
Vehicles: Exclude:				
One per household	14.33	1.29	0.22	0.12
One per adult	14.54	1.50	0.25	0.14
All	14.59	1.55	0.26	0.14

SEV is calculated separately as if denoted policy were applied in each year of the sample in a state in the contiguous United States. Mean SEV represents the cross-year average of the SEV for the sample period 1996-2015. Estimated increase in SNAP participation rate assumes that a one percentage point increase in the SEV increases the SNAP participation rate by 0.167 percentage points as estimated in Table 4. Estimated increase in number of SNAP-plausible establishments assumes that a one percentage point increase in the SEV increases the number of SNAP-plausible establishments by 0.092 per 100,000 population as estimated in Table 3.

participation rate by about 1.8 percentage points, about 16.2% of the mean rate. This would increase the number of SNAP-plausible stores by about one per 100,000 population. Assume a county with population of 100,000 and that the new SNAP beneficiaries' average benefit is $750 per year.[49] This expansion would then lead to $1.35 million more in benefits issued per year, which is plausibly enough to support the opening or stop the closing of one SNAP-plausible store – especially a smaller store like a general store.

Table 7 reports results from regressions of retail establishment count per 100,000 population on the SEV and the interaction of the SEV and the percentage of the 1990 population in each county with income between 125 and 199% of the FPL as illustrated by model (4). Positive estimates of the coefficients on the interaction term indicate that the SEV increases the number of establishments relatively more in counties where more people fell in the income range to which BBCE policies frequently expand SNAP eligibility. I find that this is the case for general stores as well as supercenters, the two retail store types which respond most strongly to SNAP expansions as indicated by Table 3. The lower panel of Table 7 illustrates the coefficient estimates on the SEV and the interaction term from the upper panel by predicting the impact of a one percentage point increase in the SEV from its mean value on establishment counts per 100,000 population in three hypothetical counties where different proportions of the population have incomes between 125 and 199% FPL. In a county where 13.6% of the population meet this criterion (the mean rate), a one percentage point increase in the SEV predicts an increase in the number of general stores per 100,000 population by 0.044, or 0.37% of the mean. The corresponding increase in the number of supercenters is about 0.005, or 0.38% of the mean. These estimates of the impacts of the SEV on store counts are larger than the corresponding

[49] The average benefit per recipient in 2018 was almost $1,500 per year (USDA FNS 2019c).

Table 7. Establishment count reduced form regressions including interaction between SEV and percentage with income between 125% and 199% of FPL

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
Reduced form regressions							
SNAP SEV	0.130	0.0595	0.144*	-0.0314***	-0.138**	0.0737	-0.230
	(0.111)	(0.0651)	(0.0860)	(0.0104)	(0.0564)	(0.130)	(0.419)
SEV* 1990 125%-199% FPL rate	-0.0106	-0.00282	-0.00838	0.00268***	0.0134***	0.00139	0.0138
	(0.00787)	(0.00416)	(0.00525)	(0.000698)	(0.00440)	(0.00930)	(0.0323)
Mean est. count per 100,000 population	22.09	9.923	32.84	1.332	11.81	86.38	288.0
Mean SEV	17.38	17.38	17.38	17.38	17.38	17.38	17.38
Mean 1990 125%-199% FPL rate	13.60	13.60	13.60	13.60	13.60	13.60	13.60
R^2	0.350	0.190	0.105	0.371	0.245	0.186	0.700
Observations	57,570	57,570	57,570	57,570	57,570	57,570	57,570

Derived estimates of effect of one percentage point increase in SEV from 17.4 (mean) to 18.4

1990 125%-199% FPL rate:	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
9.5% (-1 std. dev.)	0.029	0.033	0.064	-0.006	-0.011	0.087	-0.099
13.6% (mean)	-0.014	0.021	0.030	0.005	0.044	0.093	-0.042
17.7% (+1 std. dev.)	-0.058	0.010	-0.004	0.016	0.099	0.098	0.014

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points and indicates the percentage of the SIPP sample belonging to an eligible household when each state-year's SNAP eligibility rules are applied. The estimated percentage of county residents in 1990 with income between 125% and 199% of the federal poverty level is expressed in percentage points; only its interaction with the SEV is included in regressions as the rate does not vary by year and is therefore collinear with county fixed effects. Establishment counts are expressed as the number per 100,000 population. The second panel applies the point estimates in the first panel to estimate the average impacts of a one percentage point increase in the SEV starting from its mean value on the numbers of stores per 100,000 population in counties where 9.5%, 13.6%, or 17.7% of the population had income in the 125%-199% FPL range in 1990.

estimates from Table 3, and they are even larger for counties with 17.7% of the population falling in the income range: 0.84% and 1.2% of the mean establishment counts, respectively. These results indicate that SNAP expansions increase the numbers of these stores more in areas with relatively large federally ineligible populations that can gain eligibility if gross income limits are relaxed. This is consistent with SNAP expansions increasing participation and subsequent demand for these store's goods more in these areas.[50]

Table 8 reports the results of reduced form and IV second-stage regressions alternately using the outcomes of establishment count per 100,000 population, average annual employment as a percentage of population, or average annual earnings per employee on the SEV or predicted SNAP participation rate, respectively. These regressions use a subsample of 405 counties that have uncensored employment and payroll information available for the entire sample period and divide retail industries into four higher-level aggregations: all retail (food or non-food); grocery stores, supermarkets, and convenience stores; gas stations (with or without convenience stores); and supercenters and general stores.[51] The estimated effects of SNAP expansions or predicted SNAP participation on the numbers of establishments in Table 8 are consistent with those full-sample estimates shown in Tables 3 and 5. I find little evidence that SNAP affects the employment rate in any of the four industry aggregations I consider. A one percentage point increase in the predicted participation rate reduces the percentage of the population employed at supercenters and other general stores by 4.0% of the mean on average, but this estimate is

[50] Table B3 in Appendix B reports reduced form and IV estimates from other regressions intended to examine the heterogeneity of retailer responses by store size as measured by number of employees. In it, the outcome of SNAP-plausible or SNAP-implausible establishments per 100,000 population are divided into three size categories by number of employees at the store: 0-9, 10-49, or 50+ employees. I find suggestive evidence that smaller SNAP-plausible stores are more responsive than larger stores, consistent with the finding in Shannon et al. (2016) that greater SNAP participation predicted increased SNAP authorization among smaller stores.
[51] The counties included are shown in Figure 3. Table B1 in Appendix B shows these counties are more urban, more educated, and have higher income than the full county sample, but they are otherwise similar. Higher-level aggregations are used in order to avoid too thin of a county sample.

Table 8. Subsample establishment count, employment, and average employee earnings reduced form and second-stage regression results

	All retail, food & non-food	Grocery stores, supermarkets, & conv. stores	Gas stations	Supercenters & general stores
Establishment counts per 100,000 population				
RF: SNAP SEV	0.00484	0.0312	0.0359	0.0513**
	(0.159)	(0.0629)	(0.0335)	(0.0240)
R^2	0.794	0.503	0.587	0.653
IV: Pred. part. rate	0.0280	0.181	0.208	0.297***
	(0.918)	(0.375)	(0.184)	(0.111)
R^2	0.794	0.513	0.590	0.665
Employment rates as % of population				
RF: SNAP SEV	-0.00423	0.000528	0.0000287	-0.00302
	(0.00470)	(0.00230)	(0.000289)	(0.00341)
R^2	0.662	0.414	0.386	0.466
IV: Pred. part. rate	-0.0245	0.00305	0.000166	-0.0175
	(0.0304)	(0.0131)	(0.00166)	(0.0201)
R^2	0.659	0.410	0.386	0.432
Annual average employee earnings				
RF: SNAP SEV	-44.76***	-40.32	-36.52	39.32***
	(13.04)	(29.91)	(25.36)	(14.53)
R^2	0.473	0.408	0.265	0.229
IV: Pred. part. rate	-259.0***	-233.3*	-211.4	227.6**
	(38.22)	(139.6)	(147.5)	(101.7)
R^2	0.449	0.382	0.256	0.190
Mean est. count	370.7	33.02	33.35	10.82
Mean emp. rate	5.432	0.888	0.242	0.435
Mean emp. earnings	27,670.0	21,997.7	20,527.7	22,306.8
Mean SEV	17.67	17.67	17.67	17.67
Mean SNAP part. rate	10.89	10.89	10.89	10.89
Observations	7,695	7,695	7,695	7,695

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. Results shown are for a subsample of 405 counties with uncensored information about retail employment and payroll available for selected NAICS codes over the entire sample period of 1998 to 2016. The simulated SNAP eligibility variable (SEV) and the predicted SNAP participation rate from the first stage are expressed in percentage points. Establishment counts are expressed as the number per 100,000 population. Annual average employment rate is expressed in percentage points as percentage of population employed. Annual payroll is expressed in 2010-adjusted thousands of dollars per 100,000 population. Annual average earnings are expressed in 2010-adjusted dollars per employee. Participation rate and SEV are lagged one year.

statistically insignificant. Despite this decrease, I find that a one percentage point increase in the participation rate increases average employee earnings in these stores by $228, or about 1.0% of mean annual pay. Interestingly, I find evidence of decreases in the average employee earnings in other industries. A one percentage point increase in SNAP participation results in a reduction of the mean employee earnings in stores of all types by $259 – about 0.9% of the mean. Given SNAP's effects on the number of supercenters and general stores relative to other retailers, these effects are suggestive of SNAP participation increasing labor demand in these industries relative to others through the margins of wages or hours but not total employment. An increase in hours may be the more likely mechanism, especially given the tendency of these retail industries to employ part-time workers.

5. Robustness checks

5.1. Internal validity

Consistent identification of β_1 in the reduced form model (3) and β_1 and β_2 in model (4) relies upon several assumptions. SEV_{st} should be independent of the error term ϵ_{ct}. It must not be the case that some unobserved third factor impacts both state SNAP policy and retail businesses or that the retail business variables of interest directly impact state SNAP policy. I cannot formally test these assumptions, but I consider several possible ways they may not hold in turn.

Economic downturn tends to affect businesses negatively, and states experiencing downturns may also be more likely to expand SNAP to address expanding need. This may drive a negative relationship between the business outcomes of interest and SNAP participation tied to state-level expansions. Additionally, states that expand SNAP to a greater degree may be more likely to adopt other policies incentivizing or disincentivizing retail investment. This may include more generous administration of other safety net programs; for example, states with generous SNAP policies may also distribute more TANF funds to households, which would affect demand for food and other goods. To investigate these possibilities, I estimate IV regressions in Table 9 including additional sets of controls describing the county economic environment, the state policy environment, or participation rates in other social programs.[52] The primary findings are robust to the inclusion of any of these three sets of controls.

Additionally, I examine whether state economic conditions or policy environments predict the adoption of policies that determine the SEV in Tables 10 and 11, respectively. These tables report the results of linear probability models in which the outcomes are the presence of a BBCE or vehicle test alteration policy. I find little evidence pointing to a relationship between economic conditions and state SNAP expansions. States appear to be slightly more likely to implement BBCE expansions or vehicle test alterations in response to lagged poverty rates, but not significantly more likely. Inconsistently, higher levels of past income per capita predict BBCE expansions, and I find mixed evidence on income per capita's impact on the probability of vehicle test alteration. Similarly, I find little evidence that the policy environment variables I consider consistently predict state SNAP expansions. I therefore do not consider the exclusion of either of these control sets from the baseline models problematic.

[52] Economic controls include the unemployment rate, the poverty rate, the natural log of personal income per capita, and the natural log of non-SNAP government transfers per capita. State government controls include a dummy for the governor being a Democrat, the percentage of the state house that are Democrats, and the percentage of the state senate that are Democrats. Other social welfare program participation controls include state participation rates for TANF, SSI, and Medicaid. I include these three programs' participation rates and exclude others' participation rates as states have more discretion over the administration of these programs. Other social program participation rates are endogenous in similar ways to the SNAP participation rate, but I do not take up that issue as I include them only as controls here. Additionally, it is possible that SNAP participation increases participation in other programs through reductions of application or certification costs. If this is the case, additional buying power from other-program participation may be considered a mechanism through which SNAP impacts retailers.

Table 9. Establishment count second-stage regressions including additional control sets

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
Including economic controls							
Pred. part. rate	-0.215	0.152	0.190	0.0372	0.234*	0.435	0.0178
	(0.353)	(0.150)	(0.269)	(0.0324)	(0.121)	(0.373)	(0.918)
First-stage F-statistic	12.26	12.26	12.26	12.26	12.26	12.26	12.26
R^2	0.346	0.197	0.111	0.380	0.255	0.209	0.721
Observations	55,763	55,763	55,763	55,763	55,763	55,763	55,763
Including state government controls							
Pred. part. rate	-0.0608	0.150	0.209	0.0221	0.218**	0.530*	-0.188
	(0.315)	(0.150)	(0.209)	(0.0219)	(0.0950)	(0.299)	(0.748)
First-stage F-statistic	19.41	19.41	19.41	19.41	19.41	19.41	19.41
R^2	0.351	0.192	0.109	0.371	0.255	0.206	0.701
Observations	55,803	55,803	55,803	55,803	55,803	55,803	55,803
Including other social welfare program participation controls							
Pred. part. rate	-0.0322	0.0181	0.252	0.0245	0.220*	0.492	-0.299
	(0.351)	(0.156)	(0.246)	(0.0281)	(0.119)	(0.383)	(0.969)
Mean est. count per 100,000 population	22.09	9.923	32.84	1.332	11.81	86.38	288.0
Mean SNAP part. rate	11.12	11.12	11.12	11.12	11.12	11.12	11.12
First-stage F-statistic	17.05	17.05	17.05	17.05	17.05	17.05	17.05
R^2	0.348	0.189	0.105	0.362	0.228	0.199	0.691
Observations	54,313	54,313	54,313	54,313	54,313	54,313	54,313

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The SNAP participation rate and the simulated SNAP eligibility variable (SEV) are expressed in percentage points. Establishment counts are expressed as the number per 100,000 population. Participation rate and SEV are lagged one year. Economic controls include the unemployment rate, the poverty rate, the natural log of personal income per capita, and the natural log of non-SNAP government transfers per capita. State government controls include a dummy for the governor being a Democrat, the percentage of the state house that are Democrats, and the percentage of the state senate that are Democrats. Other social welfare program participation controls include state participation rates for TANF, SSI, and Medicaid in percentage points; I include participation rates for these three social programs and not others as states have more discretion over these program's administration.

Table 10. Regressions of SEV-determining policies on economic characteristics

	BBCE	Vehicle test alteration
Unemployment rate	0.0183	0.00132
	(0.0212)	(0.0112)
t-1	0.0192	0.00486
	(0.0185)	(0.0105)
t-2	0.00909	-0.0116
	(0.0115)	(0.00970)
t-3	0.0203	0.0108
	(0.0227)	(0.0176)
Poverty rate	0.0124	0.00408
	(0.00889)	(0.00698)
t-1	0.00822	0.00436
	(0.00781)	(0.00581)
t-2	0.0114*	0.0114**
	(0.00649)	(0.00490)
t-3	0.00888	0.0107**
	(0.00572)	(0.00475)
Ln personal income per capita	-0.326	-0.184
	(0.625)	(0.265)
t-1	0.573	0.504**
	(0.379)	(0.236)
t-2	-0.179	0.170
	(0.305)	(0.196)
t-3	1.182**	-0.721**
	(0.578)	(0.349)
Demographic controls	Yes	Yes
State and year FE	Yes	Yes
State-specific time trends	Yes	Yes
Mean of SEV-determining policy	0.462	0.797
Mean unemployment rate	5.923	5.923
Mean poverty rate	13.10	13.10
Mean ln(Personal income per capita)	10.42	10.42
R^2	0.758	0.885
Observations	912	864

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and state fixed effects, and state-specific time trends. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. "BBCE" and "Vehicle test alteration" are dummies indicating whether each state has adopted these policies in some form. Unemployment rate and poverty rate are expressed in percentage points. Log of real personal income per capita is included. Each regression includes lags from the previous three periods.

Table 11. Regressions of SEV-determining policies on policy environment characteristics

	BBCE	Vehicle test alteration
Governor Democrat	-0.0356	0.00786
	(0.0371)	(0.0202)
t-1	0.00308	0.0302*
	(0.0117)	(0.0168)
t-2	0.0273	-0.00538
	(0.0195)	(0.0125)
t-3	-0.00591	-0.0205
	(0.0282)	(0.0232)
% state house Democrats	0.000599	-0.00304
	(0.00287)	(0.00197)
t-1	-0.000894	-0.00425**
	(0.00178)	(0.00196)
t-2	-0.000465	0.0000984
	(0.00223)	(0.00108)
t-3	-0.00100	0.00203
	(0.00369)	(0.00206)
% state senate Democrats	-0.00114	-0.0000112
	(0.00290)	(0.00160)
t-1	0.00161	0.00205
	(0.00164)	(0.00145)
t-2	-0.00128	0.000389
	(0.00112)	(0.000844)
t-3	-0.00252	-0.00159
	(0.00221)	(0.00222)
Demographic controls	Yes	Yes
State and year FE	Yes	Yes
State-specific time trends	Yes	Yes
Mean of SEV-determining policy	0.462	0.797
Mean Democratic governor	0.410	0.410
Mean % Democrats in house	50.23	50.23
Mean % Democrats in senate	46.82	46.82
R^2	0.745	0.888
Observations	874	828

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and state fixed effects, and state-specific time trends. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. “BBCE” and “Vehicle test alteration” are dummies indicating whether each state has adopted these policies in some form. “Governor Democrat” is a dummy variable equal to one if the governor is a Democrat. The percentage of each state house and senate that are Democrats are expressed in percentage points. Each regression includes lags from the previous three periods.

It is possible, though somewhat unlikely, that retail businesses may collectively influence state's decisions to expand SNAP, for example through laying off employees or lowering wages such that the number of needy households significantly expands. I examine whether business responses follow or precede SNAP expansions by estimating regressions in which establishment count is modeled as a function of the SEV – lagged one year as in the baseline models – as well as several additional lags and leads of the SEV in Table 12. The inclusion of lags and leads restricts the sample period to 2000 to 2013. For comparison, the lower panel reports estimates of the baseline reduced form regressions using only the one-year lag of the SEV and the restricted sample period. If retail business responses precede SNAP expansions, I expect that estimates of the SEV lead coefficients would be large and positive, and reverse causality of this type would be an issue for my estimation.[53] This does not appear to be the case, as I find no significant evidence of relationships between the current number of retail establishments and future values of the SEV.[54]

Consistent identification of β_1 in the IV model (6) additionally relies upon several other assumptions. First, the simulated eligibility instrument SEV_{st} must have a clear, strong effect on the participation rate $SNAP_{ct}$ in the first-stage model (5). Variation in the SEV derives from state-level policies altering the portion of SIPP households eligible for SNAP. An increase in the SEV implies more generous policy and means that more real households in the state become eligible to receive benefits, all else equal. If additional households would participate when made

[53] Autocorrelation is certainly a problem in models like these with several leads and lags of the SEV. Therefore, I do not expect this model to present precise estimates of the SEV's impact, but rather to test generally for the potential for reverse causality.

[54] Examining models including lags and leads of the SEV is also interesting because it is not theoretically clear how long it would take firms to respond to changes in SNAP participation. It is possible that the full effects of SNAP participation on the operation of retail establishments would manifest in less than a year or that it might take several years, depending on retail industry. The results of Table 12 suggest that responses may be quicker than modeled in the baseline regressions due to the positive, significant coefficients on the current-period SEV for both general stores and gas stations with convenience stores.

Table 12. Reduced form regressions using lags and leads of simulated eligibility variable (SEV)

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
SNAP SEV							
t-4	0.0397	0.0331	-0.0779	0.00000431	-0.00862	-0.0161	-0.147
	(0.0455)	(0.0413)	(0.0651)	(0.00414)	(0.0164)	(0.0789)	(0.107)
t-3	-0.00554	0.0107	-0.0103	0.00192	0.0150	0.0189	-0.0458
	(0.0181)	(0.0175)	(0.0331)	(0.00206)	(0.0110)	(0.0384)	(0.0662)
t-2	-0.00136	-0.0210	0.0570*	0.00145	0.00618	0.0469	-0.0549
	(0.0206)	(0.0161)	(0.0308)	(0.00407)	(0.00862)	(0.0329)	(0.0804)
t-1	-0.00414	0.0233	-0.00733	0.00261	0.00759	0.0298	-0.0626
	(0.0236)	(0.0145)	(0.0174)	(0.00231)	(0.00756)	(0.0384)	(0.0672)
t	-0.00943	-0.00859	0.0410***	0.00112	0.0260**	0.0336	0.0418
	(0.0200)	(0.0129)	(0.0123)	(0.00318)	(0.0117)	(0.0303)	(0.0576)
t+1	-0.0494*	0.0109	0.0269	-0.00202	0.0150	0.00526	-0.0404
	(0.0289)	(0.0169)	(0.0212)	(0.00357)	(0.0122)	(0.0416)	(0.0894)
t+2	-0.00526	0.00306	-0.00658	0.000447	0.00966	0.0122	-0.0828
	(0.0337)	(0.0221)	(0.0426)	(0.00372)	(0.0140)	(0.0503)	(0.0663)
R^2	0.304	0.192	0.102	0.322	0.127	0.179	0.654
Baseline: SNAP SEV, t-1	-0.0176	0.0114	0.0449	0.00396	0.0280	0.0739	-0.0768
	(0.0414)	(0.0191)	(0.0388)	(0.00392)	(0.0187)	(0.0612)	(0.121)
Mean est. count per 100,000 population	22.03	9.959	33.05	1.297	11.69	86.60	290.2
Mean SEV	17.33	17.33	17.33	17.33	17.33	17.33	17.33
R^2	0.304	0.191	0.101	0.322	0.126	0.178	0.654
Observations	42,420	42,420	42,420	42,420	42,420	42,420	42,420

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The sample period is restricted to 2000-2013 due to the inclusion of leads and lags. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. Establishment counts are expressed as the number per 100,000 population. In the first panel, regressions include lags and leads of the SEV centered around the one-year lag of the SEV used in the baseline models. In the second panel, regressions include only the one-year lag of the SEV as in the baseline model but use the same restricted sample period for comparison.

eligible or when income or asset tests are relaxed, the SEV would be positively correlated with the real SNAP participation rate. I find that this is the case, as the SEV is strongly predictive of the county SNAP participation rate in the full sample and subsample that I consider. I discuss the first-stage results in greater detail previously in Section 4.

Second, I assume that SEV_{st} does not affect the business outcomes B_{ct} except through effects on $SNAP_{ct}$. Any retail business decisions made in response to SNAP eligibility rule changes would likely be motivated by anticipated or observed changes in demand for their goods. The determining factor of consumer demand changes of these sort would not be eligibility expansions themselves, but changes in SNAP receipt due to the expansions. Therefore, I argue that this assumption is reasonable.

5.2. External validity

Variation in the SEV derives from changes in state eligibility rules beyond the federal minimum. The SIPP households these rules are applied to can be categorized in one of three mutually exclusive and exhaustive groups: always eligible for SNAP (by meeting the federal rules in every year), never eligible for SNAP (by never meeting the federal rules or any state's rules in any year), or sometimes but not always eligible for SNAP (by meeting the federal rules or some state's rules in some years). The last group represents those households whose participation in SNAP can be "turned on" by the state eligibility expansions summarized in the SEV. The IV estimates shown in Section 4 can therefore be interpreted as local average treatment effects in that they represent the responses of retailers to changes in SNAP participation among sometimes-eligible households.[55] This interpretation requires a further

[55] Increases in the SEV may increase participation among households that are "always eligible" if it becomes less costly for these households to apply due to the expansions summarized in the SEV (e.g., they no longer must report detailed information on vehicles or other assets). However, I cannot identify these households in the SIPP like I can those who become eligible.

assumption of monotonicity: increases in the SEV should never cause households to not participate in SNAP who otherwise would. This is reasonable, as it would be unusual for households previously receiving SNAP benefits to stop because eligibility is expanded.

This raises the issue of external validity of the results: would retailers respond similarly to changes in SNAP participation among always-eligible households (e.g., due to policies impacting application costs or changes in federal eligibility or work rules)? This question could be addressed by using policy instruments directly impacting participation among always-eligible households, but the available instruments of this type are too weak for use in the context of this study.[56] I examine the characteristics of the SIPP households in each eligibility category in Table A5 in Appendix A. Relative to sometimes-eligible households, always-eligible households have on average roughly half of the total income, a third of the earned income, a hundredth of the countable non-vehicle assets, and a fourth of the vehicle equity.[57]

Given these differences, SNAP participation would likely affect these subgroups' food demand differently. Always-eligible households would receive larger average benefits, both in size and relative to their income, so changes in their participation would likely impact demand more than the changes in SNAP participation induced by the expansions considered in this study. The estimated coefficients on SNAP-plausible and SNAP-implausible retailer outcomes in Section 4 would therefore likely represent lower bounds of the effects of changes in participation among the entire eligible population. Further, while different types of households may have different demand elasticities for products from different types of stores, recent studies generally find only small differences in household choice of food retailer type by SNAP participation

[56] See Appendix B and Table B2 for further discussion.
[57] Figure A6 in Appendix A further illustrates the differences between always-eligible and sometimes-eligible households by presenting scatterplots of SIPP households' total income and countable non-vehicle assets by eligibility status.

status and income, suggesting that the individual estimated effects on industry-specific retailer outcomes are likely also lower bounds (Dong and Stewart 2012; Taillie, Grummon, and Miles 2018; Ver Ploeg et al. 2015).

5.3. Other robustness checks

I consider and present several variants of my baseline models using establishment counts as outcomes in Table B4 of Appendix B. My findings concerning SNAP's effects on the number of general stores and SNAP-plausible stores are robust to most changes I consider.[58] I also find that a one-dollar increase in SNAP benefits per capita instrumented by the SEV similarly impacts the number of stores at the state or county levels.[59]

The exclusion of state-specific time trends from the model changes the estimates significantly. Without trends, it appears that a one percentage point increase in SNAP participation increases the numbers of SNAP-plausible stores by 1.08 per 100,000 population – about twice the size of the baseline estimate. This effect is largely driven by increases in the numbers of grocery stores/supermarkets and convenience stores. As shown in Figure 5, the average SEV and SNAP participation rate trend upwards during the sample period. Accordingly, I test for multicollinearity between the SEV and state-specific time trends by calculating the variance inflation factor (VIF) for the SEV in the first stage with and without trends. Because their inclusion only increases the VIF from about 3.0 to about 4.6 and the sample covers a relatively long period, I opt to include state-specific trends in the baseline model.

[58] These changes include the use of state-level models, alternate timings of the SEV and participation rate, fewer controls, county-specific time trends in place of state-specific time trends, and a sample excluding the ten densest counties from the sample.
[59] I use a restricted county sample in analyses using benefits per capita, as this information is not available at the county level for all county-years.

Regressions including California – which is excluded from the baseline models – produce smaller estimates of the effects of SNAP on the number of general stores or SNAP-plausible stores that are not statistically significant at the 10% level. California's SSI cash-out policy during the sample period may have partially severed the relationship between SNAP expansions and retailer outcomes since those receiving SSI received only a small benefit in place of the SNAP benefit defined by the federal formula, and California's large population noticeably impacts the estimates. Regressions without population weights produce similarly smaller estimates, indicating that the retailer responses I estimate in the baseline models are driven primarily by those in counties of higher population.

6. Conclusion

In this study, I develop a novel approach to summarize detailed variation in state-level SNAP expansions through a measure of simulated eligibility. I use this measure to examine the food retail industry's responses to SNAP expansions since 1996 and resultant increases in SNAP participation. In response to an increase in SNAP participation, I find that retailers operate more stores in industries where SNAP benefits are often accepted. In particular, I find evidence that retailers operate more small general stores in response to higher SNAP participation and that average employee earnings at these stores increase. I find no evidence of a pattern of similar responses by non-food retailers.

SNAP appears to primarily impact retailers targeting lower-income customers more likely to receive benefits. I do not directly observe SNAP expansions' effects on retail sales and cannot distinguish between retailers choosing to open new stores or to keep stores open they would otherwise close. However, this study's findings are suggestive of SNAP playing a role in the location decisions of general stores and supercenters to a lesser extent, both of which have undergone tremendous growth in the last 20 years. Dollar stores targeting lower-income customers played a large part in this rapid, continuing growth.[60, 61] I assume changes in demand among lower-income customers as the primary mechanism connecting SNAP participation and the numbers of these stores, but I do not purport to rule out all other possibilities here. Further investigation is required to determine whether firms simply respond to SNAP-induced changes in demand or specifically target areas with higher SNAP participation rates when choosing new locations.

While SNAP appears to benefit some retailers, it is not clear whether its impacts on the retail environment – increases in the numbers of general stores and supercenters – benefit consumers. Higher SNAP participation does not appear to increase the number of grocery stores in operation, which are often characterized as more healthful food stores. Among other things, supercenters increase the obesity rate, housing prices, and food security, but they do not appear to strongly affect the market entry of other food retail firms (Courtemanche and Carden 2011; Pope and Pope 2015; Courtemanche et al. 2018; Ellickson and Grieco 2013). Fewer studies have examined analogous impacts of dollar stores. Dollar stores' presence is associated with decreased produce purchases and higher childhood obesity prevalence, though one study found no causal evidence that dollar store exposure contributes to childhood obesity (Cai et al. 2018; Gorski Findling et al. 2018; Drichoutis et al. 2015). Several city policymakers and media outlets have expressed concern that dollar stores' presence encourages unhealthy choices and limits

[60] In 2004, the three largest firms Dollar General, Dollar Tree, and Family Dollar operated just under half of the general stores in the United States, but in 2016 they operated more than two-thirds (Dollar Tree Stores Inc. 2005, 2017; Family Dollar Stores Inc. 2005; Dollar General Corporation 2005, 2017)

[61] Dollar General alone planned to open 1,000 stores in 2020 (Nathaniel Meyersohn, "Dollar General is opening 1,000 new stores next year," CNN Business, December 5, 2019).

access to healthy foods.[62] Others have argued that dollar stores improve food access in areas where other retailers will not go, especially low-income rural or urban areas.[63] Allcott et al. (2019) estimate that 90% of nutritional inequality between the wealthy and the poor is driven by differences in demand, while only about 10% is driven by supply-side factors like product exposure and prices, suggesting that policies restricting dollar stores and similar retailers may dampen the relationship between SNAP and these stores' prevalence but may not meaningfully improve the food retail environment or consumer nutrition. Further causal research is necessary to clarify these relationships.

Expanding SNAP makes the operation of more food retail stores feasible. If it is a goal of economic stimulus to support businesses like these, then SNAP is an effective vehicle for stimulus. State policymakers seeking a relatively cheap means of stimulus may find that expanding SNAP eligibility through one of these expansions is an attractive option. In particular, BBCE expansions waiving net income and asset tests greatly increase eligibility while also reducing administrative burden. Increasing take-up is another option. Though expanding SNAP is inexpensive for states, the same is not true for the federal government, which must pay out benefits to all who qualify and cannot simply cap spending. Federal policymakers should carefully weigh the costs of SNAP expansions against their benefits in order to determine optimal policy, especially given recently proposed changes to SNAP.

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[62] E.g., "How dollar stores prey on the poor" (Michael Sainato, The Progressive, October 1, 2019), "Dollar stores are everywhere. That's a problem for poor Americans" (Nathaniel Meyersohn, CNN Business, July 19, 2019), "Dollar stores are feeding more Americans than Whole Foods, and it's leading some communities into crisis" (Aria Bendix, Business Insider, December 10, 2018).

[63] E.g., "Unjust deserts" (Steven Malanga, City Journal, January 3, 2020), "Do dollar stores help the poor with cheap, vital staples? Or block out grocers and trigger food deserts?" (Rachel Siegel, Washington Post, February 18, 2019), "How Dollar General Is Transforming Rural America" (Frank Morris, National Public Radio, December 11, 2017).

The Trump administration proposed regulation in July 2019 that would greatly reduce BBCE by limiting the benefits conveying eligibility (Aussenberg and Falk 2019). One analysis estimates the rule would cause 3.1 million participants to lose eligibility in 2020 – though participation may fall more as BBCE lowers application and information barriers – and would lead to net federal savings of $9.4 billion over five years (USDA FNS 2019b). Assuming the effects of SNAP participation I estimate are bi-directional, the rule would reduce the number of food stores operating in the United States by almost 1,700.[64] Similarly, the administration issued a final rule initially intended to come into effect in April 2020 restricting waivers of the benefit time limit for able-bodied adults without dependents; the FNS estimates that nearly 700,000 beneficiaries will not meet work requirements and lose benefits, reducing the number of food stores similarly by about 400 (USDA FNS 2019f).[65] When evaluating changes like these to SNAP eligibility, policymakers ought to take into consideration all of the consequences of cutting SNAP – including the harm done to businesses – and weigh them against the potential financial savings

[64] A reduction of 3.1 million participating in SNAP relative to the 327.2 million U.S. population represents a 0.947 percentage point decrease in the participation rate. I apply the point estimate of a reduction of 5.4 SNAP-plausible stores per 1,000,000 population accompanying a one percentage point decrease and estimate a total 1,673.24 SNAP-plausible store decrease.

[65] A federal judge issued a preliminary injunction in March 2020 blocking the rule change from taking effect pending the outcome of a lawsuit by several states (Maria Godoy, "Judge blocks rule that would have kicked 700,000 people off SNAP," NPR, March 14, 2020).

Chapter II: Do SNAP Expansions Affect Medicaid Enrollment and Spending?

The Supplemental Nutrition Assistance Program (SNAP, formerly named the Food Stamp Program) and Medicaid play important roles in the patchwork of U.S. safety net programs. In 2017, SNAP issued $63.7 billion in benefits (or “food stamps”) to 42 million people in low-income households to be spent on food, while Medicaid and the Children’s Health Insurance Program (CHIP) (hereafter referred to collectively as “Medicaid”) provided $592 billion in health insurance coverage and other services to almost 74 million people (USDA FNS 2019c; Wolfe, Rennie, and Truffer 2018). These programs target largely overlapping populations, and household decisions to participate in either program are likely interrelated. SNAP participants are more likely to enroll in Medicaid than non-participants and vice versa due to participation in one program lowering the costs of participation in the other. Further, nutrition and health care are both determinants of health, so if SNAP improves its recipients’ health, it might reduce their perceived need for Medicaid coverage. Therefore, the aggregate effects of expanding SNAP on Medicaid enrollment and spending are unclear a priori. Measuring these effects are important to enable policymakers to design effective social policy and estimate the budget impacts of SNAP expansions.

Despite the importance of considering interactions between safety net programs, most studies of these programs examine their effects in isolation. Among the studies that consider the relationship between SNAP and Medicaid, most examine the effects of Medicaid on SNAP. These studies generally find that Medicaid enrollment increases the probability of participation in SNAP and discuss labor disincentives, awareness of benefits, lowered stigma, and program outreach as possible mechanisms (Yelowitz 1996; Baicker et al. 2014; Schmidt, Shore-Sheppard, and Watson 2019; Lanese, Fischbein, and Furda 2018; Burney, Boehm, and Lopez 2018). Only one study I identify considers the reverse causal pathway from SNAP to Medicaid, finding no evidence that state-level SNAP expansions affect the probability that households have Medicaid coverage (Han 2019).

The purpose of this study is to estimate the aggregate impacts of SNAP eligibility expansions on Medicaid enrollment and spending. I assemble annual state-level information on SNAP policies, SNAP participation, and Medicaid enrollment and spending. I construct a measure of simulated aggregate eligibility summarizing variation over time in state policies determining SNAP eligibility and representing the relative generosity of states' SNAP rules. Using this measure, I estimate the impacts of SNAP expansions on state Medicaid enrollment, enrollment of various eligibility subgroups, Medicaid spending per capita, and Medicaid spending per Medicaid enrollee.

This study makes three major contributions. First, it contributes to the broader empirical literature on social program interactions. Second, while several studies have studied the effects of Medicaid on SNAP, this study is among the first to consider the effects of SNAP on Medicaid. Specifically, this study is the first to consider the statewide effects of SNAP expansions on Medicaid enrollment and spending. Third, I construct a simulated measure of SNAP eligibility incorporating detailed variation in several kinds of state eligibility rules over the twenty years from 1996 to 2015.

I find evidence that expanding SNAP eligibility increases overall Medicaid enrollment. Enrollment increases are largest for non-disabled adults, followed by children. SNAP expansions reduce total Medicaid spending per enrollee, but not total spending per capita. Coupled with the increases in Medicaid enrollment, this suggests that SNAP expansions increase take-up of

Medicaid – especially among non-disabled adults – possibly through SNAP participation lowering barriers to Medicaid enrollment. On average these marginal Medicaid enrollees do not incur as many costs as pre-existing enrollees, at least in the short term.

1. Background

SNAP provides food-purchasing assistance to millions of eligible low-income households in the form of SNAP benefits, also known as food stamps, which can be redeemed for food for consumption at home. Benefits are federally funded through the Food and Nutrition Service (FNS) of the U.S. Department of Agriculture (USDA), but the program is jointly administered at the federal and state levels. The program has grown substantially over the last few decades: from 1996 to 2016, average participation grew by 73.3%, and benefits issued grew by 93.9%. Several program changes occurred alongside this growth, including state-level expansions to eligibility, the shift to provision of benefits through electronic benefit transfer (EBT), the shift to joint administration with other social programs, a temporary benefit increase as part of the American Recovery and Reinvestment Act (ARRA) from 2009 to 2013, and other federal changes to eligibility and benefit determination.

Medicaid is the second largest public insurance program in the United States behind Medicare. Medicaid primarily provides health insurance to millions of people with limited income and resources. It is administered by the states in conjunction with the Centers for Medicare & Medicaid Services (CMS) and jointly funded by the federal and state governments. Eligibility for Medicaid varies for different groups of people defined by law. From 1996 to 2014, these included children, parents, pregnant women, seniors aged 65 and older, and disabled people receiving Supplemental Security Income (SSI) and/or Social Security disability benefits.[66] During this period, the federal government required coverage of members of these groups with income at or below a defined percentage of the federal poverty level (FPL), but states had leeway to expand eligibility to these groups beyond the minimum required. Many did so. In some cases, states expanded eligibility to low-income, childless, non-disabled adults. Like SNAP, Medicaid grew substantially during this period. From 1995 to 2016, enrollment grew by 116.2%, and spending grew by 129.6% (Wolfe, Rennie, and Truffer 2018).

Medicaid and SNAP target overlapping low-income populations. In a sample from the Survey of Income and Program Participation (SIPP) during the Great Recession, 79% of SNAP recipient households received Medicaid coverage in some form (Moffitt 2016). Because of this overlap, it is possible that changes to one program may affect the benefits and costs households face when deciding to apply for or participate in the other. This is especially true for households who would not see a large benefit from program participation, e.g., households that would not enroll in Medicaid because they do not anticipate using many medical services. One potential mechanism for program interplay is the reduction of application or recertification costs. Applying for the first program may be costly in that it could be time-consuming, complicated, or confusing, but it may reduce the burden involved with applying for the second program. For example, applicants may be able to file the second application on the same visit, saving time and travel and costs, or they might be more familiar with the application process and the kinds of required materials during the second application. Some states reduce application and recertification costs for the second program by administering SNAP and Medicaid (among other programs) through the same agency and offering joint applications and/or joint processing of

[66] Elderly and disabled people receiving Medicaid are frequently dually eligible for Medicare and Medicaid benefits. Medicare is the primary payer for most medical services, but Medicaid can cover some benefits Medicare does not, such as long-term nursing home and home health services, and/or Medicare premiums or cost-sharing.

applications.[67] Participation in one program may lower the marginal stigma costs of applying for another as households adjust to receipt of public benefits and adjust their attitudes concerning them. Awareness of one program may also lead to awareness of the other – especially if they are administered together. Expansions of one program may therefore lead to increased participation in the other, even among those who are ineligible for the first program, all else equal. Finally, if either type of benefit reduces work incentives and subsequent earnings, then it may indirectly increase eligibility or take-up of the other. Through any combination of these mechanisms, expansions of SNAP eligibility may also increase enrollment in Medicaid and subsequent total spending on Medicaid as new enrollees utilize medical services.

