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Essays on the Social Safety Net

A dissertation submitted in partial satisfaction of the requirements
for the degree Doctor of Philosophy

in

Economics

by

Jaeyeon Shin

Committee in charge:

Professor Gordon Dahl, Co-Chair
Professor Katherine Meckel, Co-Chair
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Professor Todd Gilmer

2026

The Dissertation of Jaeyeon Shin is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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This dissertation consists of three chapters:

Chapter 1, in full, is currently being prepared for submission for publication. The dissertation author was the sole author and researcher of this paper.

Chapter 2 contains unpublished material coauthored with Shreya Bhardwaj and Katherine Meckel. The dissertation author contributed substantially to this chapter. The chapter, in full, is currently being prepared for submission for publication.

Chapter 3, in full, has been submitted for publication at the *Journal of Public Economics*. The dissertation author was the sole author and researcher of this paper.

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ABSTRACT OF THE DISSERTATION

Essays on the Social Safety Net

by

Jaeyeon Shin

Doctor of Philosophy in Economics

University of California San Diego, 2026

Professor Gordon Dahl, Co-Chair
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This dissertation consists of three chapters examining the design and impacts of safety net programs in the United States.

Chapter 1 examines how access to the Supplemental Nutrition Assistance Program (SNAP) has affected the health and economic outcomes of low-income families in the modern era. Exploiting state-level expansions under Broad-Based Categorical Eligibility (BBCE)—one of the largest SNAP reforms since the 2000s—I use a difference-in-differences design with multiple large-scale data sources on program usage and population health. The expansion increased SNAP participation

and generated spillovers to Medicaid and WIC enrollment. Increased resources improved food security and health care utilization. Although short-term health gains are modest, they are more pronounced among non-White populations, including reductions in low birth weight and obesity. Notably, I find no significant effects on employment status and only a slight decline in hours worked. These findings highlight the role of benefit transfers in supporting long-term health investments and reducing racial disparities, with little evidence of work disincentives.

Chapter 2 addresses a fundamental question in public economics: whether government services should be delivered directly or contracted out. We study the transition of Mississippi's Women, Infants, and Children (WIC) program from state-run warehouses to retail-based food distribution. Using a difference-in-differences design, we show that retail distribution reduced participation by 12–14%, with larger declines among children and disadvantaged groups, while lowering per-participant costs by 32% through warehouse elimination. Higher retail prices were offset by lower redemption rates. The evidence suggests that increased shopping burden, rather than other take-up frictions, drove the decline in participation.

Chapter 3 studies how immigration enforcement shapes participation in safety net programs within mixed-status households. I examine the responses of Hispanic citizens living with individuals facing deportation risk following the enactment of E-Verify mandates, an employment-based immigration enforcement policy targeting undocumented immigrants. Using a difference-in-differences framework, I find that E-Verify mandates reduced household income. Participation responses differ across programs. While SSI participation temporarily increased and SNAP participation remained stable, Medicaid participation declined sharply. The decline in Medicaid participation is more pronounced among groups whose household members face higher deportation risk, consistent with chilling effects limiting program take-up.

Chapter 1 BEYOND THE GROCERY AISLE: HOW SNAP EXPANSION SHAPES HEALTH

1 Introduction

Social welfare programs are designed to improve the lives of those facing economic hardship. In the United States, many of these programs began in the 1960s or 1970s, regardless of whether the benefits were provided in cash or in kind. As one example, the Supplemental Nutrition Assistance Program (SNAP) (formerly the Food Stamp Program (FSP)) is central to discussions of U.S. anti-poverty policy as the largest nutrition assistance program.[1] The stated aim of the program is to improve food security and access to a nutritious diet among low-income households (Bronchetti, Christensen, and Hoynes 2019), by issuing monthly food vouchers redeemable at grocery stores.

However, the effects of SNAP on recipients' welfare *today* are not entirely clear, as both economic conditions and the policy context have changed substantially since its introduction.[2] This raises an empirical question of whether modern expansions of SNAP are effective in reducing food insecurity and poverty, whether they generate additional downstream effects—such as improving health or reducing labor supply—and how they interact with other government programs. While prior research has established a link between the historical implementation of SNAP and long-run outcomes (Hoynes, Schanzenbach, and Almond 2016; Bailey et al. 2024), less is known about its *immediate* effects on recipients in the *modern* era (Hoynes and Schanzenbach 2015).

This paper studies one of the largest reforms to SNAP in recent decades to provide evidence on how access to the program affects recipients' health outcomes today—including food security, health care utilization, and health status—and whether it has adverse effects on labor market out-

[1] In Fiscal Year (FY) 2023, SNAP served 42.1 million individuals, with federal spending of $112.8 billion, accounting for two-thirds of the U.S. Department of Agriculture (USDA)'s total nutrition assistance expenditures (U.S. Department of Agriculture 2024a).

[2] For example, Gross Domestic Product (GDP) per capita has more than tripled (U.S. Bureau of Economic Analysis 2024), and safety net spending has increased more than fivefold (Urban Institute 2025), shifting toward means-tested and work-based programs. Most notably, SNAP participation—and thus program spending—has risen over time despite some fluctuations (U.S. Department of Agriculture 2024b).

comes. Specifically, I exploit state-level variation in SNAP eligibility expansion known as Broad-Based Categorical Eligibility (BBCE). Under BBCE, states can choose to eliminate assets tests or increase the gross income limit, effectively streamlining the income certification process by applying a single, unified cutoff. Gradually adopted by states from 2000 and onward, BBCE expanded SNAP eligibility to households with higher gross income. Prior work shows that it increased SNAP take-up (Anders and Rafkin 2024).

To study the effects of this large expansion on household health and economic outcomes, I compile large-scale data covering 1997-2016 from the following sources: USDA administrative records, the Current Population Survey (CPS)—including its Annual Social and Economic Supplement (ASEC) and Food Security Supplement (FSS)—Vital Statistics Natality Data, and the Behavioral Risk Factor Surveillance System (BRFSS). These datasets provide variables on program participation and benefit amounts, food security, health care use, newborn and adult health conditions, and labor market outcomes. In all cases, I restrict the analysis to low-income individuals who are likely affected by the eligibility expansion, identified based on *pre*-reform demographic characteristics. I estimate the prediction model with the CPS-ASEC and apply it across all other datasets.

Given the differential timing of the BBCE expansion across states, I use a difference-in-differences (DiD) framework to compare the outcomes for low-income individuals or households before and after the BBCE expansion, relative to states without the expansion. I track states three years after adoption to construct a balanced panel of state-years. To address the potential bias in using an ordinary least squares regression with two-way fixed effects (TWFE), I use the alternative DiD estimator developed by De Chaisemartin and d'Haultfoeuille (2024) to address heterogeneous treatment effects.[3]

The identifying assumption is that no unobserved, time-varying factors are correlated with the timing of the BBCE expansion. To assess the validity of my identification strategy, I estimate

[3]See De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Goodman-Bacon (2021a), Callaway and Sant'Anna (2021), Borusyak, Jaravel, and Spiess (2024), and De Chaisemartin and d'Haultfoeuille (2024) for details on the bias problem.

event-study versions of my design and find flat pre-trends across outcomes. I also control for a large number of state-level variables, including economic conditions and other policies, as robustness checks. Because the largest wave of BBCE adoption coincided with the Great Recession, I further test that there is no correlation between implementation timing and economic variables.

I first document a sharp increase in SNAP participation (an 8-12% increase across datasets) and in benefit amounts of $52.8 per year (a 9.76% increase), statistically significant at the 1% and 5% levels, respectively. There is no evidence of pre-trends, and the results remain robust across multiple specifications and extensions, including models that control for unemployment rates and other SNAP policies, and test for underreporting of SNAP participation in the CPS (Meyer, Mok, and Sullivan 2009). In line with evidence of "woodwork effects" in SNAP under BBCE (Anders and Rafkin 2024)—*already* eligible households become more likely to enroll, in addition to those at the margin of eligibility—I evaluate the reform's impact on the entire low-income population throughout the paper.

I next turn to spillovers of the reform to other safety net programs. BBCE also increased enrollment in Medicaid—which offers public health insurance—and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)—which provides nutrition assistance, health screenings, and referrals. Enrollment rose by 1.7 percentage points (a 6.24% increase) for Medicaid and 1.5 percentage points (a 10.03% increase) for WIC, statistically significant at the 1% and 5% levels, respectively. These increases primarily occurred among SNAP participants, highlighting spillover effects across programs—likely driven by increased awareness of program benefits and eligibility or reduced administrative burden through streamlined applications. These findings further provide a potential mechanism for downstream effects on health.

Next, I estimate the effects of the BBCE expansion on health outcomes: food security, health care use, and health status—separately for birth outcomes and adult health conditions. BBCE reduced the probability of low-income households experiencing low food security by 1.8 percentage points (a 8.28% decline) and very low food security by 0.8 percentage points (a 10.86% decline), representing sizable and statistically significant effects. These findings suggest improvements in the quantity, quality, and variety of food consumed among low-income households.

Furthermore, the BBCE expansion improved health care utilization across the entire low-income population. BBCE led to a statistically significant increase of 0.21 prenatal visits during pregnancy (a 2.1% increase). It also significantly raised the likelihood of having a primary care provider, beginning one year after its implementation. This increase in health care use is more pronounced among non-Whites compared to Whites, suggesting that BBCE helped reduce racial disparities in health care utilization. Additionally, the lower likelihood of missing doctor visits due to cost after BBCE supports the positive effects of increased health care provision and available resources on access to care and doctor visits.

Regarding health status, BBCE did not have statistically significant effects on birth weight or adult health conditions—medical diagnoses and self-reported health—in the overall low-income sample. Still, effects are consistently larger for non-Whites across outcomes. Most notably, BBCE significantly increased birth weight by 5.25 grams and reduced the incidence of low birth weight by 0.36 percentage points (a 2.48% decline) among Black mothers—strong indicators of infant and future health—even comparable to the effects observed during the program's initial rollout in the 1960s and 1970s (Almond, Hoynes, and Schanzenbach 2011). There is also suggestive decline in the likelihood of obesity among non-Whites.

Although the findings indicate several positive effects of anti-poverty transfers, concerns remain that safety net programs may reduce work incentives. Regarding labor market outcomes, the BBCE expansion had no substantial effect on the employment status of low-income individuals on the extensive margin of labor supply. It led to only a slight decline in their working hours by 0.3 hours per week (a 1.55% decline) on the intensive margin. The decline in hours worked was more pronounced among individuals with greater flexibility in adjusting their labor supply—those who are married, not the household head, and living with dependents under age 18—and was roughly ten times smaller than during the program's early rollout (Hoynes and Schanzenbach 2012).

In sum, this paper provides novel evidence on the impacts of a major SNAP eligibility expansion on a wide range of household outcomes, including participation in social assistance programs, health, and labor supply. The findings highlight the continued role of benefit transfers in alleviating poverty and reducing inequality, with a reduced concern for work disincentive effects relative to the past. They also capture the effects of extending benefits to near-poor families as well as to the poorest populations traditionally targeted by welfare programs.

This paper contributes to research on the causal effects of the Supplemental Nutrition Assistance Program (SNAP) on household health and economic outcomes. A first set of studies examines the program's initial rollout in the 1960s and 1970s, showing improvements in birth outcomes (Almond, Hoynes, and Schanzenbach 2011), reductions in labor supply (Hoynes and Schanzenbach 2012), and long-run gains in health and economic well-being (Hoynes, Schanzenbach, and Almond 2016; Bailey et al. 2024). By contrast, evidence on the effects of SNAP participation in the modern era is more limited. Two exceptions are (East 2020), who examines the long-run health impacts of early-life SNAP access among children of recent immigrants, and (Bronchetti, Christensen, and Hoynes 2019), who show that changes in the real value of SNAP benefits due to local food prices increase the use of preventive care among children. My study complements this work by focusing on a major eligibility reform that directly expanded program access for a broad share of low-income households, allowing for estimation of policy-relevant effects on health and economic well-being.[4]

A second strand of research studies the Broad-Based Categorical Eligibility (BBCE) reform specifically.[5] Anders and Rafkin (2024) find that BBCE raised participation among already eligible households by lowering information frictions, and Lin (2024) shows that simplifying program administration reduced application burdens and generated government savings of more than $100 million. As a related study on the participants' side, Han (2016) documents that SNAP eligibility expansion during the Great Recession reduced nonfood hardships—such as difficulties in paying for utilities and rent—among near-poor families with gross incomes between 100% and 200% of

[4] Additional studies examine outcomes for narrow groups, such as childless, non-elderly, non-disabled adults: see Dodini, Larrimore, and Tranfaglia (2024) on financial outcomes and Gray et al. (2023) on labor supply responses.

[5] A set of studies emphasize the role of SNAP policies, including BBCE, as key determinants of program participation (Klerman and Danielson 2011; Mulligan 2012; Ganong and Liebman 2018; Stacy, Tiehen, and Marquardt 2018; Dickert-Conlin et al. 2021; Jones et al. 2022).

the FPL. Han (2020) also finds increased participation in other nutritional assistance programs, including free school lunch and WIC. This paper extends this literature by providing one of the first comprehensive analyses of the health effects of SNAP expansion, a novel contribution in the modern era, and by examining the underlying mechanisms through which BBCE influences recipients' well-being.

A complementary body of work examines the effects of benefit transfers on health and household economic outcomes more broadly. Quasi-experimental evidence shows that increases in household resources improve birth outcomes, children's long-run health and educational attainment, and reduce material hardship (Dahl and Lochner 2012; Hoynes, Miller, and Simon 2015; Aizer et al. 2016; Boudreaux, Golberstein, and McAlpine 2016; Cohodes et al. 2016; Bastian and Michelmore 2018; Miller and Wherry 2019; Brown, Kowalski, and Lurie 2020; Goodman-Bacon 2021b). This paper contributes to the literature by providing new causal evidence on the effects of a large eligibility expansion in the nation's primary food assistance program, highlighting how changes in the generosity and reach of SNAP influence well-being among low-income households.

Finally, this analysis relates to broader work on the design of transfer programs. SNAP's income and asset tests have remained largely unchanged since the program's introduction, yet their effectiveness has not been rigorously reassessed in fifty years (Lin 2024). Hence, the reform provides a unique opportunity to study whether eligibility rules in SNAP effectively target resources to those in need and extend benefits to less poor families.

The remainder of the paper proceeds as follows. Section 2 describes institutional background on SNAP and the BBCE expansion. Section 3 describes the datasets, and Section 4 provides the empirical strategy. Section 5 presents the results on program participation, and Section 6 covers food security, health care utilization, and health status. Section 7 discusses labor market outcomes, and Section 8 concludes.

2 Institutional Background

2.1 Supplemental Nutrition Assistance Program (SNAP)

SNAP is one of the largest safety net programs in the U.S., with the goal of meeting the basic needs of low-income families by supplementing their grocery budgets for nutritious food. In fiscal year (FY) 2023, the program provided food benefits to about 42.1 million individuals per month, which is roughly one in eight Americans, making it the largest food assistance program in the U.S. Total federal spending on SNAP was $112.8 billion, accounting for 68% of the U.S. Department of Agriculture's (USDA) nutrition assistance spending, and the average monthly benefit per household was about $400 (U.S. Department of Agriculture 2024a).