Among SNAP's stated purposes are the improvement of nutrition and food security for needy households. Gregory, Rabbitt, and Ribar (2016) and Bitler (2016) provide overviews of the literatures examining the links between SNAP and food security and SNAP and health, respectively. When studies control for selection into SNAP, they generally find that the program is more likely to have positive impacts on food security, nutrition, and health outcomes. As nutrition, food security, and healthcare jointly determine health, it is possible that participation in SNAP may reduce the perceived need for insurance coverage through Medicaid such that participation in Medicaid falls, especially among lower-risk groups like non-disabled adults.

Even if SNAP does not reduce Medicaid enrollment, it is likely that those who decide to enroll in Medicaid due to a SNAP expansion or due to their own SNAP participation would not cost as much to insure as other Medicaid enrollees. Members of the first group, whose Medicaid enrollment is "SNAP-induced," likely have different characteristics than those who specifically

[67] FNS provides information about states engaging in joint SNAP and Medicaid application filing and/or processing in recent years in a series of "State Options Reports" (USDA FNS 2019e). This data source does not cover the years in this study's sample period. By the end of fiscal year 2017, all states but 12 had implemented one or both forms of program integration.

and intentionally seek out health insurance through Medicaid. Presumably, if an individual is eligible for Medicaid and chooses to enroll independently of other social program participation, the benefits to that individual of enrollment – health insurance coverage – outweigh the costs. Conversely, if an individual enrolls in Medicaid only because their SNAP participation lowers the costs, it is likely that individual's benefit from enrollment would on average be lower than that of the individual who enrolls independently of SNAP participation, as they would face the same costs in the absence of SNAP. Since the benefits from health insurance depend largely upon the expected utilization of healthcare, it is likely that SNAP-induced Medicaid enrollees expect to use fewer medical services – perhaps due to better overall health or barriers to access – than average enrollees. Therefore, while SNAP expansions increase Medicaid enrollment and total Medicaid spending, if they disproportionally bring in low-cost enrollees like non-disabled adults, they might lower average healthcare utilization and spending per enrollee.

Existing evidence suggests that Medicaid enrollment increases the probability of SNAP participation. Yelowitz (1996) finds evidence that expansions in Medicaid eligibility explain some of the growth in food stamp participation in the 1980s and 1990s and that the effect does not appear to be linked to a change in labor supply. Baicker et al. (2014) exploit randomized variation in Medicaid enrollment from the Oregon Health Insurance Experiment and similarly find an increase in SNAP participation with little or no change in labor outcomes. Several studies find that the Affordable Care Act (ACA) Medicaid expansions increased SNAP take-up, with Medicaid outreach being a likely mechanism and increases in SNAP participation concentrated among adults without dependents who experienced the largest increases in Medicaid eligibility (Schmidt, Shore-Sheppard, and Watson 2019; Lanese, Fischbein, and Furda 2018; Burney, Boehm, and Lopez 2018).

I identify one study considering the reverse causal pathway from SNAP to Medicaid. Han (2019) finds evidence that state-level SNAP eligibility expansions reduce the likelihood that households have private health insurance coverage and no evidence of a corresponding increase in Medicaid enrollment.

2. Data

I assemble state-level information over the years 1999 to 2012 on SNAP participation, Medicaid enrollment and spending, SNAP and Medicaid policies, and various population characteristics.[68] I exclude from the sample Alaska and Hawaii due to their different benefit formulas and Idaho due to Medicaid enrollment data quality issues.[69, 70]

I use annual average SNAP participation counts from the USDA FNS (2019c) in conjunction with intercensal population estimates to construct the SNAP participation rate as the number participating over the state population, expressed in percentage points.[71]

I use counts of Medicaid enrollees from CMS's Medicaid Statistical Information System (MSIS) (2017) to construct Medicaid enrollment rates as the number of Medicaid enrollees over the total state population, expressed in percentage points.[72, 73] MSIS also provides counts of enrollees broken down by basis of eligibility, which I use to construct enrollment rates for four

[68] Medicaid spending data is available from 1997, but the sample period is restricted as MSIS enrollment data is only available from 1999 to 2012. The restricted sample period avoids variation in Medicaid eligibility and participation resulting from ACA Medicaid expansions.
[69] The maximum allotments in Alaska and Hawaii are higher than the standard federal maximum.
[70] MSIS data showed implausible swings in Medicaid enrollment for several eligibility groups in Idaho. I perform supplementary analyses using an alternate measure of Medicaid enrollment due to the potential for other unobserved data quality issues in the MSIS enrollment data.
[71] Figure C1 in Appendix C displays variation in the SNAP participation rate between states over time.
[72] Data from the original MSIS is not available for public access as of April 2020 as CMS has decommissioned the database and is in the process of implementing the Transformed Medicaid Statistical Information System (T-MSIS) (Centers for Medicare & Medicaid Services 2019b). Historical state Medicaid enrollment counts are not currently available from T-MSIS.
[73] Figure C2 in Appendix C displays variation in the total Medicaid enrollment rate between states over time.

groups – children, non-disabled adults under age 65, seniors aged 65 or over, and blind or otherwise disabled adults – also expressed as a percentage of overall population.[74, 75, 76] In some analyses, I consider each eligibility group's share of total enrollment, which I construct as the count of enrollees in each group over total state Medicaid enrollment, expressed in percentage points. Due to potential data quality issues observed in the MSIS data, I use in some specifications an alternate measure of annual state Medicaid enrollment from the University of Kentucky Center for Poverty Research's (UKCPR) National Welfare Data (2019). I construct an alternate Medicaid enrollment rate as described above using this alternate measure as well as an alternate measure of Medicaid spending per enrollee, the construction of which I describe below.

I gather information on annual Medicaid spending by state from state expenditure reports from the Medicaid Budget and Expenditure System/State CHIP Budget and Expenditure System (CMS 2019a).[77, 78] I use this information to construct measures of Medicaid spending per capita and per enrollee as total Medicaid spending in 2010-adjusted dollars over total state population or total state Medicaid enrollment, respectively.[79]

I assemble information on a variety of state-level policies expanding SNAP eligibility in various ways over the sample period. I use this information to construct a measure I term the

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[74] These eligibility groups are mutually exclusive, but not exhaustive. They make up almost 91% of total enrollment, on average. Excluded eligibility groups include children in foster care, women screened or treated under breast and cervical cancer programs (BCCP), and unknown basis of eligibility.
[75] Figure C3 in Appendix C illustrates trends over time in the national average Medicaid enrollment rates.
[76] The MSIS enrollment data is also broken out by category other than general basis of eligibility, but this data appears to be of lower quality, and I therefore do not perform analyses using these other breakdowns.
[77] States electronically submit Form CMS-64, which includes information about total expenditures and detailed breakdowns of spending by type, as a part of the federal reimbursement process. CMS makes these forms available for public access, though they require cleaning for use in regression analysis.
[78] Form CMS-64 provides detailed breakdowns of spending by type. Due to the increasing prevalence of managed care in administering Medicaid during the sample period and the lack of spending breakdowns within managed care in this data, it is difficult to examine effects on specific types of Medicaid expenditures. I focus on total spending instead.
[79] Figures C4 and C5 in Appendix C display variation in Medicaid spending per capita and Medicaid spending per enrollee, respectively, between states over time. Figure C6 illustrates trends over time in national average Medicaid spending per capita and per enrollee.

"simulated eligibility variable" or SEV, which represents the collective generosity of these policies in a state and year. I discuss these policies and the SEV in depth in Section 3.

I use information on other state characteristics in various contexts. I compile information on state Medicaid income eligibility limits as a percentage of the federal poverty level for four groups: children, parents, pregnant women, and childless non-disabled. The primary source of this information is a dataset on state eligibility limits over time compiled by the Kaiser Family Foundation (2019).[80] I use demographic information from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System in conjunction with population data to construct estimates of the percentage of the population in each year that is living in a rural area, black, Hispanic, age 17 or younger, age 60 or older, married, foreign-born, or educated with a bachelor's degree or higher (Manson et al. 2019).[81] Other robustness checks use annual state-level information from a variety of sources.[82]

The dataset consists of 48 jurisdictions (47 states and Washington, D.C.), 14 years from 1999 to 2012, and 672 state-year observations. Information about total Medicaid enrollment, subgroup Medicaid enrollment, and Medicaid eligibility limits is unavailable for some state-years, so the sample size practically varies from 613 to 668 state-years depending on regression

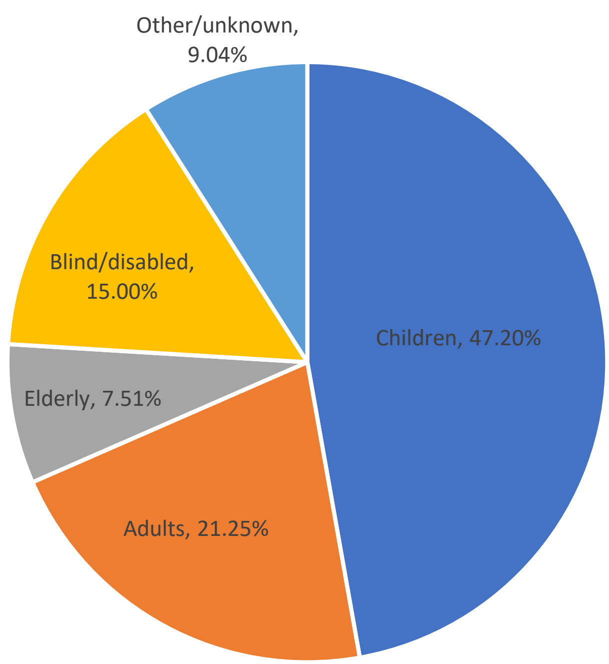
[80] The Kaiser Family Foundation (2019) provides information on state Medicaid eligibility limits for children from 2000-2019, for parents from 2002-2019, for pregnant women from 2003-2019, and for other non-disabled adults from 2011-2019. I fill in eligibility information from 1999 and later missing years using numerous state Medicaid waivers, primarily sourced from a list provided by CMS (2020). As states sometimes operate multiple Medicaid-related services for the same subgroup, I use the eligibility limits for substantial programs that would likely impact both Medicaid enrollment and spending. Because the application of these criteria can be unclear, I collect differing eligibility limits for different substantial services when they exist and consider specifications alternately using the higher of the conflicting limits, the lower of these limits, or the midpoints between these limits as controls. Baseline specifications include the midpoints between limits.

[81] Where this information is only available decennially, estimates are constructed by linear interpolation.

[82] These data include information on state unemployment rates, poverty rates, policy environments, and other social program participation information from the UKCPR's National Welfare Data (2019) as well as information on personal income and government transfers from the Bureau of Economic Analysis' Regional Economic Accounts (2019).

specification.[83] Table 13 reports the population-weighted means and standard deviations of the key variables used in the primary analyses.[84] The average SNAP participation rate is about 9.5% and the average SEV is about 16.6%. The average overall Medicaid enrollment rate is 18.8%. Figure 6 breaks down of average Medicaid enrollment over the sample period by eligibility group. On average, 47.2% of enrollees are children, 21.3% are non-disabled adults, 7.5% are seniors, and 15.0% are disabled adults. Total Medicaid spending averages $1,157 per capita or $6,330 per enrollee.

Figure 6. Medicaid enrollment by eligibility group



1999-2012 U.S. average, excluding AK, HI, and ID. Source: CMS MSIS (2017).

[83] Table C1 in Appendix C provides information about the extent of these missing variables and the limits they place on sample size for various regression specifications.
[84] Table C2 in Appendix C presents summary statistics of all variables I use in the primary analyses and robustness checks.

Table 13. Selected summary statistics

	Mean	Std. dev.	Observations
SNAP variables			
Participation rate (%)	9.519	(4.038)	672
Simulated eligibility variable (SEV) (%)	16.58	(3.934)	672
Medicaid/CHIP enrollment rate (% of total population)			
Total	18.84	(5.608)	661
Children	8.782	(2.107)	613
Adults	4.321	(2.584)	613
Elderly (age 65+)	1.396	(0.421)	613
Blind/disabled	2.812	(0.931)	613
Medicaid/CHIP spending (2010 $)			
Per capita (overall population)	1157.0	(440.0)	672
Per Medicaid enrollee	6329.9	(1947.0)	661
Medicaid eligibility limits (% of federal poverty level)			
Infants	229.9	(47.73)	668
Children aged 1-5	228.6	(49.06)	668
Children aged 6-18	228.0	(50.13)	668
Pregnant women, midpoint	198.3	(37.96)	668
Parents, midpoint	87.97	(53.54)	668
Childless non-disabled adults, midpoint	16.33	(35.78)	668
Population (unweighted)	6115908.4	(6586079.4)	672
Demographic characteristics (% of population)			
Rural	19.98	(12.16)	672
Black	12.60	(8.040)	672
Hispanic	14.76	(12.30)	672
Age 0-17	24.69	(1.805)	672
Age 60+	17.62	(2.416)	672
Married	53.42	(3.013)	672
Have bachelor's degree	26.58	(4.500)	672
Foreign-born	12.11	(7.932)	672

Statistics are weighted by state population, excluding population itself. The sample excludes Alaska and Hawaii due to different federal SNAP benefit formulas and Idaho due to Medicaid enrollment data quality issues. The sample period is 1999-2012. Medicaid enrollment and eligibility data are unavailable for some state-years at the beginning and end of the sample period, which is further detailed in Table C1 in Appendix C. Full summary statistics are available in Table C2 in Appendix C.

3. Methodology

3.1. Endogeneity of SNAP

The goal of this study is to estimate the aggregate effects of SNAP on Medicaid enrollment and spending. One approach to estimating these effects would be to estimate the fixed effects model

$$Med_{st} = \beta_0 + \beta_1 SNAP_{st} + \boldsymbol{\beta_2 X_{st}} + \boldsymbol{\beta_3 SFE_s} + \boldsymbol{\beta_4 YFE_t} + \epsilon_{st} \tag{7}$$

Here, Med_{st} represents one of the Medicaid outcomes of interest in a given state s and year t. $SNAP_{st}$ represents the SNAP participation rate. $\boldsymbol{X_{st}}$ represents a vector of covariates, while $\boldsymbol{SFE_s}$ and $\boldsymbol{YFE_t}$ represent state and year fixed effects.

Any study of the causal effects of SNAP must address the potential endogeneity of SNAP participation.[85] I have described several mechanisms through which SNAP participation may increase Medicaid enrollment and affect subsequent expenditures, but several studies find evidence that Medicaid impacts SNAP in similar ways. Estimates of β_1 from model (7) are therefore likely biased upwards. Isolating the effects of SNAP on Medicaid requires accounting for this reverse causal pathway. The ideal study would make use of randomized variation in $SNAP_{st}$ to study effects on Medicaid outcomes, but variation of this kind is not available. To address these issues, I focus instead on variation in state-level policies governing SNAP eligibility.

3.2. SNAP expansions

Under the federal SNAP rules, households are eligible if they have gross income under 130% of the FPL, net income under 100% of the FPL, and countable resources under the asset

[85] SNAP selection issues are well-documented in the literature, e.g., in the context of determining the effect of SNAP on food security (Gregory, Rabbitt, and Ribar 2016).

limit (USDA FNS 2019d).[86] Alternately, households are categorically eligible for SNAP if all household members receive Temporary Assistance for Needy Families (TANF), SSI, and/or General Assistance in some states. The benefit formula is also determined at the federal level. Each household's monthly benefit is equal to a maximum monthly allotment, which increases with household size, minus 30% of net income.

Since the enactment of welfare reform in 1996 and subsequent federal guidance, states have been given the flexibility to expand SNAP eligibility beyond the federal limits (Aussenberg and Falk 2019).[87] One option available is to alter the asset test by aligning SNAP vehicle policy with other social programs. States can increase the standard deduction applied to each vehicle's fair market value, exclude extra vehicles from the test, or eliminate vehicles from consideration. Every state has altered vehicle treatment in some way as of 2007.[88] Another option is to implement a standard medical expense deduction (SMED) that effectively reduces the net income of households with elderly or disabled members with out-of-pocket medical expenses below the deduction level.[89] 16 states have implemented SMEDs as of 2015.[90]

[86] Households with elderly (age 60 or older) or disabled members are exempt from the gross income test. Monthly net income is equal to gross income minus 20% of earned income, a standard deduction varying over time, dependent care expenses (capped in earlier years of the time period), child support expenses, out-of-pocket medical costs over $35 for elderly and disabled members, and an excess shelter deduction equal to shelter costs over half of adjusted income but no more than the upper limit. The asset limit varies by year and is higher for households with elderly or disabled members. As of 2019, the asset limit is $2,250 for households without elderly or disabled members and $3,500 for households with such members. Included in countable resources is the fair market value of owned vehicles minus a $4650 deduction per driver in the household.

[87] States are not permitted to restrict eligibility to households that are eligible under the federal rules, only expand it to those households that are ineligible under the federal rules.

[88] Figure A1 shows how states altered the treatment of vehicles over time for households without elderly or disabled members. Many states adopted less restrictive vehicle policies in the early 2000s, and most eventually moved to exclude all vehicles from the asset test.

[89] Federal SNAP rules define a person aged 60 years or more as elderly and a person receiving specific federal or state disability benefits as disabled.

[90] Figure A2 shows the 16 states that have implemented SMEDs as of 2015. Most states that implemented SMEDs did so in the late 2000s or early 2010s.

States are also able to implement "broad-based categorical eligibility" (BBCE) expansions in which they extend SNAP eligibility to households receiving certain non-cash benefits provided using TANF or maintenance-of-effort funds. States typically extend BBCE through the provision of simple benefits like brochures or referrals to telephone hotlines, making these expansions relatively inexpensive (Aussenberg and Falk 2019). Aligning SNAP eligibility to eligibility for these benefits effectively loosens or eliminates one or more of the gross income, net income, or asset tests for all or some subset of households.[91,92] From 1996 until 2015, the most common outcome of BBCE expansions was the elimination or alteration of the asset test, and the second most common outcome was a higher gross income limit. In 2015, 28 jurisdictions had expanded the gross income limit for some households without elderly or disabled members through BBCE expansions, 36 had eliminated or altered the asset test for at least some households, and 40 jurisdictions in total had implemented expansions of some type.[93]

States and their social services agencies may have several reasons to adopt the SNAP policies described here. These expansions are relatively inexpensive to states as the federal government funds SNAP benefits. They typically simplify administration, allow greater coordination between assistance programs, reduce the potential for errors in determining

[91] BBCE expansions sometimes alter the income or asset tests only for households of a certain type or alter these tests differently for households of different types, e.g. households with any elderly and/or disabled members or households with children.

[92] Despite the extension of eligibility in these ways, it is important to note that some households that are made technically eligible for SNAP cannot receive a positive benefit due to their calculated benefit, which depends on net income and household size, being at or below zero. Larger households with net incomes higher than about 100% of the federal poverty level are ineligible for a positive benefit even if they pass their state's altered gross income, net income, and asset tests. In some years of the sample period, this threshold is as high as 115%. However, smaller households of one to two members passing these tests are always eligible for a small minimum monthly benefit ranging between $10 and $16 from 1996 to 2015.

[93] Figure A3 shows the least restrictive non-elderly gross income test that may be applied to households without elderly or disabled members that are made eligible through BBCE policies. Relative to changes in vehicle asset and SMED policies over the period from 2000 to 2015, changes to the gross income test are less concentrated in timing. Adoption of more flexible gross income tests are concentrated in states in the Northeast, Upper Midwest, Southwest, and Pacific regions, with many states in the Midwest and Southeast not expanding in this way.

eligibility, and generally ease entry into SNAP for eligible households (Aussenberg and Falk 2019). It is possible states would expand SNAP in response to increasing need during economic downturns or for political reasons, but I test for these possibilities in Section 5 and find little evidence that they are driving factors.[94]

3.3 Simulated SNAP eligibility variable

States expand SNAP in the ways described above in greatly differing ways. Vehicle alterations, SMEDs, and BBCE expansions can take on very different "strengths." For instance, states could use BBCE to only increase the countable resource limit or to do away entirely with the asset test and net income test and raise the gross income test from 130% to 200% of the FPL. States frequently implement more than one type of expansion at once such that they interact with each other to determine household eligibility criteria. Some states' expansions impose different criteria for different subpopulations, e.g., households with elderly or disabled members or households with children. Due to these differences, binary indicators for whether certain types of expansions exist fail to capture the full extent of the variation in these policies.

One approach originally used to overcome issues of endogeneity between Medicaid participation and other outcomes is the construction of simulated measures of eligibility (Currie and Gruber 1996; Cutler and Gruber 1996). A simulated eligibility variable (SEV) is typically constructed as the portion of a fixed sample of people or households eligible for a program under the changing rules in place in each of several areas at different times. The sample is fixed in that it always includes the same individuals or households with the same characteristics. The only

[94] Most states do not expand SNAP to the maximum extent possible. SNAP expansions rely on alignment to other program eligibility criteria. Though the benefits these programs provide may be cheap as in the case of BBCE expansions, states must still bear the costs of providing them. Though administrative costs per case may decrease, overall administrative costs may increase if expansions greatly increase SNAP participation. Inertia or a hostile political environment may prevent some states from expanding SNAP. Further, states may expect expansions to SNAP to increase caseloads in other social programs, which would increase both financial and administrative costs.

variable factors are the changing eligibility criteria, often at the state-year-level. The use of a shared, fixed sample means that variation in the SEV derives only from changes in rules or policies, not endogenous changes in state-specific demographic or economic characteristics. Similarly, movement between states due to policy changes does not factor into the SEV's construction. The SEV therefore represents a measure of relative policy generosity that can be used to compare states over time; if it is higher in one state-year than another, that state extends eligibility to a larger portion of the common sample in that year than the other state-year.

The simulated eligibility approach is a convenient way to summarize the state rule changes I describe in a single measure. Other studies have employed simulated eligibility and benefit measures to study the effects of SNAP in various contexts (Han 2016, 2019; Leung and Seo 2019). I construct a simulated eligibility measure for use in the area-level context of this study that incorporates detailed variation in several types of state policies that affect SNAP eligibility and only counts households as eligible for SNAP if they qualify for a non-zero benefit.[95] Further, I employ this measure to estimate the impacts of expanded SNAP eligibility as well as in an instrumental variables (IV) framework in order to contextualize the reduced form estimates in terms of changes in SNAP participation tied to variation in the SEV.

I gather information from the USDA Economic Research Service's SNAP Policy Database on how states alter their BBCE and vehicle asset policies over time (2018). I gather additional details of these and other policies I require using reports from additional sources.[96] These include information such as which types of households are affected by BBCE expansions,

[95] Han's (2016, 2019) simulated eligibility measure captures variation in BBCE policies. My measure also uses variation in SMED policies and non-BBCE vehicle policies relevant to determining the eligibility of households living in states without BBCE or who are not eligible for SNAP through their state's BBCE policy. It also uses policy variation covering a longer time period. Han (2019) considers a simulated eligibility measure excluding zero-benefit households but purposefully includes these households in the baseline measure as their "technical eligibility" is relevant to their eligibility for other programs.

[96] These sources are detailed in Table A3 in Appendix A.

how many vehicles are exempted from the asset test, the size of SMEDs, and the size of allotments and standard deductions varying by household size and year. I verify these policy details and the timing of their implementation using specific state SNAP policy manuals and reports or contacting state program administrators. Specific information on these rules and their changes over time is included in Tables A1 and A2 in Appendix A.

To construct the SEV, I use a sample of households from the Survey of Income and Program Participation (SIPP) (2019) from every state and most years from 1996 to 2013.[97] The SIPP contains detailed information on household assets, income, expenses, and other characteristics necessary to determine household SNAP eligibility and benefit size. The inclusion of households from every state and many years ensures that the sample is widely representative of the United States on a national level during the sample period. To construct the SEV for a given state-year, I first adjust each household's finances for inflation to the relevant year. Then, I apply the federal and state rules in place in the given state and year to determine each households' SNAP eligibility. Since some "technically eligible" households have net income high enough to disqualify them for a positive benefit, I also calculate each eligible household's benefit according to the benefit formula in place in the relevant year. I consider only those that are also eligible for a positive benefit to be "practically eligible." I then construct SEV_{st} for the state s and year t as

$$SEV_{st} = \frac{\text{\# SIPP individuals in practically eligible households}_{\text{st}}}{\text{Total \# SIPP individuals}} \tag{8}$$

[97] The SIPP includes information on about 343,000 household-year observations composed of about 877,000 individual-year observations and covers every year from 1996 to 2013 except 2000, 2006-2008, and 2012. More information on the SIPP sample, sample exclusions, and more is included in Appendix A.

I repeat this process for each state and Washington, D.C. from 1996 to 2015. I represent SEV_{st} in percentage points, meaning it can take on values between 0 and 100. Appendix A contains an in-depth discussion on the SEV, its construction, and the policies contributing to its variation.[98]

Most of the variation in the SEV derives from BBCE expansions, especially those doing away with asset tests and/or increasing the gross income limit. Table 17, which is discussed in full in Section 4, shows how several typical expansions affect the SEV. Figure 7 illustrates interstate variation in the SEV over time. The SEV tends to increase or stay constant over time as most states only expand SNAP eligibility during the sample period, although a few states reverse expansions or change their policies such that the SEV falls. Figure 8 illustrates variation in the national average of the SEV, the average simulated federal eligibility rate – the portion of the SIPP sample that would be eligible for a positive benefit if no states expanded eligibility beyond the federal minimum – and the actual participation rate. Increases in the average value of the SEV above and beyond the simulated federal eligibility rate represent aggregate increases in SNAP policy generosity. Expansions occurred largely in two waves: vehicle test alterations and some BBCE expansions in the early 2000s and more BBCE expansions in and around the late 2000s during the Great Recession. Figure 8 also suggests a strong positive relationship between the SEV and the participation rate.

3.4 Reduced form model

To examine the impacts of SNAP eligibility expansions on Medicaid spending and enrollment, I estimate fixed effects models of the form:

$$Med_{st} = \beta_0 + \beta_1 SEV_{st} + \boldsymbol{\beta_2 X_{st}} + \boldsymbol{\beta_3 SFE_s} + \boldsymbol{\beta_4 YFE_t} + \epsilon_{st} \tag{9}$$

[98] I also construct and consider a "simulated potential benefit variable" (SPBV) representing the average monthly SNAP benefit received by households in the same common SIPP sample used to construct the SEV if every eligible household participated and received their maximum benefit. Further details are included in Appendix A.

Figure 7. Simulated SNAP eligibility variable (SEV) by state

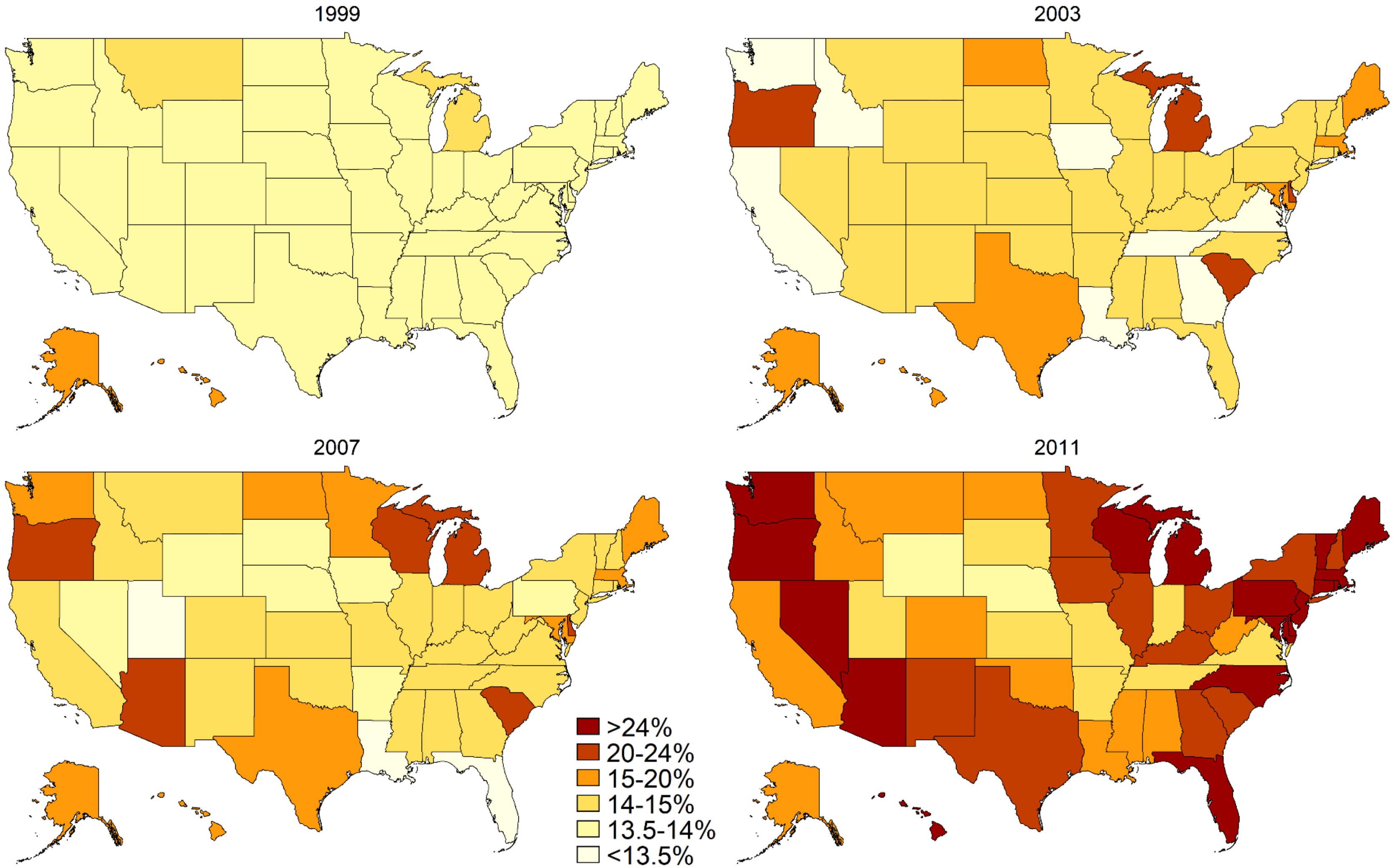
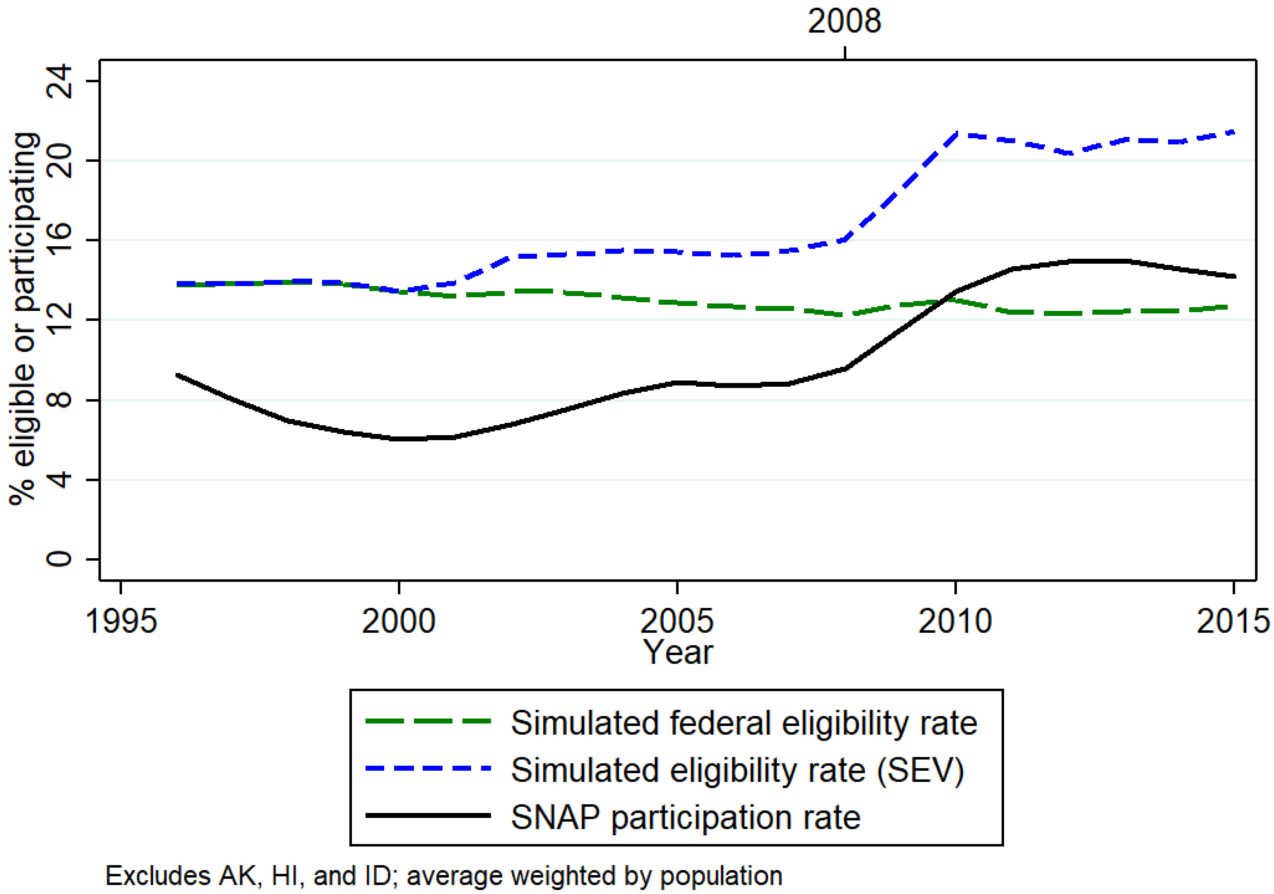


Figure 8. National simulated eligibility, simulated federal eligibility, and SNAP participation



Med_{st} represents one of the seven primary Medicaid enrollment or spending measures: total enrollment rate, child enrollment rate, adult enrollment rate, elderly enrollment rate, blind/disabled enrollment rate, spending per capita, or spending per enrollee. I also consider models using the shares of total enrollment for each of the four subgroups listed as outcomes. Baseline models include information on state-level demographic characteristics and Medicaid income eligibility limits in $\boldsymbol{X_{st}}$.[99] Baseline models also include state fixed effects $\boldsymbol{SFE_s}$ and year fixed effects $\boldsymbol{YFE_t}$ to account for time-invariant state characteristics and nationwide trends over time. Unlike in Chapter I, I opt to exclude state-specific time trends from the baseline model due to the shorter sample period.[100] Robust standard errors are clustered by state s. Results from baseline regressions are presented in Section 4. I describe and test the identification assumptions I make in detail and consider alternative models as robustness checks in Section 5.

3.5 Instrumental variables model

Reduced form estimates of β_1 from model (9) are valuable for determining if eligibility expansions impact Medicaid spending or enrollment, but it is difficult to interpret the magnitudes of these estimates. The SEV is a measure I construct solely to compare the collective generosity of SNAP policy between states and is not directly analogous to an actual eligibility rate because the SIPP oversamples low-income and low-resource households. Therefore, I estimate IV models in which the SEV instruments for the actual SNAP participation rate in order to contextualize the reduced form estimates.

I estimate the first-stage model

$$SNAP_{st} = \alpha_0 + \alpha_1 SEV_{st} + \boldsymbol{\alpha_2 X_{st}} + \boldsymbol{\alpha_3 SFE_s} + \boldsymbol{\alpha_4 YFE_t} + \varepsilon_{st} \tag{10}$$

[99] Baseline models include in $\boldsymbol{X_{st}}$ the midpoints of the upper and lower Medicaid income eligibility limits I collect (when multiple limits exist) and the percentages of the population that are living in rural areas, black, Hispanic, age 0-17, age 60+, married, educated with a bachelor's degree or higher, and foreign-born.

[100] I consider alternate models including state-specific time trends in Section 5.

to obtain $\widehat{SNAP}_{st}$, the predicted values of the participation rate $SNAP_{st}$. Baseline models include the same controls and fixed effects as model (9). Using $\widehat{SNAP}_{st}$, I then estimate second-stage models of the form:

$$Med_{st} = \beta_0 + \beta_1\widehat{SNAP}_{st} + \boldsymbol{\beta_2 X_{st}} + \boldsymbol{\beta_3 SFE_s} + \boldsymbol{\beta_4 YFE_t} + \epsilon_{st} \quad (11)$$

These models are structured the same and include the same controls as model (9) but with $\widehat{SNAP}_{st}$ in place of SEV_{st}.

I present first-stage and second-stage IV results in Section 4. I also describe the assumptions required for identification of β_1 and challenges to IV identification in Section 5.

4. Results

4.1. Hypotheses

I hypothesize that SNAP eligibility expansions – as represented by an increase in the SEV – would increase aggregate Medicaid enrollment, increase Medicaid spending per capita, and decrease Medicaid spending per enrollee.

SNAP expansions are likely to increase SNAP participation, which would also likely increase Medicaid enrollment. As described in Section 1, SNAP participation likely decreases the marginal costs of applying for and participating in Medicaid such that it may encourage new SNAP recipient households to enroll eligible members, especially among children and adults who participate in SNAP at higher rates than the elderly (Haider, Jacknowitz, and Schoeni 2003).[101] Though SNAP may improve health, existing studies do not indicate health benefits large and immediate enough to reduce the benefits that SNAP recipients would receive from

[101] SNAP participation is likely to reduce the marginal time, travel, and stigma costs of application and recertification for Medicaid, especially when these programs are jointly administered. It may also raise awareness of Medicaid eligibility and make the application process less confusing.

health insurance through Medicaid (Bitler 2016). SNAP expansions may also increase Medicaid enrollment through channels other than SNAP receipt, e.g., through raising general awareness of social programs. Therefore, there is little reason that SNAP expansions and subsequent increases in SNAP participation would decrease Medicaid enrollment, all else equal.

However, SNAP expansion-induced Medicaid enrollment increases are not likely to translate to large short-term increases in healthcare utilization or spending for two reasons. First, new Medicaid enrollees entering the program due to a SNAP expansion likely have lower need for healthcare than the average Medicaid enrollee. If someone eligible for Medicaid did have higher need and would therefore utilize more care once enrolled – e.g., due to poor health – it is more likely that they would have already enrolled in Medicaid as the benefits of enrolling, or the costs of not enrolling, would be higher. In other words, SNAP expansions may encourage lower-risk individuals to enter a previously higher-risk Medicaid risk pool, alleviating adverse selection. This would lead to an increase in total healthcare utilization but a decrease in average utilization and subsequently increase Medicaid spending per capita but decrease Medicaid spending per enrollee.

Second, new Medicaid enrollees entering the program due to a SNAP expansion may face information and access barriers to using their Medicaid benefits that those enrolled for several years have overcome. This would lead to lower healthcare utilization among these new Medicaid enrollees relative to pre-existing enrollees. As described above, this would also lead to an increase in total utilization but decrease average utilization and subsequently increase Medicaid spending per capita but decrease spending per enrollee, at least in the short term.