SNAP benefits are distributed monthly to participating households in a lump sum, with funds automatically loaded onto an Electronic Benefit Transfer (EBT) card, which works like a debit card. These benefits can only be spent on purchasing groceries at authorized retailers.[6] Recipients use their EBT card at regular store checkout lines to pay for their purchases and can supplement with other payment methods if their total purchase amount exceeds the card's balance or includes ineligible items. Most households spend more on SNAP-eligible items than their SNAP benefits cover, so they are not constrained by the in-kind nature of SNAP (relative to an equivalent transfer in cash) (Hoynes, McGranahan, and Schanzenbach 2015; Hastings and Shapiro 2018). Still, consistent with the hypothesis of mental accounting—that households treat different sources of income differently (Thaler 1999)—recent empirical evidence shows that people use SNAP benefits differently from cash assistance (Hastings and Shapiro 2018; Finkelstein, Chorniy, and Notowidigdo 2025).[7]

[6] SNAP benefits can be used to buy any food (product) for home consumption, including fruits and vegetables, meat, poultry, fish, dairy products, breads, cereals, snacks, non-alcoholic beverages, and seeds and plants that produce food for the household to eat. However, the benefits cannot be used to purchase beer, wine, liquor, cigarettes, tobacco, vitamins, medicines, supplements, live animals (except shellfish, fish removed from water, animals slaughtered prior to pick-up from the store), hot foods at the point of sale, and any non-food items such as pet foods, cleaning supplies, paper products, other household supplies, hygiene items, and cosmetics. The information is from the USDA's website: https://www.fns.usda.gov/snap/eligible-food-items.

[7] Using novel retail data, Hastings and Shapiro (2018) find that the marginal propensity to consume (MPC) food out of SNAP—that is, the increase in food expenditure from an additional dollar—is much greater than the MPC out

The funding for SNAP consists of two components. First, benefit costs represent the expenditures used to provide food assistance to participants, specifically the amount spent on food or EBT benefits. These SNAP benefits account for more than 90 percent of total program costs and are entirely funded by the federal government (Pew Research Center 2023). Although SNAP is a federal program, it is administered by state agencies through local offices, which are responsible for various operational aspects of the program—such as determining recertification periods or accepting online applications.[8] To support these activities, the second component of SNAP funding covers administrative costs, including expenses related to processing applications, determining eligibility, issuing benefits, and conducting fraud inspections. States receive reimbursement for half of these administrative costs from the federal government and are responsible for the remaining half.

SNAP is designed to prevent food insecurity, but its regulations also emphasize the role of work and responsibility. To that end, the program imposes two types of work requirements, unless an individual qualifies for an exemption. First, the general requirements apply to individuals aged 16–59 and involve registering for work, participating in Employment and Training (E&T) or workfare, accepting suitable job offers, and avoiding voluntary job loss or reductions in work hours below 30 per week without valid cause. Next, able-bodied adults (age 18-54) without dependents (ABAWDs) need to meet an additional requirement to receive SNAP benefits for more than three months within a three-year period. They must work, or participate in a work program or workfare, for at least 80 hours per month.[9]

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of cash. Also, Finkelstein, Chorniy, and Notowidigdo (2025) corroborate the non-fungibility of SNAP benefits, as SNAP does not increase emergency department visits for drug or alcohol use, unlike Supplemental Security Income (SSI) benefits provided in cash. This stands in contrast to earlier work, which aligns with theoretical predictions for inframarginal households. Hoynes and Schanzenbach (2009) find that the MPC food out of SNAP is higher, although not statistically different from the MPC food out of cash income.

[8]Other state-level SNAP policies, which may be correlated with a state's decision to adopt BBCE or influence SNAP participation and participant outcomes, will be discussed later in Section 2.3.

[9]Information about specific requirements and exemptions is available here: https://www.fns.usda.gov/snap/work-requirements.

2.2 Broad-Based Categorical Eligibility (BBCE)

Prior to 1999, SNAP eligibility was primarily determined by federal rules. However, starting in 2000, states were given the option to expand SNAP eligibility to a larger number of households through BBCE, making it one of the most significant reforms in the modern era. This change introduces a unique state-level variation in SNAP—particularly in eligibility criteria, which have not been evaluated since the program's inception in the 1960s and 1970s—within a context of limited variation for program evaluation (Hoynes and Schanzenbach 2018; Lin 2024).

SNAP eligibility is mostly established by three criteria: assets, gross income, and net income, where net income equals gross income minus deductions such as 20% of earned income, dependent care, medical expenses, and excess shelter costs. Specifically, federal law mandates that participating households must meet the following requirements: (i) assets—countable resources such as cash or money in a bank account—below the federal limit set by the USDA ($3,000 for most households or $4,500 if at least one household member is age 60 or older or disabled, as of March 2025); (ii) a gross monthly income below 130% of the Federal Poverty Level (FPL) ($3,380 per month for a family of four in FY 2025); and (iii) a net income below 100% of the FPL ($2,600 per month for a family of four in FY 2025) (U.S. Department of Agriculture 2024c).

Under BBCE, there were no changes to the net income test. However, states were given the option to eliminate the asset test or increase the gross income limit. Since previous studies consistently indicate that asset rules affect only a small number of families and contribute minimally to the recent increase in SNAP participation (Eslami 2014; Ganong and Liebman 2018; Anders and Rafkin 2024), this study focuses on the 28 states and the District of Columbia that chose to increase the gross income limit.[10] The new gross income eligibility thresholds vary, ranging from 160% to the maximum threshold of 200% of the FPL, compared to the pre-existing limit of 130%. The expanded eligibility primarily affected households with lower disposable income due to large deductions from gross income, particularly those with high medical, child care, and shelter costs

[10]Each of them additionally chose to eliminate the asset test. An additional 12 states eliminated the asset test only. 10 states did not make any changes.

(Hoynes and Schanzenbach 2015).[11]

Figure 1.1 suggests that a quasi-experimental approach is feasible by leveraging policy-induced temporal and spatial variation in states' BBCE adoption. Figure 1.1-(a) plots the change in the number of states adopting BBCE. With the exception of South Carolina, which revoked its expansion in 2009, the number of BBCE states has steadily increased. The time series graph highlights two notable jumps: (i) the initial implementation of BBCE in 2000 and (ii) the period around the Great Recession in 2009.

Regarding states' incentive to adopt BBCE, one driver could be the desire to reduce program costs. However, as described in Section 2.1, most of the total program costs consist of food benefits, which are fully reimbursed by the federal government. Although a small portion, states may seek to reduce administrative costs, as they receive only partial reimbursement (50%) from the federal government. BBCE has the potential to lower administrative costs per case, particularly by eliminating the asset test. Nevertheless, the increase in applications from expanded eligibility could raise administrative burdens, making the overall effect ambiguous ex ante. In short, the financial incentive to adopt BBCE is limited given SNAP's funding structure.

Instead, a key determinant of BBCE adoption appears to be each state's political stance toward welfare programs. Figure 1.1-(b) presents a map states categorized by their maximum gross income limit under the BBCE expansion as of 2016, which primarily includes coastal states with more generous government transfers.[12] Income eligibility thresholds vary across states: white states did not expand the gross income limit, dark blue states implemented the largest expansion up to 200% of the FPL, and other states fall in the middle, with limits ranging from 160% to 185%. By the end of the period, 55% of BBCE states had expanded income limits to the maximum threshold of 200% of the FPL.

[11] This is because both the net income test and the benefit schedule—the maximum allowance minus 30% of net income—remained unchanged.

[12] Specifically, when comparing the map of the 29 states that adopted BBCE by 2016 with the results of the 2000 U.S. presidential election, 20 of them were blue states. This suggests that while some red states, including Texas and South Carolina, also adopted the reform, the majority of treated states were liberal. Consistent with this, the BBCE adoption rate among blue states increased rapidly over time and reached 95% by 2016, compared to just over 25% in red states.

2.3 Other SNAP Policies

Since states administer the program, they can adopt different SNAP policies, leading to other SNAP policy variations during my study period. To account for these effects, I categorize twelve policies in the SNAP policy database into four groups and create four indices by taking the simple average within each group: (i) eligibility to receive benefits, (ii) transaction costs involved in applying, enrolling, or maintaining benefits, (iii) stigma associated with participation, and (iv) outreach efforts to spread information related to eligibility criteria and the process for receiving benefits (Ganong and Liebman 2018; Stacy, Tiehen, and Marquardt 2018; Dickert-Conlin et al. 2021; Jones et al. 2022).[13]

The first category (i.e., eligibility) includes three SNAP policies: whether states exempt at least one vehicle or all vehicles from the asset test, and whether states impose eligibility restrictions for non-citizens. The second category (i.e., transaction costs) includes six policies: whether states discontinue using short certification intervals of three months or less, whether states implement the simplified reporting option that reduces requirements for reporting changes in household circumstances (e.g., only requiring households to report income changes that make them ineligible for benefits between recertification dates), whether states allow households to submit SNAP applications online, whether states ease enrollment procedures for Supplemental Security Income (SSI) recipients, whether states waive the face-to-face requirement for recertification and allow phone interviews, and whether states establish call centers to handle administrative tasks related to the program. The third category (i.e., stigma) includes two policies: whether states issue a proportion of SNAP benefits via an EBT card, and whether states require fingerprinting during the application process. The fourth category (i.e., outreach) includes one policy: whether states have federally funded TV or radio ad campaigns to raise awareness of SNAP among eligible non-participants.[14]

[13] The data are available here: https://www.ers.usda.gov/data-products/snap-policy-data-sets. My findings are robust to the construction of these indices. Examples include treating the twelve policies individually without averaging, creating a simple average of all policies as a single variable, and applying the same variations to subsets of policies following Ganong and Liebman (2018).

[14] Another potential confounder is the SNAP Provisions of the American Recovery and Reinvestment Act of 2009 (ARRA). The temporary increase in maximum SNAP benefits and additional federal funding for SNAP administration applied uniformly across all states and did not vary at the state level. State-level differences in other

3 Data

To measure the impacts of BBCE adoption on household outcomes, I combine microdata on individual- and household-level program usage and health outcomes from the Current Population Survey (CPS), Vital Statistics Natality Data, and the Behavioral Risk Factor Surveillance System (BRFSS). Details on USDA administrative data are provided in the Appendix (Section A1.2.2). Summary statistics for key outcome variables are presented in Table 1.1.

3.1 Current Population Survey (CPS)

The CPS provides a rich set of demographic and economic variables for a representative sample of individuals and households. I use two types of CPS data from 1997 to 2016 to estimate the effects of BBCE on various outcomes and construct a prediction model that identifies a sample of low-income families for my analyses.

First, I use the Annual Social and Economic Supplements (CPS-ASEC), also known as the March Supplements. The CPS-ASEC provides SNAP-related variables, including whether an individual reports that household members receive SNAP benefits and for how long. Although self-reported program participation variables are subject to underreporting (Meyer, Mok, and Sullivan 2009), the CPS-ASEC complements USDA administrative data, which only report total participation counts and average benefit per recipient by state. Therefore, I use the CPS-ASEC to examine the effect of BBCE on SNAP participation and benefit amounts, participation in other safety net programs, and labor market outcomes such as employment status and weekly hours worked.

Throughout the paper, I focus on low-income families most likely to be affected by the BBCE expansion to ensure sufficient statistical power. Since income may be endogenous to the

SNAP policies—such as enrollment processes, administrative flexibility, and outreach efforts—are accounted for in the main specification, except for the suspension of the time limit for able-bodied adults without dependents (ABAWDs). ARRA initially suspended the three-month time limit for ABAWDs nationwide until September 2010. After the nationwide suspension expired, states could either continue waiving the time limits or reinstate them, leading to some state-level variation. However, my results remain robust to controlling for the effects of ABAWD waivers, based on the database of Harris (2021). To maintain consistency with previous studies on BBCE, I do not control for them in my preferred specification. The information on the ABAWD time limit is available at: https://www.cbpp.org/research/food-assistance/waivers-add-key-state-flexibility-to-snaps-three-month-time-limit.

policy, I *predict* the FPL ratio—family income divided by the FPL—using *pre*-BBCE characteristics from the CPS-ASEC (1997-1999). I then apply the same estimated prediction model to subsequent datasets to identify the low-income sample, defined as the lowest 35% of the predicted FPL ratio.[15] The full derivation, following the approach of Rittenhouse (2023), is provided in Section A1.1. Using the CPS-ASEC from 1997 to 2016, Table 1.2 presents average demographic characteristics for low-income individuals compared to the overall population in the dataset. Low-income individuals are slightly younger, more likely to be Black or Hispanic, and less likely to have a high school degree or be married, reflecting characteristics correlated with low socioeconomic status.

Next, I use the Food Security Supplements (CPS-FSS) to estimate the effect of BBCE on a household's probability of experiencing food insecurity in the past 12 months.[16] The food security measures in the CPS-FSS were developed by the Food and Nutrition Service (FNS) of the USDA to identify households that experience limited or uncertain access to adequate food for economic reasons. Food security is measured by the number of affirmative responses to 18 related questions, and households are classified into 3 categories: (i) food secure, (ii) low food secure, and (iii) very low food secure. To measure the effects on food insecurity, I create two indicators. The first measure captures low or very low food security in the past 12 months, and the other captures very low food security only.[17]

3.2 Vital Statistics Natality Data

To measure the impact of the BBCE expansion on birth weight and prenatal care, I use restricted-use individual-level Vital Statistics Natality data from 1997 to 2017. The data cover the entire universe of births in the U.S. and provide detailed information about individual births,

[15]This threshold corresponds to the share of individuals whose family income was below 200% of the FPL prior to BBCE.

[16]Between 70% and 76% of households interviewed in the December CPS complete the FSS, with rates consistent over time. For example, the 76% FSS response rate in 2022 is based on the number of households, as reported at: https://ers.usda.gov/sites/default/files/_laserfiche/DataFiles/50764/techdoc2022.pdf?v=51640.

[17]Being low food secure means that households experience a reduction in the quality, variety, and desirability of their diets, without a substantial reduction in the quantity of food intake and normal eating patterns. Being very low food secure indicates that households experience a disruption in the eating patterns of one or more household members and a reduction in food intake due to a lack of money or other resources for food.

including demographic characteristics of mothers and fathers, maternal state of residence, and birth outcomes such as birth weight, gestation weeks, and prenatal care. As outcomes for birth weight among low-income births, I use a continuous measure of birth weight and an indicator for low birth weight (birth weight $<$ 2,500 grams, approximately 5.5 lb). I also estimate the effects on the total number of prenatal visits, a potential driver of improvements in birth outcomes.

I assign the treatment timing to the beginning of the third trimester, which is the most critical period for determining birth weight (Almond, Hoynes, and Schanzenbach 2011). Since each observation reports the date of birth and gestation weeks, I calculate the conception date and add two quarters to it for all births. Then the sample is restricted to low-income households from 1997 to 2016 whose predicted FPL ratio falls within the bottom 35%, as described in Section A1.1.

3.3 Behavioral Risk Factor Surveillance System (BRFSS)

The second main source of data on health outcomes is the BRFSS (1997–2016), an annual cross-sectional telephone survey that collects individual-level health information from adults aged 18 and older. The BRFSS is currently the largest continuously conducted health survey system in the world, with more than 400,000 interviews conducted nationwide each year.

My analysis relies on the core modules conducted in every state, which include questions on various health outcomes such as medical diagnosis, physical health measures, self-reported health, and health care access and utilization. Specifically, the outcomes include indicators for asthma, diabetes, and coronary heart disease; body mass index (BMI)—calculated as weight (in kilograms) divided by height (in meters) squared; an indicator for obesity (BMI $>$ 30); indicators of poor physical or mental health in the past 30 days; and an indicator for having a primary health care provider. They largely overlap with measures used in prior studies of SNAP's effects on health (Hoynes, Schanzenbach, and Almond 2016; Bronchetti, Christensen, and Hoynes 2019) and are consistently available in the BRFSS across all years.

Additionally, the core modules provide detailed demographic characteristics and state of residence of each individual. I exclude individuals who reported being non-residents of their respective states. As with all other analyses using different datasets, I restrict the sample to low-income individuals as the lowest 35% of the predicted FPL ratio.

3.4 Time Series: SNAP Participation

Figure A1.1 shows time series graphs of SNAP participation from two different data sources. Figure A1.1-(a) presents SNAP participation rates among low-income families using the CPS-ASEC, while Figure A1.1-(b) displays the total number of SNAP participants (in millions) using USDA administrative data. The two graphs follow a similar trend, with two significant increases in SNAP participation: when BBCE was first implemented in 2000 and around the Great Recession. These patterns align with the trend in state-level BBCE adoption over time, as shown in Figure 1.1-(a).

The widening gap in SNAP participation counts between treated and never-treated states may be driven by changes in population size or the composition of people rather than the BBCE expansion, particularly around the Great Recession. To address this, Figure 1.2 presents a time series graph of the share of the predicted SNAP-eligible population among the low-income sample of the CPS-ASEC (see Section A1.1), separately by each state's BBCE adoption decision. The predicted SNAP-eligible population is defined as individuals whose predicted FPL ratio—family income divided by the FPL—falls below the gross income limit for SNAP eligibility in the pre-treatment period (1997-1999): 130% of the FPL without BBCE, and 160%, 165%, 180%, or 200% of the FPL with BBCE.

As shown by the red line, the fraction of predicted SNAP-eligible individuals remains stable in states without BBCE adoption. In contrast, the blue line—representing states with BBCE adoption—follows a different pattern, leading to a widening gap between treated and never-treated states. In addition to the two significant jumps observed in the number of states adopting BBCE (Figure 1.1-(a)) and in SNAP participation (Figure A1.1), another sharp increase in the predicted SNAP-eligible population share occurs in 2014. This is when California adopted BBCE, accounting for about 10% of the sample—the largest share among states that implemented BBCE.

4 Identification Strategy

This section describes the identification strategy employed to estimate the causal effects of the BBCE expansion on household well-being and economic outcomes of low-income families. I rely on the staggered treatment timing of the BBCE expansion across states, using a difference-in-differences (DiD) framework. The choice of specification depends on whether the outcomes are estimated at the level of individual or household i, or at the state level s as follows:

$$Y_{ist} = \alpha + \beta BBCE_{st} + \Omega' X_i + \Gamma' Z_{st} + \lambda_s + \mu_t + \epsilon_{ist} \tag{1.1}$$

$$Y_{st} = \alpha + \beta BBCE_{st} + \Gamma' Z_{st} + \lambda_s + \mu_t + \epsilon_{st} \tag{1.2}$$

$Y_{(i)st}$ is the outcome variable for (an individual or household i) in state s and year t, including program participation and SNAP benefit amounts, household food insecurity, health care use, health status, and labor market outcomes. The treatment variable, $BBCE_{st}$, is an indicator for whether state s has expanded SNAP eligibility under BBCE in year t.[18] λ_s and μ_t are state and year fixed effects, and $\epsilon_{(i)st}$ is the error term clustered at the state level. To analyze the dynamics of treatment effects and test the validity of the parallel trends assumption in the pre-treatment period, I also estimate event-study versions of Equations 1.1 and 1.2. Across all datasets, I observe two years of post-period and three years of pre-period data for treated states to construct a balanced panel of state-years.[19]

I consider various specifications with different sets of covariates. X_i represents a vector of individual- or household-level covariates, including age, gender, race and ethnicity, marital status, and education level (for households, this refers to the household head). Z_{st} is a set of state-level covariates, including unemployment rates and SNAP policy indices—eligibility, transaction costs,

[18] I also consider alternative specifications that model program participation using continuous treatment variables. These continuous treatments are defined as interactions between the BBCE indicator and either (i) the gross income limit, which increases under BBCE, or (ii) the predicted SNAP-eligible population in the given state. The results are presented in Section A1.3 and imply similar magnitudes, so I use a binary indicator in my preferred specification.

[19] Since Illinois adopted the BBCE expansion of the gross income limit in 2016, which is the last year of my data period, I exclude Illinois from the analysis to estimate dynamic treatment effects in the post-treatment period.

stigma, and outreach—defined in Section 2.3. They are included to control for their potential influence on SNAP participation (Mulligan 2012; Ganong and Liebman 2018) and the outcomes of program participants. For health outcomes, I also control for Medicaid expansion under the Affordable Care Act (ACA) to account for differential effects across states due to variations in insurance coverage generosity for low-income individuals. I use the specification with the full set of controls as the main specification, while estimates from alternative specifications with different covariates are reported as robustness checks. For all specifications, I apply person- or household-level weights from the data where applicable.

The identifying assumption is that there are no unobserved, time-varying factors correlated with the timing of BBCE adoption. Given this assumption, β is the coefficient of interest in Equations 1.1 and 1.2, capturing the effect of the BBCE expansion (on individuals or households) in treated states relative to (those in) non-treated states before and after adoption. One concern is that the largest wave of BBCE adoption occurred around the Great Recession, as shown in Figure 1.1-(a). The potential confounding effects of state economic conditions on BBCE adoption could threaten the causal interpretation of my findings. However, when I estimate the event-study model with the unemployment rate as the dependent variable, I do not find that the timing of BBCE adoption is correlated with unemployment rates (Figure A1.7). See Section A1.2 for details.

The literature on potential biases in using ordinary least squares regression with two-way fixed effects (TWFE) has expanded, particularly in cases where (i) the timing of treatment adoption varies and (ii) treatment effects are heterogeneous across units and over time (De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021; Goodman-Bacon 2021a; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024; De Chaisemartin and d'Haultfoeuille 2024). Since states adopted the BBCE expansion at different times, I use the estimator developed by De Chaisemartin and d'Haultfoeuille (2024) (DCDH) to address potential biases from the negative weighting problem in the TWFE estimator.[20]

The parameter of interest is defined as a weighted average of the average treatment effects

[20] Approximately 20% of the weights are negative across all outcomes, further supporting the adoption of this method.

for groups that switched into or out of treatment. Specifically, states adopting BBCE are compared with states that remained untreated in both years, rather than with those that had already adopted it in both years. Similarly, states that revoked the expansion are compared with those that remained treated in both years.[21]

5 Results: Program Participation

The following three sections present the main estimation results for program participation (Section 5), health outcomes (Section 6), and labor market outcomes (Section 7) following the BBCE expansion, using the DCDH estimator. In this section, I begin with the most direct effect of BBCE on SNAP participation and then examine how this effect extends to participation in other safety net programs.

5.1 SNAP Participation

Table 1.3 shows the DiD estimates for SNAP participation and benefit amounts from the CPS-ASEC, estimated using Equation 1.1. The DiD estimate in column (1) shows the effect of BBCE on SNAP participation among low-income individuals as a binary outcome. Following the BBCE expansion, I find that SNAP participation rises sharply by 2.5 percentage points, statistically significant at the 1% level. This represents an 11.74 percent increase relative to the baseline participation rate among individuals predicted to be low-income in the pre-treatment period.[22]

In columns (2) and (3), I additionally examine effects on SNAP benefit amounts (in 2016 dollars)—including zero for non-participants—and find an increase of $47.4 to $52.8 per year (a 9-10% increase across specifications), all statistically significant at the 5% level. Since the benefit schedule did not change under BBCE, this increase is driven by higher participation.

[21] Unlike other alternative DiD estimators, the DCDH estimator allows for the estimation of treatment effects when a treatment can both "turn on" and "turn off." This is relevant for BBCE, as South Carolina revoked its expansion in 2009.

[22] Since SNAP participation increased not only among the newly eligible but also among the *previously* eligible population, consistent with Anders and Rafkin (2024) (i.e., woodwork effects), I estimate the effect of BBCE on the entire low-income population throughout the paper.

Figure 1.3 shows my main event-study estimates of the impact of BBCE on SNAP participation and benefit amounts. I set the period $t = -1$ as the reference period. In each period, the blue dots show the DiD estimates for the outcome with 95% confidence intervals, based on spatial and temporal variation in the BBCE expansion. I do not find evidence of differential trends in the pre-period, supporting the identifying assumption. The estimates for post-treatment periods in Figure 1.3 show a significant increase in both outcomes over time, which is consistent with the average increase following the BBCE expansion previously shown in Table 1.3.

Table A1.1 demonstrates that the increases in SNAP participation (Panel A) and benefit amounts (Panel B) are robust to the inclusion of different covariates. In the Appendix, Sections A1.2, and A1.3 present additional robustness tests and extensions that further support the validity of my results, including: (i) similar results when compared with USDA administrative data; (ii) evidence that underreporting of SNAP participation in the CPS (Meyer, Mok, and Sullivan 2009) is not affected by BBCE; (iii) confirmatory results from several falsification tests; (iv) similar magnitudes when using a continuous treatment measure; and (v) no effect of BBCE on vendor participation.

5.2 SNAP Retailer Participation

In addition to examining the effects of the BBCE expansion on individual and household SNAP participation, I also present evidence on vendor participation. This analysis helps assess whether the reform affected access to authorized retailers, which could serve as another channel driving increased SNAP participation.

I use historical data on all SNAP-authorized retailers, which cover nearly all food stores. The data include addresses, start and end dates of SNAP authorization, and store type variables for each vendor.[23] To identify the universe of food stores where most people usually shop (i.e., usual stores), I define non-usual stores as those including specialty stores (i.e., bakery specialty,

[23]The data are publicly available here: https://www.fns.usda.gov/snap/retailer/historical-data. Store type definitions are available at: https://www.fns.usda.gov/snap/store-definitions.

fruits/vegetables specialty, meat/poultry specialty, and seafood specialty), non-retail stores (i.e., delivery routes, farmers' markets, and military commissaries), and small stores (i.e., convenience stores, small grocery stores, medium grocery stores, and combination groceries/others). Approximately 20% of SNAP stores are classified as usual stores, which include food buying co-ops, large grocery stores, super stores, supermarkets, and wholesalers.[24] I count the total number of SNAP stores, usual stores, and non-usual stores in each state-year cell and use participation counts as outcome variables.

While there is a huge increase in the number of individuals and households participating in SNAP, Table A1.2 and Figure A1.2 present no significant effects on the number of stores participating in SNAP (column (1) and Figure A1.2-(a)). By store type, there are also no significant changes in the number of SNAP vendors, for both usual stores (column (2) and Figure A1.2-(b)) and non-usual stores (column (3) and Figure A1.2-(c)). The results suggest no meaningful change in access to SNAP stores following BBCE.

5.3 Medicaid and WIC Participation

Before examining the effects on health outcomes, I investigate whether the SNAP eligibility expansion—and the resulting increase in SNAP participation—increased take-up of other welfare programs for low-income families, using the CPS-ASEC. This approach allows for a more accurate interpretation of effects on downstream outcomes and informs broader policy design by highlighting the reform's cross-program impacts. Specifically, I examine whether BBCE increased participation in Medicaid and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).

Medicaid is a federal and state public health insurance program that offers free or low-cost coverage to eligible low-income adults, children, the elderly, and people with disabilities. It provides more affordable insurance and health care services to enrollees, including prenatal doctor visits. WIC is a federal nutrition assistance program, designed to address nutritional deficiencies

[24] The store type of 1.63% of SNAP stores is unknown and classified as usual stores.

and improve the health outcomes of low-income pregnant and postpartum women and their young children. WIC benefits include monthly vouchers for nutritious foods, nutrition education, health screenings, and referrals to other social services.

Figure 1.4 and Table 1.4 show that the BBCE expansion increased Medicaid participation by 1.7 percentage points (a 6.24% increase) and WIC participation by 1.5 percentage points (a 10.03% increase).[25] The estimates are statistically significant at the 1% level for Medicaid participation and at the 5% level for WIC participation. Given that the SNAP participation rate among low-income individuals increased by 2.5 percentage points (an 11.74% increase) due to the expansion (column (1) of Table 1.3), the increases in Medicaid and WIC participation are also substantial.[26] Additional analysis in Table A1.4 examines the impact of BBCE on participation in Medicaid and WIC, accounting for whether individuals *also* participate in SNAP. The estimates suggest that the rise in Medicaid and WIC participation following BBCE is driven primarily by increased SNAP take-up—a pattern indicative of potential program spillovers.[27]

These findings motivate a further investigation of the mechanisms that enable enrollment in multiple safety net programs. SNAP participation facilitates WIC enrollment through adjunctive eligibility (Randall et al. 1998), which allows WIC applicants to bypass income documentation requirements if they are already enrolled in other means-tested programs (e.g., SNAP and Medicaid) with verified income. This highlights the role of transaction costs in reducing take-up, while also suggesting that information costs matter (Moffitt 1983; Currie 2004; Anders and Rafkin 2024), since information about WIC may be provided during SNAP enrollment.

[25] Since the variable for WIC participation is only available for females in the CPS-ASEC, I restrict the sample to females aged 15 to 39 with children in the household.

[26] Medicaid has expanded to include pregnant women and children with higher incomes, with the most significant expansion occurring under the Affordable Care Act (ACA) in 2014—which extended eligibility to individuals with family incomes at or below 138% of the FPL. As a robustness check for Medicaid participation, the estimates in Table A1.3—which exclude indicators for the ACA expansion and limit the data to pre-ACA years—show that while the ACA expansion slightly increases Medicaid participation, most of the effect is driven by the BBCE expansion itself, rather than by the ACA. The results also remain robust when controlling for state Medicaid income eligibility limits, which vary over time.

[27] In Table A1.4, the effect on Medicaid participation (column (1)) can be expressed as the sum of the estimates for those who also participate in SNAP (column (2)) and those who do not (column (3)); the same logic applies to WIC participation (columns (4)–(6)). The increases in the share of low-income individuals participating in both Medicaid and SNAP (column (2)) and in both WIC and SNAP (column (5)) account for most of the observed increases in single program participation (columns (1) and (4)).

Also, Table A1.5 provides suggestive evidence that the increase in Medicaid participation is greater in states that (i) use SNAP data to verify Medicaid eligibility, particularly for children, to facilitate enrollment or renewal (e.g., Express Lane Eligibility), and (ii) offer a combined application portal for SNAP and Medicaid.[28] In sum, these findings support the role of both the information and transaction cost channels in promoting multi-program participation through SNAP.[29]

6 Results: Health Outcomes

In previous sections, I present evidence that BBCE significantly increased participation in SNAP, Medicaid, and WIC among low-income families, suggesting the potential for improvements in food security, health care utilization, and health status. For example, greater or higher-quality food consumption can directly improve both food security and health status. Increased participation in Medicaid and WIC provides health care–related support, enhancing access to services and, in turn, improving health. Moreover, increased economic security and reduced stress from additional resources for food and medical care can facilitate better health management and investment.

To examine the effects of the modern SNAP expansion beyond nutrition assistance, this section estimates the impact of BBCE on health-related outcomes among the low-income population. I separately assess the effects on food security (Section 6.1), health care utilization (Section 6.2), and health status (Section 6.3).

6.1 Food Security

As a food assistance program, a primary goal of SNAP is to improve nutritional security. Thus, I estimate how BBCE affected low-income households' access to food and take-up as the first group of health outcomes, using the CPS-FSS. The definitions of each food security measure

[28]For example, Indiana's benefits application portal is available here: https://fssabenefits.in.gov/bp/.

[29]These findings align with cross-enrollment patterns between SNAP and Medicaid among already eligible groups (incomes below 125% of the FPL), which are correlated with state policies to reduce administrative burden, such as presumptive and continuous eligibility for Medicaid (Schmidt, Shore-Sheppard, and Watson 2024). Prior work also examines joint participation in the other direction, showing that Medicaid participation can facilitate SNAP participation, either through randomly assigned coverage (Baicker et al. 2014) or the ACA expansion (Schmidt, Shore-Sheppard, and Watson 2021; Cha and Escarce 2022).

are provided in detail in Section 3.1.