4.2. Primary results

Table 14 reports the results from regressions of the primary Medicaid enrollment and spending outcomes on the SEV, including the overall Medicaid enrollment rate; the child, non-disabled adult, senior, and blind/disabled enrollment rates; total Medicaid spending per capita; and total Medicaid spending per Medicaid enrollee. The SEV is expressed in percentage points and can range from 0 to 100. Sample size varies between these regressions due to missing observations for some variables in some state-years.[102]

I find evidence that SNAP eligibility expansions increase the overall Medicaid enrollment rate. Specifically, a one percentage point increase in the SEV – roughly 6.0% of the mean value of 16.58% – increases the Medicaid enrollment rate by about 0.13 percentage points, or about 0.7% of the mean Medicaid enrollment rate. This effect appears to be driven primarily by increases in adult and child enrollment. A one percentage point increase in the SEV increases these subgroup's enrollment as a percentage of total population by 0.060 percentage points (1.4% of the mean) and 0.033 percentage points (0.4% of the mean), respectively. I find no evidence of corresponding increases in elderly or blind/disabled Medicaid enrollment. I find evidence that SNAP expansions decrease Medicaid spending per Medicaid enrollee. A one percentage point increase in the SEV decreases spending per enrollee by about $58, or about 0.9% of the mean spending per enrollee of $6,330. Contrary to expectations, I do not find evidence that these expansions affect Medicaid spending per capita. Together, these estimates suggest that "SNAP expansion-induced" Medicaid enrollees do not utilize many covered medical services – at least not shortly after enrollment. If they did incur similar expenses to those already enrolled in Medicaid, spending per capita should increase.

[102] Table C1 in Appendix C provides information about how missing Medicaid outcomes and eligibility information affect the sample size.

Table 14. Medicaid enrollment and spending outcome regression results

	Medicaid enrollment rate (% of total population)					Medicaid total spending (2010 $)	
	Overall	Children	Adults	Elderly	Blind/disabled	Per capita	Per enrollee
SNAP SEV	0.132***	0.0329**	0.0601**	0.00764	0.00535	-1.429	-58.07***
	(0.0465)	(0.0163)	(0.0280)	(0.00501)	(0.00599)	(2.623)	(17.40)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Medicaid eligibility limit controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	18.84	8.782	4.321	1.396	2.812	1157.0	6329.9
Mean SEV	16.58	16.58	16.58	16.58	16.58	16.58	16.58
R^2	0.765	0.808	0.634	0.291	0.688	0.826	0.299
Observations	657	610	610	610	610	668	657

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. Medicaid enrollment rates represent the number of enrollees as a percentage of the overall population and are expressed in percentage points. Medicaid spending per capita or per Medicaid enrollee is expressed in 2010-adjusted dollars.

Table 15 reports abbreviated results from the first-stage regression of the SNAP participation rate on the SEV. The SEV is strongly positively correlated with the SNAP participation rate. Like the SEV, the SNAP participation rate is expressed in percentage points and can range from 0 to 100. A one percentage point increase in the SEV increases the state SNAP participation rate by 0.162 percentage points on average, or about 1.7% of the mean participation rate of 9.5%. The first-stage F-statistic of 25.4 indicates that the SEV is well-powered to instrument for SNAP participation.[103] I compare the results of first-stage regressions using the SEV and other policy instruments in Table C3 in Appendix C. The SEV I construct outperforms a variety of SNAP policy instruments used in other IV studies.

Table 15. First-stage regression results

	SNAP participation rate
SNAP SEV	0.162***
	(0.0322)
Demographic controls	Yes
Medicaid eligibility limits	Yes
State and year FE	Yes
Mean SNAP part. rate	9.519
Mean SEV	16.58
First-stage F-statistic	25.40
R^2	0.942
Observations	668

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The SNAP participation rate and the simulated SNAP eligibility variable (SEV) are expressed in percentage points. The participation rate indicates the actual percentage of the population belonging to a household that receives SNAP benefits, and the SEV indicates the percentage of the SIPP sample belonging to an eligible household when each state-year's SNAP eligibility rules are applied.

[103] The F-statistic exceeds the critical value of 16.4 to limit the maximum Wald test size distortion to 0.10 at the 5% significance level (Stock and Yogo 2005).

Table 16 reports the second-stage IV results of regressions of the primary Medicaid enrollment and spending outcomes on the predicted SNAP participation rate from the first stage.

As the IV model is just-identified, these estimates are proportional in magnitude to those reported in Table 14. Taken with those results, the IV results in Table 16 provide context for how changes in the SEV affect Medicaid enrollment and spending through SNAP expansions' effects on the participation rate. Table 16 also shows results from the naïve regressions of these same outcomes on the SNAP participation rate.

A one percentage point increase in the predicted SNAP participation rate – roughly 10.5% of the mean rate of 9.5% – is associated with a 0.89 percentage point increase in the overall Medicaid enrollment rate (4.7% of the mean), a 0.45 percentage point increase in the adult Medicaid enrollment rate (10.3% of the mean), and a 0.24 percentage point increase in the child Medicaid enrollment rate (2.8% of the mean). Similarly, a one percentage point increase in the predicted SNAP participation rate is associated with a decrease of $391 in Medicaid spending per enrollee. The estimated coefficients, particularly those on the various enrollment rates, are implausibly large. The estimated coefficient of 0.89 on the overall Medicaid enrollment rate implies that nearly 90% of individuals that begin participating in SNAP due to eligibility expansions would also newly enroll in Medicaid. This is unlikely for several reasons. Some new SNAP households would already have members enrolled in Medicaid and be unable to enroll them again. New SNAP participants would also frequently be ineligible for Medicaid as Medicaid income eligibility criteria were typically more restrictive than SNAP income eligibility criteria during the sample period for the adults driving the change in Medicaid enrollment. Even if all new SNAP participants were eligible for Medicaid, this implied take-up rate seems implausibly high, though not impossible.

Table 16. Medicaid enrollment and spending outcome second-stage and naïve regression results

	Medicaid enrollment rate (% of total population)					Medicaid total spending (2010 $)	
	Overall	Children	Adults	Elderly	Blind/disabled	Per capita	Per enrollee
Instrumental variables second stage							
Predicted SNAP part. rate	0.891***	0.244**	0.446*	0.0567	0.0397	-8.811	-391.3***
	(0.305)	(0.102)	(0.235)	(0.0416)	(0.0427)	(16.48)	(139.7)
R^2	0.755	0.823	0.580	0.261	0.679	0.826	0.175
Naive fixed effects							
SNAP part. rate	0.435***	0.211***	0.137**	0.0144	0.00309	-4.479	-132.1***
	(0.110)	(0.0515)	(0.0608)	(0.0120)	(0.0120)	(5.549)	(31.21)
Mean of dep. var.	18.84	8.782	4.321	1.396	2.812	1157.0	6329.9
Mean SNAP part. rate	9.519	9.519	9.519	9.519	9.519	9.519	9.519
R^2	0.772	0.823	0.632	0.288	0.687	0.826	0.294
Observations	657	610	610	610	610	668	657

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The predicted SNAP participation rate from the first stage and the actual SNAP participation rate are expressed in percentage points. Medicaid enrollment rates represent the number of enrollees as a percentage of the overall population and are expressed in percentage points. Medicaid spending per capita or per Medicaid enrollee is expressed in 2010-adjusted dollars.

I propose two potential explanations. First, SNAP expansions may not only make it less costly for participants to apply for Medicaid but may also raise awareness of social program eligibility or decrease the stigma of social program participation generally among participants' social networks. While SNAP eligibility can rely on household resources and other factors, Medicaid eligibility generally relies on income, so part of these increases in Medicaid take-up could be attributable to SNAP-ineligible individuals. Social network effects such as these may amplify the impacts of SNAP participation on Medicaid enrollment. Second, SNAP expansions themselves may drive significant increases in Medicaid take-up through channels other than SNAP participation. For this reason, I rely primarily on the estimates from Table 14 as measures of the causal impacts of SNAP expansions on Medicaid enrollment and spending.[104]

The estimates from the naïve regressions are similar in sign and statistical significance to the IV estimates but are uniformly smaller in magnitude. In Section 3, I describe reverse causality as an important and likely source of upward bias in these estimates, as Medicaid enrollment is likely to also increase SNAP participation. I do not rule out mechanisms other than changes in SNAP participation through which SNAP eligibility expansions may influence the Medicaid outcomes, so I therefore refrain from drawing conclusions from comparisons between the IV and naïve estimates. However, if the naïve estimates are biased upwards, they would likely represent upper bounds of the true impacts of SNAP participation on Medicaid enrollment or a lower bound of the impact on Medicaid spending per enrollee.

4.3. Other results

Table 17 outlines how several common state SNAP expansions increase the value of the SEV, increase the estimated SNAP participation rate, and affect the estimated overall Medicaid

[104] I discuss the assumptions required for identification in the reduced form and IV models further in Section 5.

Table 17. Impacts of common state policies altering SNAP eligibility

Policy	Mean SEV (%)	Increase over baseline (% points)	Est. increase in SNAP part. rate (% points)	Est. increase in Medicaid enrollment rate (% points)	Est. decrease in Medicaid spending per Medicaid enrollee (2010 $)
Baseline: no state rule change (federal minimum eligibility)	13.04	-	-	-	
BBCE: Eliminate asset and net income tests; gross income test of:					
130% FPL	17.08	4.04	0.65	0.53	-234.60
165% FPL	21.24	8.20	1.33	1.08	-476.17
185% FPL	22.77	9.73	1.58	1.28	-565.02
200% FPL	23.82	10.78	1.75	1.42	-625.99
BBCE: Eliminate asset test; net income test of 100% FPL; gross income test of:					
130% FPL	16.65	3.61	0.58	0.48	-209.63
165% FPL	18.31	5.27	0.85	0.70	-306.03
185% FPL	18.55	5.51	0.89	0.73	-319.97
200% FPL	18.63	5.59	0.91	0.74	-324.61
BBCE: Eliminate asset and net income tests; gross income test of:					
200% FPL for households with children	18.05	5.01	0.81	0.66	-290.93
200% FPL for households with elderly or disabled members; 130% FPL for others	20.74	7.70	1.25	1.02	-447.14
200% FPL for households with elderly or disabled members	18.56	5.52	0.89	0.73	-320.55
SMED of:					
$100	13.11	0.07	0.01	0.01	-4.06
$200	13.25	0.21	0.03	0.03	-12.19
Vehicles: Exclude:					
One per household	14.33	1.29	0.21	0.17	-74.91
One per adult	14.54	1.50	0.24	0.20	-87.11
All	14.59	1.55	0.25	0.20	-90.01

SEV is calculated separately as if denoted policy were applied in each year of the sample in a state in the contiguous United States. Mean SEV represents the cross-year average of the SEV for the sample period 1996-2015. Estimated increase in SNAP participation rate assumes that a one percentage point increase in the SEV increases the SNAP participation rate by 0.162 percentage points as estimated in Table 15. Estimated changes in overall Medicaid enrollment rate and Medicaid spending per enrollee assume that a one percentage point increase in the SEV increases the Medicaid enrollment rate by 0.132 percentage points or reduces Medicaid spending per enrollee by $58.07 as estimated in Table 14.

enrollment rate and spending per Medicaid enrollee. I present a common BBCE expansion as an example: eliminating the asset and net income tests and imposing a gross income test of 130% FPL. This expansion increases the value of the SEV by about 4.04 percentage points, about 24.4% of its mean value or 31.0% of its baseline value when no state expansions are in place. Applying the estimates from Tables 15 and 14, I estimate that this expansion would increase the SNAP participation rate by about 0.65 percentage points and increase the Medicaid enrollment rate by about 0.53 percentage points. In a state of 5 million people, this expansion would predict 26,500 new Medicaid enrollees, and Medicaid spending per enrollee would fall by about $235.

Table 18 presents the results of regressions using as outcomes the shares of total Medicaid enrollment belonging to each of four groups divided by basis of eligibility: children, non-disabled adults, seniors, and blind/disabled adults. As Figure 6 shows, most Medicaid enrollees according to the MSIS enrollment data are children (47.2%), followed by non-disabled adults (21.3%), disabled adults (15.0%), and seniors (7.5%). On average, a one percentage point increase in the SEV increases the proportion of Medicaid enrollment attributable to non-disabled adults by about 0.15 percentage points (0.7% of the average adult share of enrollment) and reduces the proportion of the other three groups in the sample. This reduction in enrollment share is especially large for disabled adults at about 0.08 percentage points (0.6% of the average disabled adult share of enrollment). Relative to non-disabled adults and children, seniors and disabled adults who are often dually eligible for Medicare and Medicaid use much more healthcare and cost much more to cover than the average Medicaid enrollee.[105] The introduction of more non-disabled adults to Medicaid risk pools and the subsequent shifts in enrollment

[105] For example, in fiscal year 2013, dual-eligibles made up 15% of Medicaid enrollees and 61% of aged or disabled Medicaid enrollees while they accounted for 35% of Medicaid spending (Kaiser Family Foundation 2020).

Table 18. Medicaid enrollment share regression results

	Group share of total Medicaid enrollment (%)			
	Children	Adults	Elderly	Blind/disabled
SNAP SEV	-0.118	0.149*	-0.00303	-0.0831**
	(0.0911)	(0.0892)	(0.0254)	(0.0375)
Demographic controls	Yes	Yes	Yes	Yes
Medicaid eligibility limits	Yes	Yes	Yes	Yes
State and year FE	Yes	Yes	Yes	Yes
% of total enrollment	47.20	21.25	7.509	15.00
Mean SEV	16.58	16.58	16.58	16.58
R^2	0.229	0.438	0.471	0.323
Observations	610	610	610	610

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The SNAP SEV is expressed in percentage points. Group shares represent the percentage of total Medicaid enrollees belonging each eligibility group and are expressed in percentage points.

composition are a likely mechanism through which SNAP expansions decrease average Medicaid spending per enrollee.

5. Robustness checks

5.1. Internal validity

Consistent identification of β_1 in model (9) relies upon several assumptions. SEV_{st} should be independent of the error term ϵ_{st}. It must not be the case that some unobserved third factor impacts both state SNAP policy and the Medicaid outcomes of interest or that the Medicaid outcomes directly impact state SNAP policy. I cannot formally test these assumptions, but I consider several ways they may not hold in turn.

Economic downturns tend to increase participation in both SNAP and Medicaid since both programs are means-tested and may make states more likely to expand both programs to address expanding need. Economic factors may therefore drive a positive relationship between Medicaid enrollment, Medicaid spending, and SNAP generosity. Additionally, states that expand SNAP to a greater degree may be more likely to adopt other policies increasing Medicaid enrollment and spending.[106] They may also administer other social programs more generously, which could impact Medicaid enrollment much like SNAP does. To investigate these possibilities, I estimate regressions of the primary Medicaid outcomes on the SEV including additional sets of controls describing the state economic environment, the policy environment, or participation rates in other social programs in Table 19.[107] I find that the primary findings are robust to the inclusion of any of these three sets of controls.

Additionally, I examine whether economic factors, the policy environment, or state-level Medicaid eligibility expansions predict the adoption of policies that determine the SEV in Tables 20, 21, and 22, respectively. These tables report the results of linear probability models in which the outcomes are the presence of a BBCE or vehicle test alteration policy. I find little evidence pointing to a strong relationship between economic conditions and state SNAP expansions. States appear to be slightly more likely to implement BBCE expansions in response to higher unemployment or vehicle test alterations in response to higher poverty, but not significantly so. Similarly, I find little evidence that the policy environment variables I consider consistently predict state SNAP expansions. I therefore do not consider the exclusion of either of these control sets from the baseline models problematic. In Table 22, I find some evidence that

[106] This concern is the primary motivation for including Medicaid eligibility limit controls in the baseline models. However, these controls may not fully account for other policies that could impact the Medicaid outcomes.
[107] Economic controls include the unemployment rate, the poverty rate, the natural log of personal income per capita, and the natural log of non-SNAP government transfers per capita. Policy environment controls include a dummy for the governor being a Democrat, the percentage of the state house that are Democrats, and the percentage of the state senate that are Democrats. Other social welfare program participation controls include participation rates for TANF and SSI. I include these participation rates and exclude other program participation rates as states have more discretion over the administration of these programs. Other social program participation rates are endogenous in similar ways to the SNAP participation rate, but I do not take up that issue here.

Table 19. Medicaid enrollment and spending outcome regressions including additional control sets

	Medicaid enrollment rate (% of total population)					Medicaid total spending (2010 $)	
	Overall	Children	Adults	Elderly	Blind/disabled	Per capita	Per enrollee
Including economic controls							
SNAP SEV	0.133***	0.0317**	0.0582**	0.00667	0.00586	0.226	-48.40***
	(0.0431)	(0.0154)	(0.0290)	(0.00507)	(0.00559)	(2.508)	(15.84)
R^2	0.777	0.823	0.638	0.299	0.695	0.836	0.351
Observations	657	610	610	610	610	668	657
Including state government controls							
SNAP SEV	0.128***	0.0355**	0.0553*	0.00644	0.00380	-2.832	-60.04***
	(0.0486)	(0.0180)	(0.0284)	(0.00525)	(0.00583)	(2.565)	(17.48)
R^2	0.766	0.815	0.639	0.295	0.688	0.832	0.312
Observations	630	585	585	585	585	640	630
Including other social welfare program participation controls							
SNAP SEV	0.130***	0.0264*	0.0621**	0.00681	0.00286	-1.569	-57.96***
	(0.0459)	(0.0147)	(0.0275)	(0.00499)	(0.00533)	(2.580)	(17.30)
Mean of dep. var.	18.84	8.782	4.321	1.396	2.812	1157.0	6329.9
Mean SEV	16.58	16.58	16.58	16.58	16.58	16.58	16.58
R^2	0.776	0.833	0.645	0.307	0.740	0.828	0.316
Observations	657	610	610	610	610	668	657

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The SNAP SEV is expressed in percentage points. Medicaid enrollment rates represent the number of enrollees as a percentage of the overall population and are expressed in percentage points. Medicaid spending per capita or per Medicaid enrollee is expressed in 2010-adjusted dollars. Economic controls include the unemployment rate, the poverty rate, the natural log of personal income per capita, and the natural log of non-SNAP government transfers per capita. State government controls include a dummy for the governor being a Democrat, the percentage of the state house that are Democrats, and the percentage of the state senate that are Democrats. Other social welfare program participation controls include state participation rates for TANF and SSI in percentage points; I include participation rates for these social programs and not others as states have more discretion over these programs' administration.

Table 20. Regressions of SEV-determining policies on economic characteristics

	BBCE	Vehicle test alteration
Unemployment rate	0.0208	0.00764
	(0.0218)	(0.0145)
t-1	0.0235	-0.00388
	(0.0216)	(0.0105)
t-2	0.00962	-0.000382
	(0.0186)	(0.0126)
t-3	0.000968	0.00185
	(0.0251)	(0.0158)
Poverty rate	0.000446	0.00223
	(0.0116)	(0.00618)
t-1	-0.00424	0.00640
	(0.00947)	(0.00537)
t-2	0.00671	0.0101**
	(0.00865)	(0.00495)
t-3	0.00143	0.0140**
	(0.00851)	(0.00595)
Ln personal income per capita	-0.809	-0.0131
	(0.545)	(0.420)
t-1	0.526	0.351
	(0.519)	(0.415)
t-2	0.0320	0.509*
	(0.550)	(0.287)
t-3	0.785	-0.747*
	(0.756)	(0.429)
Demographic controls	Yes	Yes
State and year FE	Yes	Yes
Mean of SEV-determining policy	0.359	0.784
Mean unemployment rate	6.244	6.244
Mean poverty rate	13.08	13.08
Mean ln(Personal income per capita)	10.43	10.43
R^2	0.552	0.793
Observations	672	672

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls and year and state fixed effects. Regressions are not weighted by population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. "BBCE" and "Vehicle test alteration" are dummies indicating whether each state has adopted these policies in some form. Unemployment rate and poverty rate are expressed in percentage points. Log of real personal income per capita is included. Each regression includes lags from the previous three periods.

Table 21. Regressions of SEV-determining policies on policy environment characteristics

	BBCE	Vehicle test alteration
Governor Democrat	-0.00748	-0.0321*
	(0.0379)	(0.0177)
t-1	-0.00295	0.0271
	(0.0120)	(0.0178)
t-2	0.0149	-0.0277
	(0.0266)	(0.0197)
t-3	0.00532	-0.0307
	(0.0382)	(0.0248)
% state house Democrats	0.0000808	-0.00302
	(0.00420)	(0.00258)
t-1	0.000511	-0.00428*
	(0.00155)	(0.00234)
t-2	0.00000634	-0.00141
	(0.00311)	(0.00160)
t-3	0.000815	0.00390
	(0.00418)	(0.00281)
% state senate Democrats	0.00120	0.000210
	(0.00335)	(0.00245)
t-1	0.00180	0.00188
	(0.00137)	(0.00171)
t-2	-0.00113	0.00258
	(0.00249)	(0.00222)
t-3	-0.00227	-0.00222
	(0.00286)	(0.00248)
Demographic controls	Yes	Yes
State and year FE	Yes	Yes
Mean of SEV-determining policy	0.359	0.784
Mean Democratic governor	0.476	0.476
Mean % Democrats in house	52.91	52.91
Mean % Democrats in senate	49.80	49.80
R^2	0.542	0.798
Observations	644	644

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls and year and state fixed effects. Regressions are not weighted by population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. “BBCE” and “Vehicle test alteration” are dummies indicating whether each state has adopted these policies in some form. “Governor Democrat” is a dummy variable equal to one if the governor is a Democrat. The percentage of each state house and senate that are Democrats are expressed in percentage points. Each regression includes lags from the previous three periods.

Table 22. Regressions of SEV-determining policies on Medicaid eligibility limits

	BBCE	Vehicle test alteration
Infants	0.00796*	0.000553
	(0.00446)	(0.00180)
t-1	-0.000378	-0.00146
	(0.00323)	(0.00187)
t-2	0.00196	0.00224
	(0.00222)	(0.00186)
t-3	-0.000927	-0.000117
	(0.00152)	(0.00199)
Children aged 1-5	-0.00715	-0.000260
	(0.00465)	(0.00190)
t-1	-0.00384	-0.000672
	(0.00470)	(0.00274)
t-2	0.000150	-0.00117
	(0.00281)	(0.00216)
t-3	0.000307	-0.000632
	(0.00267)	(0.00209)
Children aged 6-18	0.000863	-0.000396
	(0.000964)	(0.000699)
t-1	0.00347	0.00149
	(0.00254)	(0.00142)
t-2	-0.00116	-0.000473
	(0.00150)	(0.00127)
t-3	-0.000491	0.00159
	(0.00219)	(0.00165)
Pregnant women, midpoint	-0.00200**	0.00122
	(0.000922)	(0.000829)
t-1	0.00285**	0.0000241
	(0.00119)	(0.000584)
t-2	-0.000749	-0.000973
	(0.000679)	(0.000888)
t-3	-0.000480	-0.000100
	(0.00136)	(0.000576)
Parents, midpoint	-0.00356***	0.000515
	(0.000920)	(0.00130)
t-1	0.00450***	-0.00118
	(0.00102)	(0.00191)
t-2	-0.00304***	0.0000380
	(0.00105)	(0.00101)
t-3	0.00321***	0.000430
	(0.00119)	(0.000596)
Childless non-disabled adults, midpoint	0.00383***	-0.00168*
	(0.000988)	(0.000930)
t-1	-0.00164*	0.00107
	(0.000859)	(0.000781)
t-2	-0.000712	0.00111*
	(0.000756)	(0.000616)
t-3	-0.00136	-0.000546
	(0.00139)	(0.000494)
R^2	0.571	0.745
Observations	618	618

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. All regressions include demographic controls and year and state fixed effects. Regressions are not weighted by population. The simulated SNAP eligibility variable (SEV) is expressed in percentage points. "BBCE" and "Vehicle test alteration" are dummies indicating whether each state has adopted these policies in some form. Medicaid eligibility limits are shown for various eligibility groups and are expressed as the percentage of the federal poverty level. Each regression includes lags from the previous three periods.

Medicaid eligibility expansions for groups other than children affect the likelihood a state implements a BBCE expansion, but the mixed estimates do not point to a consistent positive or negative influence of these state expansions.

Along the same lines, Medicaid generosity may influence state's decisions to expand SNAP. States substitute between social programs (Marton and Wildasin 2007). Though SNAP benefits are financed at the federal level, states face administration costs and therefore may substitute to some degree between SNAP and Medicaid. In this way, Medicaid generosity – and subsequent increases in enrollment and spending – may impact the SEV. I examine whether changes in Medicaid outcomes follow or precede SNAP expansions by estimating regressions in which the outcome is modeled as a function of the SEV as well as several lags and leads of the SEV in Table 23. If changes in Medicaid outcomes precede SNAP expansions, reverse causality of this type may be an issue for estimation.[108] This is not the case, as I find no evidence of relationships between these outcomes and future values of the SEV.[109]

Consistent identification of β_1 in the IV model (11) additionally relies upon several other assumptions. First, the simulated eligibility instrument SEV_{st} must have a clear, strong effect on the participation rate $SNAP_{ct}$ in the first-stage model (10). Variation in the SEV derives from state-level policies altering the portion of SIPP households eligible for SNAP. An increase in the SEV implies more generous policy and means that more real households in the state become eligible to receive benefits, all else equal. If additional households would participate when made eligible or when income or asset tests are relaxed, the SEV would be positively correlated with

[108] Autocorrelation is certainly a problem in models like these with several leads and lags of the SEV. Therefore, I do not expect this model to present precise estimates of the SEV's impact, but rather to test generally for the potential for reverse causality.
[109] Examining models including lags and leads of the SEV is also interesting because it is not theoretically clear how long it would take SNAP expansions to impact Medicaid spending and enrollment. Table 23 suggests that the full impacts of SNAP expansions on Medicaid enrollment and spending are not instantaneous and may take several years to materialize.

Table 23. Medicaid enrollment and spending outcome regressions including lags and leads of SEV

	Medicaid enrollment rate (% of total population)					Medicaid total spending (2010 $)	
	Overall	Children	Adults	Elderly	Blind/disabled	Per capita	Per enrollee
SNAP SEV							
t-3	0.0932	-0.00198	0.0220	0.00606	0.00194	0.528	-32.55
	(0.0749)	(0.0267)	(0.0306)	(0.00386)	(0.00659)	(3.600)	(25.77)
t-2	0.128***	0.0201	0.0216	0.00594	0.0109**	2.069	-27.58
	(0.0484)	(0.0148)	(0.0174)	(0.00367)	(0.00525)	(2.417)	(18.70)
t-1	0.0639*	0.0110	0.0354	0.00645**	0.00793*	-4.954*	-48.52***
	(0.0340)	(0.0106)	(0.0225)	(0.00298)	(0.00431)	(2.902)	(18.24)
t	0.0632*	0.0222**	0.0151	0.000594	-0.00131	3.019	-7.072
	(0.0357)	(0.0103)	(0.0130)	(0.00218)	(0.00356)	(2.461)	(10.40)
t+1	-0.0376	0.00610	0.0198	0.00180	0.00348	-4.409**	-12.52
	(0.0430)	(0.0131)	(0.0140)	(0.00403)	(0.00433)	(1.808)	(15.60)
t+2	0.0394	-0.00701	0.0185	0.00494	-0.00711*	0.687	-17.62
	(0.0390)	(0.00933)	(0.0121)	(0.00354)	(0.00424)	(1.746)	(16.42)
t+3	-0.0200	-0.0175	0.0203	-0.00996	-0.00400	3.704*	14.04
	(0.0419)	(0.0181)	(0.0159)	(0.00706)	(0.00566)	(2.086)	(12.68)
R^2	0.781	0.811	0.646	0.312	0.700	0.831	0.347
Baseline: SNAP SEV, t	0.132***	0.0329**	0.0601**	0.00764	0.00535	-1.429	-58.07***
	(0.0465)	(0.0163)	(0.0280)	(0.00501)	(0.00599)	(2.623)	(17.40)
Mean of dep. var.	18.84	8.782	4.321	1.396	2.812	1157.0	6329.9
Mean SEV	16.58	16.58	16.58	16.58	16.58	16.58	16.58
R^2	0.765	0.808	0.634	0.291	0.688	0.826	0.299
Observations	657	610	610	610	610	668	657

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. The sample period is restricted due to the inclusion of leads and lags. The simulated SNAP eligibility variable (SEV) are expressed in percentage points. Medicaid enrollment rates represent the number of enrollees as a percentage of the overall population and are expressed in percentage points. Medicaid spending per capita or per Medicaid enrollee is expressed in 2010-adjusted dollars. In the first panel, regressions include lags and leads of the SEV centered around the current-period SEV. In the second panel, regressions include only the current-period SEV as in the baseline model.

the real SNAP participation rate. I find that this is the case and show the first-stage results in Table 15.

Second, SEV_{st} should not affect the Medicaid outcomes Med_{st} except through effects on $SNAP_{st}$. I argue that increased SNAP participation is the primary mechanism through which SNAP eligibility expansions would increase Medicaid enrollment and subsequent Medicaid spending, but it is possible that the expansions themselves may increase Medicaid enrollment, even if new enrollees are not SNAP recipients. For example, SNAP expansions may raise general awareness of the availability of Medicaid, even among households not participating in SNAP. It is possible then that this assumption is not met, and I accordingly treat the IV estimates as illustrative instead of causal.

5.2. External validity

Variation in the SEV derives from changes in state eligibility rules beyond the federal minimum. The SIPP households these rules are applied to can be categorized in one of three mutually exclusive and exhaustive groups: always eligible for SNAP (by meeting the federal rules), never eligible for SNAP (by meeting the federal rules in every year), never eligible for SNAP (by never meeting the federal rules or any state's rules in any year), or sometimes but not always eligible for SNAP (by meeting the federal rules or some state's rules in some years). The last group represents those households whose SNAP eligibility and subsequent participation can be "turned on" by the state eligibility expansions summarized in the SEV. The effects shown in Section 4 can therefore be interpreted as local average treatment effects in that they represent changes in Medicaid enrollment and spending in response to changes in SNAP eligibility and participation among sometimes-eligible households.[110] This interpretation requires monotonicity: increases in the SEV should not cause households to lose eligibility for or not participate in SNAP who otherwise would. This is reasonable, as it would be unusual for households previously receiving SNAP benefits to stop because eligibility is expanded.

This raises the issue of external validity of the results: would Medicaid enrollment and spending change in the same way if the SEV represented the expansion of SNAP eligibility to always-eligible households (e.g., if SNAP participation was prohibitively costly in terms of time or stigma costs then made less so)? This question could be addressed by using policy instruments directly impacting these costs among always-eligible households, but the available instruments of this type are too weak for use in the context of this study.[111] I examine the characteristics of the SIPP households in each eligibility category in Table A5 in Appendix A. Relative to sometimes-eligible households, always-eligible households have on average roughly half of the total income, a third of the earned income, a hundredth of the countable non-vehicle assets, and a fourth of the vehicle equity.[112]

Given these differences, expansions of SNAP among these populations would likely have different impacts on aggregate Medicaid enrollment and spending. Always-eligible individuals would tend to be more frequently eligible for Medicaid due to their lower income but are also more likely to have already enrolled in Medicaid. SNAP expansions affecting always-eligible populations might therefore increase Medicaid enrollment and associated increases in spending

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[110] Increases in the SEV may increase participation among households that are "always eligible" if it becomes less costly for these households to apply due to the expansions summarized in the SEV (e.g., they no longer must report detailed information on vehicles or other assets). However, I cannot identify these households in the SIPP like I can those who become eligible.
[111] See Appendix C and Table C3 for further discussion.
[112] Figure A6 in Appendix A further illustrates the differences between always-eligible and sometimes-eligible households by presenting scatterplots of SIPP households' total income and countable non-vehicle assets by eligibility status.

relatively more or less on average. I therefore refrain from commenting on whether the estimates in Section 4 would hold for any theoretical expansions of SNAP eligibility to households across the distributions of income and assets, and I urge caution in interpreting them as such. Rather, they should be interpreted as the aggregate impacts of expanding SNAP eligibility to populations who would not be eligible under the federal criteria.

5.3. Other robustness checks

I consider and present several variants of the baseline models in Table C4 of Appendix C. The primary findings are robust to most changes I consider, including the exclusion of Medicaid income eligibility limit controls, demographic controls, or both; the use of alternate Medicaid eligibility controls; the exclusion of California from the sample due to its SSI cash-out policy during the sample period; the inclusion of Alaska and Hawaii in the sample; the use of a uniform sample in which any missing observations are excluded; and not weighting regressions by population.

The inclusion of state-specific time trends from the model removes statistical significance from the estimates of the impacts of the SEV on the primary Medicaid outcomes, but these estimates generally maintain their sign. Given the upward trends in SNAP eligibility, Medicaid enrollment, and SNAP enrollment over the sample period, it is not surprising that the addition of trends absorbs some of the identifying variation in the SEV. The inclusion of trends also generally reduces the size of the estimates, especially for the model with adult Medicaid enrollment as the outcome. The point estimate of the coefficient on the SEV when trends are included is reduced almost to zero, which also accounts for the reduction in the estimated impact of the SEV on overall Medicaid enrollment.

Because of potential data quality issues in the MSIS enrollment data, I also estimate two regressions using outcomes constructed with an alternate measure of state-level Medicaid enrollment from the UKCPR. The coefficient estimates from these regressions are consistent with those using the original measures of the overall Medicaid enrollment rate and Medicaid spending per enrollee.

6. Conclusion

In this study, I develop an approach to summarize detailed variation in state-level SNAP expansions through a measure of simulated eligibility. I use this measure to estimate the aggregate impacts of SNAP expansions on Medicaid enrollment and spending. I find that expanding SNAP eligibility increases Medicaid enrollment – especially enrollment of adults – and reduces average Medicaid spending per enrollee.

Interactions between SNAP and Medicaid have important policy implications. Recent ACA Medicaid eligibility expansions as well as the increasing prevalence of joint social program administration means that these programs may be even more likely to affect each other in obvious and nonobvious ways. Further, as the first- and second-largest means-tested programs in the United States by both spending and recipients, these interactions have implications for program design as well as for federal and state budgets. Policymakers aiming to increase Medicaid take-up may find that increasing SNAP take-up or expanding eligibility are effective ways to do so. State policymakers may find this an especially attractive option since states only finance SNAP administration, not benefits. Conversely, it may be difficult for policymakers to target expansion of eligibility or take-up in only one program without increasing take-up – and the subsequent cost of – the other.

Previous studies have found that Medicaid enrollment or expansions increase SNAP take-up, but only one I identify examines whether SNAP eligibility expansions impact Medicaid enrollment. Han (2019) finds no evidence that SNAP BBCE expansions affect the probability that a household has Medicaid coverage for its members. I find conflicting evidence that SNAP expansions do increase aggregate Medicaid enrollment, potentially due to my incorporation of non-BBCE SNAP eligibility expansions in the SEV, use of state-level administrative SNAP and Medicaid participation data, and use of a slightly different sample period. Further evidence is needed to clarify this relationship between SNAP and Medicaid, particularly in the post-ACA period in which many states have significantly expanded Medicaid eligibility.

Further investigation in this area should also clarify the mechanisms through which SNAP and Medicaid interact. One potentially major mechanism is integrated administration of these programs by the states. Increasingly, state social services agencies have adopted joint program applications, processing of applications, and/or recertification, all of which have the potential to significantly reduce the hassle costs of multiple social program participation. Another way to further explore SNAP's impacts on Medicaid would be to examine the impacts of SNAP expansions on Medicaid-covered healthcare utilization as well as subcategories of Medicaid spending in the short term and long term. For example, SNAP may increase administrative spending while not greatly increasing spending on actual healthcare if it increases take-up of Medicaid among low-risk individuals. Disentangling the mechanisms through which SNAP impacts Medicaid would be especially valuable to state social services administrators and other relevant policymakers.

Chapter III: The Impacts of the Introduction of the Food Stamp Program on Mortality

The U.S. mortality rate declined substantially over the twentieth century by about one to two percent per year, with earlier reductions largely attributable to fewer deaths from infectious disease and later reductions to improved medical care and the prevention of infant mortality (Cutler and Meara 2001). A growing body of literature documents the impacts of the 1960s "War on Poverty" era domestic programs on health and mortality, especially of infants and children.[113] The modern Food Stamp Program (FSP) – renamed the Supplemental Nutrition Assistance Program (SNAP) in 2008 – was introduced during this time period with the intention of improving nutrition among low-income households through food-purchasing assistance. In practice, the FSP increased the resources available to many of the United States' poorest families by supplementing their food-purchasing dollars with food stamps. Given the relationship between income and health, it is likely that the FSP played some role in reducing mortality.[114]

Several studies have estimated the effects of the FSP rollout from 1961 to 1975 on various health outcomes. Almond, Hoynes, and Schanzenbach (2011) find increases in mean birthweight and small reductions in neonatal mortality among pregnancies in areas where the FSP had been introduced during its rollout period. Currie and Moretti (2008) apply an analogous method to analyze the impacts of the FSP rollout in California and find a reduction in average birth weight that appears to be driven by increased fertility, especially among black teens. Hoynes, Schanzenbach, and Almond (2016) find that exposure to the FSP in utero or early

[113] E.g., Medicaid (Goodman-Bacon 2018; Wherry and Meyer 2016; Brown, Kowalski, and Lurie 2015), Medicare (Finkelstein and McKnight 2008), Head Start (Ludwig and Miller 2007), and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) (Hoynes, Page, and Stevens 2011).
[114] Many studies have examined the relationships between health and income or mortality, both in causal and descriptive frameworks. Khullar and Chokshi (2018) provide a recent overview.

childhood reduces incidence of adult metabolic syndrome. Only one study I identify examines the mortality impacts of adult food stamp receipt in a causal framework, finding that it reduces the risk of death (Heflin, Ingram, and Ziliak 2019).

The purpose of this study is to determine whether the FSP's introduction contributed to a decrease in the aggregate mortality rate. I follow previous work exploiting variation in access to food stamps from the county-level rollout of the FSP (Hoynes and Schanzenbach 2009). I combine county-level information on the timing of the rollout in combination with death counts from the period. I construct mortality rates representing total deaths as well as deaths broken down by gender, age range, racial group, or cause of death. Because the FSP's effects on mortality may take a long time period to be realized and may not be linear over time, I estimate regressions modeling each mortality rate as a function of the FSP being in place for several different periods of time. Because high-poverty areas likely would have benefitted from access to food stamps the most, I estimate separate regressions for a subsample of high-poverty counties.

This study makes three major contributions. First, this study contributes to the broader literature on the health and mortality impacts of "War on Poverty" era social programs. Second, this study focuses on health outcomes other than those for infants and children. This study is only one of two to examine the effects of adult exposure to the FSP on mortality and the first to examine these effects in the context of the program's rollout. Third, this study incorporates as outcomes both the overall mortality rate and various subgroup-specific and cause-specific mortality rates, which allows investigation of potential mechanisms through which the FSP's introduction may have reduced overall mortality.

I find no statistically significant evidence that the introduction of the FSP reduced mortality rates among the full county sample. However, I find evidence that the program's operation reduced mortality over time in counties with the highest pre-rollout poverty rates. Mortality reductions in these counties are driven by reductions in deaths of males, blacks, and those aged 0 to 19. I also find limited evidence that the FSP reduced deaths in these areas from major cardiovascular diseases, suicides, and non-motor vehicle accidents.