Table 1.5 shows that the BBCE expansion reduced the probability of households experiencing low food security in the past 12 months by 1.8 percentage points (column (1)) and very low food security by 0.8 percentage points (column (2)), statistically significant at the 1% and 5% levels, respectively. These point estimates correspond to a 8.28 percent and 10.86 percent decline in food insecurity relative to the pre-treatment means, representing sizable effects in percentage terms. The event-study estimates in Figure 1.5 confirm this reduction in food insecurity, preceded by flat pre-trends for both outcome indicators.[30]

The decrease in the fraction of households with food insecurity indicates improvements in access to food following BBCE. This can be attributed to increased participation in SNAP and WIC providing nutrition benefits, with the latter driven by potential spillovers within the social safety net, as discussed in Section 5.3. Participation in Medicaid can also play a role by reducing out-of-pocket medical costs and increasing available resources.

6.2 Health Care Utilization

The increase in program participation (Section 5)—including Medicaid and WIC, which directly facilitate health care access—motivates examining the effects of BBCE on health care utilization. This analysis helps illuminate the frictions that low-income households face in obtaining medical care—distinct from health status—with the potential to further improve health in the long run.

For health care utilization, I examine two outcomes from restricted-use Natality data and the BRFSS: the number of prenatal visits and the likelihood of having a personal doctor or primary

[30] Unlike my findings, Han (2016) finds no decline in food insecurity and Han (2020) finds no increase in Medicaid. Both papers use the Survey of Income and Program Participation (SIPP), with around 7,300 and 24,000 observations, whereas I use data sets 40 to 55 times larger, covering 20 years and providing greater statistical power. Moreover, both papers restrict their samples to households with gross income between 100% and 200% of the FPL and define the treatment variable as the share of newly eligible households. As a result, they miss potential woodwork effects Anders and Rafkin (2024), where *already* eligible households respond to BBCE. I instead include *all* already eligible groups to capture full effects of the policy. Finally, my sample construction is based on *predicted* income—unlike actual income, where the treatment sample is not held fixed—to avoid potential endogenous responses to the policy, such as participation in other government programs or changes in labor supply.

health care provider. Figure 1.6-(a) and column (1) of Table 1.6 show that the number of prenatal visits increased by 0.21 (a 2.1% increase), with strong statistical significance at the 1% level. Column (4) indicates a 1.1 percentage-point increase in the probability of low-income adults having a health care provider. While this average effect is imprecise, Figure 1.6-(b), which illustrates dynamic effects, shows a statistically significant increase of 2.2 percent starting one year after the expansion.

When investigating heterogeneous effects by race, Table 1.6 reports greater improvements in health care utilization among non-Whites compared to Whites, both for prenatal care and access to health care providers. Columns (2) and (3) show that the number of prenatal visits increased by 0.24 for non-Whites (a 2.42% increase), which is larger than the 0.13 increase for Whites (a 1.23% increase), both in absolute terms and as a percentage.

In columns (5) and (6), the BBCE expansion increased the probability of having a health care provider for both Whites and non-Whites. However, the 2 percentage-point increase for non-Whites (a 3.05% increase) is more than double the increase for Whites and is estimated with greater precision. Since non-Whites had lower average values in the pre-treatment period and experienced greater increases in both outcomes following the BBCE expansion, these findings suggest that BBCE also reduced racial disparities in health care utilization while improving overall use.

Also, Figure A1.3 shows a decline in the share of low-income adults who report missing doctor visits due to cost in the past year. The 0.5 percentage point decline represents roughly a 3 percent reduction relative to the pre-treatment period mean of 18.7%. The increase in household resources, along with the reduction in out-of-pocket medical expenses (through Medicaid or WIC), may explain the decline in the financial burden of doctor visits.

My findings on health care utilization are consistent with those of Bronchetti, Christensen, and Hoynes (2019), although their focus is on *children's* outcomes. The authors estimate that a 10 percent increase in SNAP's purchasing power—driven by changes in local food prices—increases annual checkups by 8.1 percent and any doctor's visits by 3.4 percent.

6.3 Health Status

I then examine how BBCE influenced the health status of low-income individuals, separately for birth outcomes (Section 6.3.1) and adult health conditions (Section 6.3.2).

6.3.1 Birth Outcomes

Using restricted-use birth certificate data, I first examine the effect of BBCE on birth weight, which is a common indicator of newborn health (Almond, Hoynes, and Schanzenbach 2011) and is also linked to later life human capital formation. Table 1.7 shows that BBCE did not lead to significant improvements in birth weight for the overall low-income sample. The DiD estimates for the the continuous measure of birth weight below 3,200 grams (approximately 7.1 lb; column (1)) and for the indicator of low birth weight (birth weight $<$ 2,500 grams, approximately 5.5 lb; column (5)) are both small in magnitude, along with small standard errors.

When estimating heterogeneous impacts by race, however, I find larger and statistically significant improvements among Black mothers (columns (3) and (7)) following the BBCE expansion, who represent the most vulnerable group, compared to modest effects among White mothers (columns (2) and (6)). Among Black mothers, the birth weight of babies with lower-than-normal weight increased by 5.25 grams, and more importantly, the share of low birth weight births declined by 0.36 percentage points (a 2.48% decline).[31] The event-study graph for Black mothers in Figure 1.7-(c) shows a sharp and persistent drop in the share of low birth weight births, preceded by flat pre-trends. I further confirm that these findings are not driven by changes in fertility or the composition of mothers (Figure A1.4).

My findings align with Almond, Hoynes, and Schanzenbach (2011) in that the introduction of the FSP improved birth outcomes, including the lower share of low birth weight. After adjusting the estimates by SNAP participation rate for the corresponding sample, they find a reduction in low birth weight of 7 to 8 percent for Whites and 5 to 12 percent for Blacks. Although I find statistically

[31] This estimate remains statistically significant even after adjusting for multiple hypothesis testing with the Bonferroni correction (Anderson 2008).

significant improvements only among Blacks, the 0.36 percentage point decline corresponds to an 7.1 percent reduction when similarly adjusted using the CPS-ASEC. This supports the continued role of benefit transfers including SNAP in improving birth outcomes in the modern era.

6.3.2 Adult Health Conditions

In addition to the improvements in the health status of the youngest population—children born to Black mothers—discussed in the previous section (Section 6.3.1), the final group of health outcomes focuses on the health status of the older population.

For health conditions of low-income adults, I investigate the effect of the BBCE expansion on three groups of outcomes using the BRFSS. I first estimate how BBCE affected the incidence of medical diagnoses for certain diseases, including asthma, diabetes, and coronary heart disease. In columns (1)–(3) of Table A1.6, the BBCE expansion reduced the share of individuals ever diagnosed with these diseases by 2 to 3 percentage points across all three disease types.[32] Still, these effects are imprecise.

Next, I estimate the effect of the BBCE expansion on self-reported health among adults. In Table A1.6, columns (4) and (5) show that BBCE resulted in a slight decline in body mass index (BMI)—measured as weight (in kilograms) divided by height (in meters) squared—and a reduction in the probability of being obese (BMI $>$ 30), although the coefficients are not statistically significant.[33] Columns (6) and (7) indicate no significant impacts of BBCE on indicators of poor health in the past 30 days, assessed separately for physical and mental health. Event-study graphs for the outcomes in Table A1.6 are presented in Figure A1.5.

Table A1.7 presents the results on health outcomes discussed above by race. Compared to Whites, non-Whites experienced larger declines in the incidence of medical diagnoses (Panel A) and self-reported poor health (Panel B). Although most effects are imprecisely estimated, the 1.1

[32]Since health care utilization for low-income households improved following the BBCE expansion (Section 6.2), the observed reduction in diagnoses is unlikely to reflect underdiagnosis and more likely indicates a decline in disease occurrence.

[33]When estimating the treatment effect on BMI, observations in the top 1% of the distribution—those with a BMI greater than 48.55—are coded as missing values and excluded from the analyses.

percentage point decline in obesity for non-Whites (column (2) of Panel B) is statistically significant at the 5% level and represents a 3.86 percent reduction relative to the pre-treatment mean, taken at face value. Since obesity can lead to other conditions—including diabetes, cardiovascular disease, respiratory issues, reproductive health problems, and mental health disorders—this reduction may translate into meaningful health gains among non-Whites.[34]

One potential concern with income transfers is that recipients might use their increased financial resources in ways that ultimately harm their health. To address this, I examine whether BBCE led to any behavioral changes related to smoking or alcohol consumption, as an example of risky behaviors. Table A1.6 shows that BBCE did not significantly increase the share of low-income individuals who had smoked in the past 30 days (column (8)) or the number of alcoholic drinks they consumed per occasion in the past 30 days (column (9)). These findings indicate no adverse effects of benefit transfers on vice goods.

To sum up, BBCE did not lead to significant improvements in health conditions for the overall sample of low-income adults, although larger—albeit mostly statistically insignificant—effects are observed among the non-White population. Similar to the results on health care utilization in Section 6.2, my findings on adult health conditions also align with those of Bronchetti, Christensen, and Hoynes (2019), although I focus on health outcomes for adults rather than *children*. They find that the local purchasing power of SNAP is not significantly associated with children's health status—including obesity and self-reported health—except for a reduction in school days missed due to illness.

While it is challenging to detect substantial effects on health outcomes within just a few years of policy implementation for the overall low-income sample, positive effects may emerge in the long run, reflecting improvements in food security and health care utilization—even among low-income Whites. Previous studies document the long-term impacts of the initial rollout of the

[34] In unreported results, I construct separate indices for health conditions based on medical diagnoses, self-reported health, and a combined measure. Each index is calculated as the sum of standardized outcomes, following the approach of Bronchetti, Christensen, and Hoynes (2019) and Bailey et al. (2024). For the overall sample, and for the combined index by race, no significant patterns emerge. However, among non-Whites, the self-reported health index shows an immediate improvement after the reform, gradually returning to zero over time. In contrast, the medical diagnosis index shows no initial response but improves progressively following the reform's implementation.

FSP, showing that children with access to FSP have a lower incidence of metabolic syndrome (Hoynes, Schanzenbach, and Almond 2016) and have higher life expectancy in adulthood (Bailey et al. 2024).[35] Accounting for long-run impacts, including improvements in health-related outcomes, the Marginal Value of Public Funds (MVPF) for the initial rollout of SNAP increases substantially from 1.04 to 62.25 (Hendren and Sprung-Keyser 2020; Bailey et al. 2024).

7 Results: Work Disincentives

Despite meaningful improvements in several health outcomes, safety net programs may also have the unintended consequence of reducing work incentives. For this reason, this section estimates the effects of BBCE on labor market outcomes to examine potential side effects.

Table 1.8 presents the results of BBCE's impact on labor market outcomes among low-income individuals, focusing on employment status and weekly hours of work. The definition of hours worked per week includes zero hours for individuals who did not report any employment during the year. Column (1) of Table 1.8 indicates that BBCE had no substantial effect on the probability of being employed for low-income individuals as a binary outcome. The 0.3 percentage point decline is neither statistically nor economically significant relative to the pre-treatment mean.

In contrast, column (2) reveals that the BBCE expansion led to a slight reduction in weekly working hours by 0.3 hours (a 1.55% decline). This finding indicates a modest but statistically significant adjustment in labor supply on the intensive margin without notable changes on the extensive margin. The decline in hours worked likely reflects both the income effect of higher non-labor income and the substitution effect associated with the benefit tax on earnings (Bitler, Cook, and Rothbaum 2021). Event-study graphs confirm the absence of confounding pre-trends for both employment status (Figure 1.8-(a)) and weekly hours of work (Figure 1.8-(b)).

Next, I examine the heterogeneous effects of the BBCE expansion on hours worked, considering household structure and demographic characteristics of low-income families. In Table A1.8,

[35] The outcomes used to construct the metabolic syndrome index in (Hoynes, Schanzenbach, and Almond 2016) largely overlap with those in my study, including obesity, diabetes, heart disease, heart attack, and high blood pressure.

the decline in working hours is concentrated among individuals living with dependents under age 18 (columns (1) and (2)), those who are not the household head (columns (3) and (4))—and thus are more likely to be secondary earners—and married individuals (columns (5) and (6)), although the latter two estimates are not statistically significant. These findings suggest that labor supply responses tend to be larger among individuals with greater flexibility to adjust their labor supply. Specifically, those with dependents may reduce their labor supply to spend more time with their children, facilitated by easier access to food. By contrast, SNAP recipients without dependents may find it more difficult to reduce their labor supply because of the ABAWD work requirements, as discussed in Section 2.1.

Hoynes and Schanzenbach (2012) find that the initial introduction of the FSP in the 1960s and 1970s had no significant impact on the labor supply of the overall sample but reduced both employment and hours worked among single mothers, who were more likely to participate in the program.[36] The implementation of the FSP reduced weekly work hours by 3.5 hours among families headed by an unmarried woman as an Intent-to-Treat (ITT) estimate, which translates to 9.7 hours as a Treatment-on-the-Treated (TOT) estimate. This reduction is substantially larger than my estimate of 0.3 hours per week (ITT) or 1.1 hours per week (TOT). The smaller magnitude of my estimate suggests that the potential adverse effects of transfer programs—such as reduced work incentives, discouraged labor supply, and lower tax revenues—are less pronounced today than in earlier periods of program implementation, reflecting the transition to more work-based program designs.

[36]Their estimates for the overall sample, though very small in magnitude, do not indicate a negative effect. This is likely due to the composition of their sample, which includes high-income individuals who are less relevant to the program, with only 3 percent reporting food stamp receipt. As another distinction, I find that the decline is more pronounced among married individuals than unmarried ones. Consistent with the findings from all of my heterogeneity analyses, married individuals may find it easier to reduce their labor supply due to the presence of a partner who can also work or contribute financially, unlike unmarried individuals who may still need to work. Since my sample consists only of those likely to be affected by the reform in terms of predicted income, unlike that of Hoynes and Schanzenbach (2012), household structure could play a larger role in determining the effects of benefit receipt.

8 Conclusion

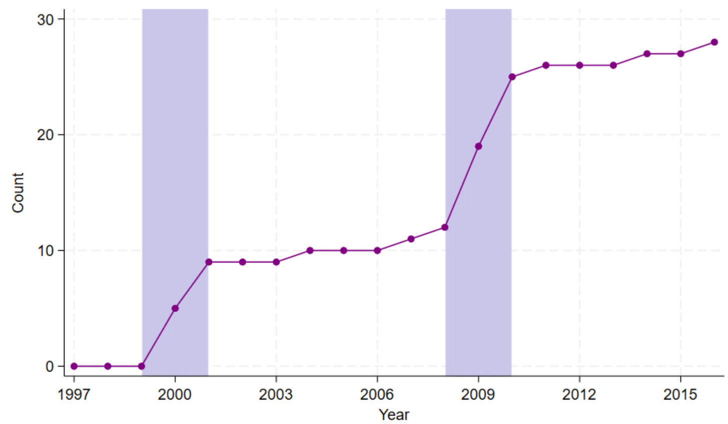
This paper examines how recent eligibility expansions in SNAP affected a wide range of outcomes for low-income households, including program take-up, participation in other benefit programs, health outcomes, and labor supply. Using a difference-in-differences (DiD) approach, I leverage variation in the timing of BBCE adoption across states, one of the largest SNAP reforms in the modern era.

Following the BBCE expansion, SNAP participation and benefit amounts among low-income individuals increased sharply, with spillover effects increasing participation in Medicaid and WIC. Additional nutrition benefits and resources led to meaningful improvements in food insecurity and health care utilization, including greater use of prenatal care and primary care providers. While short-term impacts on birth outcomes and adult health status for the overall low-income sample are modest, improvements—particularly in low birth weight and obesity—are more pronounced among non-Whites, suggesting that BBCE helped reduce racial disparities in health. Lastly, BBCE had no significant effect on employment status, but it led to a slight decline in hours worked—about one-tenth of what was observed during the program's early rollout in the 1960s and 1970s. In sum, the modern SNAP expansion appears to have achieved many goals, generating substantial benefits while only producing modest work disincentive effects.