1. Background

1.1. The introduction of the Food Stamp Program

The stated purposes of the FSP upon its introduction were to strengthen the agricultural economy and providing improved nutrition to low-income households. The program made physical food stamps available to income- and resource-eligible households, which could be spent like cash at authorized retailers on foods for consumption at home, excluding imported foods and alcoholic drinks. Until program changes implemented in 1979, households were required to purchase their food stamps by paying an amount proportionate to the expected food expenditures of a household of their size and income.[115] In return, they would receive a greater amount in food stamps intended to allow them to purchase enough food for a nutritionally adequate diet. Food stamps were funded by the federal government and jointly administered by the federal and state governments (USDA FNS 2018a).

Pilot versions of the FSP began in 1961.[116] Through 1963, pilots were expanded from eight initial counties to 43 counties and cities (USDA FNS 2018a). The passage of the Food

[115] Exceptions were made for some households receiving Aid to Families with Dependent Children (AFDC) benefits or without cash income.
[116] Prior to the FSP's introduction, the Commodity Distribution Program (CDP) provided food assistance to low-income households in some counties. As a part of that program, the federal government purchased surplus food commodities to support agricultural prices and then distribute the food to the poor. Compared to the FSP, the CDP was fairly limited in scope and benefits provided. The implementation of the FSP in a county required the discontinuation of the CDP. Assuming the CDP improved health outcomes, this would tend to bias estimates in this study toward zero.

Stamp Act of 1964 secured funding for three years to give additional localities the option to initiate the FSP. From 1964 on, new counties implemented the program at a steady rate, and participation and benefits issued grew rapidly (Berry 1984). Funding for the program was renewed over the following years, and 1973 amendments to the act required all counties to implement the FSP by 1975.[117] In the years of its rollout, the FSP was relatively popular among the American public. Congressmen generally sought to introduce it in their districts to build favor with their electorates and gain publicity (Berry 1984). The growth of the FSP was therefore governed less by demand and more by funding limits. Counties could not join the program until the USDA selected them to do so, and there were always waiting lists during the rollout period (Berry 1984).

The fact that local and national political decision-making was involved in the FSP's implementation raises concerns that the variation afforded by the rollout may be biased. For instance, there may have been more political pressure to join the FSP in areas where more constituents were poor, non-white, and elderly, as these groups benefited more from the program. These population characteristics are correlated with health outcomes, so estimates of the effects of early adoption of the program could be biased towards more negative health outcomes. Hoynes and Schanzenbach (2009) address this possibility by estimating a model in which the time to adoption is modeled as a function of various pre-rollout county characteristics. While they find that counties with a higher percentage of black, young, old, and/or poor residents are more likely to implement the program sooner, they also find that county characteristics explain only a small portion of the variation in time to adoption.

[117] Other "War on Poverty" programs introduced during this period include Medicare (1965), Medicaid (1966-1982), and Head Start (1965-1980).

1.2. The Food Stamp Program and mortality

The introduction of food stamps during the 1960s and 1970s would likely impact recipients' health through two mechanisms. First, receipt of food stamps may improve the diet of household members. Especially for the poorest households who receive the largest benefits, access to food stamps would tend to enable the purchase of enough food for a healthy diet. Access to food stamps may also encourage a more nutritionally complete diet including more expensive perishable foods. Insofar as food stamps reduce food insecurity, they are also likely to improve health.[118] Second, food stamp receipt may affect health by increasing a household's disposable income. Economic theory suggests that households who receive an in-kind transfer like food stamps would spend less of their own income on food and direct it toward other purposes (Southworth 1945). "Inframarginal" households that normally spend more on food than they would receive in food stamps could entirely replace a previous food spending with food stamps if they so choose.[119] In effect, food stamp receipt frees up income for other consumption, which could be health-promoting (e.g., medical care), health-harming (e.g., cigarettes), or health-neutral.[120, 121]

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[118] More recent studies that address the selection of more food insecure households into food assistance programs generally find that food stamp receipt reduces food insecurity (Gregory, Rabbitt, and Ribar 2016). Previous studies have consistently found negative associations between food insecurity and health, so it is reasonable to expect that the Food Stamp Program might improve health through this channel (Gundersen and Ziliak 2015).
[119] Hoynes and Schanzenbach (2009) find evidence that most households during the rollout period are inframarginal and that unconstrained households alter their consumption in response to food stamp income much like they would cash income.
[120] I argue that income freed up by food stamp receipt is more likely to be health-promoting than health-harming for those poorer households that would receive food stamps. If it is partially health-harming, this may manifest as an increase in mortality from liver disease or malignant neoplasms over time, which I can observe in the framework of this study.
[121] It is theoretically possible that food stamp receipt could harm health. Food stamp receipt may worsen diet if recipients substitute away from nutritious food toward less nutritious, more convenient foods, leading to unhealthy weight and other health conditions. Evidence using post-rollout data generally does not find that food stamps increase obesity (Gundersen 2016). Food stamp availability may disincentivize labor force participation, causing recipients to forgo so-called "healthy worker benefits" like employer-sponsored health insurance, social activity, or physical activity. Food stamp recipients may experience stigma, which would lead to stress and potentially risky coping behaviors like smoking or drinking. Finally, food stamp receipt may lead to uneven food consumption over the monthly benefit cycle, which may adversely impact health (Wilde and Ranney 2000). Despite these possibilities,

Existing evidence suggests that the introduction of the FSP had generally positive or neutral effects on the health outcomes of those exposed in utero or early childhood nationwide, including increases in mean birthweight, reductions in neonatal mortality, and reduced incidence of metabolic syndrome in later adulthood (Almond, Hoynes, and Schanzenbach 2011; Hoynes, Schanzenbach, and Almond 2016). No studies I identify examine the effect of adult exposure to the FSP rollout. Using post-rollout variation in program participation, other studies draw mixed findings on the impacts of food stamp receipt on various adult health outcomes. Many such studies do not account for household selection into food stamp participation, which is a major hurdle to the identification of its causal effects on health (Bitler 2016).[122] Studies that do account for selection in some way typically find that food stamp receipt is more likely to improve adult health. For example, Gregory and Deb (2015) estimate in an instrumental variables framework that food stamp receipt improves self-assessed health, increases the probability of reporting excellent or very good health, reduces sick days spent in bed, reduces emergency and diagnostic office-based doctor and outpatient visits, and increases checkups among nonelderly adults.[123]

Mortality rates are useful to measure the aggregate health of populations. Relative to other health measures like self-assessed health status, mortality is an objective outcome but also severe – perhaps so severe that the FSP rollout may not significantly affect its likelihood. I identify three studies estimating the relationship between food stamp receipt and mortality. Two

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it is unlikely that the FSP rollout would harm health or increase the mortality rate in the context of this study, especially since the relevant margin of variation is access to the FSP as opposed to changes in participation.

[122] The presence of a negative relationship between food stamp receipt and health does not imply that food stamps worsens health. Those who experience extreme financial hardship are more likely to both participate in social programs like the FSP and to have poorer health for a variety of reasons. Reverse causality is also possible in that poor health may decrease income, necessitating food stamp participation. Even when compared to eligible non-participants, food stamp recipients are more likely to be female, younger, parents to more children, nonwhite, noncitizens, poorer, and uninsured. Recipients presumably differ in unobservable ways as well.

[123] Yen, Bruce, and Jahns (2012) also employ an instrumental variables framework to estimate the health impacts of SNAP, finding decreases in self-assessed health. However, their sample only includes recipients in Tennessee.

studies estimate a positive relationship between food stamp receipt and the likelihood of death – especially from heart disease, stroke, and diabetes – but these studies do not account for selection into program participation (Krueger et al. 2004; Conrad et al. 2017). Heflin, Ingram, and Ziliak (2019) account for selection into food stamp participation using state-level SNAP policy instruments and find that food stamp receipt reduces the overall risk of death as well as the risk of "deaths of despair" from liver disease/cirrhosis, poisoning, or suicide among adults aged 40-64. The reduction in deaths of despair among non-elderly adults is suggestive of SNAP's role in the social safety net, mitigating economic or other hardships. Given these findings, it is possible that the FSP's introduction may have reduced mortality rates through similar mechanisms.

2. Data

I assemble county-level information on the timing of the introduction of the FSP, annual mortality rates, and various population characteristics and economic conditions. The panel dataset includes information on 1,716 counties and spans the years 1969 to 1978.

I use information on the month and year that the FSP began in each county. This information is retrieved from an online dataset accompanying Hoynes and Schanzenbach (2009).[124] The dates of program implementation range from May 1961 to March 1975 and are available for most counties.[125] Table 24 shows the number of counties that implemented the FSP in each year during the rollout period, dividing these counties into three groups: all counties for which the timing of introduction is available, counties included in this study's sample (described

[124] The information on timing of FSP implementation is originally sourced from several USDA year-end reports from the rollout period on county food stamp caseloads.
[125] Exceptions include Alaska (due to inconsistencies between FSP service areas and local areas during the rollout period) and ten local areas spread between several states. This information is available for most kinds of non-county local areas, including Washington, D.C. and most independent cities in Virginia.

Table 24. County introductions of Food Stamp Program by year

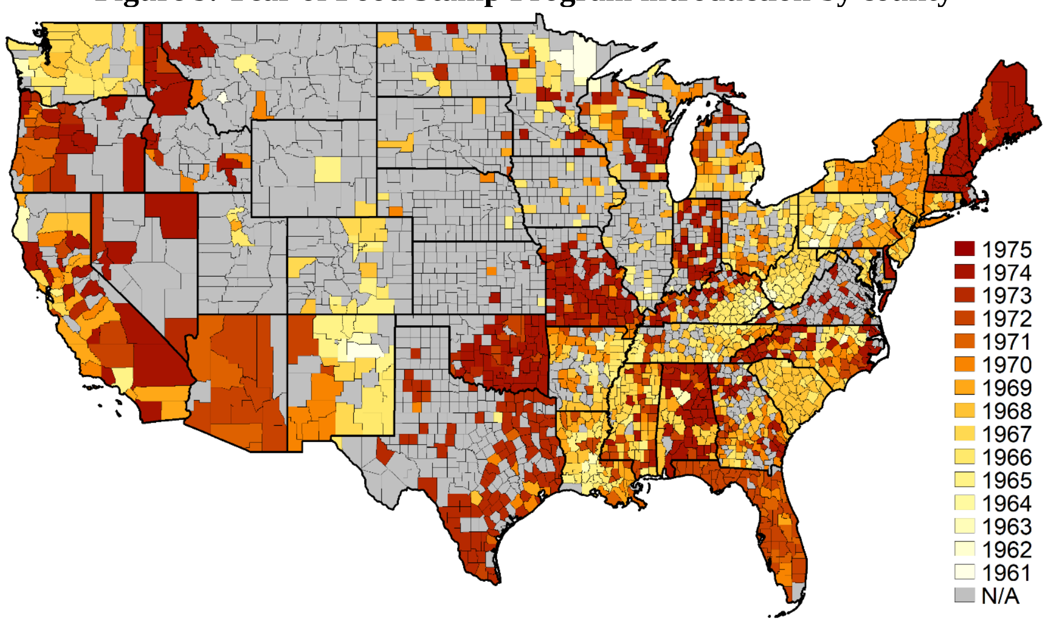
	All counties		Full county sample		High-poverty county sample	
Year	# introducing FSP	Percent (%)	# introducing FSP	Percent (%)	# introducing FSP	Percent (%)
1961	9	0.29	9	0.52	2	0.47
1962	9	0.29	9	0.52	2	0.47
1963	25	0.81	22	1.28	8	1.87
1964	0	0.00	0	0.00	0	0.00
1965	142	4.57	99	5.77	42	9.81
1966	285	9.18	169	9.85	52	12.15
1967	374	12.05	192	11.19	53	12.38
1968	369	11.89	156	9.09	56	13.08
1969	369	11.89	117	6.82	17	3.97
1970	392	12.63	189	11.01	31	7.24
1971	60	1.93	47	2.74	6	1.40
1972	208	6.70	171	9.97	54	12.62
1973	325	10.47	142	8.28	41	9.58
1974	535	17.24	393	22.90	64	14.95
1975	2	0.06	1	0.06	0	0.00
Total	3,104	100.00	1,716	100.00	428	100.00

“All counties” includes all counties for which FSP rollout timing information is available. “Full county sample” includes those counties used in this study’s sample. “High-poverty county sample” includes those counties in this study’s sample with 1960 poverty rates in the highest 25%.

below), and counties included in this study's high-poverty subsample (described below). Other than the pilot program period from 1961 to 1964, county FSP adoption is spread throughout the rollout period with some clustering in the mid-1960s and in 1974 after the program was mandated. Figure 9 illustrates the timing of the county rollout for counties included in this study's sample. In some states, counties tend to adopt the program in clusters, but there is still substantial within-state variation in year of adoption.

I use death and population counts by county and year gathered from the Centers for Disease Control and Prevention Wide-ranging Online Data for Epidemiologic Research (CDC WONDER) system (2019).[126] Death and population counts are provided for various population subgroups. I construct mortality rates for each county and year as the number of deaths per 100,000 members of the population or relevant population subgroup. These subgroups include genders (male and female), racial groups (white, black, and "other"), and five- to ten-year age

Figure 9. Year of Food Stamp Program introduction by county



The FSP was introduced in all counties during this period. Grayed out counties are not included in study sample.

[126] Specifically, I gather mortality information from the Compressed Mortality File. The earliest consistent years available are 1968 to 1978.

ranges. I aggregate age ranges up to three wider ranges: 19 or younger, 20 to 64, and 65 or older. Death counts are also provided for specific causes of death, which are designated by International Classification of Diseases-8 (ICD-8) code. I collapse death counts into ten broader causes of death, including malignant neoplasms, diabetes mellitus, major cardiovascular diseases, stroke, pneumonia and influenza, chronic liver disease and cirrhosis of the liver, motor vehicle accidents, other accidents, suicide, and homicide and legal intervention.[127] I define and describe the mortality rates I consider as outcomes in Table 25.[128]

I use information on other county characteristics in various contexts. I gather annual county-level information from the Bureau of Economic Analysis' Regional Economic Accounts (2019). This data is available beginning in 1969 and includes information on total personal income and government transfers, which I use to construct measures of real personal income and non-food stamp transfers per capita. I collect information about county-level demographic characteristics in 1960 from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System (Manson et al. 2019). This information includes the total population and the number of people that are under age 5, age 65 or older, nonwhite, or living in rural areas.[129] I use this information to construct measures of the log of population and the percentages of each county with each of these characteristics. I also collect county poverty rates from the 1960 Decennial Census (2018), which I use as a control or to stratify the county sample in some analyses. Finally, I collect information on the timing of the state-level rollout of

[127] The inclusion of eight major causes of death – malignant neoplasms, cardiovascular diseases, pneumonia and influenza, liver disease and cirrhosis, motor vehicle accidents, other accidents, suicides, and homicides and legal intervention – follow groups defined by Ruhm (2000). The inclusion of two additional causes of death – diabetes and stroke – are motivated by the findings of Conrad et al. (2017).

[128] Figure D1 in Appendix D shows average trends in the overall mortality rate during the sample period for both the full county sample and high-poverty county subsample (described below). Figure D2 illustrates variation in the overall mortality rate between counties throughout the sample period.

[129] This measure represents those living in rural non-farm areas as I lack information about the total rural population at the county-level.

Table 25. Mortality rate details

Mortality rate categorization	Breakdowns
Overall	-
Gender	• Male • Female
Race	• Black • White • Other race
Age	• 0-19 years old • 20-64 years old • 65+ years old
Internal causes	• Malignant neoplasms (140-209) • Diabetes (250) • Major cardiovascular disease (390-448) • Stroke (432-434, 436) • Pneumonia and flu (470-486) • Liver disease and cirrhosis (571)
External causes	• Motor vehicle accidents (E810-E823) • Other accidents (E800-E807, E825-E949) • Suicide (E950-E959) • Homicide and legal intervention (E960-E978)

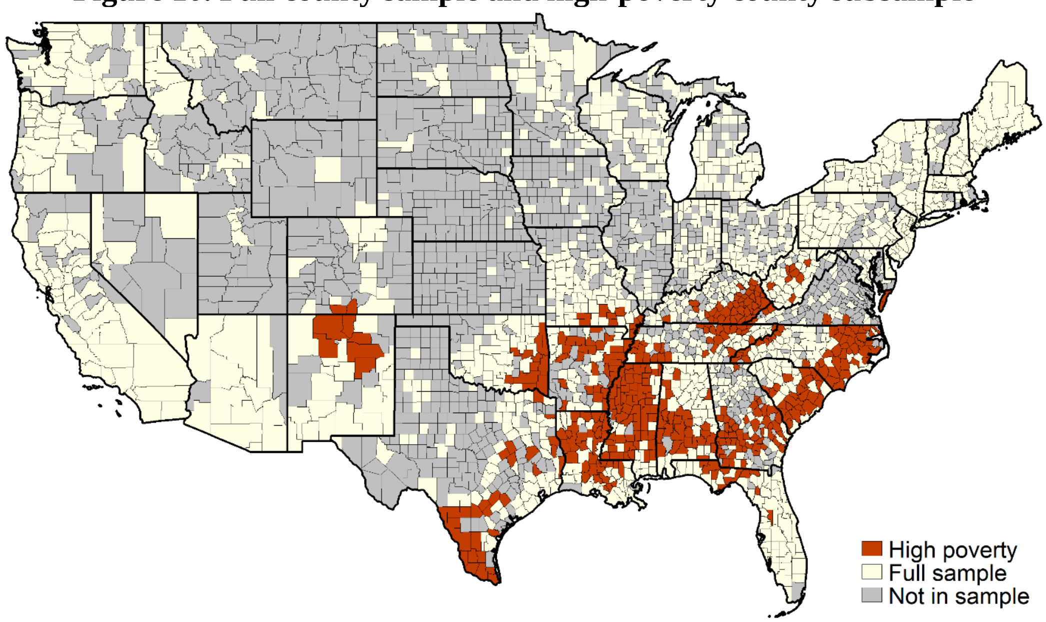
Mortality rates are defined as the number of deaths per 100,000 members of the relevant population and are not adjusted for age makeup of the county population. For overall and cause-specific mortality rates, the population used to construct the mortality rate is the total county population. For mortality rates of specific gender, race/ethnicity, or age groups, the population used to construct the mortality rate is the number of people of the specified gender, race group, or age range.

Cause-specific mortality rates aggregate deaths of similar underlying cause as classified by International Classification of Disease-8 (ICD-8) code, shown in parentheses. Deaths in the CDC WONDER system are assigned a singular code representing “underlying” cause, meaning the disease or injury which initiated the sequence of events leading directly to death or the circumstances of the accident or violence which produced the fatal injury. The same death is not classified under multiple ICD-8 codes.

Medicaid from 1966 to 1982 in order to control for the program's potential impacts on mortality in some specifications (Kaiser Commission on Medicaid and the Uninsured 2012).

I exclude from the county sample those counties with missing FSP timing, mortality, or other data over the 1969 to 1978 period.[130] I also exclude counties whose borders change during that period. The total population of the county sample was 176.3 million in 1970, or almost 86% of the U.S. population of 205.1 million. Additionally, I define a subsample of "high-poverty" counties which includes those counties whose 1960 poverty rates fall in the highest 25%. The panel dataset consists of 1,716 counties over ten years for a total of 17,160 county-year-level observations. The high-poverty subsample includes 428 of these counties and a total of 4,280 county-year-level observations. Figure 10 illustrates the location of counties in the full sample and the high-poverty subsample. High-poverty counties are concentrated in the Southeast with a minority located in the Southwest.

Figure 10. Full county sample and high-poverty county subsample



High-poverty counties are defined as those with 1960 poverty rates in the highest 25% of the study's county sample.

[130] Most excluded counties are missing information on annual income or government transfers.

Table 26 reports the population-weighted summary statistics of the key variables I use in the analyses for both the full county sample and the high-poverty subsample.[131] The mean year of FSP implementation was 1969 for both samples. The average overall mortality rate was 904 deaths per 100,000 population in the full sample and 1,018 deaths in the high-poverty sample. Average subgroup- and cause-specific mortality rates are generally higher in the high-poverty sample. Exceptions include the mortality rate of those aged 65 and over as well as rates of death from malignant neoplasms, liver disease and cirrhosis, and suicides. Counties in the high-poverty subsample differ on average from those in the full sample on several observable characteristics. They tend to have fewer residents and lower income per capita, and their residents are more likely to be nonwhite, rural, and/or impoverished.

3. Methodology

3.1. Baseline model

The goal of this study is to estimate the impacts of the FSP rollout on overall, subgroup, and cause-specific mortality rates. Since mortality is an extreme health outcome and it may take an extended period of exposure to food stamps to alter individual risk of death, I aim to estimate both the short-term and long-term mortality impacts of the FSP rollout. My primary approach is to estimate fixed effects models of the form:

$$MRATE_{ct} = \beta_0 + \sum_{i=1}^{5} \beta_{1i}\, FSP_{cti} + \boldsymbol{\beta_2 X_{ct}} + \boldsymbol{\beta_3 CFE_c} + \boldsymbol{\beta_4 YFE_t} + \epsilon_{ct} \qquad (12)$$

The outcome $MRATE_{ct}$ represents one of the mortality rates in a county c and year t, expressed as the number of deaths per 100,000 population. FSP_{cti} represents one of several indicators of how many years have passed since the introduction of the FSP in county c. The

[131] Table D1 in Appendix D presents summary statistics of all variables I use in the analyses.

Table 26. Selected summary statistics

	Full county sample		High-poverty county sample	
	Mean	Std. dev.	Mean	Std. dev.
FSP rollout				
Year of introduction:				
Weighted by population	1968.7	(3.308)	1968.9	(3.278)
Unweighted	1969.9	(3.270)	1969.3	(3.308)
=1 if FSP was introduced:				
1 or 2 years ago	0.128	(0.334)	0.131	(0.338)
3 or 4 years ago	0.177	(0.382)	0.180	(0.385)
5 or 6 years ago	0.164	(0.370)	0.163	(0.369)
7 or 8 years ago	0.149	(0.356)	0.136	(0.343)
9 or more years ago	0.217	(0.412)	0.214	(0.410)
Years since introduction	5.186	(3.883)	5.030	(3.837)
Mortality rates: deaths per 100,000 population				
Overall	903.8	(205.2)	1017.9	(208.9)
Female	783.7	(173.2)	833.9	(190.0)
Male	1031.0	(255.2)	1212.2	(265.2)
Black	929.2	(251.3)	1088.3	(236.9)
White	909.0	(220.4)	996.6	(239.8)
Other race	365.0	(296.4)	639.2	(480.9)
0-19	137.6	(40.47)	183.7	(65.26)
20-64	526.6	(126.3)	662.4	(163.4)
65+	5598.1	(624.6)	5558.0	(766.7)
Malignant neoplasms	168.9	(41.19)	155.1	(42.94)
Diabetes	17.28	(7.747)	21.60	(12.16)
Major cardiovascular disease	466.6	(126.9)	520.4	(139.9)
Stroke	55.67	(23.37)	84.20	(41.56)
Pneumonia & influenza	27.63	(11.44)	33.31	(18.22)
Liver disease & cirrhosis	15.94	(8.580)	11.11	(7.064)
Motor vehicle accidents	23.50	(11.59)	39.82	(18.98)
Other accidents	26.62	(10.43)	39.19	(17.74)
Suicide	12.24	(5.043)	12.06	(7.412)
Homicide & legal intervention	10.28	(7.910)	14.76	(9.185)
1960 poverty rate (%)	21.13	(13.31)	60.92	(6.256)
Population (unweighted)	106715.9	(302240.6)	20986.2	(19158.9)
Counties	1,716		428	
Years	10		10	
Observations	17,160		4,280	

Mortality rates are weighted by the county population used as the denominator in their construction: either the total population for the overall or cause-specific mortality rates or the relevant subgroup population for subgroup-specific mortality rates. Other statistics are weighted by total county population unless otherwise noted. The sample excludes Alaska and counties for which data is not available for the entire period. The sample period is 1969-1978. Full summary statistics are displayed in Table D1 in Appendix D.

baseline models include five mutually exclusive dummy variables indicating the number of years since the FSP was introduced in a county, divided into two-year bins. Specifically, these variables are equal to one if the FSP was introduced one or two years ago (i.e., in year $t-1$ or $t-2$), three or four years ago, five or six years ago, seven or eight years ago, or nine or more years ago. The estimates of the coefficients β_{1i} on these dummies represent the effects of the FSP being in place for the given number of years on each mortality rate, relative to counties which have either not yet implemented the FSP or implement it in the current year t. Taken collectively, these estimates can be used to trace out the effects of the FSP rollout on each mortality rate over time, which need not be a linear function of the time the FSP has been in place.

Model (12) includes a vector of county-level economic and demographic covariates, represented by $\boldsymbol{X_{ct}}$. Baseline models include annual measures of of real personal income per capita, real government transfers to individuals per capita, as well as interactions between linear time trends and several pre-rollout 1960 county characteristics.[132, 133] $\boldsymbol{CFE_c}$ and $\boldsymbol{YFE_t}$ represent county fixed effects and year fixed effects, respectively. Baseline regressions are weighted by the

[132] Nominal dollar values are adjusted for inflation and represented in 1960-adjusted dollars. Government transfers per capita are broken out into income maintenance benefits excluding food stamps, medical benefits, retirement and disability insurance benefits, unemployment insurance compensation, veteran's benefits, education and training assistance, and other transfers. 1960 county characteristics interacted with time trends include the log of population, the percentage of people under age 5, the percentage of people aged 65 or older, the percentage of people that are non-white, the percentage of people living in rural, non-farm areas, and the poverty rate.

[133] The inclusion of individual government transfer controls is intended to disentangle the effects of the FSP from any effects of the expansion of other social programs during the time period. Hoynes and Schanzenbach (2009) find that pre-rollout characteristics are correlated with time until FSP introduction, though they explain little overall variation in the time until introduction. This motivates the inclusion of time trend-interacted pre-rollout county characteristics as controls.

county population or subgroup population corresponding to the mortality rate used as an outcome.[134]

The FSP rollout may affect mortality rates in some areas differently than it would in others. Given that the FSP targets low-income, low-resource households, poorer counties would likely benefit more from access to the program. A larger share of the population in high-poverty areas would be eligible to receive food stamps relative to low-poverty areas, which likely means that a larger share would also participate in the program in these areas. The average benefit per participating household may also be larger in high-poverty areas since benefit size decreases with income. Therefore, the introduction of the FSP may subsequently improve health and/or reduce mortality rates more in high-poverty areas. In order to determine whether this is the case, I estimate model (12) separately for each mortality outcome for a subsample of high-poverty counties. I define counties as "high-poverty" if they have pre-rollout 1960 poverty rates in the highest quartile.

Results from baseline regressions using the full and high-poverty county samples are presented alongside each other in Section 4. I discuss challenges to identification at the end of Section 4.

3.2. Alternative models

I consider several alternatives to model (12) that differently model the impact of the FSP or alter other aspects of the model. I estimate alternative models of the form:

$$MRATE_{ct} = \beta_0 + \delta_0 FSP_{ct} + \beta_1 FSPyrs_{ct} + \beta_2 FSPyrs_{ct}^2 + \boldsymbol{\beta_3 X_{ct}} + \boldsymbol{\beta_4 CFE_c} + \boldsymbol{\beta_5 YFE_t} + \epsilon_{ct} \quad (13)$$

[134] Models using the overall mortality rate or specific causes of death use the total county population. Models using subgroup mortality rates use the relevant subgroup population, which is also used to construct the mortality rate. For example, models using the male, white, or age 20-64 mortality rates are weighted by the male population, white population, or adult population aged 20-64, respectively.

This model includes three outcomes of interest. The first is a dummy variable FSP_{ct}, which is equal to one if the FSP is introduced in year t in county c or in any other previous year. δ_0 represents the average effect of the FSP being in place at all during year t. The second is $FSPyrs_{ct}$ which represents the number of years since the year in which the FSP began. For example, $FSP_{ct} = 2$ for a county in 1970 if the FSP was introduced there in 1968. If the FSP was instead introduced in 1970 or in any future year, $FSP_{ct} = 0$. The third is the square of $FSPyrs_{ct}$. β_1 and β_2 together represent the effect of the FSP being in place for several years and model the mortality effects of the program as a linear or quadratic function of time since introduction. Model (13) is otherwise structured the same and contains the same controls as model (12).

Additionally, I estimate alternate versions of model (12) to test the sensitivity of the main results to various specifications. Results from model (13) and these other alternative models are presented in Section 4 following the main results.

4. Results

4.1. Hypotheses

I hypothesize that the introduction of the FSP reduces the overall mortality rate. As discussed in Section 1, access to the FSP is likely to improve health, which would reduce mortality over time. The program is intended to improve the nutrition of low-income recipients and is particularly likely to meet this goal among the poorest recipients. Therefore, I hypothesize that the FSP's introduction reduces mortality among those aged 19 or younger – who are likely to see larger health improvements from increased nutrition – and that it reduces mortality from causes of death such as major cardiovascular diseases or diabetes. The first effect is likely to occur relatively quickly, and the second set of effects is more likely to manifest over time.

Since the FSP frees up recipients' income, its introduction may reduce mortality through increased consumption of health-promoting goods like medical care. It is also possible that recipients may use freed-up income for health-harming consumption such as unhealthy food, tobacco, or alcohol. If this is the case, the FSP's introduction may increase mortality rates from causes such as diabetes, malignant neoplasms, and liver disease, though competing reductions in these rates from other changes in consumption may lead to small or undetectable net changes.

The FSP may also reduce deaths of despair by functioning as a safety net for households facing economic or other hardship. Access to food stamps may reduce adverse impacts on mental health and increases in risky coping behaviors accompanying short- or long-term financial hardship. Therefore, the FSP rollout may decrease mortality from suicides, other accidents (a category that includes poisoning and drug overdose), or liver disease.

Last, I hypothesize that reductions in mortality rates as described above are likely larger in high-poverty counties than among the full sample. The FSP rollout is likely to have the largest effects in these counties because the health impacts of access to food stamps would likely be largest for the poorest households and because a greater share of the population in these counties are eligible to receive food stamps relative to the full sample. For similar reasons, the FSP rollout may have larger mortality impacts on more-vulnerable population subgroups like blacks and the elderly.

4.2. Main results

Tables 27 through 31 report the main results from regressions of the county-level mortality rates I consider on dummy variables indicating how many years have passed since the introduction of the FSP as shown by model (12). Mortality rates are expressed as the number of deaths per 100,000 members of the population in all regressions. Each of these tables reports results from regressions using the full county sample and from those using only the high-poverty sample alongside each other.

Table 27 reports the results of regressions using the overall mortality rate that includes deaths from the entire population from all causes, the female rate, and the male rate. I do not find evidence that the introduction of the FSP reduces any of these mortality rates among the full sample in the post-rollout period considered. However, I do find evidence that the FSP rollout significantly decreases the overall mortality rate over time in high-poverty counties. I estimate that the FSP reduces the mortality rate by 21.2 deaths five to six years after its introduction, or about 2.1% of the mean rate of 1,018 deaths per 100,000. I estimate that this mortality reduction grows over time. Seven to eight years after its introduction, I estimate that the FSP reduces the mortality rate by 29.4 deaths, or about 2.9% of the mean. Nine or more years after its introduction, I estimate a reduction of 33.9 deaths, or about 3.3% of the mean. The estimated reduction in the overall mortality rate in high-poverty counties appears to be driven entirely by reductions in the male mortality rate. I find no evidence of mortality reductions related to the FSP introduction among females in either sample, but I find that the FSP's introduction significantly reduces the male mortality rate in high-poverty counties. Five to six years after its introduction, the FSP reduces the male mortality rate in these counties by 41.4 deaths, or 3.4% of the mean rate of 1,212 deaths per 100,000. After being in place for nine or more years, the FSP reduces this rate by about 74.3 deaths, or 6.1% of the mean.

Table 28 reports the results of regressions using black, white, and "other" racial group-specific mortality rates. I find no statistically significant evidence that the FSP's introduction affects any of these rates in the full county sample. Among the high-poverty county sample, I find a pattern of estimated reductions in the black mortality rate much like the pattern of

Table 27. Overall and gender-specific mortality rate regression results

	Full sample			High-poverty sample		
	Overall	Female	Male	Overall	Female	Male
Years since FSP introduction:						
1 or 2	2.115	1.156	2.901	-4.214	0.249	-8.782
	(2.414)	(2.598)	(3.124)	(7.596)	(8.844)	(11.38)
3 or 4	-0.450	-1.309	0.195	-8.527	4.654	-22.59
	(3.100)	(3.322)	(4.039)	(9.798)	(11.21)	(14.63)
5 or 6	-0.317	-0.893	-0.0617	-21.20*	-2.402	-41.44**
	(4.000)	(4.259)	(5.155)	(12.53)	(14.63)	(18.45)
7 or 8	-2.001	-2.296	-2.101	-29.40*	-10.04	-50.33**
	(5.029)	(5.488)	(6.399)	(15.72)	(18.36)	(22.96)
9 or more	-3.068	-4.604	-1.961	-33.89*	3.440	-74.25***
	(6.004)	(6.517)	(7.762)	(19.82)	(22.97)	(28.79)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
1960 controls * t	Yes	Yes	Yes	Yes	Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean mortality rate	903.8	783.7	1031.0	1017.9	833.9	1212.2
R^2	0.453	0.232	0.423	0.252	0.132	0.188
Observations	17,160	17,160	17,160	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP dummies indicate how long ago the FSP was rolled out in each county.

Table 28. Race group-specific mortality rate regression results

	Full sample			High-poverty sample		
	Black	White	Other race	Black	White	Other race
Years since FSP introduction:						
1 or 2	8.435	1.154	2.614	2.419	-10.69	-11.02
	(5.659)	(2.687)	(14.59)	(14.71)	(8.648)	(53.73)
3 or 4	3.970	-0.393	-23.40	-18.16	-7.154	-10.08
	(7.425)	(3.445)	(16.94)	(18.75)	(11.08)	(66.06)
5 or 6	12.54	-0.789	-25.27	-43.90*	-15.30	68.24
	(9.572)	(4.422)	(20.58)	(24.24)	(14.13)	(85.22)
7 or 8	8.434	-1.110	-13.17	-61.45**	-18.09	68.73
	(11.50)	(5.585)	(24.72)	(29.98)	(17.86)	(111.0)
9 or more	20.89	-4.263	-29.60	-81.47**	-17.08	59.43
	(14.22)	(6.704)	(32.58)	(37.46)	(22.44)	(130.2)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
1960 controls * t	Yes	Yes	Yes	Yes	Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean mortality rate	929.2	909.0	365.0	1088.3	996.6	639.2
R^2	0.164	0.374	0.0675	0.111	0.175	0.0212
Observations	17,160	17,160	17,160	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP dummies indicate how long ago the FSP was rolled out in each county.

reductions in the male mortality rate. Five to six years after its introduction, the FSP reduces the black mortality rate in these counties by 43.9 deaths, or 4.0% of the mean rate of 1,088 deaths per 100,000. After nine or more years since its introduction, the FSP reduces this rate further by about 81.5 deaths, or 7.5% of the mean. I find no statistically significant evidence of corresponding reductions in the other groups' mortality rates.

Table 29 reports the results of regressions using age group-specific mortality rates. I find evidence that the FSP's introduction reduces the mortality rate of those under age 20 in the full county sample. Unlike the estimates in Tables 27 and 28, I estimate a statistically significant reduction in this mortality rate starting just one to two years after the FSP begins in a county. Specifically, I estimate that the FSP being in place for that long reduces the rate by 3.6 deaths on average, or 2.6% of the mean of 138 deaths per 100,000 population. I estimate that the impacts of the FSP's introduction increase over time. Nine or more years after its introduction, I estimate that the FSP reduces this rate by about 7.1 deaths, or 5.1% of the mean. I find evidence of a similar pattern of reductions in mortality of those aged 0 to 19 in the high-poverty county sample. After being in place for nine years or more, I estimate that the FSP reduces that mortality rate by 34.5 deaths, or 18.8% of the mean rate of 184 deaths per 100,000 population. I find no evidence of mortality reductions for those aged 20 to 64 or 65 or older in the full county sample. Among high-poverty counties, I estimate coefficients for these age groups suggestive of mortality reductions over time, but only two estimates for those aged 65 or older are weakly statistically significant. I estimate a reduction in the elderly mortality rate in high-poverty counties as high as 179.8 deaths seven or eight years after the introduction of the FSP. This effect is 3.2% of the mean rate of 5,558 deaths per 100,000 population.

Table 29. Age group-specific mortality rate regression results

	Full sample			High-poverty sample		
	0-19	20-64	65+	0-19	20-64	65+
Years since FSP introduction:						
1 or 2	-3.644***	3.267*	22.30	-7.713	0.273	-19.31
	(1.071)	(1.848)	(15.78)	(5.029)	(8.220)	(52.48)
3 or 4	-3.752***	1.570	10.18	-13.22**	-6.418	-37.22
	(1.396)	(2.403)	(20.48)	(6.133)	(10.61)	(67.73)
5 or 6	-4.494**	2.464	11.28	-16.54**	-9.448	-143.6*
	(1.773)	(3.092)	(27.15)	(7.903)	(13.43)	(86.54)
7 or 8	-6.555***	0.0208	5.321	-22.40**	-17.40	-179.8*
	(2.206)	(3.850)	(35.30)	(9.729)	(17.09)	(108.3)
9 or more	-7.052***	0.570	-14.39	-34.53***	-26.58	-131.9
	(2.699)	(4.691)	(43.37)	(11.96)	(21.28)	(136.3)
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes
1960 controls * t	Yes	Yes	Yes	Yes	Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean mortality rate	137.6	526.6	5598.1	183.7	662.4	5558.0
R^2	0.386	0.607	0.526	0.211	0.401	0.258
Observations	17,160	17,160	17,160	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects.
Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP dummies indicate how long ago the FSP was rolled out in each county.

Table 30 reports the results of regressions using mortality rates from the six internal causes of death I consider: malignant neoplasms, diabetes mellitus, major cardiovascular diseases, stroke, pneumonia and influenza, and liver disease and cirrhosis. In the full sample, I find evidence of an increase in deaths from diabetes. I estimate that the FSP increases the diabetes mortality rate by 0.39 deaths (2.3% of the mean) one to two years after its introduction and 0.96 deaths (4.6% of the mean) seven or eight years after its introduction. I find no statistically significant evidence of a pattern of impacts over time on the other five mortality rates in either sample. However, in the high-poverty sample, I estimate coefficients of size and magnitude that are suggestive of reductions over time in deaths from diabetes, cardiovascular disease, and liver disease and cirrhosis.