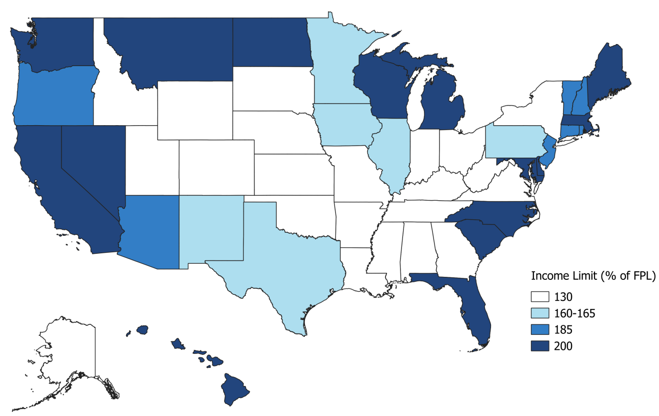
This paper has several policy implications. First, given the scale and coverage of SNAP, evaluating the effects of its recent large expansion advances our understanding of government program operations. On the cost side, expanding eligibility raises government expenditures by increasing program participation. However, this rise in spending should be weighed against substantial benefits. Evidence highlights that benefit transfers including SNAP function as a public investment with long-term gains—improving health outcomes that reduce future medical costs, enhance labor market productivity, and increase tax revenue—beyond providing immediate nutritional support. Thus, this paper contributes to measuring the MVPF for SNAP expansion in recent years, which offers an estimate that informs current policy debates and is comparable to the program's introduction in the 1960s and 1970s.

Moreover, the findings suggest that welfare programs have implications for addressing inequality. Health disparities—particularly those linked to race and socioeconomic status—are key drivers of income inequality. The stronger health gains among disadvantaged groups support the role of public programs in both alleviating poverty and advancing equity. Future research should evaluate the long-run effects of the BBCE expansion to better understand its broader implications, assess the value of integrated program design, and conduct a cost-benefit analysis to identify the optimal use of government funds.

Figures and Tables



(a) Number of States with BBCE Expansion



(b) Map of States with BBCE Expansion

Figure 1.1: States with BBCE Expansion

Notes: Figure 1.1-(a) displays time series graphs of the number of states that adopted the BBCE expansion of the asset test and the gross income rule over time. The x-axis represents calendar years. The light purple areas denote two major expansions: (i) the initial implementation in 2000, and (ii) the period around the Great Recession in 2009. Figure 1.1-(b) presents a state-level map illustrating the maximum gross income limit under the BBCE expansion up to 2016. States in dark blue implemented the largest expansion by raising the limit to 200% of the FPL, those in blue expanded the limit to 185%, those in light blue to between 160% and 165%, and those in white maintained the gross income limit at 130%, with no expansion.

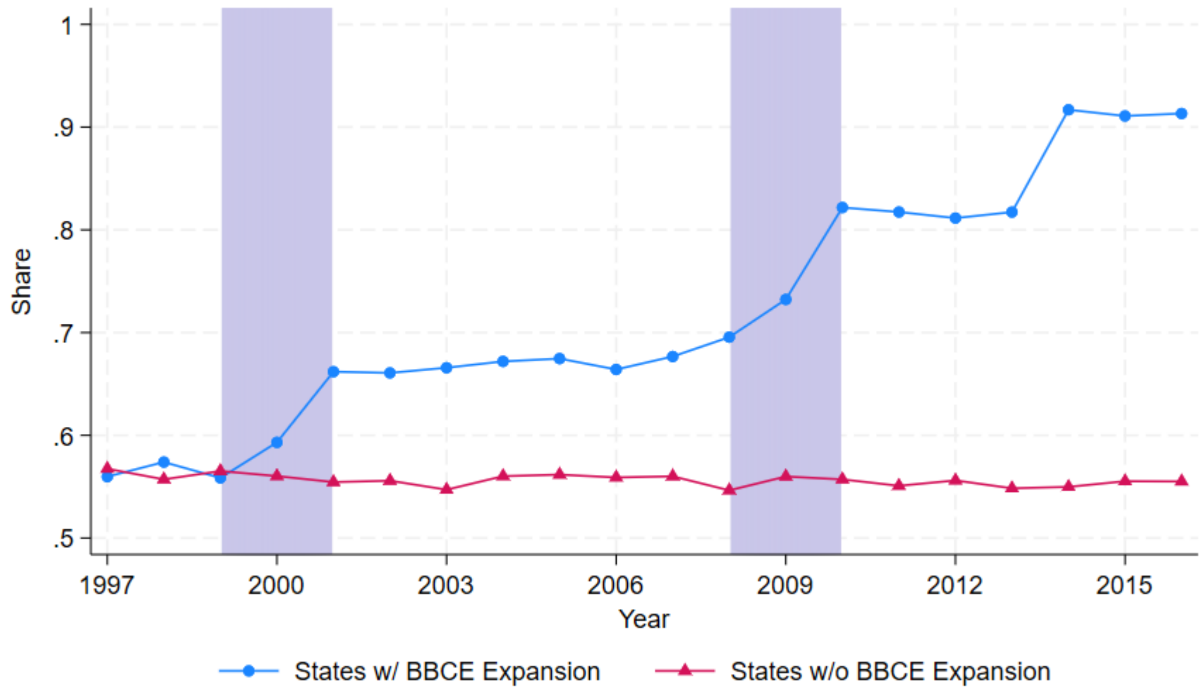
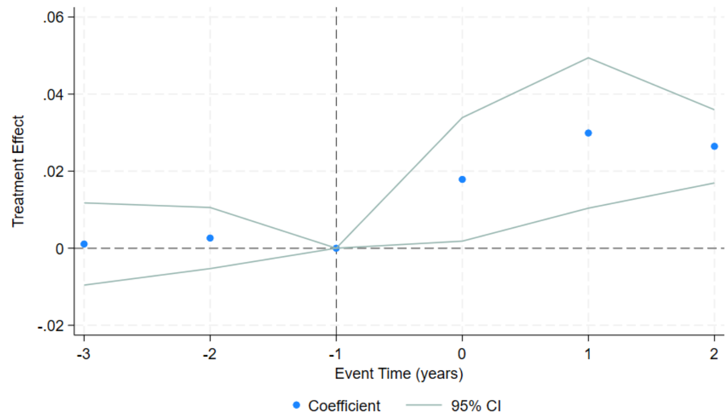
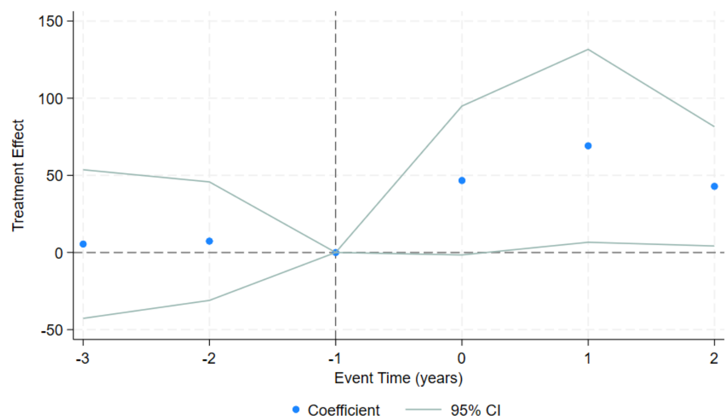


Figure 1.2: Time Series Graph of Predicted SNAP-Eligible Population Share

Notes: Figure 1.2 displays a time series graph of the share of the predicted SNAP-eligible population among the low-income sample of the CPS-ASEC (see Section A1.1), separately by each state's BBCE adoption decision. The predicted SNAP-eligible population is defined as individuals whose predicted FPL ratio—family income divided by the FPL—falls below the gross income limit for SNAP eligibility in the pre-treatment period (1997-1999): 130% of the FPL without BBCE, or 160%, 165%, 180%, or 200% of the FPL with BBCE. The x-axis represents calendar years. The blue line shows the average share of predicted SNAP-eligible population for states that adopted BBCE at some point during the sample period (i.e., treated). The red line shows the average share of predicted SNAP-eligible population for states that have never adopted BBCE (i.e., never-treated). The light purple areas denote two major expansions: (i) the initial implementation in 2000, and (ii) the period around the Great Recession in 2009.



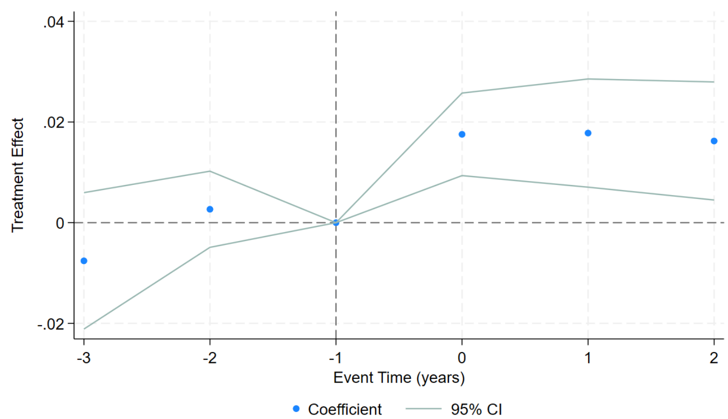
(a) SNAP Participation (binary)



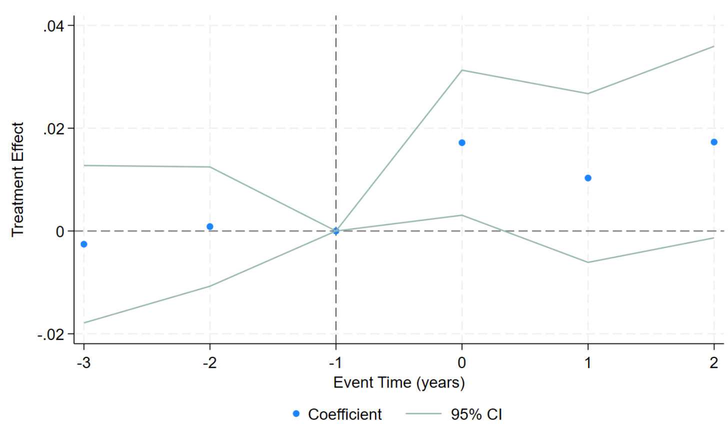
(b) SNAP Benefit Amounts

Figure 1.3: Effect of BBCE Expansion on SNAP Participation and Benefit Amounts

Notes: Figure 1.3 displays the estimation results of the impact of the BBCE expansion on SNAP participation and household SNAP benefit amounts (in 2016 dollars) using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on low-income individuals' SNAP participation as a binary outcome (Figure 1.3-(a)) and reported SNAP benefit amounts per year (Figure 1.3-(b)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.



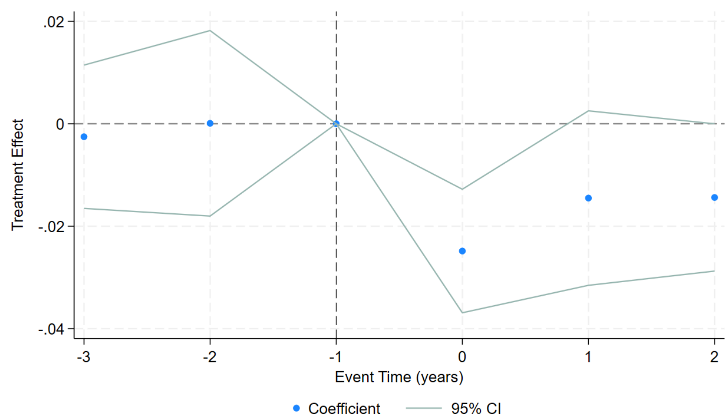
(a) Medicaid Participation (binary)



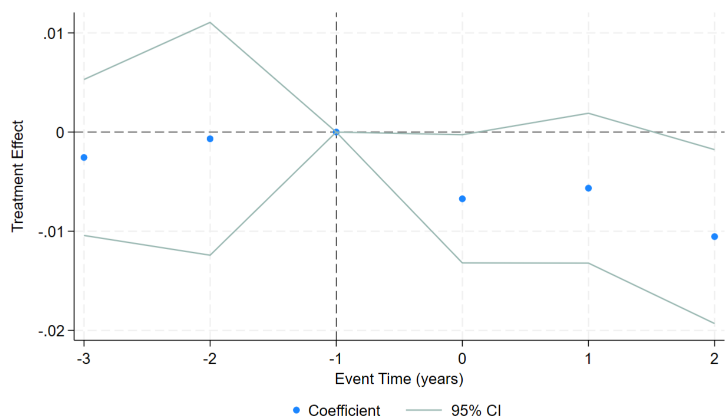
(b) WIC Participation (binary)

Figure 1.4: Effect of BBCE Expansion on Medicaid and WIC Participation

Notes: Figure 1.4 displays the estimation results of the impact of the BBCE expansion on enrollment in other safety net programs using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on Medicaid participation (binary) (Figure 1.4-(a)) and WIC participation (binary) (Figure 1.4-(b)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. In Figure 1.4-(b), the sample is restricted to females aged 15 to 39 with children in the household. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



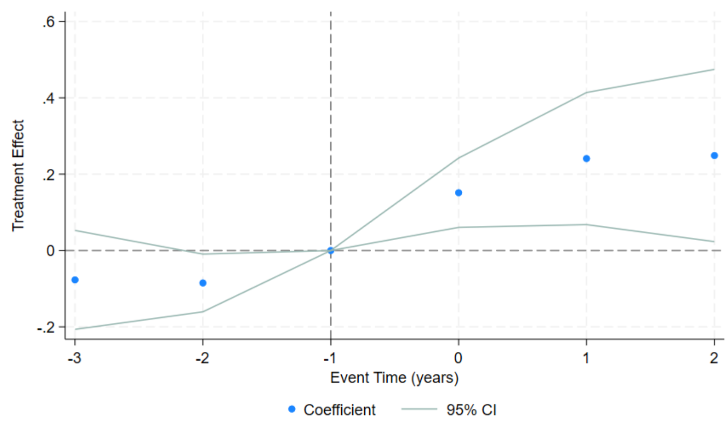
(a) Low Food Security (binary)



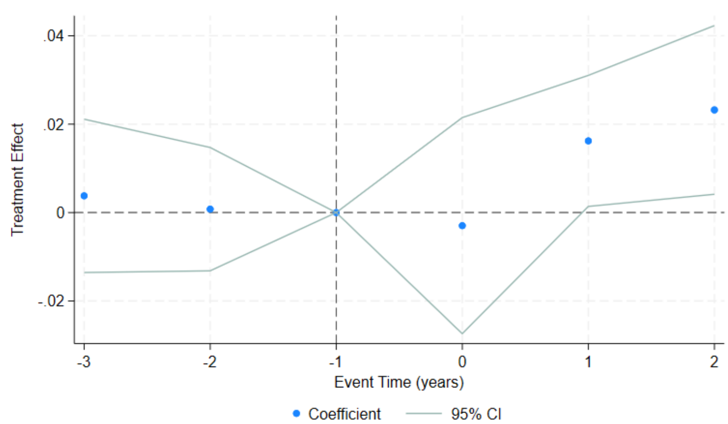
(b) Very Low Food Security (binary)

Figure 1.5: Effect of BBCE Expansion on Household Food Security

Notes: Figure 1.5 displays the estimation results of the impact of the BBCE expansion on household food security using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on the probability of households experiencing very low or low food security (Figure 1.5-(a)) and very low food security (Figure 1.5-(b)), as defined in Section 3.1. The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income households, as defined in Section A1.1, using the CPS-FSS from 1997 to 2016. Standard errors are clustered at the state level. CPS-FSS household-level weights are applied. Household-level covariates include gender, race/ethnicity, marital status, and education level of household head, number of children, and number of family members. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