Table 31 reports the results of regressions using mortality rates from the four external causes of death I consider: motor vehicle accidents, other accidents, suicide, and homicide and legal intervention. In the full sample, I find evidence of an increase in deaths from homicide and legal intervention. One to two years following the introduction of the FSP, this mortality rate increases by about 0.56 deaths (5.5% of the mean), and by five to six years following the FSP's introduction, this rate increases by 0.80 deaths (7.7% of the mean). In the high-poverty sample, I find evidence of a pattern of reductions in mortality from non-motor vehicle accidents. I estimate that this reduction increases with time since the FSP's introduction. One to two years after the FSP's introduction, the other accident mortality rate falls by about 2.6 deaths, or 6.7% of the mean rate of 39.2 deaths per 100,000 population. After nine or more years since the program's introduction, this mortality rate falls by about 7.2 deaths, or 18.5% of the mean rate. I estimate coefficients of size and magnitude that are suggestive of reductions in the suicide rate following

Table 30. Regressions of mortality rates from internal causes on Food Stamp Program introduction indicators

	Cancer	Diabetes	Cardio. disease	Stroke	Pneumonia & flu	Liver
Full sample						
Years since FSP intro:						
1 or 2	-0.174	0.394*	2.451	0.646	-0.252	-0.255
	(0.716)	(0.231)	(1.660)	(0.436)	(0.351)	(0.254)
3 or 4	0.0684	0.585*	-0.167	0.886	-0.934**	0.00506
	(0.960)	(0.311)	(2.296)	(0.573)	(0.442)	(0.325)
5 or 6	-0.620	0.753*	0.571	1.246*	-0.735	0.128
	(1.229)	(0.409)	(3.017)	(0.741)	(0.565)	(0.407)
7 or 8	-0.712	0.964*	-0.208	1.445	-0.983	-0.122
	(1.549)	(0.522)	(3.864)	(0.915)	(0.708)	(0.505)
9 or more	-1.305	0.788	-2.454	1.288	-0.742	0.192
	(1.874)	(0.681)	(4.780)	(1.167)	(0.915)	(0.642)
Mean mortality rate	168.9	17.28	466.6	55.67	27.63	15.94
R^2	0.238	0.0958	0.471	0.142	0.187	0.111
Observations	17,160	17,160	17,160	17,160	17,160	17,160
High-poverty sample						
Years since FSP intro:						
1 or 2	-3.359	-0.506	-1.431	2.362	-0.273	-0.478
	(2.643)	(1.054)	(5.307)	(2.325)	(1.399)	(0.676)
3 or 4	1.441	-0.510	-9.142	2.852	-0.928	-0.801
	(3.439)	(1.289)	(6.882)	(2.887)	(1.711)	(0.874)
5 or 6	-1.206	-0.955	-14.12	1.445	-1.380	-0.847
	(4.375)	(1.632)	(8.899)	(3.688)	(2.189)	(1.100)
7 or 8	-1.809	-0.503	-23.01**	0.464	-0.0871	-1.742
	(5.600)	(2.035)	(10.99)	(4.618)	(2.726)	(1.395)
9 or more	-1.887	-1.814	-18.19	2.451	-0.154	-1.398
	(6.923)	(2.486)	(13.78)	(5.843)	(3.335)	(1.733)
Mean mortality rate	155.1	21.60	520.4	84.20	33.31	11.11
R^2	0.0670	0.0462	0.242	0.0800	0.109	0.0552
Observations	4,280	4,280	4,280	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP dummies indicate how long ago the FSP was rolled out in each county.

Table 31. Regressions of mortality rates from external causes on Food Stamp Program introduction indicators

	Motor veh. acc.	Other acc.	Suicide	Homicide & LI
Full sample				
Years since FSP intro:				
1 or 2	-0.282	0.0939	0.163	0.565**
	(0.270)	(0.317)	(0.189)	(0.261)
3 or 4	0.0214	0.399	0.109	0.650*
	(0.358)	(0.442)	(0.252)	(0.351)
5 or 6	-0.537	0.434	0.0699	0.795*
	(0.447)	(0.571)	(0.322)	(0.466)
7 or 8	-0.876	-0.140	-0.365	0.846
	(0.537)	(0.728)	(0.415)	(0.598)
9 or more	-0.867	-0.185	-0.781	1.026
	(0.674)	(0.918)	(0.491)	(0.780)
Mean mortality rate	23.50	26.62	12.24	10.28
R^2	0.158	0.0850	0.0418	0.135
Observations	17,160	17,160	17,160	17,160
High-poverty sample				
Years since FSP intro:				
1 or 2	0.211	-2.626*	-0.791	0.469
	(1.490)	(1.456)	(0.675)	(0.827)
3 or 4	1.392	-3.463*	-0.546	0.184
	(1.926)	(1.857)	(0.916)	(1.022)
5 or 6	-2.126	-3.991	-2.694**	-0.119
	(2.434)	(2.471)	(1.147)	(1.270)
7 or 8	-1.579	-6.052**	-1.528	-0.259
	(3.016)	(3.058)	(1.411)	(1.616)
9 or more	-1.918	-7.245*	-2.036	-1.406
	(3.704)	(3.781)	(1.760)	(1.985)
Mean mortality rate	39.82	39.19	12.06	14.76
R^2	0.123	0.0424	0.0360	0.0434
Observations	4,280	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP dummies indicate how long ago the FSP was rolled out in each county.

the FSP's introduction in high-poverty counties, but all but one of these estimates are statistically insignificant.

4.3. Results from alternative models

Tables 32 and 33 report results from regressions of the county-level mortality rates on three variables of interest as shown in model (13): an indicator equal to one if the FSP was introduced in a county in the current year or earlier, a variable equal to the number of years since the FSP was introduced, and a variable equal to the square of the number of years since introduction. Both tables report results for the full county sample in the top panel and the high-poverty county sample in the bottom panel. Table 32 reports results for the overall mortality rate and those rates broken down by gender, racial group, or age group, and Table 33 reports results for the mortality rates broken down by internal or external cause of death.

Generally, the estimates reported in Table 32 support the main findings. For example, I find no evidence that the introduction of the FSP reduces the overall mortality rate in the full county sample, but I do estimate that among the high-poverty county sample, the FSP reduces the overall mortality rate over time. For comparison, Table 27 places the impact of the FSP being in place for 10 years as a reduction in the mortality rate of about 33.9 deaths per 100,000 population (3.3% of the mean). Applying the point estimates in Table 32 would predict a much larger reduction of 82.5 deaths (8.1% of the mean).[135] Similarly, Table 32 shows that most of the reductions in the overall mortality rate can be attributed to reductions in the male and black mortality rates. In the full sample, the FSP being in place reduces the mortality rate of those aged 0 to 19 and reduces it further the longer it is in place. In the high-poverty sample, I estimate similar effects, though they are statistically insignificant. Interestingly, I find evidence that the

[135] $-5.477 * 1 - 8.852 * 10 + 0.115 * 10^2 = -82.497$

Table 32. Alternate overall and subgroup-specific mortality rate regression results

	Overall	Female	Male	Black	White	Other race	0-19	20-64	65+
Full county sample									
FSP in place	2.892	5.651	-0.337	13.85*	1.111	7.877	-3.316**	4.715*	46.20**
	(3.562)	(3.593)	(4.692)	(8.299)	(4.158)	(14.31)	(1.679)	(2.798)	(21.72)
Years since intro	-0.518	0.637	-1.848	4.037	-0.496	-4.885	-1.120**	0.560	4.881
	(1.124)	(1.184)	(1.489)	(3.367)	(1.248)	(4.642)	(0.545)	(0.935)	(7.421)
Years since intro,	0.00788	0.0380	-0.0226	-0.0145	-0.00912	0.280	0.0388*	-0.00948	0.208
squared	(0.0478)	(0.0522)	(0.0594)	(0.108)	(0.0529)	(0.209)	(0.0202)	(0.0353)	(0.351)
Mean mortality rate	903.8	783.7	1031.0	929.2	909.0	365.0	137.6	526.6	5598.1
R^2	0.453	0.232	0.423	0.163	0.374	0.0660	0.386	0.607	0.525
Observations	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160
High-poverty county sample									
FSP in place	-5.477	0.820	-12.25	-8.010	-8.941	-42.53	-2.163	5.159	-31.83
	(10.96)	(12.43)	(16.69)	(22.38)	(12.70)	(81.31)	(7.323)	(11.93)	(76.99)
Years since intro	-8.852**	-2.846	-15.43**	-23.08***	-4.182	24.61	-4.104	-1.892	-71.91**
	(4.213)	(4.959)	(6.132)	(8.911)	(4.849)	(33.96)	(2.543)	(4.482)	(30.39)
Years since intro,	0.115	0.0728	0.160	0.200	0.0823	-0.301	0.00397	0.0874	1.698**
squared	(0.131)	(0.156)	(0.191)	(0.253)	(0.150)	(1.047)	(0.0868)	(0.139)	(0.851)
Mean mortality rate	1017.9	833.9	1212.2	1088.3	996.6	639.2	183.7	662.4	5558.0
R^2	0.251	0.129	0.187	0.111	0.174	0.0206	0.210	0.400	0.257
Observations	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP in place dummy indicates that the FSP was rolled out in the current year or earlier. Years since intro takes on non-negative values representing how many years ago the FSP was introduced.

Table 33. Alternate cause-specific mortality rate regression results

	Cancer	Diabetes	Cardio. disease	Stroke	Pneumonia & flu	Liver	Motor veh. acc.	Other acc.	Suicide	Homicide & LI
Full county sample										
FSP in place	-0.368	0.0915	3.356	-0.923	-0.475	-0.745	-0.0527	0.226	0.115	0.687*
	(1.031)	(0.292)	(2.361)	(0.614)	(0.506)	(0.486)	(0.368)	(0.409)	(0.261)	(0.377)
Years since intro	-0.339	0.163	-0.0933	-0.121	-0.158	-0.0972	-0.162	0.166	-0.166*	0.299**
	(0.345)	(0.104)	(0.815)	(0.219)	(0.171)	(0.130)	(0.134)	(0.152)	(0.0887)	(0.122)
Years since intro, squared	-0.00888	-0.0183***	0.0140	-0.0312***	0.0110*	-0.00510	0.000200	-0.0104	-0.00403	-0.00629
	(0.0151)	(0.00500)	(0.0376)	(0.00898)	(0.00656)	(0.00475)	(0.00481)	(0.00678)	(0.00404)	(0.00611)
Mean mortality rate	168.9	17.28	466.6	55.67	27.63	15.94	23.50	26.62	12.24	10.28
R^2	0.238	0.0973	0.471	0.143	0.187	0.111	0.158	0.0845	0.0409	0.136
Observations	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160
High-poverty county sample										
FSP in place	-6.149*	-1.344	-0.952	-0.525	-4.158**	-2.013**	4.620**	-0.322	-0.487	1.569
	(3.407)	(1.478)	(7.479)	(3.283)	(1.929)	(0.893)	(2.194)	(2.024)	(1.029)	(1.085)
Years since intro	0.0250	-0.547	-5.861**	-0.891	-1.683**	-0.857***	1.040	-0.487	-0.718*	0.289
	(1.320)	(0.560)	(2.839)	(1.168)	(0.741)	(0.330)	(0.807)	(0.796)	(0.369)	(0.382)
Years since intro, squared	-0.0588	-0.00779	0.215**	-0.0117	0.00591	0.00842	0.0318	0.0203	0.0194	0.00412
	(0.0466)	(0.0171)	(0.0981)	(0.0382)	(0.0228)	(0.0126)	(0.0259)	(0.0250)	(0.0122)	(0.0143)
Mean mortality rate	155.1	21.60	520.4	84.20	33.31	11.11	39.82	39.19	12.06	14.76
R^2	0.0657	0.0458	0.241	0.0789	0.110	0.0569	0.121	0.0409	0.0281	0.0420
Observations	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample or a subsample of counties whose 1960 poverty rates fall in the highest quartile. Regressions and mean mortality rates are weighted by the total county population or subgroup population used to construct each mortality rate. Mortality rates are expressed as the number of deaths per 100,000 members of the total population or subgroup population. FSP in place dummy indicates that the FSP was rolled out in the current year or earlier. Years since intro takes on non-negative values representing how many years ago the FSP was introduced.

FSP being in place increases the elderly mortality rate in the full sample while decreasing that rate over time in the high-poverty sample.

The estimates reported in Table 33 are somewhat less consistent with the findings concerning cause-specific mortality rates presented in Tables 30 and 31. I estimate that the FSP being in place reduces the malignant neoplasm in high-poverty counties by 6.1 deaths per 100,000 population, or 4.0% of the mean. Unlike in the main findings, I do not find statistically significant evidence of an increase in deaths from diabetes. I estimate that the FSP reduces deaths from major cardiovascular diseases over time in high-poverty counties, with the size of this reduction falling over time. I also find evidence that the FSP being in place reduces deaths from pneumonia and influenza as well as from liver disease and cirrhosis, with these reductions growing over time. As in the main findings, I find some evidence that the FSP reduces the suicide rate over time in the high-poverty county sample, but I also find similar evidence of a reduction over time in the full county sample. Also consistent with the main findings, I find evidence that the FSP being in place increases deaths from homicide and legal intervention in the full county sample with this increase growing over time.

I test the sensitivity of the main results by estimating regressions altering some aspect of model (12). Tables D2 and D3 in Appendix D report the results of these regressions using the overall mortality rate and the full or high-poverty county samples, respectively. I estimate regressions including state-year-level fixed effects in place of year fixed effects in order to control for state-year-level unobservable characteristics; including indicators representing whether Medicaid has been introduced in a state or whether it has been in place for several years; including county-specific time trends in place of interactions between time trends and pre-rollout characteristics; altering the baseline control set by excluding controls or including alternate versions of the economic controls; or that are not weighted by population. None of these alterations meaningfully change the estimates for either the full or high-poverty county samples, though some cause the high-poverty county subsample estimates to lose their statistical significance.

4.4. Caveats

I identify several threats to the validity of this study's findings. First, I assume that the operation of the FSP in a county is a measure of access to food stamps. This is an imprecise measure. People move into and out of counties during the rollout period, so some will have had access to food stamps for a longer or shorter time than the number of years the FSP has been in place in a county. The longer the period since the FSP's introduction, the more movement that will have occurred and the more imprecise this measure. At best, movement into and out of counties is unrelated to the presence or absence of the FSP, meaning that the estimated mortality impacts of the FSP's introduction would be unbiased but less precise. However, movement may be related to the FSP rollout. For instance, poorer households who would benefit from the program may move into areas where it is introduced earlier. If migrant households also have a higher average risk of death, this would bias the estimates in the direction of earlier FSP adoption increasing the mortality rate. Accordingly, I suggest a greater degree of caution in interpreting the estimates of the FSP being in place for longer periods of time.

Second, this study's identification strategy relies on the assumption that there are no trends in mortality rates associated with the timing of FSP adoption. Availability of county-level mortality and economic data restricts the sample period to 1969 through 1978. Table 24 shows that by 1969, the FSP had been introduced in 45% of counties in this study's sample and 54% of counties in the high-poverty subsample. I therefore do not observe most counties' pre-rollout mortality rates or trends in these rates. Further, almost all counties that introduce the FSP after 1969 do so by 1974 at the latest, meaning that I only observe mortality rates up to five years prior to the FSP's introduction. Counties that adopt the FSP earlier tend to be more populous, urban, black, and poor (Hoynes and Schanzenbach 2009). They may therefore differ from late-adopting counties in pre-rollout mortality rates or mortality trends, which could invalidate the above assumption. In order to address these concerns, I would ideally perform an event study-like analysis including dummies indicating the number of years since the FSP's introduction as in the baseline model (12) as well as dummies indicating the number of years until the FSP's introduction. I should estimate no positive or negative effects of the FSP on mortality rates ahead of its introduction. Given the overall trends in mortality during the 1960s and 1970s, the length of time the FSP would need to be in place to affect mortality, and potential differences between early and late adopters, a convincing analysis would require detailed county-level mortality data covering more time prior to 1969.

Last, I urge caution in applying this study's findings to other contexts. Although SNAP is the modern-day equivalent of the FSP, it is not likely that the mortality impacts of introducing SNAP today would be equivalent. Modern-day SNAP differs significantly in several ways, including provision through electronic benefit transfer, the lack of a purchase requirement, higher benefit levels, and expanded eligibility, among others. Mortality rates are also lower today than in the "War on Poverty" era for several reasons (including less severe poverty and food insecurity, though SNAP's operation partially determines these factors), so the rollout of the program would likely result in smaller mortality improvements. Additionally, the composition of the high-poverty county sample may affect interpretation of the estimated mortality impacts in these counties. As Figure 10 shows, this sample is concentrated in the Southeast. It may be the case that the FSP's mortality impacts are larger among this subsample due to unobservable regional characteristics instead of poverty itself.

5. Conclusion

In this study, I estimate the impact of the rollout of the FSP on various county-year level mortality rates over time. I consider mortality rates broken down by population subgroup and by cause of death in order to examine the different mechanisms through which the FSP might affect the overall mortality rate. I do not find evidence that the FSP's introduction reduced mortality among the full county sample, but I do find that it reduced mortality over time in a subsample of high-poverty counties. Mortality reductions in these counties are primarily driven by reductions in the mortality of males, blacks, and those aged 0 to 19. I also find some evidence of a reduction in deaths from cardiovascular disease, suicides, and non-motor vehicle accidents.

I estimate large mortality impacts of the FSP's introduction, though the magnitudes of these impacts are generally in-line with other estimates of the mortality effects of "War on Poverty" era social programs.[136] In various specifications, I estimate that the FSP being in place for 5 to 6 years reduces the mortality rate in high-poverty counties by 1.4% to 2.1% of the mean rate.[137] The FSP being in place for 9 or more years reduces this rate even further by 1.7% to 3.5%.[138] In a typical year during the sample period, about 92,000 people died in these high-poverty areas.[139] After ten years in place, I estimate that the FSP may have saved as many as

[136] E.g., Goodman-Bacon (2018) estimates that Medicaid's introduction led to an 11% reduction in the non-white child mortality rate.
[137] From Table D3, the weighted point estimates range from -14.3 to -21.4 in columns 1-7.
[138] From Table D3, the weighted point estimates range from -17.1 to -35.4 in columns 1-7.
[139] The total average population in the high-poverty counties was just under 9,000,000. An average rate of 1,017.9 deaths per 100,000 population implies an average of 91,611 total annual deaths.

3,200 lives annually.[140] Therefore, I conclude that the FSP – like other antipoverty programs of the era – contributed to reductions in the mortality rate.

[140] A 3.5% decrease in 91,611 deaths is 3,206.4 deaths avoided.

Appendix A: Construction of the Simulated Eligibility Variable (SEV)

Table A1. Changes in federal SNAP rules, 1996-2015

		Resource limits ($)			Dependent care deduction caps ($)		
Year	Vehicle FMV exclusion ($)	No elderly or disabled	Any elderly	Any disabled	Infants	Other	Excess shelter deduction cap ($)
1996	4600	2000	3000	2000	200	175	247
1997	4650	2000	3000	2000	200	175	250
1998	4650	2000	3000	2000	200	175	250
1999	4650	2000	3000	2000	200	175	275
2000	4650	2000	3000	2000	200	175	275
2001	4650	2000	3000	2000	200	175	340
2002	4650	2000	3000	3000	200	175	354
2003	4650	2000	3000	3000	200	175	367
2004	4650	2000	3000	3000	200	175	378
2005	4650	2000	3000	3000	200	175	388
2006	4650	2000	3000	3000	200	175	400
2007	4650	2000	3000	3000	200	175	417
2008	4650	2000	3000	3000	200	175	431
2009	4650	2000	3000	3000	None	None	446
2010	4650	2000	3000	3000	None	None	459
2011	4650	2000	3000	3000	None	None	458
2012	4650	2000	3250	3250	None	None	459
2013	4650	2000	3250	3250	None	None	469
2014	4650	2000	3250	3250	None	None	478
2015	4650	2250	3250	3250	None	None	490

Rules shown are for fiscal years, not calendar years. Dollar values are nominal. Values of deductions and deduction caps shown are per month, not per year. Dependent care deduction caps were eliminated in fiscal year 2009. I only show rules above that change over the 1996-2015 period for the contiguous United States. FMV stands for fair market value.

Table A1. Changes in federal SNAP rules, 1996-2015 (continued)

	Standard deduction ($) for households of size:				Maximum allotment ($) for households of size:								
Year	1-3	4	5	6+	1	2	3	4	5	6	7	8	+
1996	134	134	134	134	119	218	313	397	472	566	626	716	90
1997	134	134	134	134	120	220	315	400	475	570	630	720	90
1998	134	134	134	134	122	224	321	408	485	582	643	735	92
1999	134	134	134	134	125	230	329	419	497	597	659	754	94
2000	134	134	134	134	127	234	335	426	506	607	671	767	96
2001	134	134	134	134	130	238	341	434	515	618	683	781	98
2002	134	134	134	134	135	248	356	452	537	644	712	814	102
2003	134	134	147	168	139	256	366	465	553	663	733	838	105
2004	134	134	149	171	141	259	371	471	560	672	743	849	106
2005	134	134	153	175	149	274	393	499	592	711	786	898	112
2006	134	134	157	179	152	278	399	506	601	722	798	912	114
2007	134	139	162	186	155	284	408	518	615	738	816	932	117
2008	134	143	167	191	162	298	426	542	643	772	853	975	122
2009*	144	147	172	197	188	345	494.5	628	745.5	895	989	1130	141
2010	141	153	179	205	200	367	526	668	793	952	1052	1202	150
2011	142	153	179	205	200	367	526	668	793	952	1052	1202	150
2012	147	155	181	208	200	367	526	668	793	952	1052	1202	150
2013	149	160	187	214	200	367	526	668	793	952	1052	1202	150
2014*	152	163	191	219	189.92	348.67	499.42	635	753.58	904.33	999.75	1142.42	142.67
2015	155	165	193	221	194	357	511	649	771	925	1022	1169	146

Rules shown are for fiscal years, not calendar years. Dollar values are nominal. Values of deductions and allotments shown are per month, not per year. Maximum allotments change once each between the beginning and end of fiscal years 2009 and 2014. For these years, I construct averages of the maximum allotments weighted by the percentage of the year each allotment was in place. I present those averages here and use them in the construction of the simulated eligibility measure. For households with more than 8 members, maximum allotment is equal to the allotment for households of 8 members plus the number of members in excess of 8 multiplied by the last "+" column, e.g. the maximum allotment for a household of 10 in 2015 is $1,169 + $146 × 2 = $1,461.

Table A1. Changes in federal SNAP rules, 1996-2015 (continued)

	Monthly federal poverty level (FPL) ($) for households of size:								
Year	1	2	3	4	5	6	7	8	+
1996	623	836	1050	1263	1476	1690	1903	2116	214
1997	645	864	1082	1300	1519	1737	1955	2174	219
1998	658	885	1111	1338	1565	1791	2018	2245	227
1999	671	905	1138	1371	1605	1838	2071	2305	234
2000	687	922	1157	1392	1627	1862	2097	2332	235
2001	696	938	1180	1421	1663	1905	2146	2388	242
2002	716	968	1220	1471	1723	1975	2226	2478	252
2003	739	995	1252	1509	1765	2022	2279	2535	257
2004	749	1010	1272	1534	1795	2057	2319	2580	262
2005	776	1041	1306	1571	1836	2101	2366	2631	265
2006	798	1070	1341	1613	1885	2156	2428	2700	272
2007	817	1100	1384	1667	1950	2234	2517	2800	284
2008	851	1141	1431	1721	2011	2301	2591	2881	290
2009	867	1167	1467	1767	2067	2367	2667	2967	300
2010	903	1215	1526	1838	2150	2461	2773	3085	312
2011	903	1215	1526	1838	2150	2461	2773	3085	312
2012	908	1226	1545	1863	2181	2500	2818	3136	319
2013	931	1261	1591	1921	2251	2581	2911	3241	330
2014	958	1293	1628	1963	2298	2633	2968	3303	335
2015	973	1311	1650	1988	2326	2665	3003	3341	339

Rules shown are for fiscal years, not calendar years. Dollar values are nominal. Values of the FPL shown are per month, not per year. For households with more than 8 members, FPL is equal to the FPL for households of 8 members plus the number of members in excess of 8 multiplied by the last “+” column, e.g. the FPL for a household of 10 in 2015 is $3,341 + $339 × 2 = $4,019.

Table A2. State SNAP expansions, 1996-2015

State	Month/ year	BBCE expansions: BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Other expansions: Non-BBCE vehicle rule/asset limit	SMED ($)
AL	09/2001						Exclude all vehicles	
	02/2010	X	All elderly or disabled	200	100	None		
			No or some elderly or disabled	130	None	None		
	10/2014							165
AK*	09/2001						Exclude one vehicle per driver*	
AZ	06/2003						Exclude all vehicles	
	06/2007	X	All	185	None	None		
AR	09/2001						Exclude one vehicle per household	
	11/2011							103
CA	01/2004						Exclude all vehicles	
	07/2009	X	Any children under 18	130	None	None		
	04/2011	X	All	130	None	None		
	05/2013	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
	07/2014	X	All	200	None	None		
CO	09/2001						Exclude one vehicle per household	
	09/2002						Exclude all vehicles	
	03/2011	X	Any elderly or disabled	200	100	None		
			No elderly or disabled	130	100	None		
CT	09/2002						Exclude equity value of one vehicle up to $9,500	
	05/2007						Exclude all vehicles	
	07/2009	X	All	185	None	None		
DE*	02/2000*	X	All	200*	None	None		
DC	09/2001						Exclude all vehicles	
	04/2010	X	All	200	None	None		

BBCE stands for broad-based categorical eligibility. GI and NI stand for gross income and net income, respectively. SMED stands for standard medical expense deduction.

Table A2. State SNAP expansions, 1996-2015 (continued)

		BBCE expansions					Other expansions	
State	Mo./year	BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Non-BBCE vehicle rule/asset limit	SMED ($)
FL*	09/2001						Count equity value of vehicles after excluding combined equity value of as many vehicles as there are drivers up to $8500*	
	01/2009						Exclude all vehicles	
	07/2010	X	All	200	None	None		
GA	12/2005						Exclude all vehicles	
	03/2008	X	All elderly or disabled	200	None	None		
			No or some elderly or disabled	130	None	None		
HI	09/2002						Exclude all vehicles	
	10/2010	X	All	200	None	None		
ID	05/2007						Exclude one vehicle per adult	
	06/2009	X	All	130	100	None		
	06/2011	X	Any elderly or disabled	None	100	Same as non-BBCE	Exclude one vehicle per adult; $5,000 asset limit	
			No elderly or disabled	130	100	Same as non-BBCE		
	11/2013							144
IL	09/2001						Exclude one vehicle per household plus one vehicle per driver	
	03/2010	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
	10/2010							210
IN	01/2002						Exclude all vehicles	
IA	06/2004						Exclude one vehicle per household	
	10/2007							105
	01/2011	X	All	160	None	None		
KS	09/2001						Exclude all vehicles	
	10/2010							140
	04/2014						Exclude one vehicle per household	

Table A2. State SNAP expansions, 1996-2015 (continued)

State	Mo./year	BBCE expansions: BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Other expansions: Non-BBCE vehicle rule/asset limit	SMED ($)
KY	09/2001						Exclude all vehicles	
	06/2010	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
LA	09/2001						Exclude all vehicles	
	05/2010	X	Any elderly or disabled	None	100	None		
			No elderly or disabled	130	100	None		
	08/2014	X	All	BBCE ended; revert to federal rules				
ME*	09/2000*	X	Any children 18 and in high school or under*	200*	None	None		
	12/2006	X	Any children 18 and in high school or under*	185	None	None	Exclude one vehicle per household	
	08/2010	X	All	185	None	None		
MD*	03/2001	X	Any children under 18 or aged 18-19 and graduating in 19th year*	200	None	None		
	10/2001						Exclude all vehicles	
	10/2010	X	All	200	None	None		
MA	09/2001						Exclude all vehicles	
	11/2001	X	Any children under 19	200	None	None		
	04/2008							
	06/2008	X	Any elderly or disabled or any children under 19	200	None	None		90
			No elderly, disabled, or under 19	130	100	None		
	03/2014						155	155

Table A2. State SNAP expansions, 1996-2015 (continued)

State	Mo./year	BBCE expansions: BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Other expansions: Non-BBCE vehicle rule/asset limit	SMED ($)
MI*	01/1996*						Exclude one vehicle per household	
	10/1999						Revert to federal rules	
	10/2000*	X	All	200*	None	None		
	10/2011	X	All	200	None	Same as non-BBCE	Exclude $15,000 combined FMV of vehicles; $5,000 asset limit	
	01/2012	X	All	200	None	Same as non-BBCE	Exclude one vehicle plus $15,000 combined FMV of remaining vehicles; $5,000 asset limit	
MN*	06/2003						Exclude $7,500 FMV from each vehicle*	
	12/2006	X	Any elderly or disabled	165	None	$7,000 after excluding all vehicles		
			No elderly or disabled	130	None	$7,000 after excluding all vehicles		
	11/2010	X	All	165	None	None		
	11/2011						Revert to federal rules	
MS	06/2003						Exclude all vehicles	
	06/2010	X	Any elderly or disabled	None	100	None		
			No elderly or disabled	130	100	None		
MO	09/2001						Exclude all vehicles	
	09/2011							165
MT*	02/1996*						Exclude one vehicle per household	
	06/2004						Exclude all vehicles	
	03/2009	X	Any elderly or disabled	None	100	None		
			No elderly or disabled	185	100	None		
	09/2010	X	All	200	100	None		

Table A2. State SNAP expansions, 1996-2015 (continued)

		BBCE expansions					Other expansions	
State	Mo./year	BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Non-BBCE vehicle rule/asset limit	SMED ($)
NE	01/2002						Exclude FMV of one vehicle up to $12,000	
	10/2011	X	Any elderly or disabled	None	100	$25,000 after excluding non-liquid assets		
			No elderly or disabled	130	100	$25,000 after excluding non-liquid assets		
NV	09/2001						Exclude one vehicle per household	
	04/2009	X	All	200	None	None		
NH*	09/2001						Exclude one vehicle per adult	
	12/2003							83
	05/2009	X	Any dependent children under age 22*	185	None	None		
	10/2015							115
NJ	09/2001						Exclude FMV of one vehicle up to $9,500	
	05/2007						Exclude all vehicles	
	04/2010	X	All	185	None	None		
NM	01/2002						Exclude all vehicles	
	04/2010	X	All	165	None	None		
NY	01/2002						Exclude one vehicle per driver; count equity value of other vehicles	
	01/2008	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
	03/2009	X	Any elderly or disabled or any dependent care expenses	200	None	None		
			No elderly, disabled, or dependent care expenses	130	None	None		
NC	09/2001						Exclude one vehicle per adult	
	01/2009						Exclude all vehicles	
	07/2010	X	All	200	None	None		

Table A2. State SNAP expansions, 1996-2015 (continued)

		BBCE expansions					Other expansions	
State	Mo./year	BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Non-BBCE vehicle rule/asset limit	SMED ($)
ND*	10/2000*	X	All	None	100*	None		
	10/2010	X	All	200	100	None		
	04/2013							165
OH	09/2001						Exclude all vehicles	
	10/2008	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
OK	09/2001						Exclude equity value of one vehicle up to $5,000	
	09/2002						Exclude one vehicle per adult; count combined equity of other vehicles in excess of $5,000	
	01/2009						Exclude all vehicles	
	06/2009	X	Any elderly or disabled	None	100	None		
			No elderly or disabled	130	100	None		
OR	12/2000	X	All	185	None	None		
	10/2001						Count equity value of vehicles in excess of $10,000	
PA	09/2001						Exclude one vehicle per household	
	10/2008	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
	08/2009	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	160	None	None		
	06/2012	X	Any elderly or disabled	200	None	$9,000 after excluding one vehicle		
			No elderly or disabled	160	None	$5,500 after excluding one vehicle		
	05/2015	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	160	None	None		

Table A2. State SNAP expansions, 1996-2015 (continued)

State	Mo./year	BBCE expansions: BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Other expansions: Non-BBCE vehicle rule/asset limit	SMED ($)
RI	06/2003						Exclude one vehicle per adult up to a maximum of two	
	04/2009	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	185	None	None		
	10/2012							141
SC	04/2001	X	All	200	None	None		
	10/2001						Exclude one vehicle per driver	
	04/2009	X	Any elderly or disabled	200	None	None		
			No elderly or disabled	130	None	None		
SD	09/2001						Exclude one vehicle per household	
	05/2008							165
TN	12/2003						Exclude all vehicles	
TX	09/2001	X	All	165	None	Same as non-BBCE	Exclude FMV of $15,000 for first vehicle, $4,650 for all others, and all non-vehicle non-liquid assets; $5,000 asset limit	
	10/2007							102
UT	09/2001						Exclude FMV of $8,000 for first vehicle	
	01/2007						Count equity value of first vehicle in excess of $8,000 and equity value of other vehicles	
	10/2007						Exclude all vehicles	
VT	09/2001						Exclude one vehicle per adult up to a maximum of two	
	12/2008							138
	01/2009	X	All	185	None	None		

Table A2. State SNAP expansions, 1996-2015 (continued)

		BBCE expansions					Other expansions	
State	Mo./year	BBCE change	Applicable households	GI limit (%FPL)	NI limit (%FPL)	Asset limit	Non-BBCE vehicle rule/asset limit	SMED ($)
VA	07/2003						Exclude one vehicle per household and any vehicles with FMV of $7,500 or less	
	12/2003						Exclude all vehicles	
	10/2011							140
WA	05/2004	X	All	130	None	None		
	10/2008	X	All	200	None	None		
WV	09/2001						Exclude all vehicles	
	10/2008	X	All	130	None	None		
	05/2013	X	All elderly or disabled and no earned income	200	None	None		
			No or some elderly or disabled or some earned income	130	None	None		
WI	09/2001						Exclude all vehicles	
	06/2004	X	All	200	None	None		
WY	09/2001						Exclude combined FMV of $12,000 for first two vehicles if household includes married couple; otherwise exclude FMV of $12,000 for first vehicle only	
	01/2006							103
	10/2011						Exclude all vehicles	

Table A2. State SNAP expansions, 1996-2015: Notes

General	Some states reference the number of household drivers in their rules, which generally means the number of adults plus the number of licensed teenagers using household vehicles for purposes of transportation to work, school, etc. I cannot determine which teenagers are drivers and therefore use the number of adults in place of the number of drivers in my baseline simulated eligibility measure.
AK	Technically, AK excludes all vehicles necessary for transport to meet basic needs. I have no indicator for the necessity of household vehicles, so I therefore choose to exclude one vehicle per driver.
DE	I am unable to resolve conflicting information concerning the start date of BBCE in DE; other sources place the start date at 08/2001. Technically, DE would not have required a gross income test from 02/2000 to 08/2001, but I impose a 200% limit on gross income during this time period.
FL	FL's vehicle rule technically only allows the exclusion of the combined equity value of as many vehicles as there are drivers that meet work requirements, or one vehicle if no drivers meet work requirements. I do not consider work requirements and instead use the number of adults in the construction of the simulated eligibility measure.
ME	I am unable to resolve conflicting information concerning the start date of BBCE in ME; other sources place the start date at 12/2001. Technically, ME would not have required a gross income test from 09/2000 to 12/2001, but I impose a 200% limit on gross income during this time period. ME's restriction to households with children also requires that they live with a parent or caretaker relative; I do not apply this restriction in the construction of the simulated eligibility measure.
MD	MD's restriction to households with children requires those children to be "related" to applicant; I do not apply this restriction or the graduation restriction in the construction of the simulated eligibility measure.
MI	I am unable to independently determine the start date of MI's 1996 vehicle rule and therefore follow the SNAP Policy Database. I am unable to resolve conflicting information concerning the start date of BBCE in MI; other sources place the start date at 10/2001. Technically, MI would not have required a gross income test from 10/2000 to 10/2001, but I impose a 200% limit on gross income during this time period.
MN	MN's vehicle rule technically counts the "loan value" of each vehicle in excess of $7,500 toward the asset limit; I lack information on the loan value and instead use the fair market value in the construction of the simulated eligibility measure.
MT	I am unable to independently determine the start date of MT's 1996 vehicle rule and therefore follow the SNAP Policy Database.
NH	NH's restriction to households with children requires those children be "dependent" and to contain a relative to the dependent child; I do not apply these restrictions in the construction of the simulated eligibility measure.
ND	I am unable to resolve conflicting information concerning the start date of BBCE in ND; other sources place the start date at 10/2001. Technically, ND would not have required a net income test from 10/2000 to 10/2001, but I impose a 100% limit on net income during this time period.

Table A3. Federal and state SNAP policy sources

Sources	Information
SNAP Policy Database (USDA ERS 2018)	I use this database from the USDA Economic Research Service (ERS) as my primary source of information on state Supplemental Nutrition Assistance Program (SNAP) policies and gather information from other sources to supplement it. It provides monthly state-level information on the treatment of vehicles and broad-based categorical eligibility (BBCE) policies, as well as other state policies I do not use to construct the SEV. The variables provided generally summarize these policies and do not provide all details needed to use to determine household eligibility.
Mathematica Policy Research Reports (Trippe and Gillooly 2010; Laird and Trippe 2014)	I use these reports to fill in details about state-level treatment of vehicles and BBCE policies and information about the timing of policy changes. These reports aggregate information from the state agencies administering SNAP concerning current and past BBCE changes and non-BBCE vehicle exclusions as of 2010 and 2014. They provide detailed information about the groups various BBCE expansions apply to; the gross income, net income, and asset tests applied to these groups; and the specifics of non-BBCE vehicle policies among other policy details.
Aussenberg and Falk (2019)	I use this report to fill in details about state BBCE policies as of 2018.
Horng and Dean (2002, 2008)	I use these reports to fill in details about state treatment of vehicles as of 2002 and 2008.
Technical Documentation for the Supplemental Nutrition Assistance Program Quality Control Database and the QC Minimodel (n.d.)	I use this technical documentation for SNAP quality control data as my primary source of information on state standard medical expense deductions (SMEDs). It provides annual information from 1996 to 2016 about which states have implemented SMEDs as well as their size and implementation date.
Various state SNAP policy manuals, reports, and memos; state SNAP administrators	I use a combination of state-issued SNAP policy manuals, reports, and memos to verify the timing and details of the state SNAP policy changes outlined above when the timing and/or details of those changes are unclear from the sources listed above or when those sources contradict each other. When those sources cannot clarify the timing or details of state policies, I contact state SNAP administrators directly by phone or e-mail.
Food Stamp Act of 1977 and Amendments (2004); USDA FNS (2018a, 2018b, 2019d, n.d.)	I gather information on federal eligibility and benefit determination using the Food Stamp Act and its amendments in conjunction with articles from the USDA FNS on the legislative history of SNAP, federal eligibility determination, the federal net income and benefit formulas, and how determinants of those formulas vary over time (e.g., the values of the standard deduction and maximum allotment by household size, various deduction caps, and the resource limit).
HHS ASPE (2019)	I gather information on the federal poverty level over time from in order to compare household gross and/or net income to the federal and state limits.

Figure A1. Least restrictive treatment of vehicles in asset test for non-elderly, non-disabled households

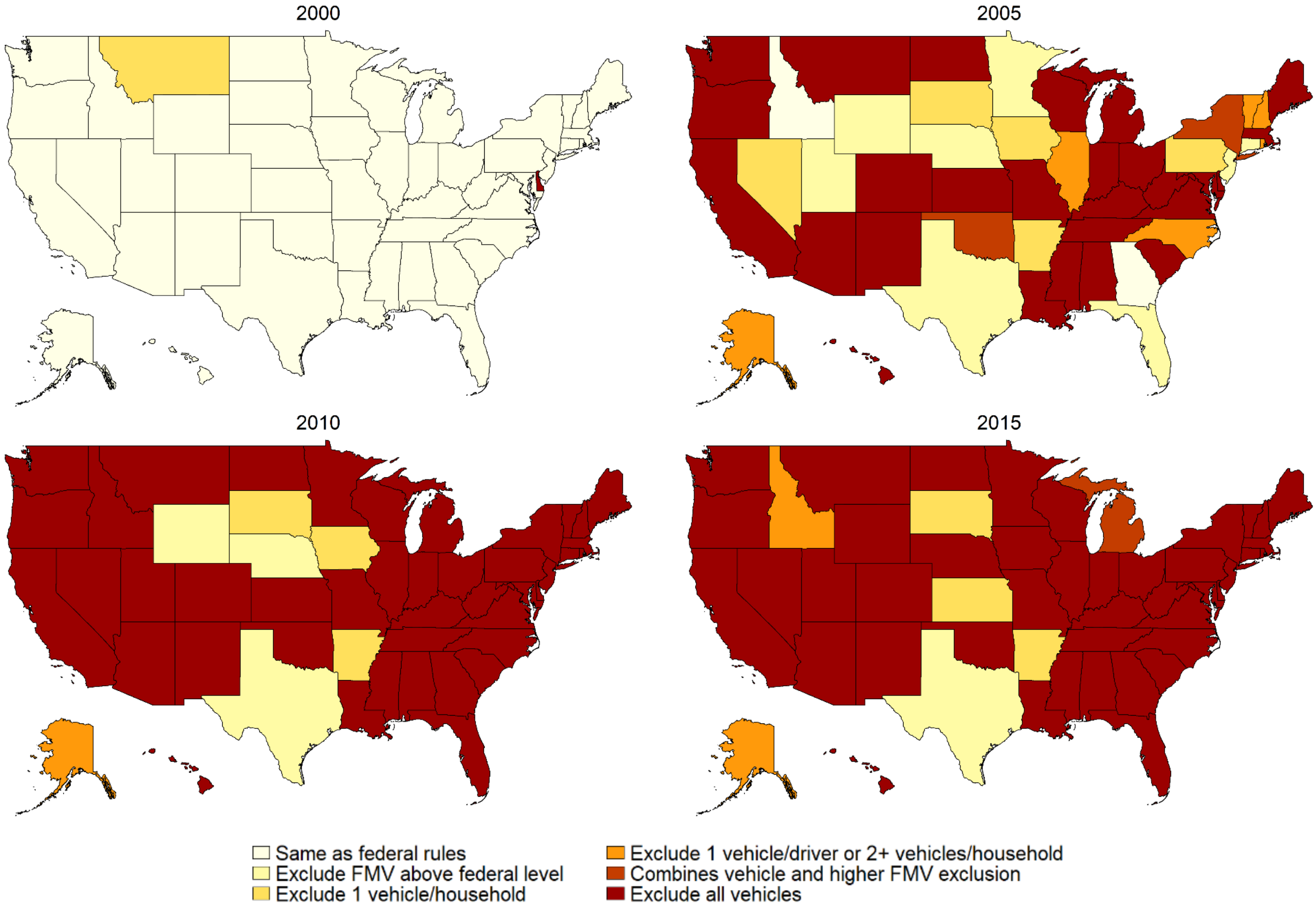
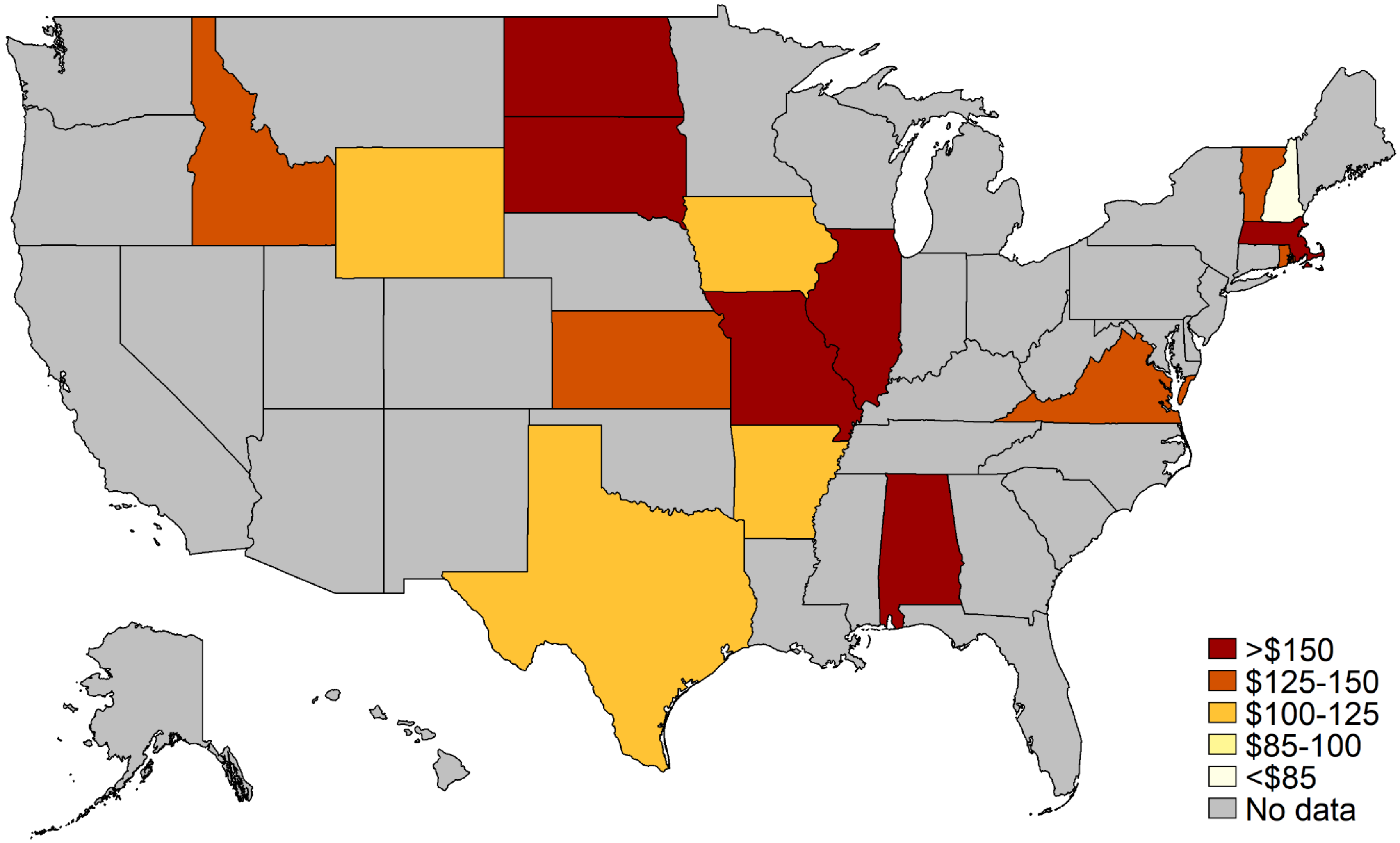
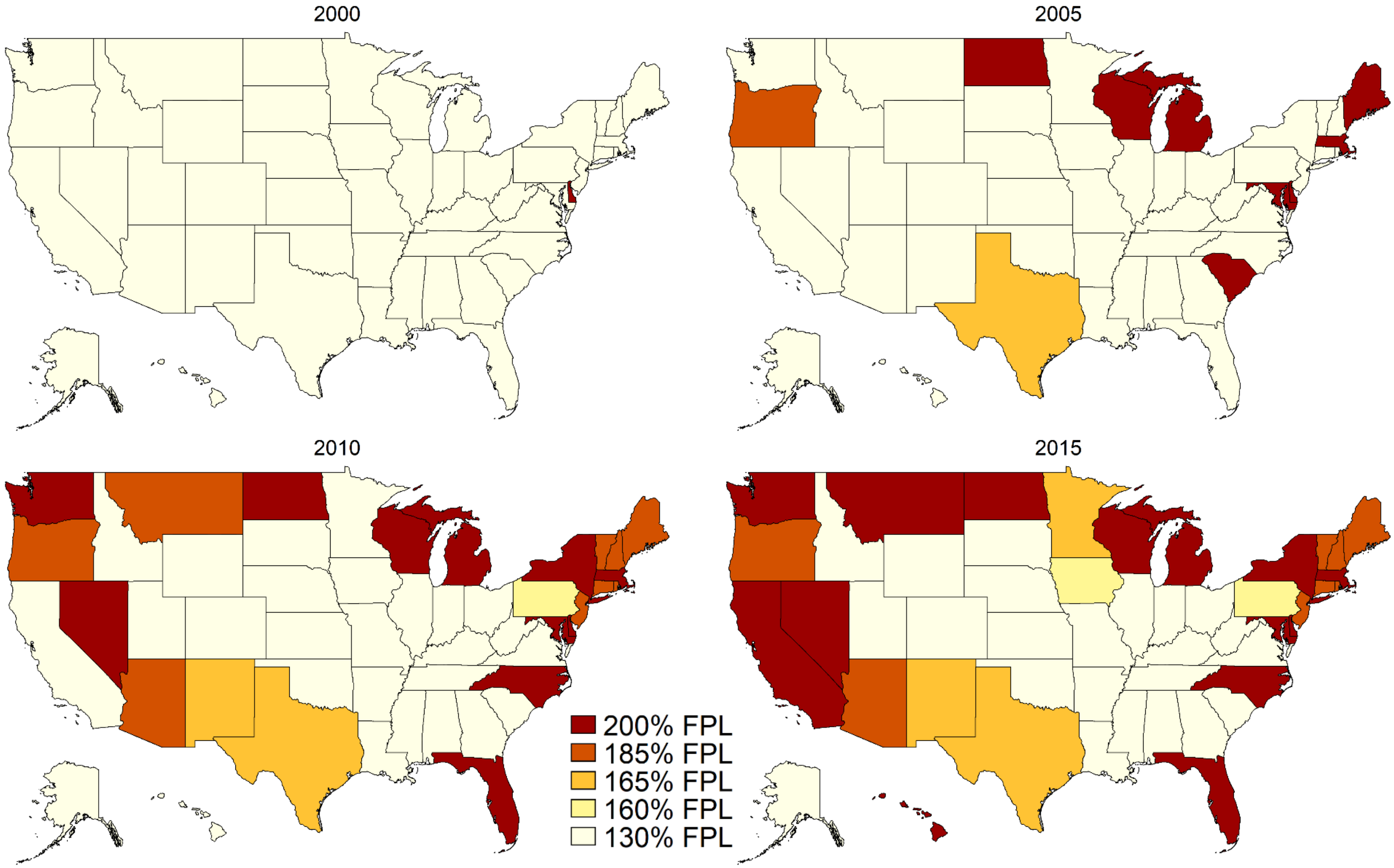


Figure A2. Standard medical expense deductions for elderly and/or disabled households



SMEDs shown are those in place at the end of 2015 and are in nominal dollar values.

Figure A3. Least restrictive gross income limits for non-elderly, non-disabled households



1. SIPP household sample

To construct the simulated eligibility variable (SEV), I apply the annual state and federal rules detailed above to a common sample of households. I use a sample of households from the Survey of Income and Program Participation (SIPP) (2019) containing households from all 50 states and Washington, D.C. and spanning the years 1996-1999, 2001-2005, 2009-2011, and 2013.[141] I use the SIPP because it includes individual-level and household-level information I need to determine households' SNAP eligibility and potential benefit size. I include households from different states and years to achieve representation of the United States on a national level over this study's sample period. The households in the sample are diverse enough in terms of income, expenses, assets, and other characteristics to provide ample variation to the simulated eligibility measure as I apply different rules to the sample.

The SIPP consists of a series of household panels, each lasting 2.5 to 4 years with waves lasting four months (12 months for the most recent 2014 panel). For each household in the sample, I combine information over three waves (or use information from one wave for the 2014 panel) to construct household-year-level observations and assign to each observation the year most closely corresponding with the period covered by those 12 months.[142] For all years but 2013, I merge these collapsed observations with annual information from periodic topical modules containing information on household assets, liabilities, and certain expenses.[143]

[141] I exclude years from my sample for which there is no corresponding topical module on assets, liabilities, and expenses available.

[142] Accordingly, households appear multiple times in the sample, although they appear with different income, assets, expenses, and other characteristics insofar as these changed over the panel. No household-year-level observations are repeated. Specifically, in my constructed sample: 1996, 1997, 1998, and 1999 are waves 1-3, 4-6, 7-9, and 10-12 of the 1996 panel, respectively. 2001, 2002, and 2003 are waves 1-3, 4-6, and 7-9 of the 2001 panel, respectively. 2004 and 2005 are waves 1-3 and 4-6 of the 2004 panel, respectively. 2009, 2010, and 2011 are waves 2-4, 5-7, and 8-10 of the 2008 panel, respectively.

[143] These topical modules correspond to the last wave of each three-wave group used to construct reference years.

I drop households that do not merge to a topical module or that do not have a reference person with complete information for 12 months. I do not separate households into sub-households or sub-families due to a lack of necessary group-level information for these units. For individual and household characteristics that I cannot reasonably average across months, I use information from the last month of the reference year.[144] I average monthly information on income over the 12 months in each year to construct average monthly gross income and earned income. I convert information provided for only the last month of the reference year, the four months in the linked topical module, or for the year to a monthly level when appropriate.[145] I apply information on assets and liabilities recorded in the topical module to the corresponding reference year.[146] Information on vehicles is available for up to three vehicles per household on vehicle fair market value, debt, equity, and whether each vehicle's primary use is for business purposes and/or the transportation of disabled household members.[147]

Summary statistics for the SIPP household sample are presented in Table A4. 34% of households include children, 36% include elderly members (defined as those aged over 60 for the purposes of determining SNAP eligibility), 19% include members with a disability, and 47% include elderly or disabled members.[148] The average monthly gross income (total income less certain exclusions, e.g. the income of household members aged under 18 and in high school) was

[144] This information includes age/elderly status, disability status, presence of a married couple, presence of children, household size, school attendance, and receipt of TANF or SSI benefits.

[145] This information includes dependent care costs, child support costs, shelter costs, and out-of-pocket medical costs.

[146] This information includes breakdowns of assets and liabilities at the household and individual levels, which I aggregate into countable and non-countable resources according to federal or state SNAP rules.

[147] Detailed vehicle information is only available for up to three "standard" vehicles, which includes cars, trucks, and vans. While some information is available for other types of vehicles, it is not consistently available throughout all years or at a detailed enough level to use. I therefore limit each household to the three standard vehicles for which information is available. 5.3% of the households in the sample have more than three vehicles.

[148] I count individuals as elderly if they are 60 years old or older in the last month of the reference year in line with federal SNAP rules. I count individuals as disabled if they report a disability in the last month of the reference year as I lack information on receipt of the specific benefits that confer disability status in line with federal SNAP rules.

Table A4. SIPP household sample summary statistics

	Mean	Std. dev.	Min.	Max.	N
# members	2.56	1.49	1	20	342,615
Any < 18	0.34	0.48	0	1	342,615
# < 18	0.66	1.09	0	12	342,615
Any elderly	0.36	0.48	0	1	342,615
# elderly	0.50	0.73	0	6	342,615
Any disabled	0.19	0.39	0	1	342,615
# disabled	0.22	0.50	0	7	342,615
Any elderly or disabled	0.47	0.50	0	1	342,615
# elderly or disabled	0.66	0.79	0	7	342,615
# adults	1.90	0.84	0	11	342,615
Out-of-pocket medical costs ($)	185.96	267.61	0	4,492.96	161,652
Dependent care costs ($)	18.53	126.23	0	4,867.38	342,615
Child support ($)	15.76	107.32	0	2,970.18	342,615
Shelter costs ($)	908.06	856.60	0	41,185.48	342,615
Total income ($)	5,206.34	4,969.95	0	100,220.97	342,615
Gross income ($)	5,190.42	4,962.57	0	99,441.26	342,615
Earned income ($)	4,126.77	5,058.59	0	97,468.84	342,615
Countable non-vehicle assets ($)	84,190.49	1,090,296.28	0	272,134,144	342,615
Countable non-vehicle liquid assets ($)	56,044.61	1,068,687.51	0	271,916,768	342,615
Any vehicles	0.85	0.36	0	1	342,615
# vehicles	1.52	0.94	0	3	342,615
Total fair market value of vehicles ($)	11,278.72	11,114.51	0	126,645.36	342,615
Total equity in vehicles ($)	7,526.65	8,655.48	0	126,645.36	342,615
Average FMV per vehicle ($)	7,347.12	5,348.83	0	46,331.32	291,208
Average equity per vehicle ($)	4,938.56	4,540.75	0	46,331.32	291,208
# household-years	342,615				
# individual-years	876,544				

Unit of observation is the household-year. Dollar values adjusted to 2010 dollars. Out-of-pocket medical costs shown for households with elderly and/or disabled members only. Average FMV or equity per vehicle shown for households with vehicles only.

$5,190 in 2010-adjusted dollars. The average amount of non-vehicle assets counted toward the federal SNAP asset limit was $84,190, though 50% of households had $3,276 in these assets or less. 85% of households had at least one vehicle, and households had 1.52 vehicles on average. The average fair market value per vehicle was $7,347, and the average equity per vehicle was $4,939.

2. Simulated eligibility variable construction

I construct the SEV for each state s and year t as

$$SEV_{st} = \frac{\text{\# SIPP individuals in practically eligible households}_{st}}{\text{Total \# SIPP individuals} = 876{,}544}$$

I define "practically eligible" to mean that a household is both determined eligible for SNAP by the rules in place in a given state and year and would receive a positive monthly benefit if they applied for SNAP. The second part of this definition is necessary as some state-level expansions during the sample period can technically grant eligibility to households who would receive a zero SNAP benefit according to the federally defined benefit formula. Generally, this is the case for households with three or more members and net income of about 115% the federal poverty level (FPL). Households with one or two members are always eligible for a minimum benefit of $10 to $16 during the sample period. I construct the SEV as the percentage of the total number of individuals comprising the household-year observations. Therefore, households with more members are weighted more heavily than households with fewer members and the SEV more closely resembles the SNAP participation rate, which is the percentage of individuals in the population participating in the program.

SNAP eligibility is determined at the household level. I follow a basic process to determine each SIPP household's practical eligibility in each state and year.

2.1 Adjust for inflation

First, I adjust each household's finances for inflation. I use the BLS Consumer Price Index for urban consumers to adjust dollar values of household income, assets, and expenses from the original year to the relevant year. This is important because while some rules reference measures like the FPL that are adjusted on a yearly basis, others reference static dollar values.

2.2 Apply federal rules

Second, I apply federal rules to determine household eligibility at the federal level. If households are eligible at the federal level, they are eligible at the state level as states cannot implement rules that restrict eligibility. Federal rules allow for eligibility through two pathways.

The first pathway is through "categorical eligibility." Households are categorically eligible for SNAP if all members receive Supplemental Security Income (SSI) or Temporary Assistance for Needy Families (TANF) cash assistance (or General Assistance in some states). The SIPP provides information on individual receipt of SSI and TANF, so I consider a household eligible if all members report receiving one or both types of assistance.

The second pathway is through meeting each of three tests. The first of these tests is a gross income test**.** Households must have gross income of 130% of the FPL or below. Gross income is a household's total income less income from excluded sources. Several types of income are excluded – most of which are typically small relative to overall income – but I define gross income for my purposes as total household income less the income of household members under age 18 and enrolled in school, as the SIPP provides the information necessary to exclude this kind of income. I calculate average monthly gross income for each SIPP household and compare it to 130% of the FPL. If it is equal or below, the household passes this test. If a household includes elderly or disabled members, it is not subject to the gross income test and need only pass the net income and asset tests. The gross income test does not change during the sample period.

The second of these tests is a net income test. Households must have net income of 100% of the FPL or below. Net income is a household's gross income minus allowable deductions. These deductions include 20% of earned income, a standard deduction, a dependent care deduction (subject to a cap before 2009), child support payments, out-of-pocket medical expenses for elderly or disabled household members above $35 per month, and excess shelter costs (subject to a cap).[149] I detail changes in the standard deduction, dependent care deduction cap, and excess shelter costs deduction cap in Table A1. I calculate average monthly net income for each SIPP household and compare it to 100% of the FPL. If it is equal or below, the household passes this test.

The third and last of these tests is an asset test, also called a countable resource test. Households must have countable resources below the asset limit. The asset limit varies depending on whether a household includes elderly or disabled members. For most of the sample period, the limit was $2,000 for households without such members and $3,000 for households with any such members. I detail changes in the limit in Table A1. Countable resources generally include those resources available to the household to use to purchase food. These resources exclude assets like a first home and lot, personal property, and retirement savings. The excess value of vehicles is counted toward the asset limit, after excluding certain licensed vehicles, including those used to produce income, those used to transport disabled household members, or

[149] In some states, child support is excluded from gross income instead of deducted to determine net income, but the result is the same for determining net income. I do not distinguish between excluding and deducting states. Shelter costs include house mortgage, rent, taxes, and/or certain utilities. The excess shelter cost deduction is calculated as the value of shelter costs in excess of half of the sum of gross income less the earned income, standard, dependent care, child support, and out-of-pocket medical expenses. Households with elderly or disabled members are not subject to an excess shelter costs deduction cap.

those that would sell for less than $1,500, among other criteria. For as many vehicles as there are drivers in the household, the excess value is the fair market value (FMV) over $4,650.[150] If the household has more vehicles than drivers, the excess value for those vehicles is the greater of the equity value or the FMV over $4,650. I calculate the excess value of vehicles in each year. As there is no way to discern the vehicles a household would choose to exclude or those vehicles to which it would apply the FMV test as opposed to the combined FMV/equity test, I choose to match vehicles to the combination of available tests given a household's characteristics that minimizes the excess vehicle value and subsequently minimizes countable resources. I then add this excess value to the household's countable non-vehicle resources and compare the sum to the relevant asset limit. If it is equal or below, the household passes this test.

2.3 Apply state rules

States implement various expansions which in practice loosen one or more of the tests mentioned above, which I detail in Table A2. These expansions include broad-based categorical eligibility (BBCE) expansions, alterations to the valuation of vehicles, and standard medical expense deductions (SMEDs). BBCE expansions eliminate or raise the gross income, net income, and/or asset limits for all households or for a subset of households. These kinds of expansions may make households eligible who would qualify for a benefit if not for being above one of the income or asset limits. If a BBCE expansion applies to a household and that household meets the altered gross income, net income, and/or asset tests, they are eligible for SNAP (but may not be practically eligible).

Outside of BBCE expansions, states can align their vehicle rules to those used in other programs like TANF. In this way, states can eliminate vehicles from the asset test entirely,

[150] The limit was $4,600 in 1996 before being changed to $4,650 in 1997 and subsequent years.

exclude one or more vehicles, or increase the deduction applied to vehicles' FMV or equity value. States can also implement SMEDs, which are a type of standard deduction optional for households with elderly or disabled members to take in place of the deduction for monthly out-of-pocket medical expenses above $35 used to calculate net income. Households with such members and out-of-pocket medical expenses below the value of the SMED plus $35 would find it advantageous to take the standard deduction as it lowers their net income and may make their household eligible for SNAP or increase their benefit. Households may become eligible given the alterations to the asset or net income test that these tests imply.

I apply the SNAP rules in place in each state and year in order to determine the eligibility of each SIPP household in each state and year.

2.4 Determine potential benefit and practical eligibility

If I determine a SIPP household as eligible in a state and year by the federal or state rules in place, I then determine the potential benefit the household would receive if they participated in SNAP in that state and year. The monthly benefit amount is equal to a maximum allotment minus 30% of net income.[151] There is no minimum allotment except for households with one or two members. If a household is determined to be eligible at the federal and/or state level and has a positive potential benefit, I designate it "practically eligible."

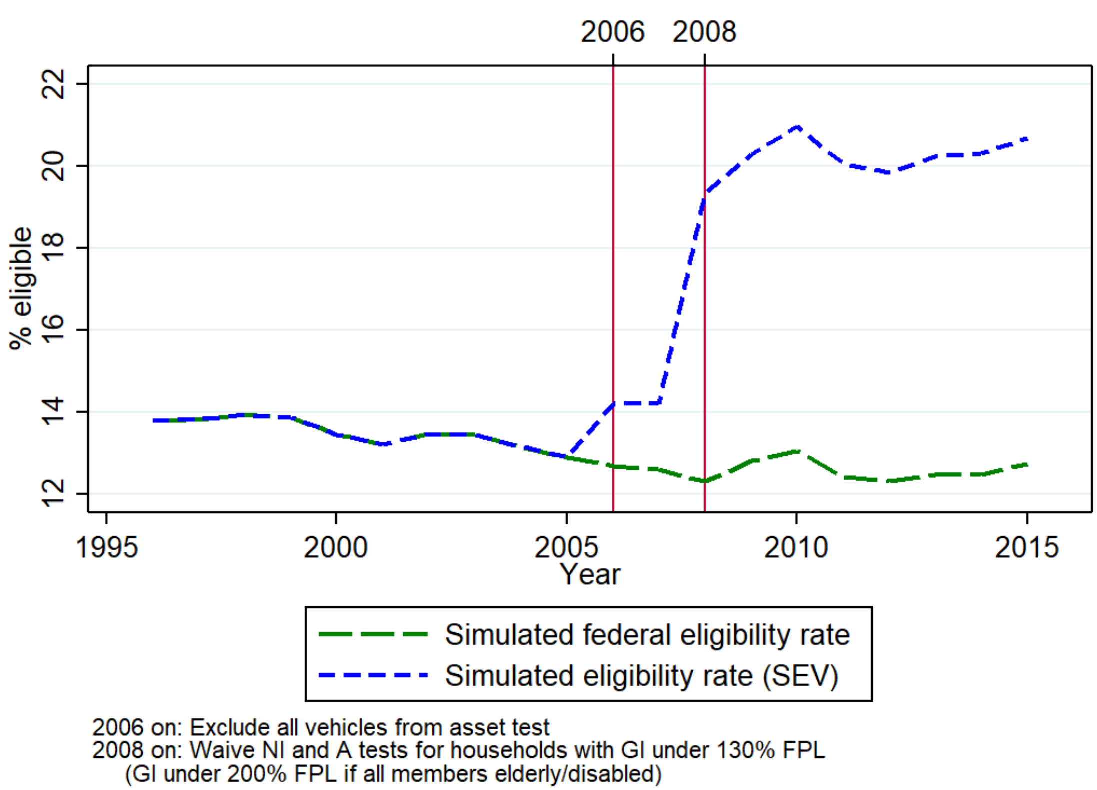
I then sum the number of SIPP individuals in each practically eligible SIPP household under the rules in place in the given state and year and divide that by the total number of SIPP individuals. The resulting number, which I express in percentage points, is the SEV for that state and year. I repeat this process for all state-year combinations in the sample.

[151] Changes in maximum allotments over time are detailed in Table A1.

2.5 Example

I present Georgia as an example to illustrate this process. Figure A4 shows the value of the SEV in Georgia from 1996 to 2015 as well as the simulated federal-rule eligibility rate, or the percentage of the SIPP sample that would be eligible for a positive benefit if Georgia did not expand SNAP. Prior to 2006, Georgia did not alter the SNAP rules in any way, so SIPP households would only be eligible in Georgia if they were also eligible under the federal rules. Therefore, the SEV and the simulated federal-rule eligibility rate were equal. In 2006, Georgia began excluding all vehicles from the asset test. SIPP households would then be eligible if they were eligible at the federal level or if they met the federal gross income and net income tests as well as the altered asset test that excludes vehicles. The SEV increased in value beyond the simulated federal-rule eligibility rate during this time period by about 1.5 percentage points, representing the households that were made eligible by the loosened state vehicle rule. In 2008, Georgia implemented a BBCE expansion which made a household eligible if a) it had gross income under 130% of the FPL or b) all household members were elderly or disabled and the household had gross income under 200% of the FPL. Households meeting either of these conditions were not subject to the net income or asset test. If SIPP households met either of these conditions or met the gross income, net income, and altered vehicle tests, they would then be eligible, although they must be eligible for a positive potential benefit to be determined practically eligible. As more pathways to eligibility were opened during this period, the value of the SEV increased even further beyond the federal-rule baseline in 2008.

Figure A4. Georgia simulated eligibility and simulated federal eligibility rates



3. Simulated potential benefit variable construction

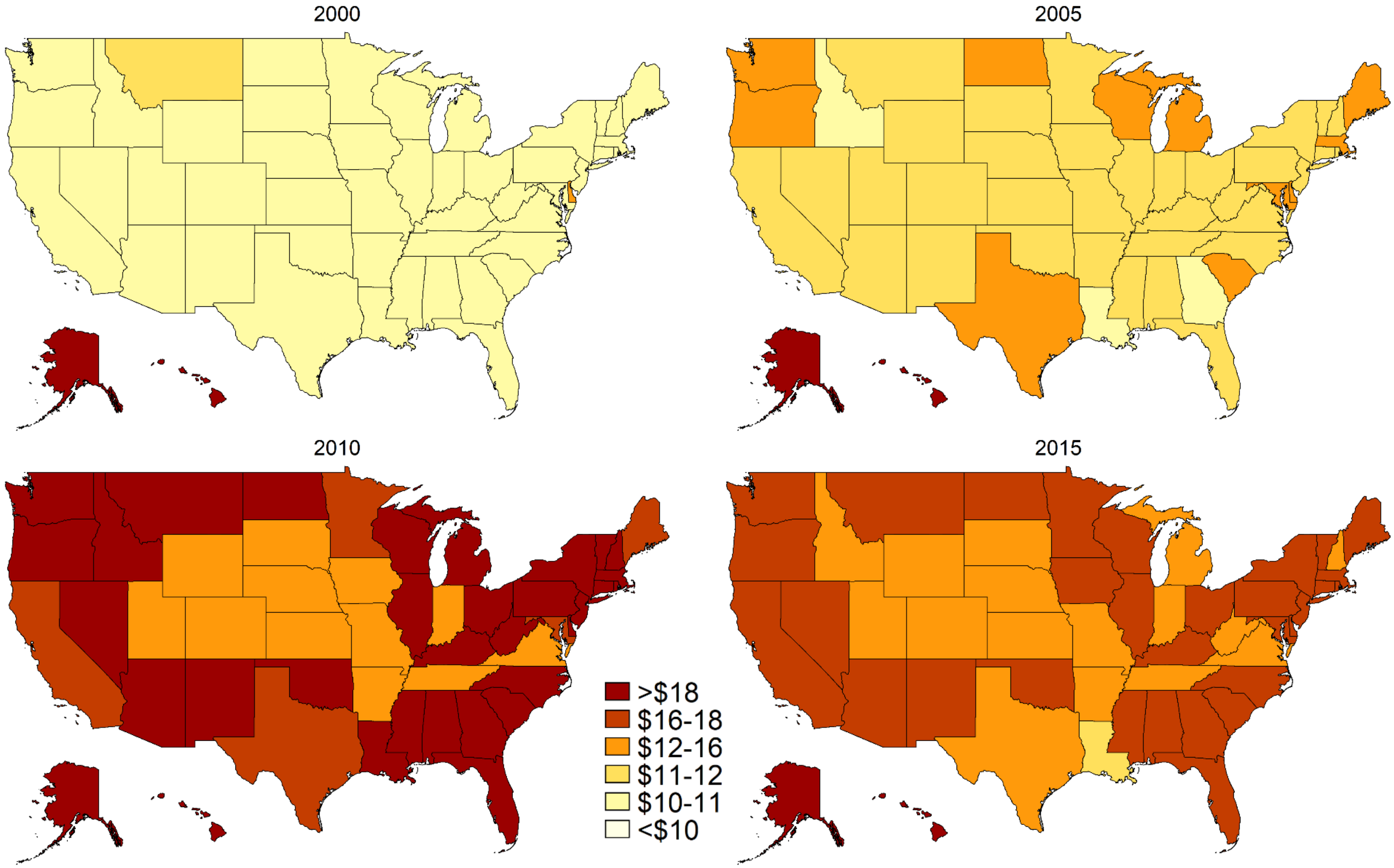
I also construct a measure I term the "simulated potential benefit variable" (SPBV) for each state s and year t as

$$SPBV_{st} = \frac{\text{Sum of potential SIPP household SNAP benefits}_{st}}{\text{Total \# SIPP individuals} = 876{,}544}$$

In step 4 detailed above in section 2, I describe the calculation of each eligible household's potential monthly SNAP benefit. I sum these household benefits as determined under the rules in place in each state and year and divide this sum by the number of SIPP individuals. The resulting measure is the SPBV, which more accurately represents the average simulated potential benefits per capita for SIPP households in each state and year. This measure varies on the extensive margin with the number of practically eligible households as well as the intensive margin with changes in the maximum allotment and net income determination. I consider this measure as an instrument in place of the SEV in some specifications.

Figure A5 shows the value of the SPBV over time, adjusted to 2010 dollars. As its value varies directly with changes in the number of households eligible, it follows similar patterns between states and over time as the SEV, as shown in Figures 4 and 7.

Figure A5. Simulated SNAP potential benefit variable (SPBV) by state



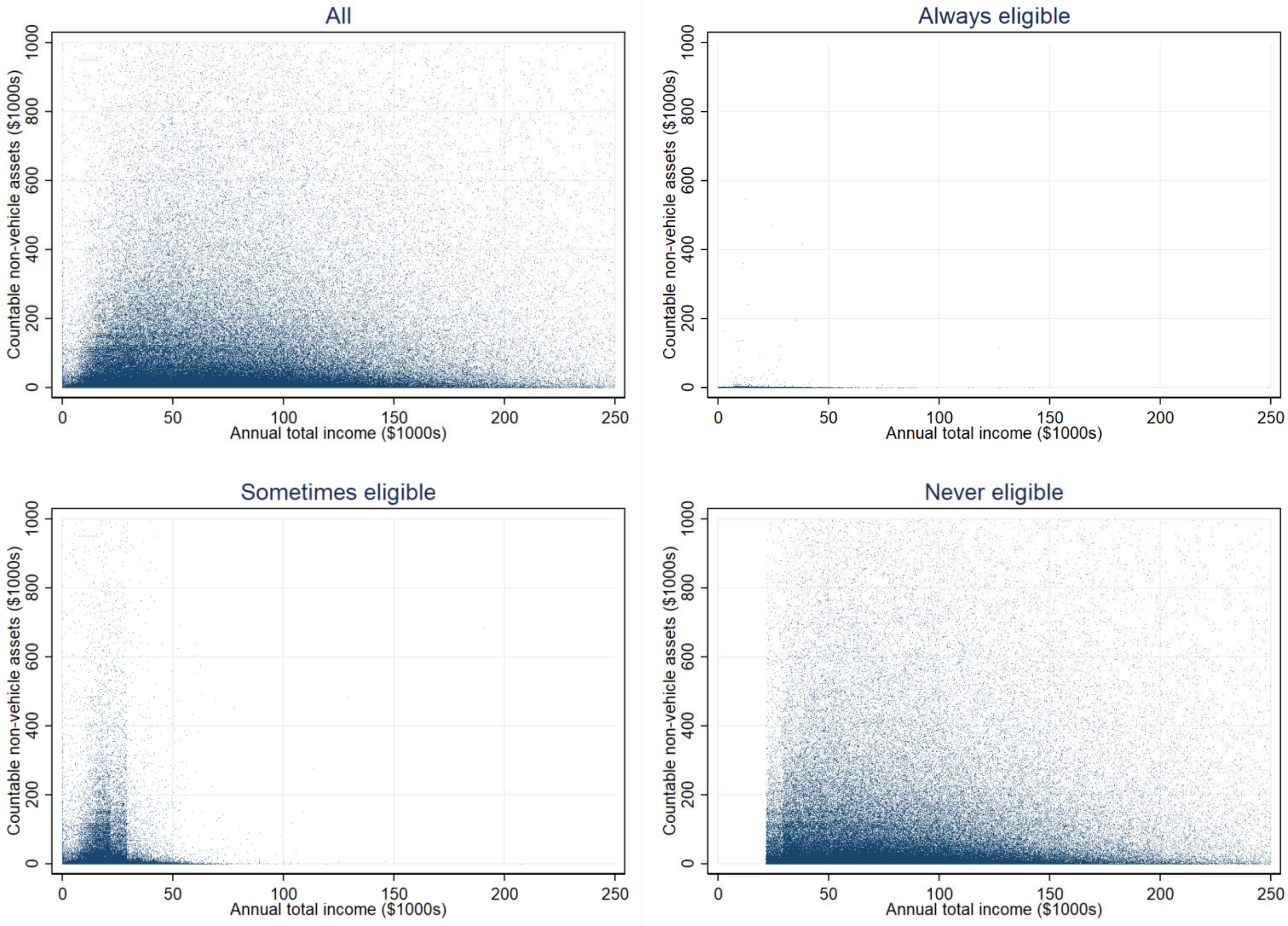
SPBV is represented in 2010-adjusted dollars per capita.

Table A5. Selected SIPP household sample summary statistics by eligibility status

	All	Always eligible	Sometimes eligible	Never eligible
# members	2.558	2.391	2.313	2.648
	(1.485)	(1.743)	(1.644)	(1.381)
Any < 18	0.345	0.391	0.297	0.348
	(0.475)	(0.488)	(0.457)	(0.476)
Any elderly	0.364	0.388	0.509	0.325
	(0.481)	(0.487)	(0.500)	(0.468)
Any disabled	0.235	0.451	0.249	0.193
	(0.424)	(0.498)	(0.433)	(0.394)
Any elderly or disabled	0.499	0.688	0.632	0.432
	(0.500)	(0.463)	(0.482)	(0.495)
Annual total income	62.48	13.17	21.67	81.31
($1000s)	(59.64)	(8.360)	(10.35)	(61.85)
Annual earned income	49.52	5.812	11.43	66.68
($1000s)	(60.70)	(8.852)	(13.82)	(64.75)
Countable non-vehicle	84.19	0.315	39.80	110.2
assets ($1000s)	(1090.3)	(9.029)	(670.3)	(1257.5)
Any vehicles	0.850	0.507	0.813	0.921
	(0.357)	(0.500)	(0.390)	(0.269)
# vehicles	1.523	0.650	1.222	1.755
	(0.940)	(0.748)	(0.842)	(0.877)
Total equity in vehicles	7526.9	1374.3	5699.2	9088.6
($)	(8655.5)	(2368.1)	(6832.5)	(9177.0)
Observations	342,605	43,831	58,101	240,673

Unit of observation is the household-year. Means are shown with standard deviations in parentheses. Dollar values are adjusted to 2010 dollars. The leftmost column shows statistics for all households in the SIPP used to construct the SEV. The other columns show statistics for households that are determined eligible for SNAP in all state-years, in some but not all state-years, or in no state-years, excluding the rules in Alaska and Hawaii from consideration. Full SIPP household summary statistics are presented in Table A4.

Figure A6. Total income and non-vehicle countable resources of SIPP households by eligibility status



Scatterplots show households' non-vehicle assets that are countable in determining SNAP eligibility and annual total income. The plot labeled "All" displays this information for all household-years in the SIPP used to construct the SEV, while the other plots display this information for subcategories of these household-years as defined in Table A5.