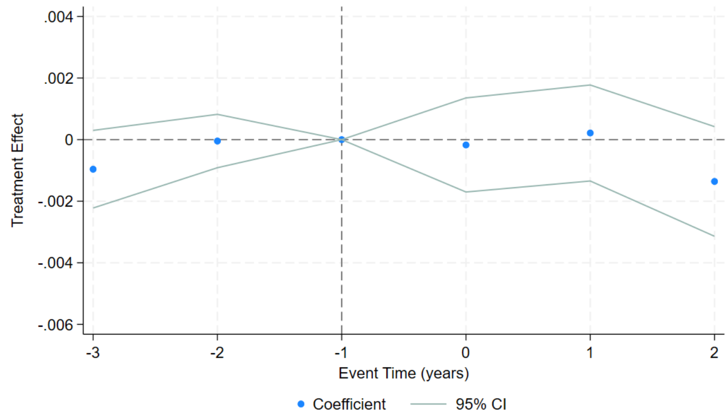
(a) Number of Prenatal Visits



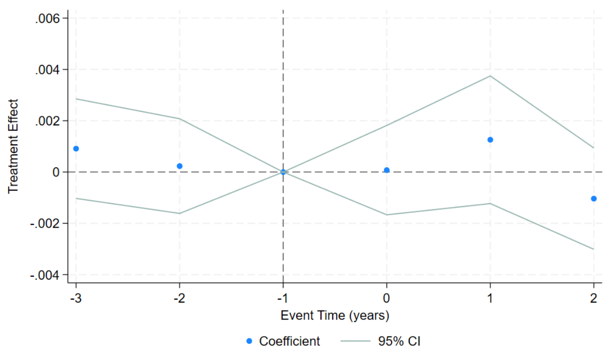
(b) Any Primary Care Provider (binary)

Figure 1.6: Effect of BBCE Expansion on Health Care Utilization

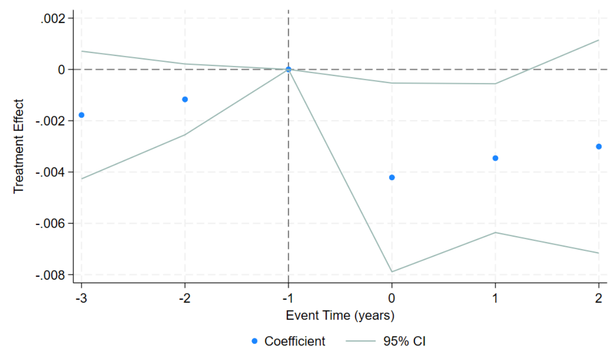
Notes: Figure 1.6 displays the estimation results of the impact of the BBCE expansion on health care utilization using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024). Each figure shows the effect on the number of prenatal visits during pregnancy based on Equation 1.2 (Figure 1.6-(a)) and whether having a personal doctor or health care provider (binary) based on Equation 1.1 (Figure 1.6-(b)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the restricted-use Vital Statistics Natality Data (Figure 1.6-(a)) and the BRFSS (Figure 1.6-(b)) from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied (Figure 1.6-(b) only). Individual-level covariates include age, gender, race/ethnicity, marital status, and education level (Figure 1.6-(b) only). State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



(a) All



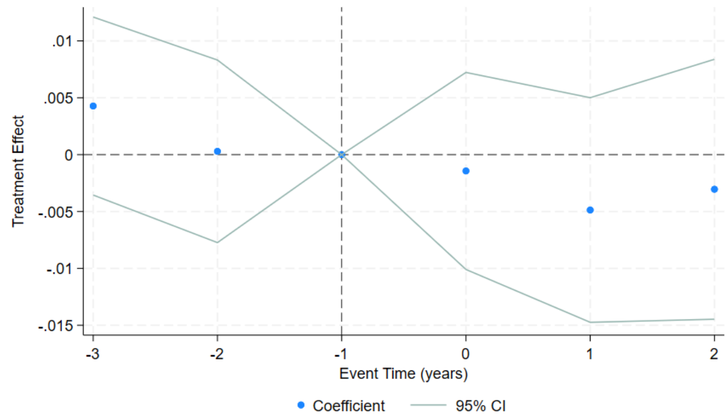
(b) White



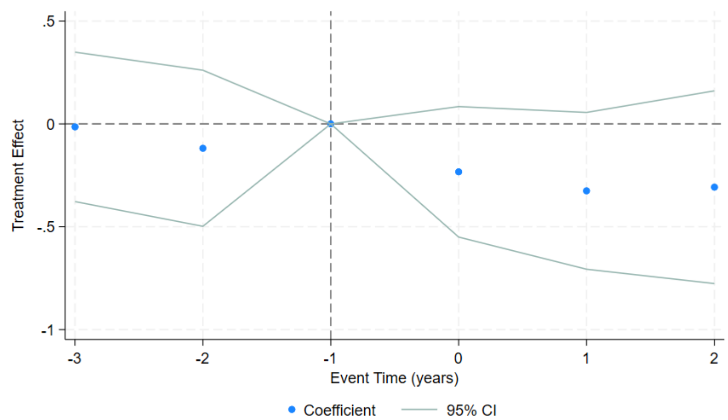
(c) Black

Figure 1.7: Effect of BBCE Expansion on Low Birth Weight

Notes: Figure 1.7 displays the estimation results of the impact of the BBCE expansion on low birth weight (binary), both for the full sample and by race, using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on the incidence of low birth weight ($<$ 2,500 g $\fallingdotseq$ 5.5 lb) among all mothers (Figure 1.7-(a)), White mothers (Figure 1.7-(b)), and Black mothers (Figure 1.7-(c)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the restricted-use Vital Statistics Natality Data from 1997 to 2016. Standard errors are clustered at the state level. Individual-level covariates include age, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



(a) Employment Status (binary)



(b) Hours Worked (per week)

Figure 1.8: Effect of BBCE Expansion on Labor Market Outcomes

Notes: Figure 1.8 displays the estimation results of the impact of the BBCE expansion on labor market outcomes using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on employment status (binary) (Figure 1.8-(a)) and hours worked (per week) (Figure 1.8-(b)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table 1.1: Descriptive Statistics (1997-2016)

Variable	Mean
A. CPS-ASEC	
SNAP participation (binary)	0.231
SNAP benefit amounts (in 2016 dollars)	601.379
Medicaid participation (binary)	0.290
WIC participation (binary)	0.131
Employment status (binary)	0.499
Hours worked (per week)	18.279
Observations	1,317,100
B. CPS-FSS	
Low food security (binary)	0.219
Very low food security (binary)	0.075
Observations	289,237
C. Vital Statistics Natality Data	
Low birth weight (binary)	0.094
Birth weight (in grams)	3,203.208
Number of prenatal visits	10.108
Observations	26,910,878
D. BRFSS	
Ever diagnosed with asthma (binary)	0.138
Ever diagnosed with diabetes (binary)	0.099
Ever diagnosed with coronary heart disease (binary)	0.038
Body mass index (BMI)	27.236
Obesity (BMI $>$ 30) (binary)	0.276
Poor physical health (past 30 days) (binary)	0.373
Poor mental health (past 30 days) (binary)	0.379
Ever smoked (past 30 days) (binary)	0.266
Number of drinks (per occasion; past 30 days)	1.191
Having a primary care provider (binary)	0.726
Observations	2,048,505

Notes: Table 1.1 reports average characteristics from various data sources between 1997 and 2016. The sample consists of low-income individuals, as defined in Section A1.1.

Table 1.2: Descriptive Statistics: Demographics (CPS-ASEC)

	(1) All	(2) Low-Income
Age	33.671	27.509
Male	0.485	0.439
White	0.643	0.449
Black	0.109	0.162
Hispanic	0.168	0.299
No High School Diploma	0.396	0.602
High School Diploma	0.219	0.235
Some College or Above	0.385	0.163
Married	0.411	0.228
Observations	3,763,147	1,317,100

Notes: Table 1.2 reports average characteristics for observations in the CPS-ASEC from 1997 to 2016. Averages are reported separately for two groups: all observations (column (1)) and low-income families (column (2)), as defined in Section A1.1.

Table 1.3: Effect of BBCE Expansion on SNAP Participation and Benefit Amounts

	Participation	Amount	
	(1)	(2)	(3)
	Part. = 1	Reported	Winsorized
BBCE	0.025***	52.809**	47.431**
	(0.007)	(21.391)	(19.254)
Baseline Controls	YES	YES	YES
Pre-Period Mean	0.211	541.088	509.164

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.3 reports the estimation results of the impact of the BBCE expansion on SNAP participation and household SNAP benefit amounts (in 2016 dollars) using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for SNAP participation as a binary outcome (column (1)), reported SNAP benefit amounts per year (column (2)), and winsorized benefit amounts where extreme values are replaced at the 2.5th percentile in each tail of the distribution (column (3)). The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table 1.4: Effect of BBCE Expansion on Medicaid and WIC Participation

	(1) Medicaid	(2) WIC
BBCE	0.017***	0.015**
	(0.004)	(0.007)
Baseline Controls	YES	YES
Pre-Period Mean	0.275	0.149

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.4 reports the estimation results of the impact of the BBCE expansion on enrollment in other satefy net programs using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for Medicaid participation (column (1)) and WIC participation (column (2)) as a binary outcome. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. In column (2), the sample is restricted to females aged 15 to 39 with children in the household. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table 1.5: Effect of BBCE Expansion on Household Food Security

	(1) Low Food Security	(2) Very Low Food Security
BBCE	-0.018***	-0.008**
	(0.006)	(0.003)
Baseline Controls	YES	YES
Pre-Period Mean	0.213	0.072

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.5 reports the estimation results of the impact of the BBCE expansion on household food security using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for the probability of households experiencing very low or low food security (column (1)) and very low food security (column (2)), which are defined in Section 3.1 as binary outcomes. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-FSS from 1997 to 2016. Standard errors are clustered at the state level. CPS-FSS household-level weights are applied. Household-level covariates include gender, race/ethnicity, marital status, and education level of household head, number of children, and number of family members. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table 1.6: Effects of BBCE Expansion on Health Care Utilization

	Number of Prenatal Visits			Any Primary Care Provider		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	White	Others	All	White	Others
BBCE	0.213***	0.130*	0.240***	0.011	0.008	0.020*
	(0.080)	(0.070)	(0.088)	(0.009)	(0.008)	(0.011)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	10.163	10.591	9.950	0.727	0.785	0.668

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.6 reports the estimation results of the impacts the BBCE expansion on health care utilization, both for the full sample and by race, using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Columns (1)-(3) present the DiD estimates for the number of prenatal visits during pregnancy based on Equation 1.2. Columns (4)-(6) present the DiD estimates for whether having a personal doctor or health care provider (binary) based on Equation 1.1. The sample consists of low-income individuals, as defined in Section A1.1, using the restricted-use Vital Statistics Natality Data (columns (1)-(3)) and the BRFSS (columns (4)-(6)) from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied (columns (4)-(6) only). Individual-level covariates include age, gender, race/ethnicity, marital status, and education level (columns (4)-(6) only). State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table 1.7: Effect of BBCE Expansion on Birth Weight

	Birth Weight (continuous)				Low Birth Weight (binary)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	White	Black	Hispanic	All	White	Black	Hispanic
BBCE	0.831	0.451	5.249*	-1.258	-0.0004	0.0001	-0.0036**	0.0013
	(1.349)	(2.276)	(2.833)	(2.124)	(0.0007)	(0.0009)	(0.0016)	(0.0010)
Baseline Controls	YES	YES	YES	YES	YES	YES	YES	YES
Pre-Period Mean	2,722	2,729	2,652	2,778	0.095	0.091	0.143	0.066

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.7 reports the estimation results of the impact of the BBCE expansion on birth weight, both for the full sample and by race and ethnicity, using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Columns (1)-(4) present the DiD estimates for the continuous measure of birth weight below 3,200 g ($\fallingdotseq$ 7.1 lb). Columns (5)-(8) present the DiD estimates for the incidence of low birth weight (birth weight $<$ 2,500 g $\fallingdotseq$ 5.5 lb) (binary). The sample consists of low-income individuals, as defined in Section A1.1, using the restricted-use Vital Statistics Natality Data from 1997 to 2016. Standard errors are clustered at the state level. Individual-level covariates include age, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

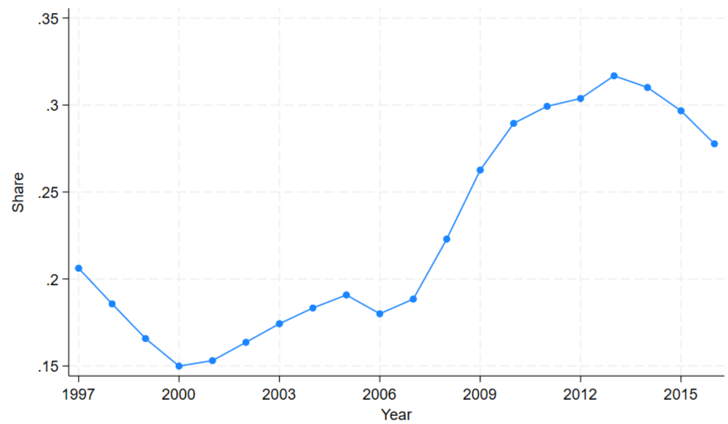
Table 1.8: Effect of BBCE Expansion on Labor Market Outcomes

	(1) Employment	(2) Hours Worked
BBCE	-0.003	-0.285*
	(0.004)	(0.158)
Baseline Controls	YES	YES
Pre-Period Mean	0.501	18.455

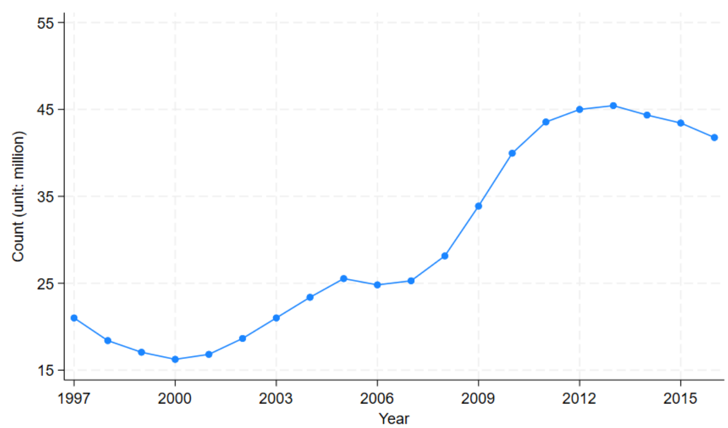
Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 1.8 reports the estimation results of the impact of the BBCE expansion on labor market outcomes using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for employment status (binary) (column (1)) and hours worked (per week) (column (2)). The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Appendix



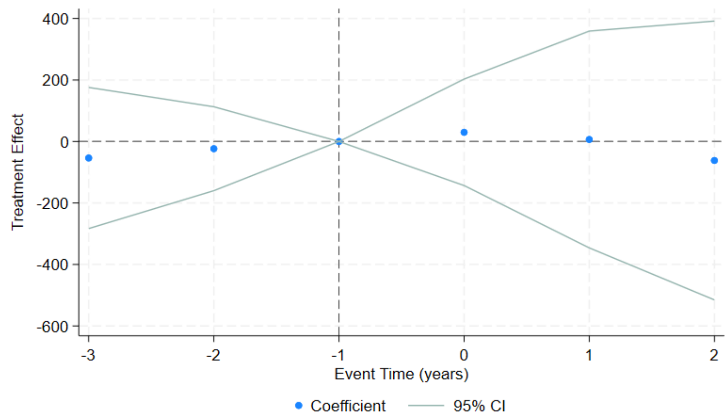
(a) SNAP Participation Rate



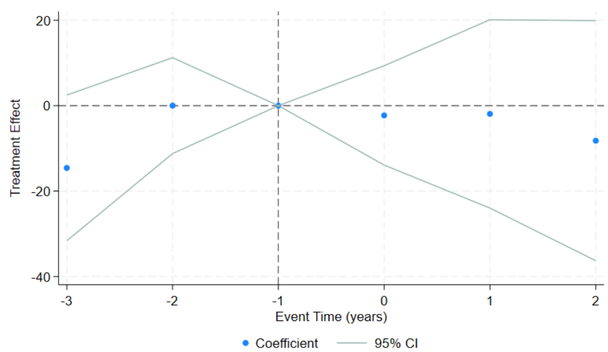
(b) SNAP Participation Count

Figure A1.1: Time Series Graphs of SNAP Participation

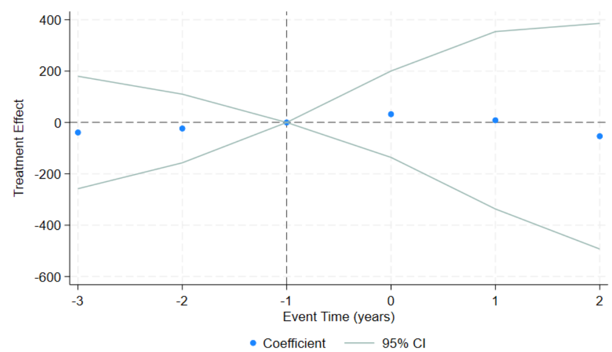
Notes: Figure A1.1 displays time series graphs of SNAP participation based on two different data sources. Figure A1.1-(a) shows SNAP participation rates among low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Figure A1.1-(b) shows the total number of SNAP participants (in millions) using USDA administrative data from 1997 to 2016. In both figures, the x-axis represents calendar years.