Appendix B: Chapter I Supplementary Material

Figure B1. SNAP participation rate by county

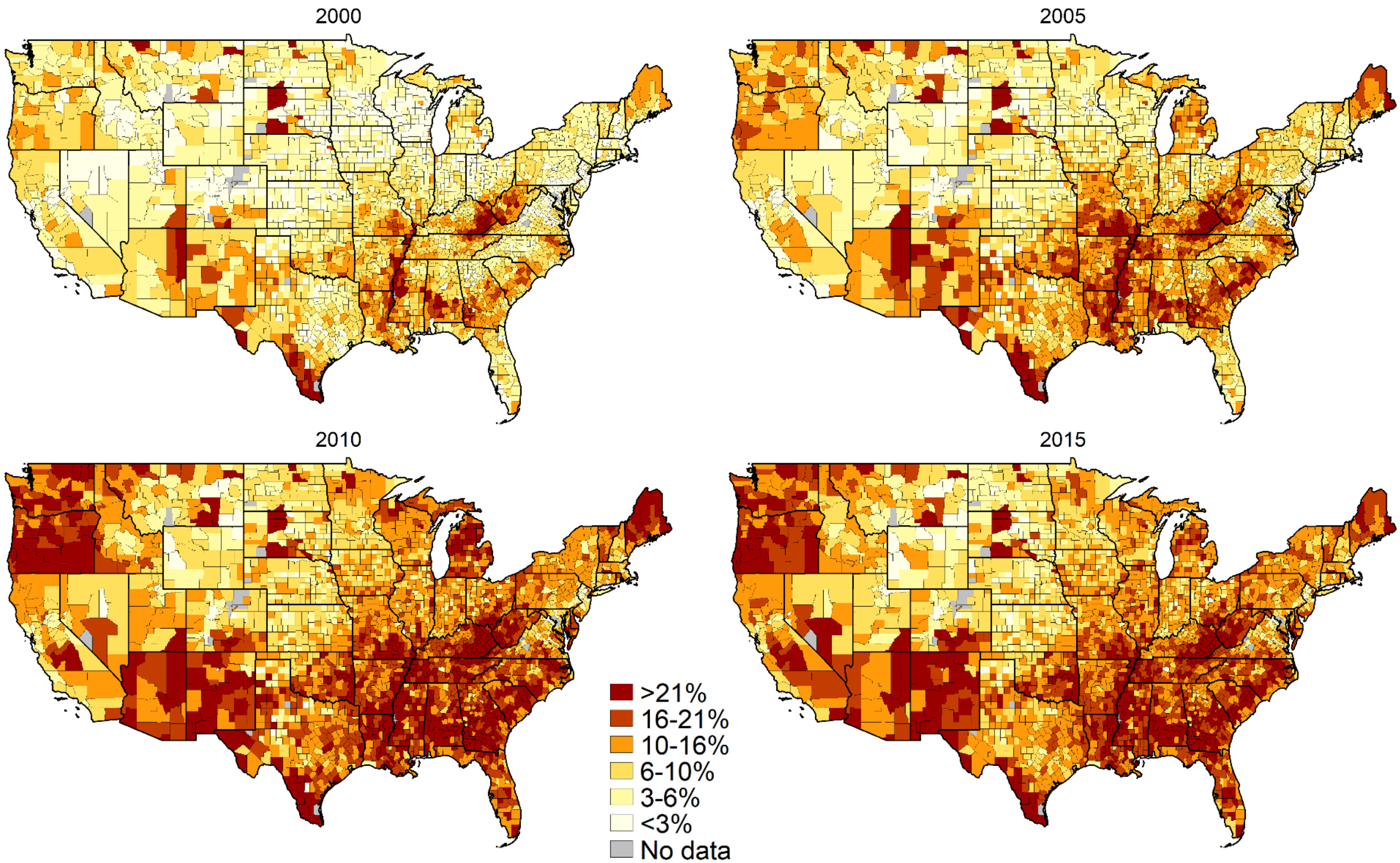


Figure B2. Number of SNAP-plausible establishments per 100,000 population by county

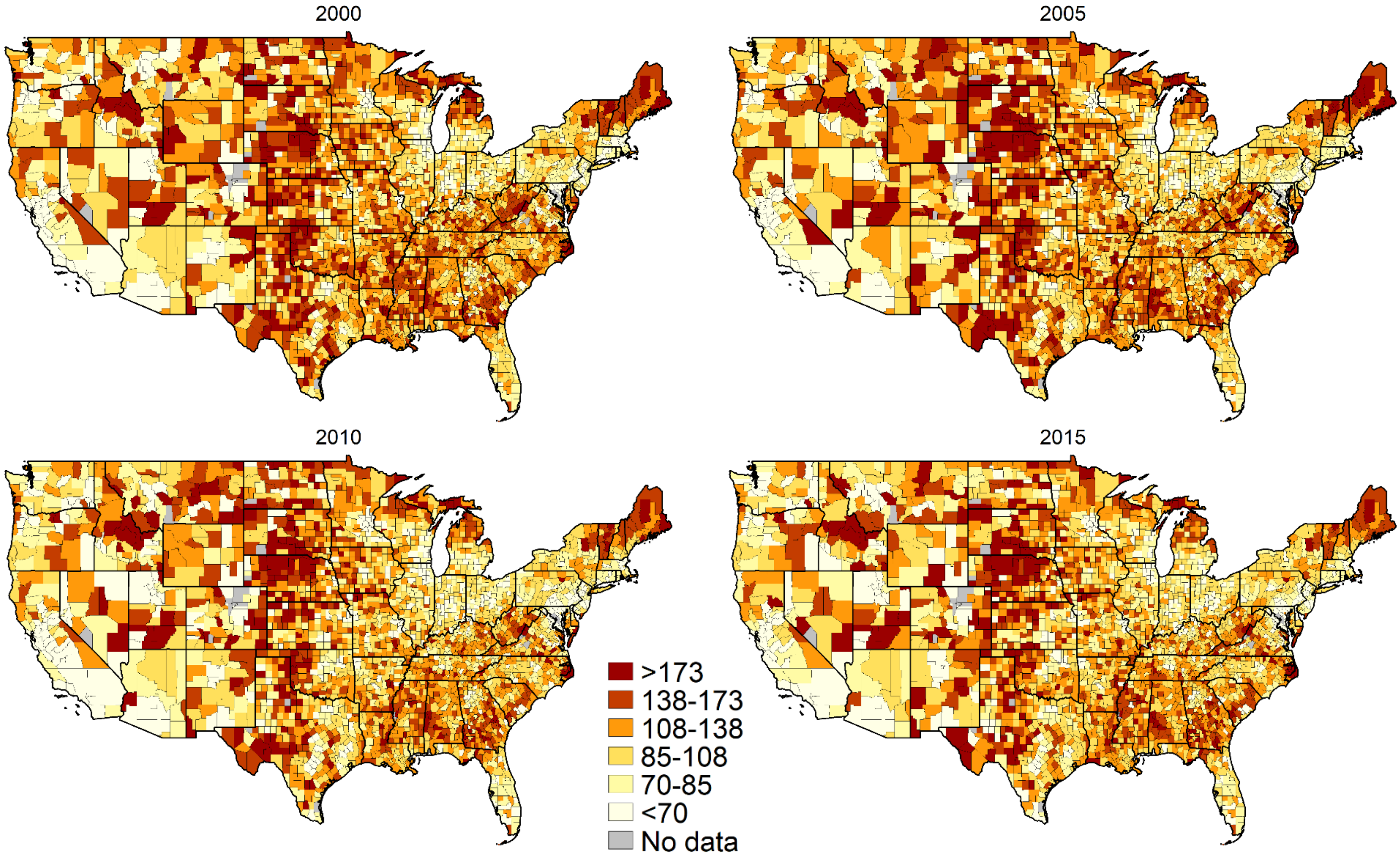


Figure B3. Number of SNAP-implausible establishments per 100,000 population by county

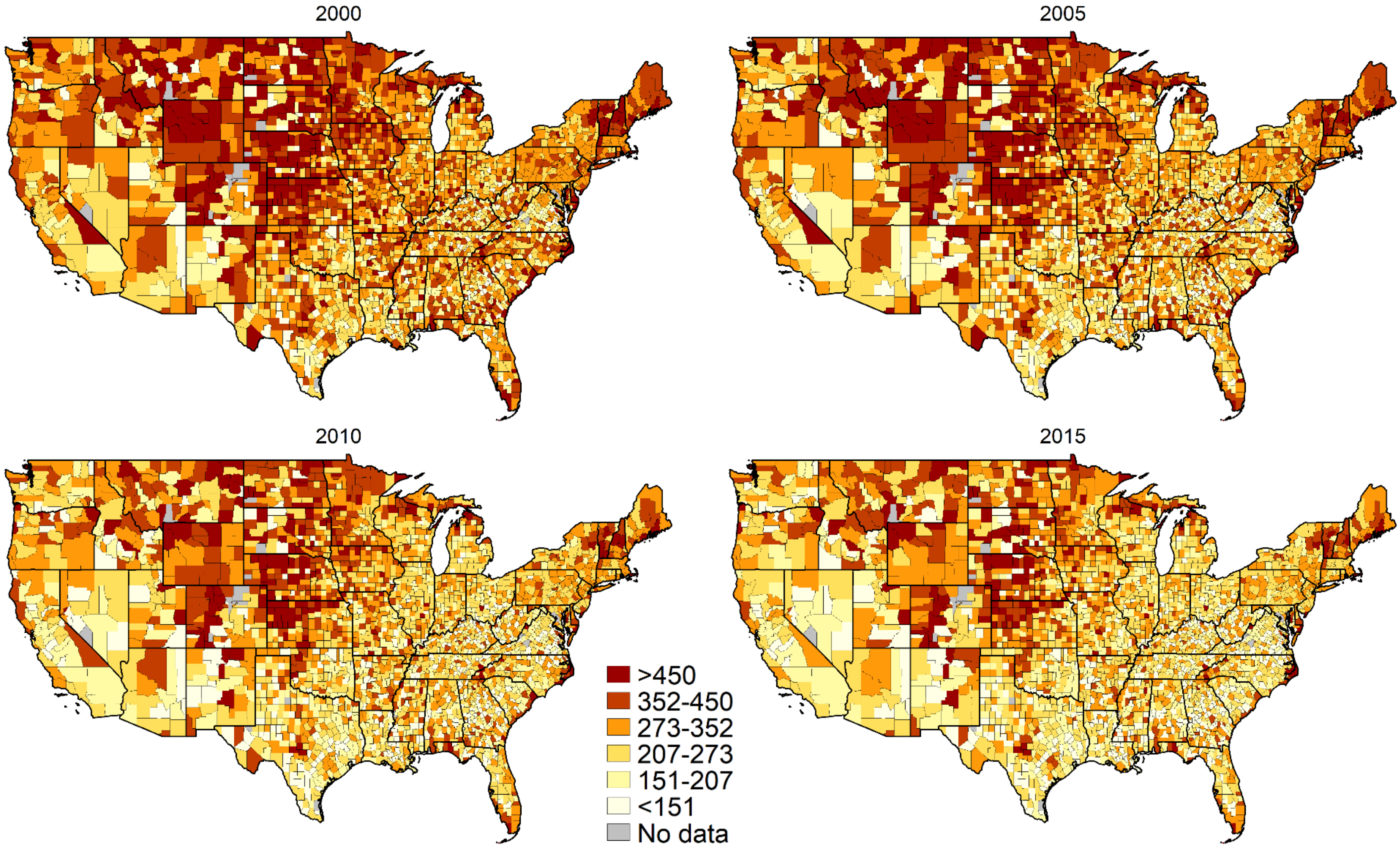
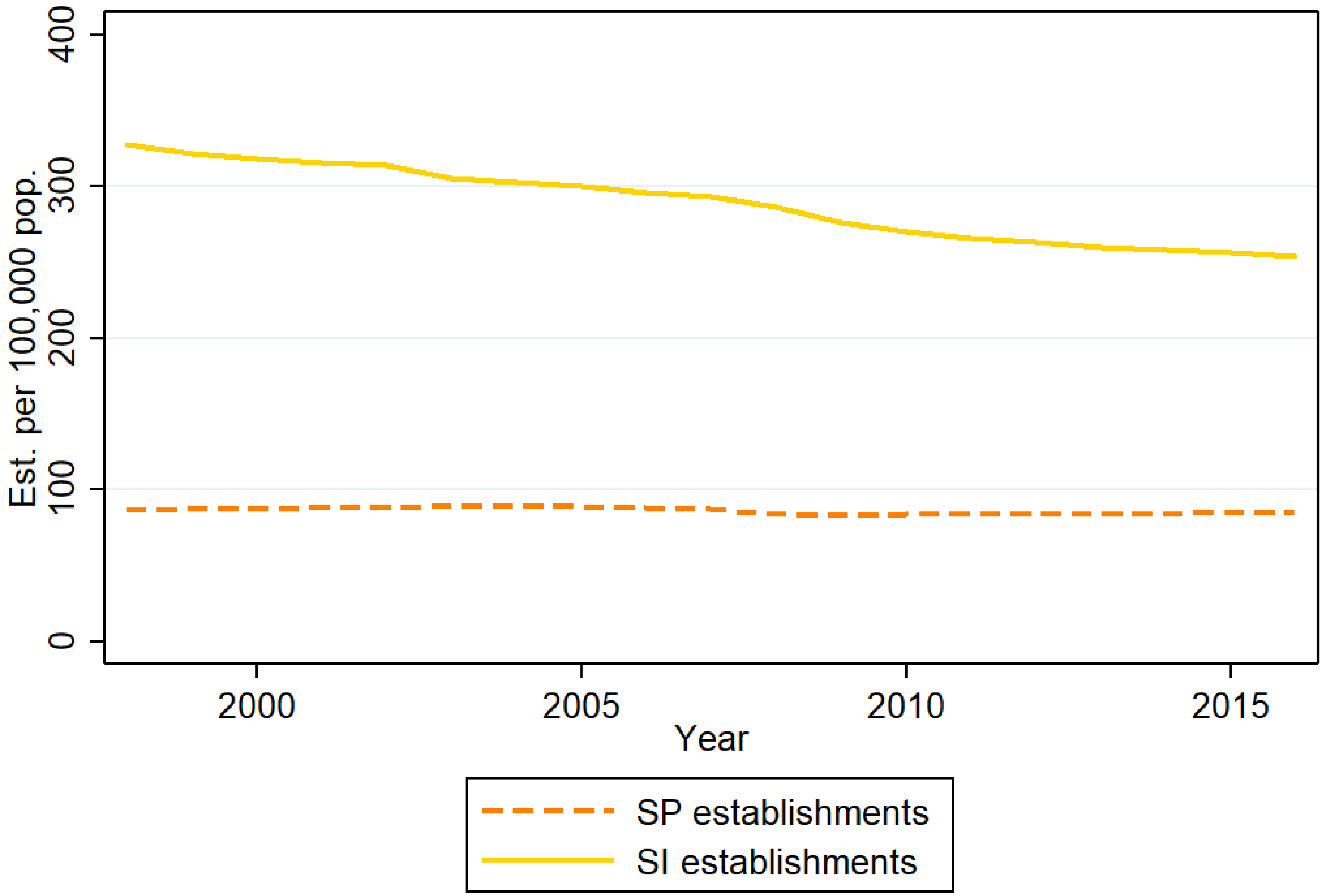


Figure B4. National SNAP-plausible and SNAP-implausible establishments per 100,000 population



Excludes AK, HI, and CA; average weighted by population

Table B1. Full summary statistics

	Full county sample		Uncensored county sample	
	Mean	Std. dev.	Mean	Std. dev.
SNAP variables				
Participation rate (%)	11.12	(6.643)	10.89	(6.372)
Benefits per capita (2010 $)	11.92	(8.347)	11.81	(8.028)
Simulated eligibility variable (SEV) (%)	17.38	(4.227)	17.67	(4.344)
Simulated potential benefit variable (2010 $)	13.51	(3.048)	13.61	(3.111)
Establishment counts per 100,000 population				
Grocery stores & supermarkets	22.09	(12.18)	22.35	(12.20)
Convenience stores	9.923	(6.423)	10.67	(5.430)
Gas stations w/ conv. stores	32.84	(17.77)	26.01	(12.31)
Supercenters & warehouse clubs	1.332	(1.160)	1.239	(0.832)
General stores	11.81	(7.339)	9.580	(4.278)
SNAP-plausible stores	86.38	(26.32)	79.37	(17.55)
SNAP-implausible stores	288.0	(88.10)	291.4	(68.78)
Establishment counts per 100,000 population by employment size group				
SNAP-plausible stores, 0-9 emp.	58.35	(22.25)	54.66	(17.60)
SNAP-plausible stores, 10-49 emp.	20.54	(10.91)	17.03	(5.947)
SNAP-plausible stores, 50+ emp.	7.484	(2.655)	7.680	(2.018)
SNAP-implausible stores, 0-9 emp.	206.1	(69.43)	203.0	(53.59)
SNAP-implausible stores, 10-49 emp.	70.00	(23.66)	74.25	(18.77)
SNAP-implausible stores, 50+ emp.	11.88	(5.914)	14.06	(4.161)
Employment rates as % of population				
All retail (food & nonfood)	-	-	5.432	(1.223)
Grocery stores, supermarkets, and convenience stores	-	-	0.888	(0.234)
Combined gas stations	-	-	0.242	(0.120)
Supercenters & general stores	-	-	0.435	(0.254)
Annual payroll per 100,000 population in 2010-adjusted $1000s				
All retail (food & nonfood)	-	-	151452.6	(47294.4)
Grocery stores, supermarkets, and convenience stores	-	-	19529.8	(5770.7)
Combined gas stations	-	-	4876.4	(2504.4)
Supercenters & general stores	-	-	9599.8	(5539.3)

Table B1. Full summary statistics (continued)

	Full county sample		Uncensored county sample	
	Mean	Std. dev.	Mean	Std. dev.
Annual average employee earnings in 2010-adjusted dollars				
All retail (food & nonfood)	-	-	27670.0	(4034.7)
Grocery stores, supermarkets, and convenience stores	-	-	21997.7	(3291.9)
Combined gas stations	-	-	20527.7	(3624.3)
Supercenters & general stores	-	-	22306.8	(3741.6)
Population (unweighted)	85360.4	(234710.7)	408407.9	(524664.6)
Demographic characteristics as % of population				
Rural	21.92	(25.74)	8.747	(11.61)
Black	13.28	(13.28)	15.07	(12.02)
Hispanic	12.31	(14.57)	15.01	(15.51)
Age 0-17	24.22	(3.069)	24.21	(2.867)
Age 60+	18.52	(4.665)	17.77	(4.186)
Married	53.16	(6.726)	51.65	(5.943)
Have bachelor's degree	26.95	(10.43)	30.29	(9.183)
Foreign-born	10.17	(9.735)	13.34	(10.43)
Economic characteristics				
Poverty rate (%)	13.75	(5.538)	13.38	(5.205)
Unemployment rate (%)	6.012	(2.380)	5.918	(2.227)
Personal income per capita 2010 $)	33711.3	(12455.2)	37081.2	(12766.7)
Non-SNAP government transfers per capita (2010 $)	6080.3	(1733.8)	6006.0	(1677.1)
% with income of 125-199% of FPL in 1990	13.60	(4.083)	12.08	(3.293)
Policy environment characteristics				
Governor is Democrat	0.406	(0.491)	0.402	(0.490)
State house Democrats (%)	50.10	(13.71)	50.91	(13.88)
State senate Democrats (%)	46.62	(13.90)	46.73	(13.74)
Other program state participation rates (%)				
TANF	1.286	(0.848)	1.318	(0.845)
SSI	2.375	(0.712)	2.353	(0.627)
Medicaid	16.16	(5.196)	16.17	(5.175)
Counties	3,030		405	
Years	19		19	
Observations	57,570		7,695	

Statistics are weighted by county population, excluding population itself. The full sample excludes Alaska, Hawaii, California, and counties that either change borders during the sample or for which data is not available for the entire period. The uncensored county subsample includes the 405 counties with uncensored information about retail employment and payroll available for selected NAICS codes over the entire sample period. The sample period is 1998-2016, although simulated eligibility and benefits instruments are not available for 2016.

1. SEV performance compared to other instruments

I compare the performance of the simulated eligibility variable (SEV) as an instrument for the SNAP participation rate to the performance of the simulated potential benefit variable (SPBV) I describe in Appendix A and other state-level SNAP policy variables from the SNAP Policy Database (USDA ERS 2018).[152] Relative to the SEV and SPBV which primarily derive their variation from rules affecting eligibility, most of these policies affect the information available to households or the costs to households of applying, certifying, or recertifying. Several studies have used sets of these policies in an IV framework to estimate SNAP's impacts in various contexts, typically at the individual- or household-level as opposed to the area-level business impacts I consider.[153]

I estimate first-stage regressions, each modeling the county SNAP participation rate as a function of one policy instrument and the covariates in my baseline models. I present the coefficient estimates and first-stage F-statistics from these regressions in Table B2. The SEV is individually the strongest instrument among those I consider, with an F-statistic of 17.09. The SPBV is the only other instrument with an F-statistic above 10. All other policy variables are underpowered for use as instruments in the context of this study.[154]

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[152] In the order they are shown in Table B2, the policy instruments I consider are the SEV, the SPBV, a dummy for a BBCE expansion of any type, a dummy for the state operating call centers, a dummy for the state operating a Combined Application Project for SSI recipients, the average certification period in months for SNAP units with earnings/with elderly members/without earnings, the proportion of the dollar value of benefits accounted for by EBT, a dummy for the state having a waiver to use a telephone interview in lieu of an in-person interview at initial certification/recertification, a dummy for the state requiring fingerprinting of applicants, a dummy for the state allowing online application, outreach spending per capita in thousands of 2010-adjusted dollars, a dummy for the state using simplified reporting that reduces requirements for households with earnings to report changes in household circumstances, a dummy for the state excluding all vehicles from the asset test, and a dummy for the state excluding any number or exempting any value of vehicles beyond the federal minimum.

[153] E.g., Meyerhoefer and Pylypchuk (2008); Yen et al. (2008); Ratcliffe, McKernan, and Zhang (2011); Gregory and Coleman-Jensen (2013); Gregory and Deb (2015); Almada, McCarthy, and Tchernis (2016).

[154] These patterns of relative strength and weakness generally hold in equivalent state-level first-stage regressions.

Table B2. Comparative first-stage regression results with alternate instruments

Instrument	SNAP part. rate	F-statistic	Instrument	SNAP part. rate	F-statistic
SNAP SEV	0.171*** (0.0413)	17.09	Telephone: initial certification	-0.409 (0.538)	0.577
SNAP SPBV	0.348*** (0.0963)	13.02	Telephone: recertification	-0.116 (0.382)	0.0931
BBCE	0.936*** (0.329)	8.100	Fingerprinting	0.741 (0.626)	1.403
Call centers	-0.241 (0.420)	0.329	Online application	-0.593* (0.332)	3.189
CAP	-0.0122 (0.502)	0.000592	Outreach spending per capita	-0.00774 (0.00873)	0.786
Average cert. period: households w/ earnings	0.0739 (0.0461)	2.570	Simplified reporting	-0.0311 (0.242)	0.0165
Average cert. period: households w/ elderly	0.0179 (0.0353)	0.256	Excludes all vehicles	0.885** (0.349)	6.446
Average cert. period: households w/o earnings	-0.0343 (0.0990)	0.120	Alters vehicle treatment	0.0568 (0.257)	0.0490
EBT	-0.0575 (0.558)	0.0106	Observations	54,540 or 57,570	
			Mean SNAP part. rate	11.12	

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
First-stage F-statistics shown beside coefficient estimates.
Each coefficient is from a separate first-stage regression using just one instrument. All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. Regressions lag participation rate and instrument by one year as in baseline models. SNAP participation rate is expressed in percentage points. Each regression uses a sample of 54,540 county-year observations except for those using the SEV or SPBV. See Appendix B, Section 1 for a brief discussion of the policy instruments considered here.

Table B3. Establishment counts broken down by number of employees second-stage and reduced form regression results

	SNAP-plausible stores with:			SNAP-implausible stores with		
	0-9 emp.	10-49 emp.	50+ emp.	0-9 emp.	10-49 emp.	50+ emp.
Reduced form						
SNAP SEV	0.0696	0.0204	0.00196	0.0803	-0.103	-0.0258*
	(0.0687)	(0.0351)	(0.0180)	(0.0959)	(0.0676)	(0.0144)
Mean SEV	17.38	17.38	17.38	17.38	17.38	17.38
R^2	0.155	0.0720	0.0637	0.673	0.279	0.306
Instrumental variables second stage						
Predicted SNAP part. rate	0.408	0.119	0.0115	0.470	-0.603	-0.151
	(0.385)	(0.202)	(0.107)	(0.596)	(0.418)	(0.0968)
Mean est. count per 100,000 population	58.35	20.54	7.484	206.1	70.00	11.88
Mean SNAP part. rate	11.12	11.12	11.12	11.12	11.12	11.12
R^2	0.169	0.0720	0.0625	0.674	0.266	0.302
Observations	57,570	57,570	57,570	57,570	57,570	57,570

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
All regressions include demographic controls, year and county fixed effects, and state-specific time trends. All regressions are weighted by county population. The SEV and predicted SNAP participation rate from the first stage are expressed in percentage points. Establishment counts are expressed as the number per 100,000 population and only include those establishments with a number of employees falling within the designated ranges: 0-9 employees, 10-49, or 50 or more employees. Participation rate and SEV are lagged one year.

Table B4. Other robustness checks

	Grocery stores & supermarkets	Convenience stores	Gas stations w/ conv. stores	Supercenters & warehouse clubs	General stores	SNAP-plausible stores	SNAP-implausible stores
Baseline							
County-level	-0.0547	0.131	0.197	0.0225	0.225**	0.538*	-0.283
	(0.316)	(0.129)	(0.196)	(0.0238)	(0.0966)	(0.289)	(0.722)
State-level	-0.0878	0.143	0.308	0.0316	0.295**	0.749*	-0.353
	(0.326)	(0.165)	(0.246)	(0.0229)	(0.123)	(0.398)	(0.936)
Altered timing of SNAP participation rate and SEV							
Not lagged	-0.132	0.0965	0.259	0.0198	0.246**	0.417	-0.173
	(0.289)	(0.157)	(0.174)	(0.0239)	(0.108)	(0.272)	(0.770)
Lagged three-period	-0.0168	0.0389	0.259	0.0133	0.246***	0.683**	-0.286
moving average	(0.337)	(0.137)	(0.276)	(0.0277)	(0.0869)	(0.341)	(0.654)
No demographic	-0.134	0.112	0.209	0.0273	0.243***	0.479*	-0.378
controls	(0.299)	(0.123)	(0.198)	(0.0217)	(0.0930)	(0.256)	(0.769)
Altered time trends							
No state-specific trends	0.668*	0.328**	0.185	-0.0415*	0.0497	1.079***	0.480
	(0.377)	(0.129)	(0.221)	(0.0250)	(0.129)	(0.416)	(1.277)
County-specific trends	-0.101	0.124	0.190	0.0289	0.247**	0.511	-0.471
	(0.296)	(0.121)	(0.203)	(0.0196)	(0.0988)	(0.316)	(0.733)
Benefits per capita as outcome							
Limited county sample	-0.198	0.139	0.167	0.0313	0.313**	0.485	-0.665
	(0.441)	(0.192)	(0.245)	(0.0335)	(0.157)	(0.458)	(1.057)
State-level	-0.0821	0.134	0.288	0.0296	0.276**	0.700*	-0.330
	(0.298)	(0.150)	(0.225)	(0.0232)	(0.111)	(0.395)	(0.875)
Alternate samples							
Includes CA	-0.159	0.0791	0.130	0.0386*	0.132	0.177	-0.495
	(0.229)	(0.0942)	(0.138)	(0.0202)	(0.0899)	(0.305)	(0.505)
Excludes densest	-0.167	0.127	0.154	0.0249	0.241**	0.412	-0.343
counties	(0.343)	(0.129)	(0.225)	(0.0254)	(0.103)	(0.300)	(0.755)
No population weights	-0.159	0.0791	0.130	0.0386*	0.132	0.177	-0.495
	(0.229)	(0.0942)	(0.138)	(0.0202)	(0.0899)	(0.305)	(0.505)

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Baseline regressions include demographic controls, year fixed effects, county or state fixed effects, and state-specific time trends. Baseline regressions are weighted by county or state population and lag the SNAP participation rate and SEV by one period. Regressions are at the county level unless otherwise stated. The SNAP participation rate and SEV are expressed in percentage points. Benefits per capita are expressed in 2010-adjusted dollars. Establishment counts are expressed as the number per 100,000 population. Each coefficient and standard error pair are from a separate regression. Each row deviates from the baseline model in some way as described. These deviations include using the state sample with state-level variables, using current-period participation rate and SEV, using a moving average of participation rate and SEV from the previous three periods, excluding demographic controls, excluding state-specific time trends, using county-specific trends in place of state-specific, using benefits per capita in place of participation rate as the outcome (with a restricted county sample for which this information is available), including California counties in the sample, excluding the ten densest counties (as of 2010), and not weighting by population.

Appendix C: Chapter II Supplementary Material

Figure C1. SNAP participation rate by state

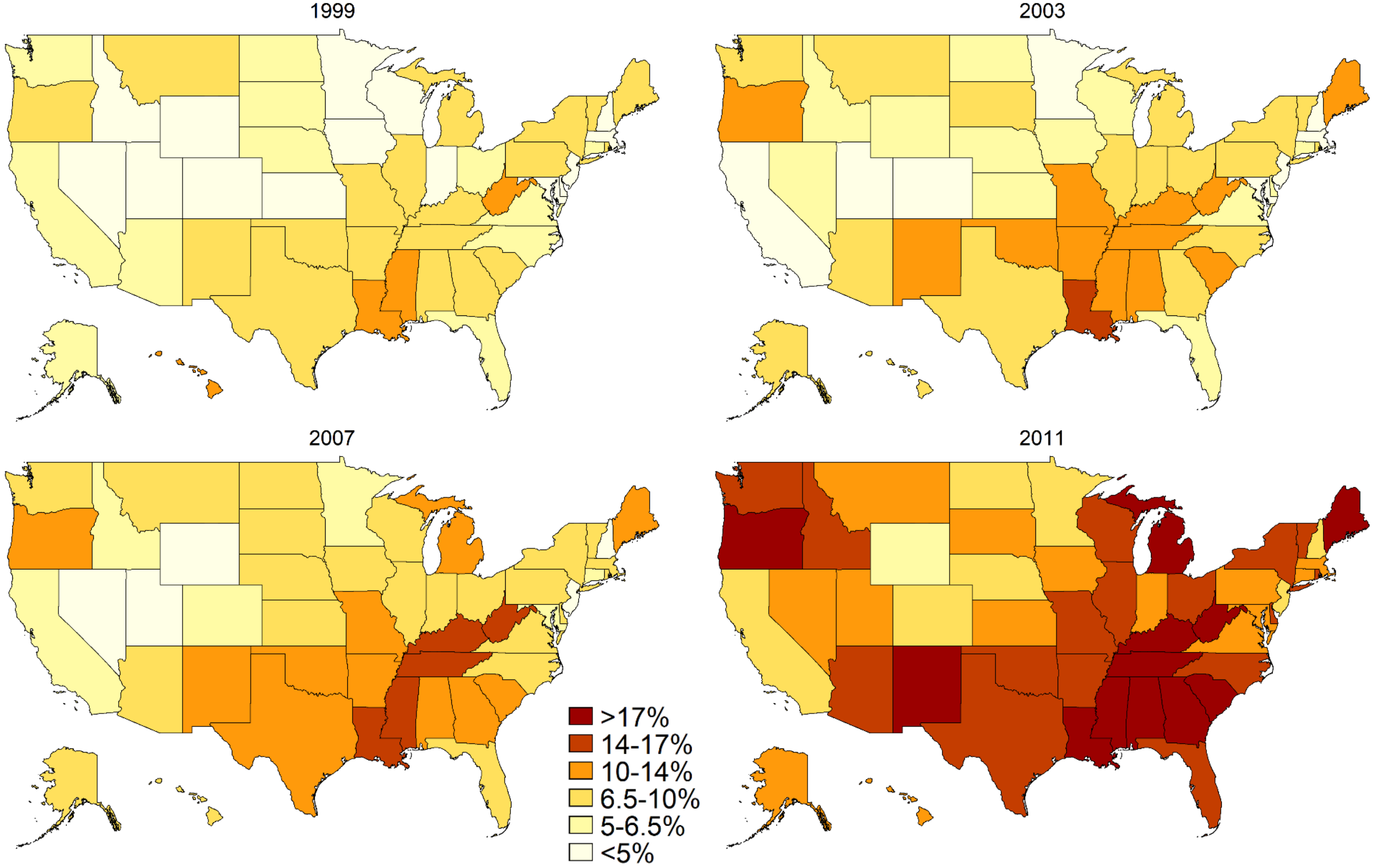


Figure C2. Medicaid enrollment rate by state

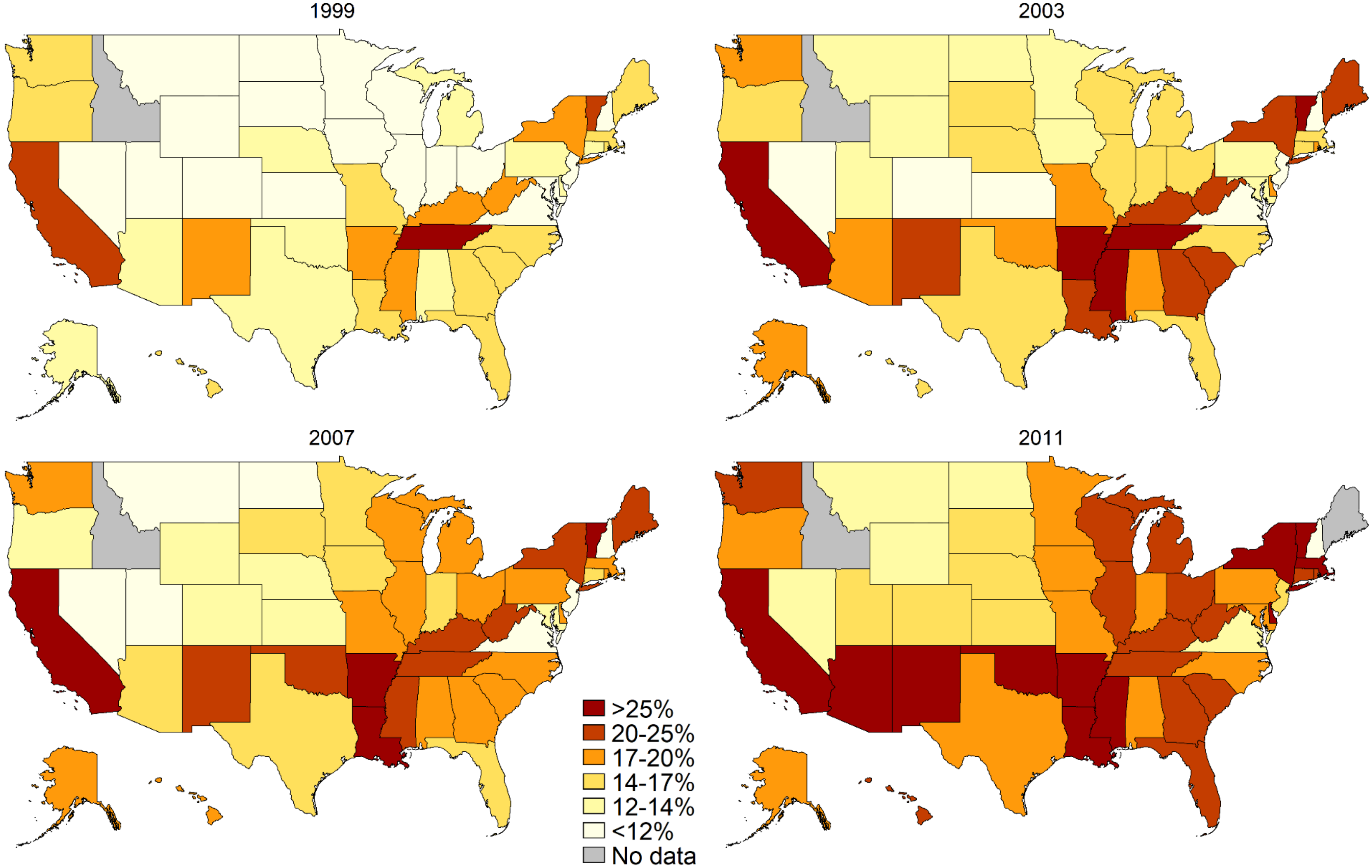
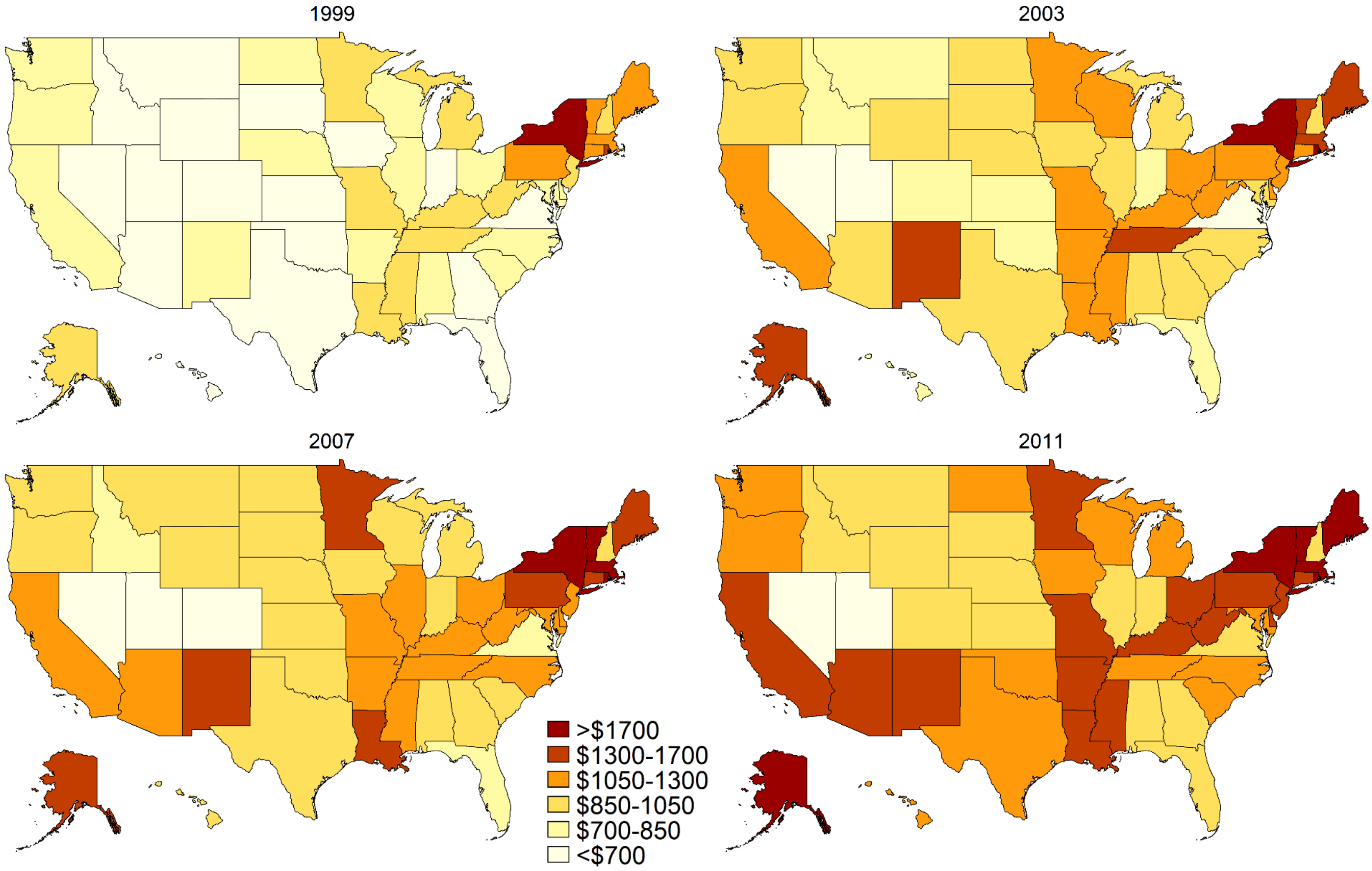
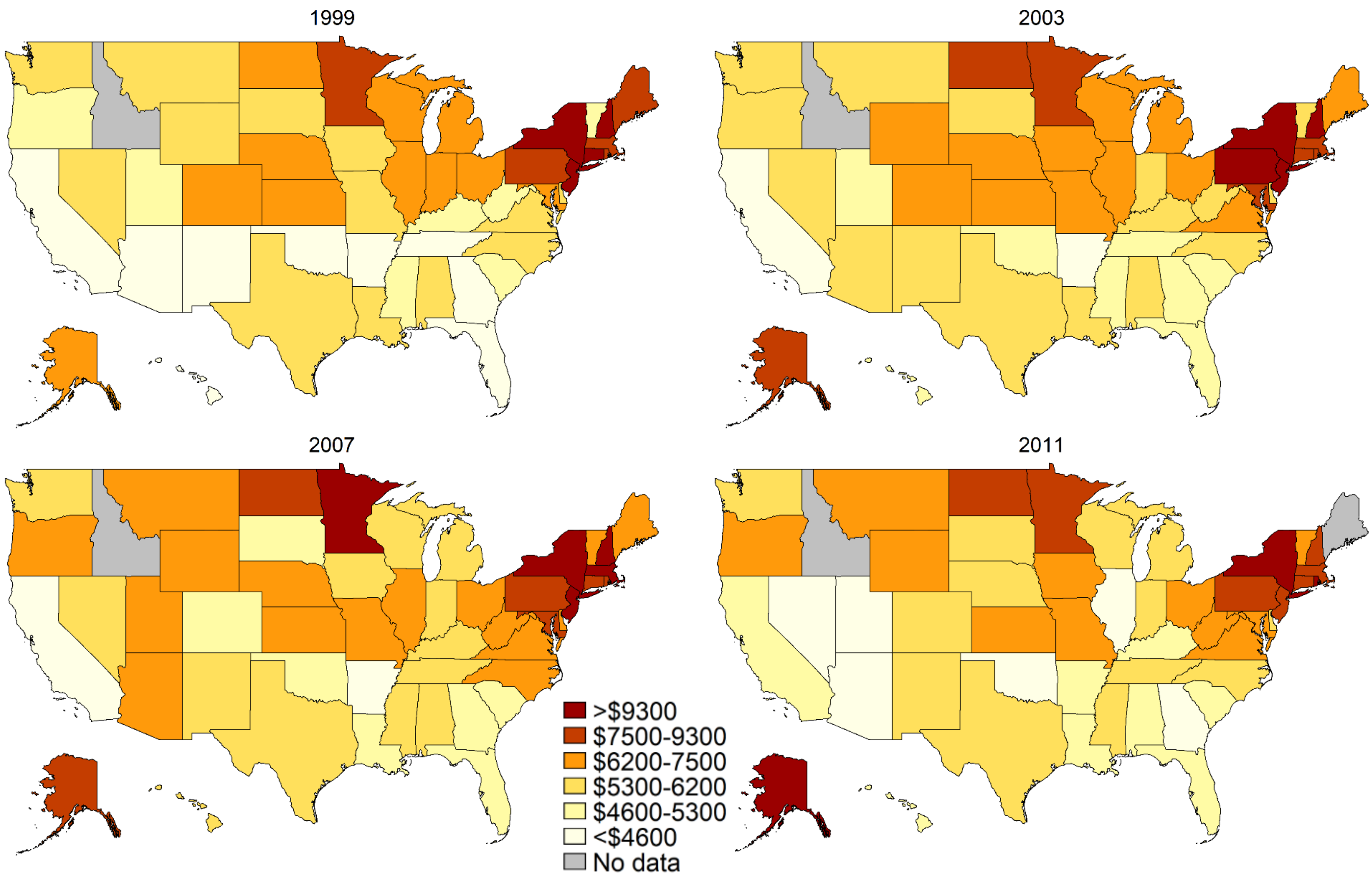