(a) All Stores



(b) Usual Stores (20%)



(c) Non-Usual Stores (80%)

Figure A1.2: Effect of BBCE Expansion on SNAP Retailer Participation

Notes: Figure A1.2 displays the estimation results of the impact of the BBCE expansion on SNAP vendor counts using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.2. Each figure shows the effect on the number of all stores (Figure A1.2-(a)), usual stores (Figure A1.2-(b)), and non-usual stores (Figure A1.2-(c)) participating in SNAP, as defined in Section 5.2. The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of SNAP-authorized retailers, using the Historical SNAP Retailer Locator Data from 1997 to 2016. Standard errors are clustered at the state level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

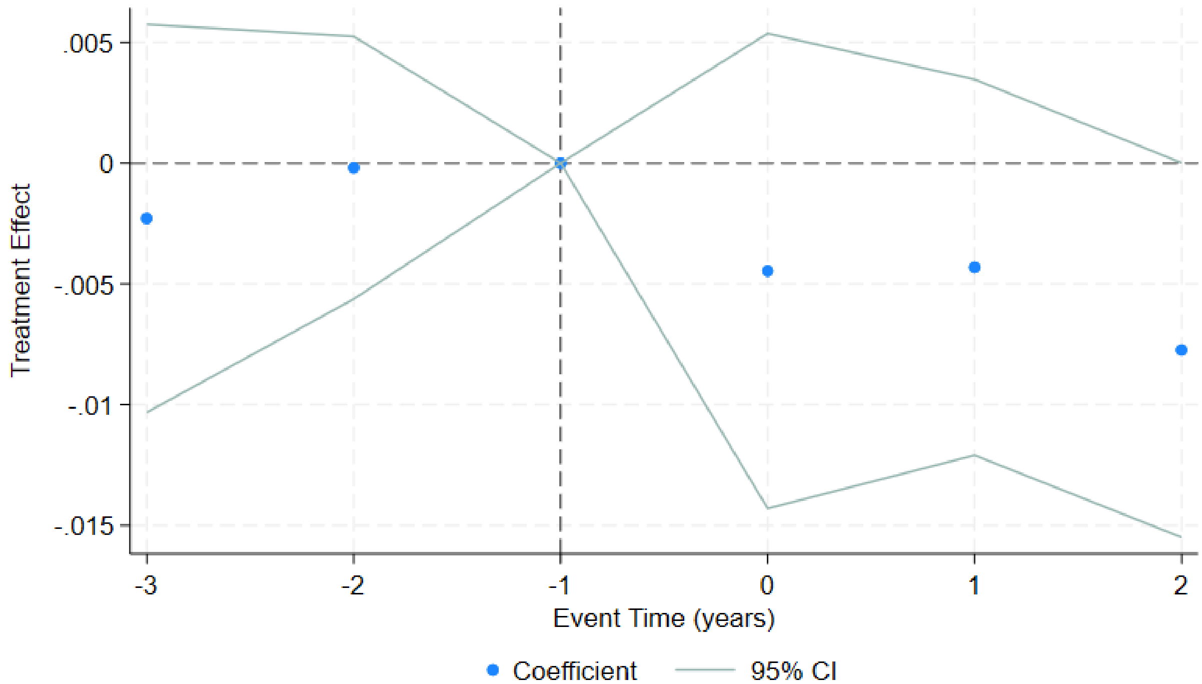
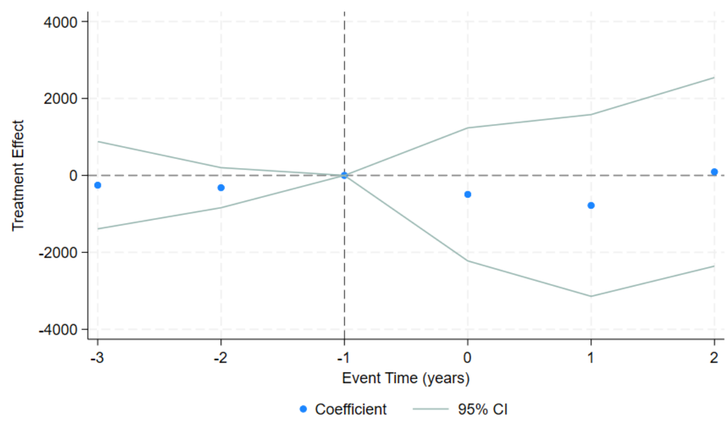
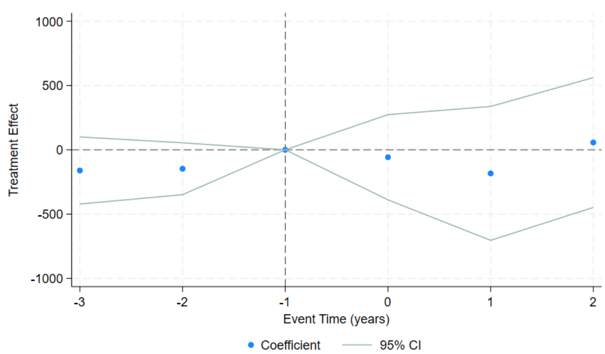


Figure A1.3: Effect of BBCE Expansion on Financial Barrier to Medical Care

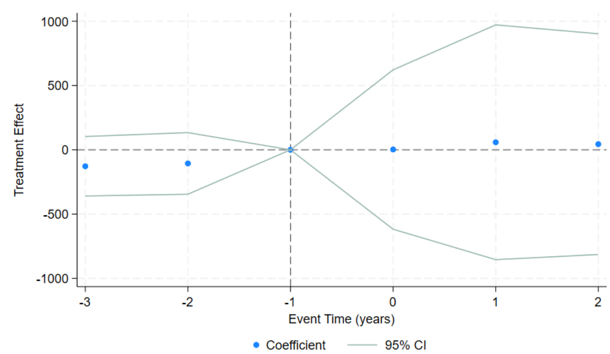
Notes: Figure A1.3 displays the estimation results of the impact of the BBCE expansion on cost-related barriers to medical care using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. The figure shows the effect on the probability that low-income adults report missing doctor visits due to cost in the past year (binary). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the BRFSS from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



(a) All



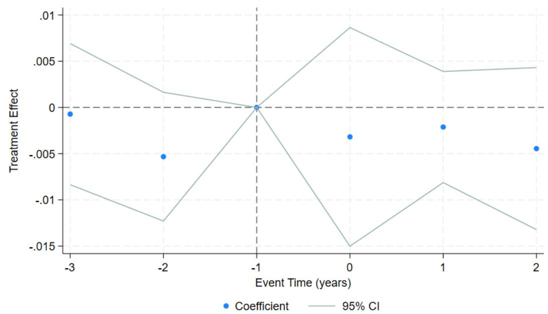
(b) White



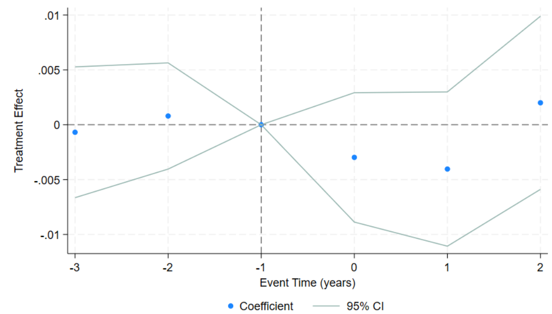
(c) Black

Figure A1.4: Effect of BBCE Expansion on Fertility

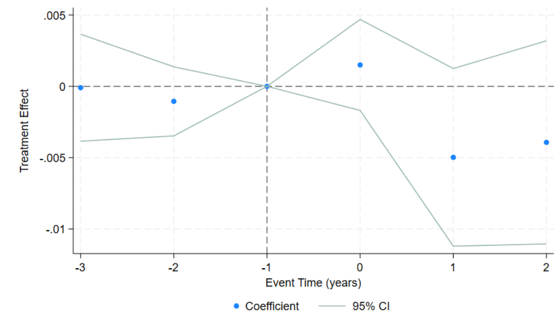
Notes: Figure A1.4 displays the estimation results of the impact of the BBCE expansion on fertility, both for the full sample and by race, using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.2. Each figure shows the effect on the total number of births among all mothers (Figure 1.7-(a)), White mothers (Figure 1.7-(b)), and Black mothers (Figure 1.7-(c)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the restricted-use Vital Statistics Natality Data from 1997 to 2016. Standard errors are clustered at the state level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).



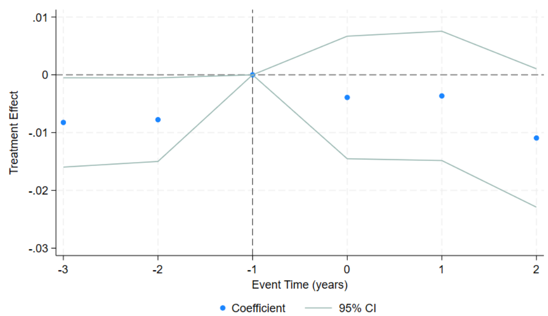
(a) Asthma (binary)



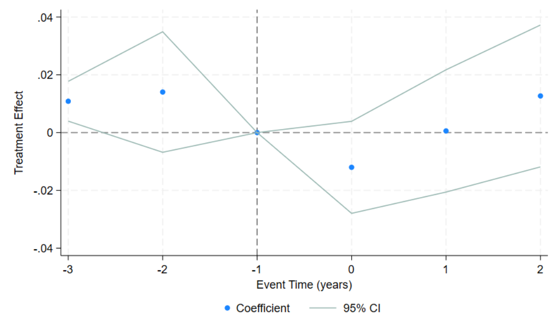
(b) Diabetes (binary)



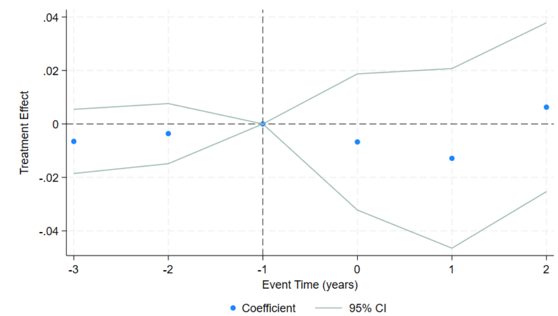
(c) Coronary Heart Disease (binary)



(d) Obesity (binary)



(e) Poor Physical Health (30d) (binary)



(f) Poor Mental Health (30d) (binary)

Figure A1.5: Effect of BBCE Expansion on Health Conditions

Notes: Figure A1.5 displays the estimation results of the impact of the BBCE expansion on health conditions using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each figure shows the effect on the probability of ever being diagnosed with the disease (binary) (Figure A1.5-(a), (b), (c)), the probability of being obese (binary) (Figure A1.5-(d)), and the probability of reporting poor physical or mental health in the past 30 days (binary) (Figure A1.5-(e), (f)). The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample consists of low-income individuals, as defined in Section A1.1, using the BRFSS from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table A1.1: Robustness: Effect of BBCE Expansion on SNAP Participation

	(1)	(2)	(3)	(4)	(5)
A. Participation = 1					
BBCE	0.019***	0.024***	0.024***	0.025***	0.025***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
CONTROLS					
Individual	NO	YES	YES	YES	YES
State					
Unemployment	NO	NO	YES	NO	YES
SNAP Policies	NO	NO	NO	YES	YES
Pre-Period Mean			0.211		
B. Benefit Amounts					
BBCE	37.914**	51.175**	51.663**	52.276**	52.809**
	(17.567)	(20.985)	(21.151)	(21.465)	(21.391)
CONTROLS					
Individual	NO	YES	YES	YES	YES
State					
Unemployment	NO	NO	YES	NO	YES
SNAP Policies	NO	NO	NO	YES	YES
Pre-Period Mean			541.088		

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.1 reports the estimation results of the impact of the BBCE expansion on SNAP participation and household SNAP benefit amounts (in 2016 dollars) with a different combination of covariates using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each panel presents the DiD estimates for SNAP participation as a binary outcome (Panel A) and reported SNAP benefit amounts per year (Panel B). The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table A1.2: Effect of BBCE Expansion on SNAP Retailer Participation

		Store Type	
	(1)	(2)	(3)
	All	Usual	Non-Usual
BBCE	-8.482	-4.133	-4.349
	(165.353)	(10.299)	(160.897)
Baseline Controls	YES	YES	YES
Pre-Period Mean	3,917.017	802.660	3,114.357

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.2 reports the estimation results of the impact of the BBCE expansion on SNAP vendor counts using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.2. Each column presents the DiD estimate for the number of all stores (column (1)), usual stores (column (2)), and non-usual stores (column (3)) participating in SNAP, as defined in Section 5.2. The sample consists of SNAP-authorized retailers, using the Historical SNAP Retailer Locator Data from 1997 to 2016. Standard errors are clustered at the state level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table A1.3: Robustness: Effect of BBCE Expansion on Medicaid Participation

	Post-ACA Period (1997-2016)		Pre-ACA Period (1997-2013)	
	(1)	(2)	(3)	(4)
	All States	All States	All States	w/o CA
BBCE	0.017***	0.021***	0.015***	0.014***
	(0.004)	(0.005)	(0.005)	(0.005)
CONTROLS				
Baseline	YES	YES	YES	YES
ACA Expansion	YES	NO	N/A	N/A
Pre-Period Mean	0.275	0.275	0.275	0.267

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.3 reports the estimation results of the impact of the BBCE expansion on Medicaid participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for Medicaid participation as a binary outcome, using different specifications and data periods. Columns (1) and (2) present estimates from the full sample period (1997–2016), and columns (3) and (4) present estimates from the pre-ACA period (1997–2013). Column (1) presents the estimate from my preferred specification (column (1) of Table 1.4). Column (2) presents the estimate without controlling for the ACA expansion. Column (3) presents the baseline estimate for the pre-ACA period. Column (4) presents the estimate after excluding California (CA), which adopted BBCE after 2013. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA) (column (1) only).