Figure C3. Medicaid spending per capita by state



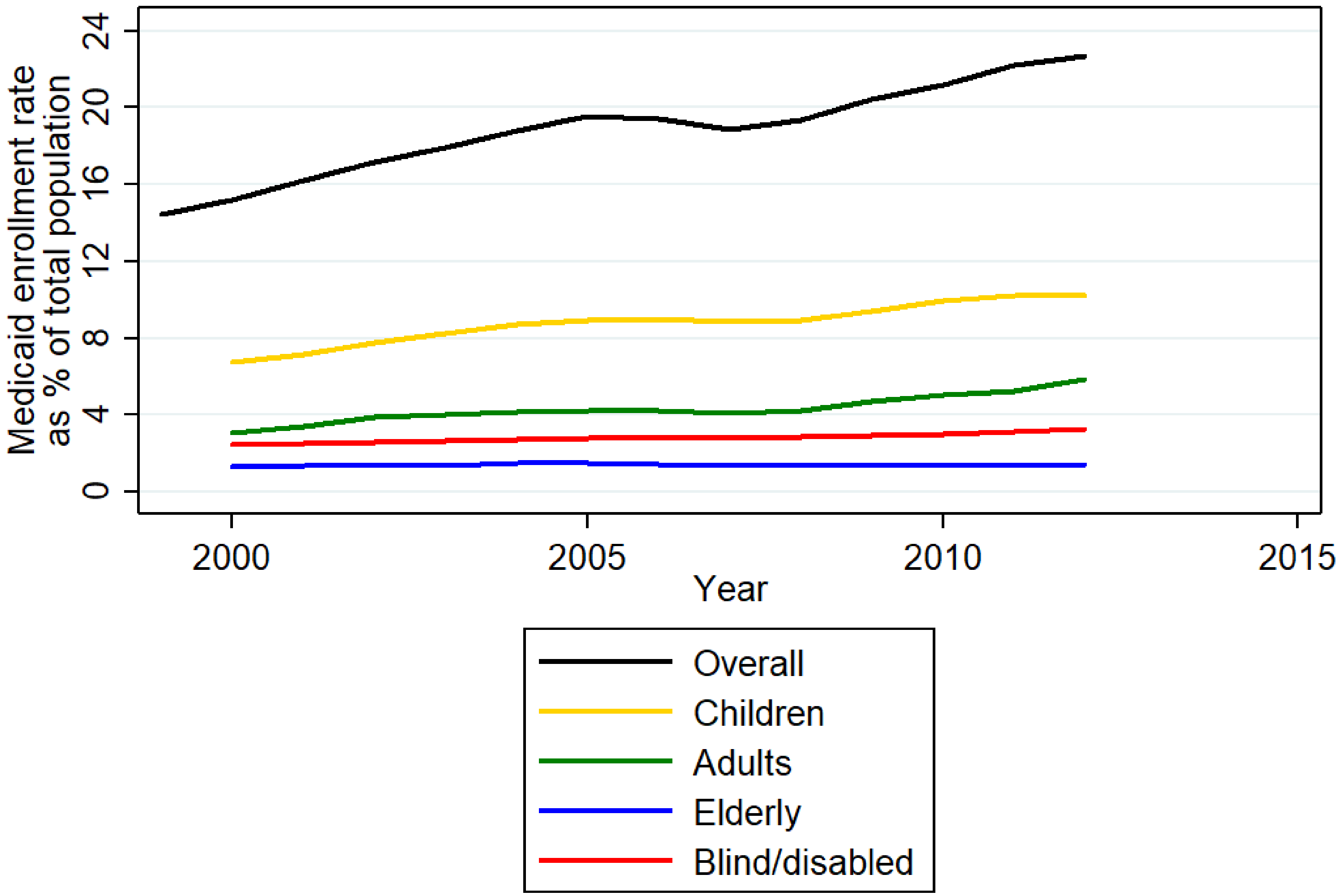
Spending is represented in 2010-adjusted dollars per capita.

Figure C4. Medicaid spending per enrollee by state



Spending is represented in 2010-adjusted dollars per Medicaid enrollee.

Figure C5. National Medicaid enrollment rate and eligibility group enrollment rates



Excludes AK, HI, and ID; average weighted by population

Figure C6. National Medicaid spending per capita and per enrollee

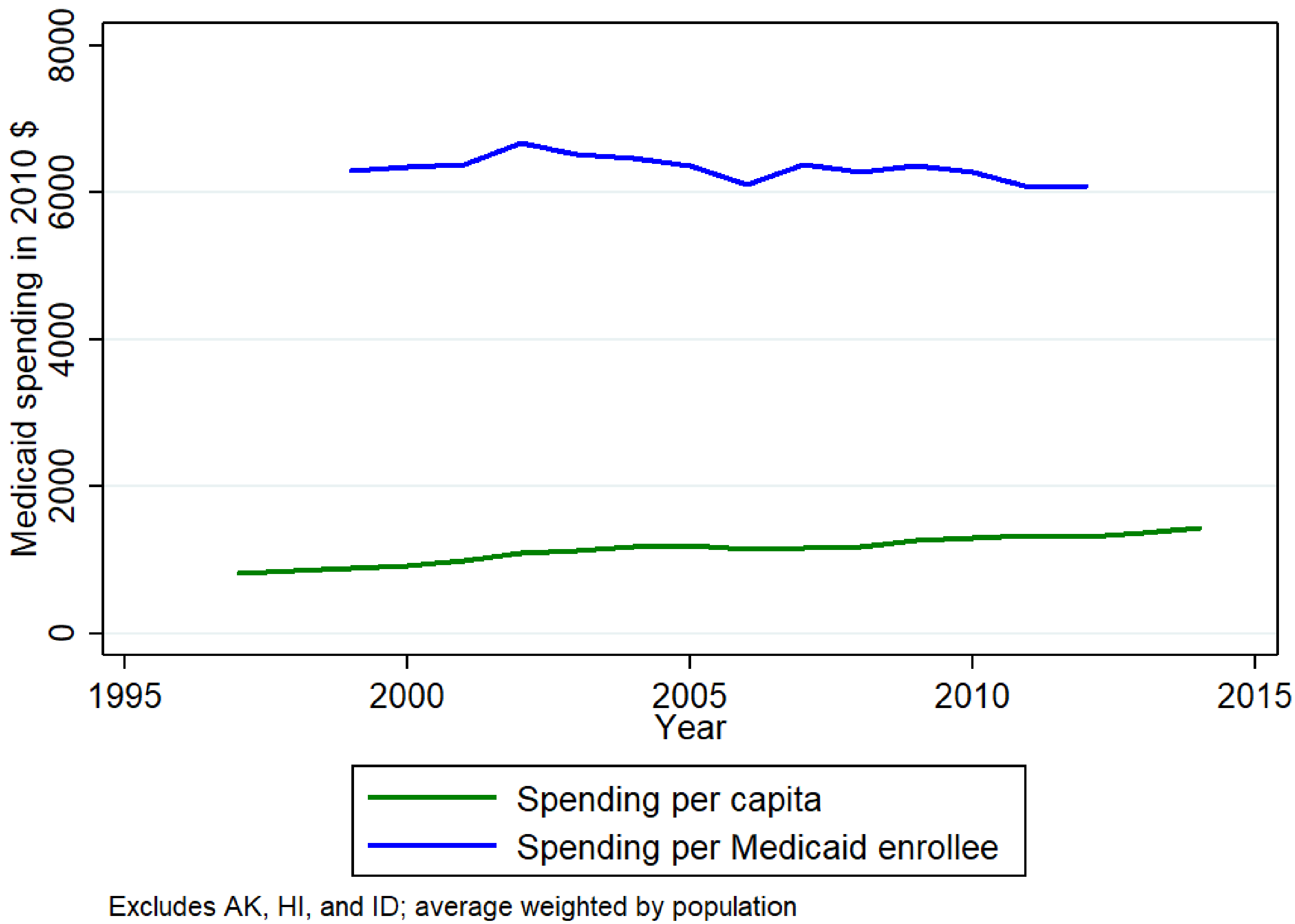


Table C1. Missing Medicaid enrollment and eligibility information

Category	Restricts regressions using:	Total non-missing observations	Missing state-years
Total observations	Any outcome	672	-
Total Medicaid enrollment count	Overall Medicaid enrollment rate or Medicaid spending per enrollee	661	2011: ME 2012: AZ, CO, DC, FL, KS, LA, ME, MA, TX, UT
Group Medicaid enrollment count	Child, adult, elderly, or blind/disabled Medicaid enrollment rates	613	1999: All states 2011: ME 2012: AZ, CO, DC, FL, KS, LA, ME, MA, TX, UT
Medicaid eligibility limits	Any outcome, in baseline models (included as controls)	668	1999-2002: TN

Full state sample includes Washington, DC and all states but Alaska, Idaho, and Hawaii. Sample period consists of the 14 years from 1999 to 2012, where available. There are therefore 672 observations in the full sample (48 states multiplied by 14 years), though no baseline regression uses all observations due to the missing Medicaid eligibility limit controls in early years in Tennessee.

Table C2. Full summary statistics

	Mean	Std. dev.	Observations
SNAP variables			
Participation rate (%)	9.519	(4.038)	672
Simulated eligibility variable (SEV) (%)	16.58	(3.934)	672
Medicaid/CHIP enrollment rate (% of total population)			
Total	18.84	(5.608)	661
Children	8.782	(2.107)	613
Adults	4.321	(2.584)	613
Elderly (age 65+)	1.396	(0.421)	613
Blind/disabled	2.812	(0.931)	613
Medicaid/CHIP spending (2010 $)			
Per capita (overall population)	1157.0	(440.0)	672
Per Medicaid enrollee	6329.9	(1947.0)	661
Medicaid/CHIP eligibility limits (% of federal poverty level)			
Infants	229.9	(47.73)	668
Children aged 1-5	228.6	(49.06)	668
Children aged 6-18	228.0	(50.13)	668
Pregnant women, midpoint	198.3	(37.96)	668
Parents, midpoint	87.97	(53.54)	668
Childless non-disabled adults, midpoint	16.33	(35.78)	668
Medicaid/CHIP enrollment composition (% of total enrollment)			
Children	47.20	(8.299)	613
Adults	21.25	(7.200)	613
Elderly (age 65+)	7.509	(1.917)	613
Blind/disabled	15.00	(3.806)	613
Alternative Medicaid measures using UKCPR state-year enrollment data			
Medicaid enrollment rate, UKCPR	15.95	(4.988)	668
Medicaid spending per enrollee, UKCPR	7489.2	(2170.1)	668
Population (unweighted)	6115908.4	(6586079.4)	672

Table C2. Full summary statistics (continued)

	Mean	Std. dev.	Observations
Demographic characteristics (% of population)			
Rural	19.98	(12.16)	672
Black	12.60	(8.040)	672
Hispanic	14.76	(12.30)	672
Age 0-17	24.69	(1.805)	672
Age 60+	17.62	(2.416)	672
Married	53.42	(3.013)	672
Have bachelor's degree	26.58	(4.500)	672
Foreign-born	12.11	(7.932)	672
Economic characteristics			
Poverty rate (%)	13.08	(2.986)	672
Unemployment rate (%)	6.244	(2.239)	672
Personal income per capita (2010 $)	34203.0	(5531.6)	672
Non-SNAP/Medicaid government transfers per capita (2010 $)	4644.1	(915.7)	672
Policy environment characteristics			
Governor is Democrat (1=Yes)	0.476	(0.500)	658
Fraction of State House that is Democrat	52.91	(12.49)	644
Fraction of State Senate that is Democrat	49.80	(13.29)	644
Other program state participation rates (% of population)			
TANF	1.596	(1.024)	672
SSI	2.459	(0.750)	672

Statistics are weighted by state population, excluding population itself. The sample excludes Alaska and Hawaii due to different federal SNAP benefit formulas and Idaho due to Medicaid enrollment data quality issues. The sample period is 1999-2012. Medicaid enrollment and eligibility data are unavailable for some state-years at the beginning and end of the sample period, which is further detailed in Table C1.

1. SEV performance compared to other instruments

I compare the performance of the simulated eligibility variable (SEV) as an instrument for the SNAP participation rate to the performance of the simulated potential benefit variable (SPBV) I describe in Appendix A and other state-level SNAP policy variables from the SNAP Policy Database (USDA ERS 2018).[155] Relative to the SEV and SPBV which primarily derive their variation from rules affecting eligibility, most of these policies affect the information available to households or the costs to households of applying, certifying, or recertifying. Several studies have used sets of these policies in an IV framework to estimate SNAP's impacts in various contexts, typically at the individual- or household-level as opposed to the state-level impacts on Medicaid spending and enrollment I consider.[156]

I estimate first-stage regressions, each modeling the state SNAP participation rate as a function of one policy instrument and the covariates in my baseline models. I present the coefficient estimates and first-stage F-statistics from these regressions in Table C3. The SEV is individually the strongest instrument among those I consider, with an F-statistic of 25.40. The SPBV is the only other instrument with an F-statistic above 10. All other policy variables are underpowered for use as instruments in the context of this study.

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[155] In the order they are shown in Table C3, the policy instruments I consider are the SEV, the SPBV, a dummy for a BBCE expansion of any type, a dummy for the state operating call centers, a dummy for the state operating a Combined Application Project for SSI recipients, the average certification period in months for SNAP units with earnings/with elderly members/without earnings, the proportion of the dollar value of benefits accounted for by EBT, a dummy for the state having a waiver to use a telephone interview in lieu of an in-person interview at initial certification/recertification, a dummy for the state requiring fingerprinting of applicants, a dummy for the state allowing online application, outreach spending per capita in thousands of 2010-adjusted dollars, a dummy for the state using simplified reporting that reduces requirements for households with earnings to report changes in household circumstances, a dummy for the state excluding all vehicles from the asset test, and a dummy for the state excluding any number or exempting any value of vehicles beyond the federal minimum.

[156] E.g., Meyerhoefer and Pylypchuk (2008); Yen et al. (2008); Ratcliffe, McKernan, and Zhang (2011); Gregory and Coleman-Jensen (2013); Gregory and Deb (2015); Almada, McCarthy, and Tchernis (2016).

Table C3. Comparative first-stage regression results with alternate instruments

Instrument	SNAP part. rate	F-statistic	Instrument	SNAP part. rate	F-statistic
SNAP SEV	0.162*** (0.0322)	25.40	Telephone: initial certification	-0.192 (0.416)	0.213
SNAP SPBV	0.270*** (0.0760)	12.65	Telephone: recertification	0.225 (0.281)	0.642
BBCE	0.415* (0.224)	3.433	Fingerprinting	-0.339 (0.485)	0.490
Call centers	0.237 (0.308)	0.594	Online application	-0.0269 (0.299)	0.00808
CAP	0.449 (0.325)	1.900	Outreach spending per capita	-0.0183* (0.00995)	3.368
Average cert. period: households w/ earnings	0.124*** (0.0408)	9.170	Simplified reporting	0.896*** (0.329)	7.434
Average cert. period: households w/ elderly	0.00804 (0.0271)	0.0878	Excludes all vehicles	0.206 (0.304)	0.462
Average cert. period: households w/o earnings	0.0462 (0.0554)	0.697	Alters vehicle treatment	0.224 (0.250)	0.801
EBT	-0.641 (0.412)	2.426	Observations	668	
			Mean SNAP part. rate	9.519	

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
Each coefficient is from a separate first-stage regression using just one instrument. All regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. All regressions are weighted by state population. SNAP participation rate is expressed in percentage points. See Appendix C, Section 1 for a brief discussion of the policy instruments considered here.

Table C4. Other robustness checks

	Medicaid enrollment rate (% of total population)					Medicaid total spending (2010 $)	
	Overall	Children	Adults	Elderly	Blind/disabled	Per capita	Per enrollee
Baseline	0.132***	0.0329**	0.0601**	0.00764	0.00535	-1.429	-58.07***
	(0.0465)	(0.0163)	(0.0280)	(0.00501)	(0.00599)	(2.623)	(17.40)
Altered controls							
No Medicaid elig. controls	0.158***	0.0372*	0.0816**	0.00943	0.0106	-1.238	-68.49***
	(0.0570)	(0.0204)	(0.0319)	(0.00577)	(0.00939)	(3.009)	(18.73)
Upper-limit Medicaid elig. controls	0.137***	0.0353**	0.0636**	0.00773	0.00489	-1.054	-58.82***
	(0.0468)	(0.0163)	(0.0283)	(0.00503)	(0.00600)	(2.741)	(17.51)
Lower-limit Medicaid elig. controls	0.124***	0.0294*	0.0553**	0.00713	0.00548	-1.871	-56.70***
	(0.0460)	(0.0167)	(0.0279)	(0.00503)	(0.00599)	(2.482)	(17.45)
No demographic controls	0.0977**	0.00717	0.0626**	0.00916	-0.00174	-0.389	-44.94***
	(0.0484)	(0.0186)	(0.0299)	(0.00573)	(0.00565)	(2.914)	(16.41)
No controls other than state and year fixed effects	0.125**	0.00989	0.0934***	0.0120	0.00813	-0.618	-64.77***
	(0.0608)	(0.0215)	(0.0351)	(0.00800)	(0.0108)	(3.310)	(20.21)
Add state-specific time trends	0.0154	0.0210	-0.00330	0.00602	-0.00114	-3.880*	-30.13**
	(0.0476)	(0.0206)	(0.0214)	(0.00529)	(0.00532)	(2.279)	(14.88)
Altered sample							
Uniform, non-missing sample	0.129***	0.0329**	0.0601**	0.00764	0.00535	-1.251	-56.64***
	(0.0441)	(0.0163)	(0.0280)	(0.00501)	(0.00599)	(2.531)	(17.00)
Exclude CA	0.112***	0.0162	0.0622**	0.00882*	0.00402	-1.385	-53.14***
	(0.0400)	(0.0150)	(0.0269)	(0.00527)	(0.00631)	(2.677)	(16.08)
Include AK and HI	0.132***	0.0334**	0.0606**	0.00747	0.00491	-1.349	-57.01***
	(0.0448)	(0.0156)	(0.0269)	(0.00485)	(0.00580)	(2.484)	(16.96)
Alternate outcomes using UKCPR Medicaid enrollment counts	0.0954**	-	-	-	-	-	-70.55***
	(0.0408)						(21.29)
No population weights	0.158***	0.0133	0.0917***	0.0122*	0.00501	0.729	-58.90***
	(0.0485)	(0.0139)	(0.0332)	(0.00670)	(0.00662)	(2.924)	(19.85)

Standard errors, heteroskedasticity-robust and clustered by state, are in parentheses. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Baseline regressions include demographic controls, Medicaid eligibility controls, and year and state fixed effects. Baseline regressions are weighted by state population. The SNAP SEV is expressed in percentage points. Medicaid enrollment rates represent the number of enrollees as a percentage of the overall population and are expressed in percentage points. Medicaid spending per capita or per Medicaid enrollee is expressed in 2010-adjusted dollars. Each coefficient and standard error pair are from a separate regression. Each row deviates from the baseline model in some way as described. These deviations include excluding the Medicaid income eligibility limit controls, using the version of these controls that resolves conflicting information by using the higher or lower income eligibility limit, excluding demographic controls, excluding all controls except state and year fixed effects, including state-specific time trends, using a uniform sample excluding state-years with any missing values for any Medicaid outcome or control, excluding California from the sample, including Alaska and Hawaii in the sample, using estimates of Medicaid enrollment from UKCPR's National Welfare Data in place of the enrollment information sourced from MSIS, and not weighting regressions by population.

Appendix D: Chapter III Supplementary Material

Figure D1. Full-county sample and high-poverty county subsample mortality rates

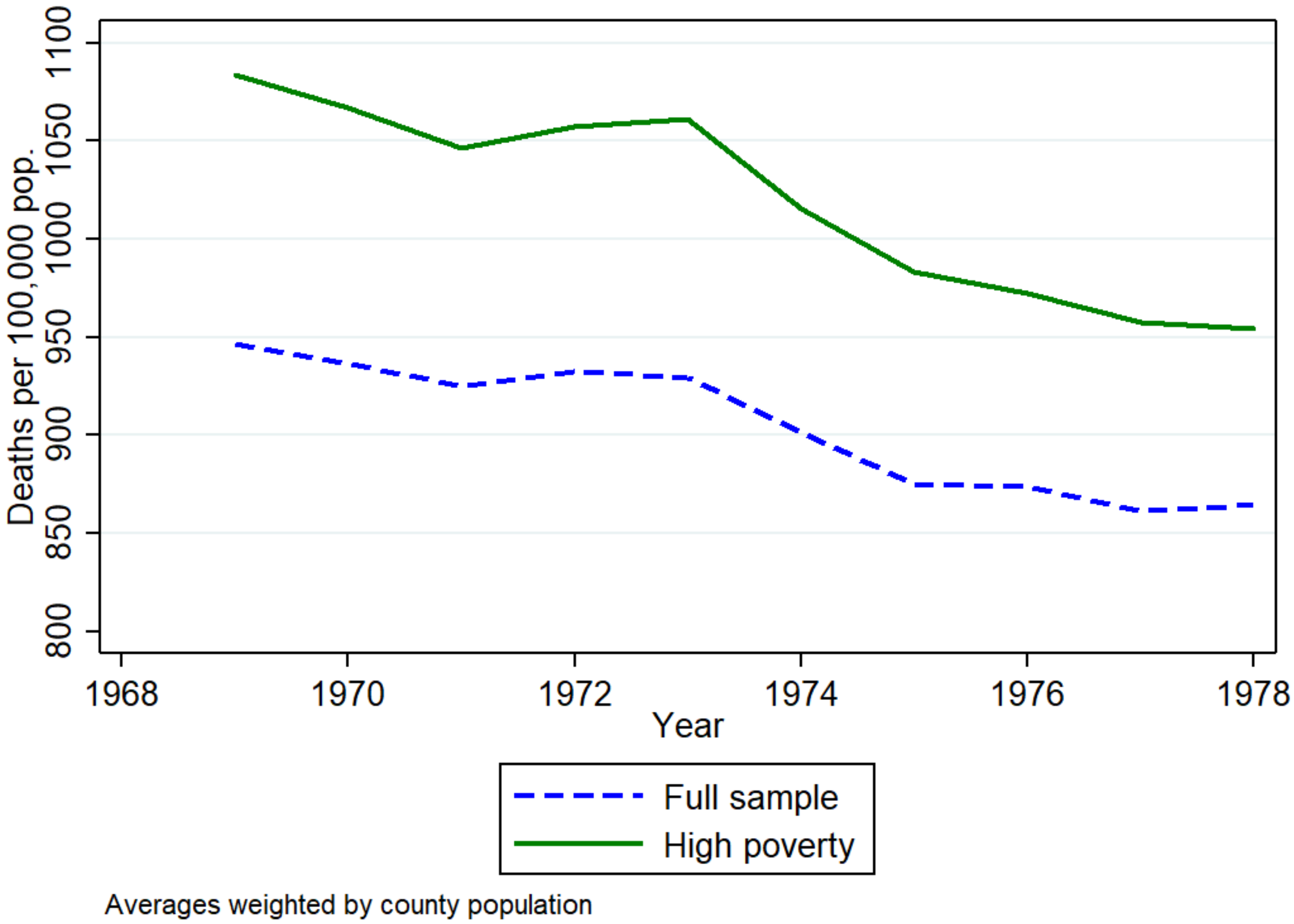

Figure D2. Overall mortality rate per 100,000 population by county

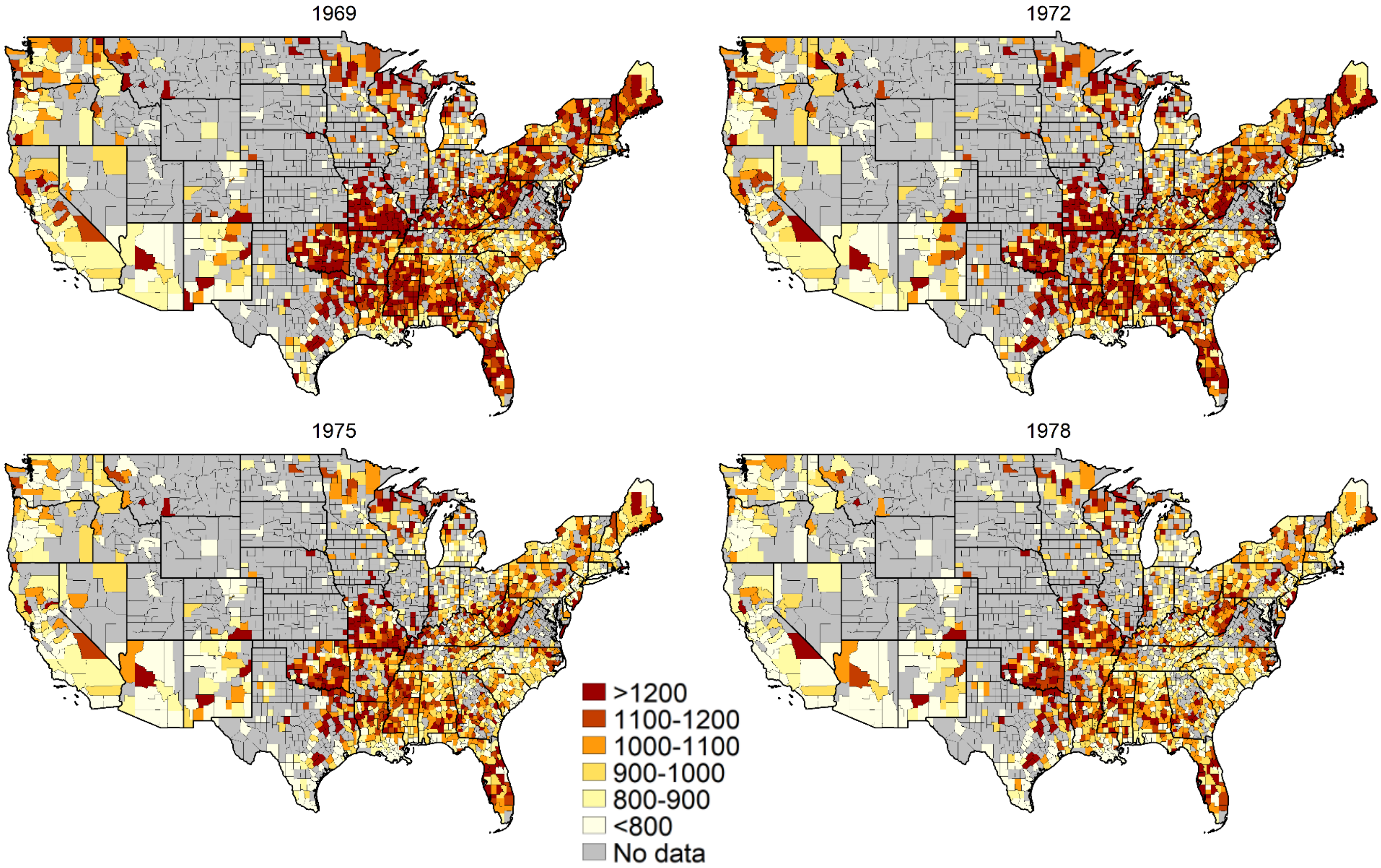

Table D1. Full summary statistics

	Full county sample		High-poverty county sample	
	Mean	Std. dev.	Mean	Std. dev.
FSP rollout				
Year of introduction:				
Weighted by population	1968.7	(3.308)	1968.9	(3.278)
Unweighted	1969.9	(3.270)	1969.3	(3.308)
=1 if FSP was introduced:				
1 or 2 years ago	0.128	(0.334)	0.131	(0.338)
3 or 4 years ago	0.177	(0.382)	0.180	(0.385)
5 or 6 years ago	0.164	(0.370)	0.163	(0.369)
7 or 8 years ago	0.149	(0.356)	0.136	(0.343)
9 or more years ago	0.217	(0.412)	0.214	(0.410)
=1 if FSP is in place	0.885	(0.319)	0.869	(0.337)
Years since introduction	5.186	(3.883)	5.030	(3.837)
Mortality rates: deaths per 100,000 population				
Overall	903.8	(205.2)	1017.9	(208.9)
Female	783.7	(173.2)	833.9	(190.0)
Male	1031.0	(255.2)	1212.2	(265.2)
Black	929.2	(251.3)	1088.3	(236.9)
White	909.0	(220.4)	996.6	(239.8)
Other race	365.0	(296.4)	639.2	(480.9)
0-19	137.6	(40.47)	183.7	(65.26)
20-64	526.6	(126.3)	662.4	(163.4)
65+	5598.1	(624.6)	5558.0	(766.7)
Malignant neoplasms	168.9	(41.19)	155.1	(42.94)
Diabetes	17.28	(7.747)	21.60	(12.16)
Major cardiovascular disease	466.6	(126.9)	520.4	(139.9)
Stroke	55.67	(23.37)	84.20	(41.56)
Pneumonia & influenza	27.63	(11.44)	33.31	(18.22)
Liver disease & cirrhosis	15.94	(8.580)	11.11	(7.064)
Motor vehicle accidents	23.50	(11.59)	39.82	(18.98)
Other accidents	26.62	(10.43)	39.19	(17.74)
Suicide	12.24	(5.043)	12.06	(7.412)
Homicide & legal intervention	10.28	(7.910)	14.76	(9.185)

Table D1. Full summary statistics (continued)

	Full county sample		High-poverty county sample	
	Mean	Std. dev.	Mean	Std. dev.
Annual economic controls in 2010-adjusted dollars per capita				
Personal income	3488.8	(760.6)	2134.0	(384.1)
Government transfers to individuals:				
Income maintenance (excluding food stamps)	0.0416	(0.0293)	0.0465	(0.0195)
Medical benefits	0.0720	(0.0433)	0.0535	(0.0228)
Retirement and disability insurance	0.169	(0.0553)	0.145	(0.0525)
Unemployment insurance	0.0250	(0.0189)	0.0150	(0.0108)
Veteran's	0.0308	(0.00853)	0.0345	(0.0112)
Education and training assistance	0.00609	(0.00432)	0.00447	(0.00480)
Other	0.000745	(0.00399)	0.000702	(0.00205)
1960 county characteristics				
Younger than age 5 (%)	11.47	(1.510)	12.19	(1.869)
Age 65 or older (%)	8.919	(2.554)	8.794	(2.458)
Nonwhite (%)	11.31	(11.87)	32.29	(23.25)
Rural, non-farm (%)	20.93	(20.15)	48.24	(19.60)
Poverty rate (%)	21.13	(13.31)	60.92	(6.256)
Population (unweighted)	87853.0	(263010.2)	20169.4	(16883.7)
Population (annual, unweighted)	106715.9	(302240.6)	20986.2	(19158.9)
Counties	1,716		428	
Years	10		10	
Observations	17,160		4,280	

Mortality rates are weighted by the county population used as the denominator in their construction: either the total population for the overall or cause-specific mortality rates or the relevant subgroup population for subgroup-specific mortality rates. Other statistics are weighted by total county population unless otherwise noted. The sample excludes Alaska and counties for which data is not available for the entire period. The sample period is 1969-1978.

Table D2. Robustness checks using full county sample

	(1) Overall	(2) Overall	(3) Overall	(4) Overall	(5) Overall	(6) Overall	(7) Overall	(8) Overall
1 or 2	2.115	3.503	2.241	2.079	-0.656	-3.004	-0.580	3.271
	(2.414)	(2.336)	(2.404)	(2.404)	(1.795)	(3.524)	(2.935)	(2.954)
3 or 4	-0.450	1.730	-0.324	-0.488	-2.908	-7.951*	-4.963	1.786
	(3.100)	(3.029)	(3.089)	(3.090)	(2.536)	(4.534)	(3.830)	(3.942)
5 or 6	-0.317	0.00126	-0.126	-0.363	-2.141	-6.238	-6.948	0.485
	(4.000)	(3.971)	(3.988)	(3.993)	(3.326)	(5.842)	(4.944)	(5.095)
7 or 8	-2.001	-3.098	-1.750	-2.059	-2.410	-6.026	-9.002	-3.673
	(5.029)	(4.727)	(5.016)	(5.018)	(4.274)	(7.381)	(6.186)	(6.346)
9 or more	-3.068	-3.440	-2.752	-3.146	1.541	-2.430	-8.896	1.116
	(6.004)	(5.724)	(5.988)	(5.991)	(5.097)	(8.384)	(7.114)	(8.068)
Personal income	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Government transfers, breakdown	Yes	Yes	Yes	Yes	Yes			Yes
Government transfers, combined							Yes	
1960 controls * t	Yes	Yes	Yes	Yes			Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-specific time trends					Yes			
State-year FE		Yes						
Medicaid in place for 1 year or more			Yes					
Medicaid in place for 5 years or more				Yes				
Mean mortality rate	903.8	903.8	903.8	903.8	903.8	903.8	903.8	903.8
Adjusted R^2	0.376	0.422	0.376	0.376	0.497	0.211	0.341	0.201
Observations	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
Baseline regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the full county sample. Baseline regressions are weighted by county population. The outcome is the overall mortality rate, expressed as the number of deaths per 100,000 members of the population. FSP dummies indicate how long ago the FSP was rolled out in each county. Columns 2-8 alter the baseline model. (2) includes state-year fixed effects. (3) includes a dummy =1 if Medicaid was introduced in the state in the previous year or earlier. (4) includes a dummy =1 if Medicaid was introduced in the state 5 years ago or earlier. (5) includes county-specific time trends in place of interactions between pre-rollout characteristics and time trends. (6) excludes baseline controls except county or year fixed effects. (7) includes combined real non-food stamp government transfers per capita in place of individual transfer categories. (8) is not weighted by county population.

Table D3. Robustness checks using high-poverty county sample

	(1) Overall	(2) Overall	(3) Overall	(4) Overall	(5) Overall	(6) Overall	(7) Overall	(8) Overall
1 or 2	-4.214	1.076	-3.533	-4.372	-10.62	-2.899	-2.139	7.134
	(7.596)	(9.121)	(7.655)	(7.609)	(8.492)	(7.590)	(7.599)	(8.914)
3 or 4	-8.527	-5.679	-8.000	-8.426	-8.833	-5.442	-6.374	1.236
	(9.798)	(11.56)	(9.852)	(9.817)	(11.11)	(9.801)	(9.799)	(11.37)
5 or 6	-21.20*	-17.08	-20.68	-21.40*	-14.28	-14.66	-19.60	-8.279
	(12.53)	(14.74)	(12.58)	(12.54)	(13.87)	(12.59)	(12.58)	(14.50)
7 or 8	-29.40*	-26.77	-29.00*	-29.49*	-15.94	-20.84	-29.45*	-14.85
	(15.72)	(17.83)	(15.76)	(15.73)	(17.15)	(15.79)	(15.84)	(17.97)
9 or more	-33.89*	-35.38	-33.70*	-33.92*	-17.08	-23.72	-34.47*	-15.72
	(19.82)	(22.20)	(19.86)	(19.84)	(20.81)	(19.80)	(19.90)	(22.42)
Personal income	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Government transfers, breakdown	Yes	Yes	Yes	Yes	Yes			Yes
Government transfers, combined							Yes	
1960 controls * t	Yes	Yes	Yes	Yes			Yes	Yes
County and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-specific time trends					Yes			
State-year FE		Yes						
Medicaid in place for 1 year or more			Yes					
Medicaid in place for 5 years or more				Yes				
Mean mortality rate	1017.9	1017.9	1017.9	1017.9	1017.9	1017.9	1017.9	1017.9
Adjusted R^2	0.139	0.183	0.139	0.139	0.221	0.125	0.134	0.108
Observations	4,280	4,280	4,280	4,280	4,280	4,280	4,280	4,280

Standard errors, heteroskedasticity-robust and clustered by county, are in parentheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.
Baseline regressions include annual economic controls, 1960 pre-rollout characteristics interacted with linear time trends, and county and year fixed effects. Regressions are estimated using the high-poverty county sample. Baseline regressions are weighted by county population. The outcome is the overall mortality rate, expressed as the number of deaths per 100,000 members of the population. FSP dummies indicate how long ago the FSP was rolled out in each county. Columns 2-8 alter the baseline model. (2) includes state-year fixed effects. (3) includes a dummy =1 if Medicaid was introduced in the state in the previous year or earlier. (4) includes a dummy =1 if Medicaid was introduced in the state 5 years ago or earlier. (5) includes county-specific time trends in place of interactions between pre-rollout characteristics and time trends. (6) excludes baseline controls except county or year fixed effects. (7) includes combined real non-food stamp government transfers per capita in place of individual transfer categories. (8) is not weighted by county population.

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Vita

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