Table A1.4: Effect of BBCE Expansion on Joint Program Participation: SNAP, Medicaid, WIC

	Medicaid			WIC		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	w/ SNAP	w/o SNAP	All	w/ SNAP	w/o SNAP
BBCE	0.017***	0.014***	0.003	0.015**	0.011***	0.003
	(0.004)	(0.003)	(0.003)	(0.007)	(0.004)	(0.004)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.275	0.143	0.133	0.149	0.076	0.072

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.4 reports the estimation results of the impact of the BBCE expansion on joint program participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for Medicaid or WIC participation as a binary outcome, accounting for whether individuals also participate in SNAP. Column (1) presents the baseline estimate for Medicaid participation, without accounting for participation in SNAP (column (1) of Table 1.4). Column (2) presents the estimate for participation in both Medicaid and SNAP, while column (3) presents the estimate for participation in Medicaid but not in SNAP. Column (4) presents the baseline estimate for WIC participation, without accounting for participation in SNAP (column (2) of Table 1.4). Column (5) presents the estimate for participation in both WIC and SNAP, while column (6) presents the estimate for participation in WIC but not in SNAP. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. In columns (4)-(6), the sample is restricted to females aged 15 to 39 with children in the household. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table A1.5: Heterogeneous Effects of BBCE Expansion on Medicaid Participation

	Utilization of SNAP Data		Combined Application Portal	
	(1)	(2)	(3)	(4)
	YES	NO	YES	NO
BBCE	0.020**	0.015***	0.019***	0.014
	(0.008)	(0.006)	(0.006)	(0.009)
Baseline Controls	YES	YES	YES	YES
Pre-Period Mean	0.288	0.269	0.267	0.290

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.5 reports the estimation results of the heterogeneous impacts of the BBCE expansion on Medicaid participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for Medicaid participation as a binary outcome, using different subsamples of states. Columns (1) and (2) present estimates based on whether states use SNAP data to verify Medicaid eligibility, particularly for children, to facilitate enrollment or renewal (e.g., Express Lane Eligibility). Columns (3) and (4) present the estimates based on whether states offer a combined application portal for SNAP and Medicaid. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA)).

Table A1.6: Effect of BBCE Expansion on Health Conditions

	Medical Diagnosis			Self-Reported Health				Risky Behavior	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Asthma	Diabetes	CHD	BMI	Obesity	Physical ($-$)	Mental ($-$)	Smoker	# Drinks
BBCE	-0.003	-0.002	-0.002	-0.015	-0.006	-0.001	-0.005	0.003	0.023
	(0.004)	(0.003)	(0.002)	(0.077)	(0.005)	(0.009)	(0.014)	(0.004)	(0.059)
Baseline Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.136	0.095	0.038	27.115	0.267	0.371	0.378	0.272	1.188

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.6 reports the estimation results of the impact the BBCE expansion on health conditions using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Each column presents the DiD estimate for the probability of ever being diagnosed with asthma (column (1)), diabetes (column (2)), or coronary heart disease (CHD) (column (3)), BMI (column (4)), the probability of being obese (column (5)), the probability of reporting poor physical health (column (6)) or mental health (column (7)) in the past 30 days, having ever smoked in the past 30 days (column (8)), and the number of alcoholic drinks per occasion in the past 30 days (column (9)). All outcomes are binary except for columns (4) and (9). The sample consists of low-income individuals, as defined in Section A1.1, using the BRFSS from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table A1.7: Heterogeneous Effects of BBCE Expansion on Health Conditions

A. Medical Diagnosis of Diseases

	Asthma		Diabetes		Coronary Heart Disease	
	(1)	(2)	(3)	(4)	(5)	(6)
	White	Others	White	Others	White	Others
BBCE	-0.002	-0.008	0.002	-0.004	0.001	-0.002
	(0.005)	(0.006)	(0.002)	(0.005)	(0.011)	(0.003)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.144	0.129	0.080	0.110	0.042	0.034

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B. Self-Reported Health

	Obesity		Poor Physical Health		Poor Mental Health	
	(1)	(2)	(3)	(4)	(5)	(6)
	White	Others	White	Others	White	Others
BBCE	0.002	-0.011**	-0.000	-0.005	-0.007	-0.014
	(0.006)	(0.006)	(0.008)	(0.010)	(0.011)	(0.013)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.236	0.294	0.378	0.363	0.386	0.372

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.7 reports the estimation results of the heterogeneous impacts of the BBCE expansion on health conditions by race, using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. In Panel A, each column presents the DiD estimate for the probability of ever being diagnosed with asthma (columns (1) and (2)), diabetes (columns (3) and (4)), and coronary heart disease (columns (5) and (6)). In Panel B, each column presents the DiD estimate for the probability of being obese (columns (1) and (2)) and the probability of reporting poor physical health (columns (3) and (4)) or mental health (columns (5) and (6)) in the past 30 days. All outcomes are binary. The sample consists of low-income individuals, as defined in Section A1.1, using the BRFSS from 1997 to 2016. Standard errors are clustered at the state level. BRFSS person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA).

Table A1.8: Heterogeneous Effects of BBCE Expansion on Hours Worked

	Living w/ Dependents		Relation to HH Head		Marital Status	
	(1)	(2)	(3)	(4)	(5)	(6)
	Yes	No	Head	Other	Married	Unmarried
BBCE	-0.368***	-0.201	-0.173	-0.388**	-0.588	-0.112
	(0.126)	(0.284)	(0.290)	(0.162)	(0.486)	(0.218)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	21.599	13.421	19.000	17.880	23.363	16.115

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.8 reports the estimation results of the heterogeneous impacts of the BBCE expansion on hours worked (per week) across different subsamples, using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1. Columns (1) and (2) present the DiD estimates after splitting the sample into those living with dependents under age 18 and those without dependents, respectively. Columns (3) and (4) present the DiD estimates by relationship to household head. Columns (5) and (6) present the DiD estimates by marital status. The sample consists of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

A1.1 Sample Construction

I construct a sample of low-income individuals or families to identify those most likely to be affected by the BBCE expansion to ensure sufficient statistical power for the estimation. A key challenge is that income may be endogenous to the BBCE expansion. To address this, I predict the FPL ratio—calculated as family income divided by the poverty level—using characteristics of the sample *prior to* the implementation of BBCE, so that these predictions remain unaffected by the policy. I follow the approach proposed by Rittenhouse (2023).[37]

I begin with a sample of households with children under age 18 from the *pre*-treatment period of the CPS-ASEC from 1997 to 1999.[38] Next, I divide the sample into a training set (75%) and a validation set (25%) and run a linear regression on the training set using the following equation:

$$Y_i = \alpha + \beta X_i + \delta X_i \times X_i \tag{1.3}$$

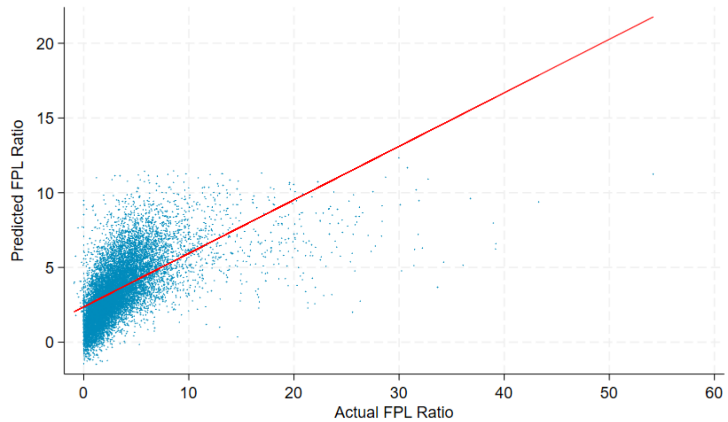
Y_i is the FPL ratio of household i, serving as the outcome variable of interest. X_i is a vector of observable characteristics, including the household head's and spouse's age, gender, race and ethnicity, education, number of children under age 18, number of family members, and state, with one level of interaction included. I obtain predicted values of the FPL ratio by applying the estimated regression coefficients to the entire dataset. Finally, I define households in the lowest 35% of the predicted FPL ratio as low-income families, as this corresponds to the proportion of individuals whose family income was below 200% of the poverty level before BBCE. The same procedure is applied to all analyses using different data, except when using USDA administrative data on statewide SNAP participation counts for individuals and vendors.

The scatter plots in Figure A1.6 compare predicted and actual FPL ratios in the validation sample (25%) of the CPS-ASEC. Each blue dot in Figure A1.6-(a) corresponds to an observation,

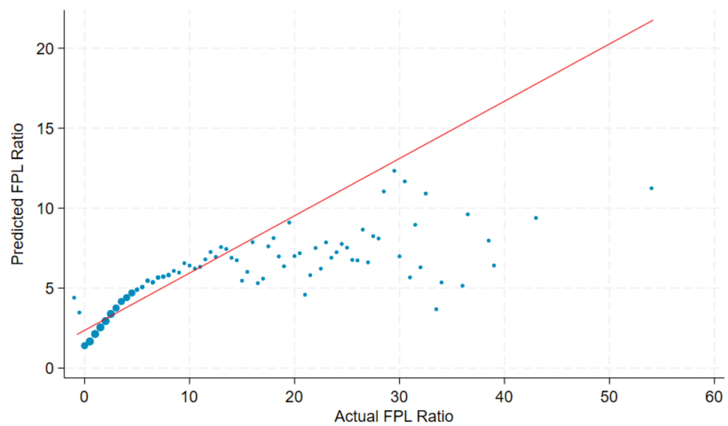
[37] In the referenced paper, this procedure aims to estimate the tax value of birth and define "low-income" births by predicting household income.

[38] This is because SNAP benefits may be limited to three months within a three-year period for households without a child under age 18, unless they meet work requirements or qualify for an exemption.

while Figure A1.6-(b) displays the average predicted values within bins of actual FPL ratios, with bin lengths of 50 percentage points. Both figures demonstrate that the estimation strategy is effective in ordering observations by the FPL ratio, particularly within the low-income population. The out-of-sample R-squared is 0.351.



(a) Scatter Plot of Raw Data



(b) Binned Scatter Plot

Figure A1.6: Out-of-Sample CPS-ASEC Predictions

Notes: Figure A1.6 compares predicted and actual FPL ratios in the validation sample (25%) of the CPS-ASEC from 1997 to 1999. The predicted FPL ratio is estimated using Equation 1.3. The x-axis represents the true FPL ratio, while the y-axis shows the predicted value. In Figure A1.6-(a), each blue dot represents an individual observation. In Figure A1.6-(b), each blue dot represents the average predicted values within bins of actual FPL ratios, with bin widths of 50 percentage points. In both figures, the red line represents the line of best fit through the data. The out-of-sample R-squared is 0.351.

A1.2 Robustness Checks

A1.2.1 Testing for Confounds

My identifying assumption is that the timing of the BBCE expansion across states is uncorrelated with each state's time-varying determinants of the outcomes of interest. One concern is that the second major BBCE expansion occurred around the Great Recession, potentially confounding the relationship between policy adoption and my outcomes. To address this, I estimate an event-study version of the main specification based on Equation 1.2 without controls, using the unemployment rate as the dependent variable. Figure A1.7 shows no strong evidence that state unemployment rates follow different trends depending on the timing of BBCE adoption, both over the entire data period (Figure A1.7-(a)) and in the restricted period from 2007 onward (Figure A1.7-(b)). Notably, there are no positive pre-trends, even when I restrict the sample to later years. This suggests that unemployment rates—representing a key measure of state economic conditions—were not a primary driver of BBCE adoption, consistent with the findings of Anders and Rafkin (2024).

A1.2.2 Comparison with U.S. Department of Agriculture (USDA) Data

As robustness checks on the first stage effects of the reform using the CPS-ASEC in Section 5.1, I rely on administrative data from the USDA covering the period from 1997 to 2016. The data on SNAP participation counts are aggregated at the level of month and participant's state of residence. In the administrative data, a participant in a given month is defined as an individual who has been issued SNAP food instruments (i.e., paper vouchers or EBT cards) for that month. I use the yearly level data to ensure consistency with other results from different data sources. I also draw on the average benefit amount for SNAP-participating households in each state and year to validate the treatment effects on the reported value of SNAP benefits.

Table A1.9 shows the DiD estimates for the natural log of the number of individuals (Panel A) and households (Panel B) participating in SNAP (i.e., participation counts) with different covariates, estimated using Equation 1.2. In my preferred specification with full controls in column (4), BBCE increased SNAP participation by 7.2 to 7.5 percent on average, with both estimates statistically significant at the 1% level. Compared to the estimate using the CPS-ASEC in column (1) of Table 1.3 as a binary outcome (i.e., an 11.7% increase relative to the baseline mean), these estimates are somewhat smaller but lie within the 95% confidence interval. The corresponding event-study figures, which show flat pre-trends and sharp dynamic increases, are presented in Figure A1.8.

Next, the effect of BBCE on SNAP benefit amounts from the CPS-ASEC ranges from \$47.4 to \$52.8 per year (a 9-10% increase across specifications), as shown in columns (2) and (3) of Table 1.3. Since the estimates are based on survey participants' reported values, I also compare them to the average benefit amount for SNAP-participating households from the USDA data. Prior to BBCE adoption, the average yearly benefit amount for participating households is \$3,167 (in 2016 dollars), and the SNAP participation rate of low-income families from the CPS-ASEC is 21.1 percent. Following the policy adoption, SNAP participation rates increased by 7.2 percent for households as discussed above. Adjusting the average benefit amount using the sample mean and the treatment effect yields an increase of \$48.1 in household SNAP benefits. This suggests that the effect sizes of reported benefits from the CPS-ASEC align with those from the administrative data.

A1.2.3 Underreporting of SNAP participation

Underreporting of program participation variables in survey data, including the CPS, is well documented in the literature (Meyer, Mok, and Sullivan 2009). Although the CPS-ASEC complements USDA administrative data with detailed demographic variables, my estimates of SNAP participation could be biased if the degree of underreporting responds to the BBCE expansion.

Therefore, I directly test whether this measurement error is significantly affected by BBCE, using the administrative data that are not subject to this issue. In particular, I measure the degree of underreporting of SNAP participation as the ratio of SNAP participation between the CPS-ASEC and the administrative data (multiplied by 1,000 as a scaling factor). The numerator is the reported

SNAP participation count of low-income individuals from the CPS-ASEC, and the denominator is the statewide SNAP participation count from the administrative data.

Table A1.10 shows that the SNAP participation ratio does not change significantly following BBCE, under different combinations of controls. This holds both in terms of statistical significance and the magnitudes of the coefficients. These findings suggest that, even with the well-established underreporting of SNAP participation in the CPS, this measurement error is not correlated with the BBCE expansion and therefore does not bias my estimates.

A1.2.4 Falsification Tests

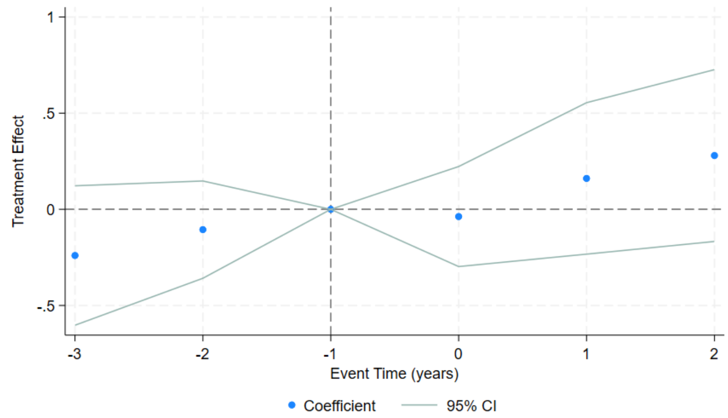
To ensure that my results capture true policy effects, I conduct two types of falsification tests to evaluate the impact of BBCE expansion on SNAP participation.

I first perform a permutation test using two data sources: the CPS-ASEC and USDA administrative data. In this test, I randomly reassign states to treated or untreated groups, while keeping the original BBCE adoption dates unchanged. The average effect of BBCE on SNAP participation under these counterfactual assignments should be close to zero. I reassign treatment status 1,000 times and plot the estimates of each counterfactual policy effect as a histogram in Figure A1.9, separately for each data source.

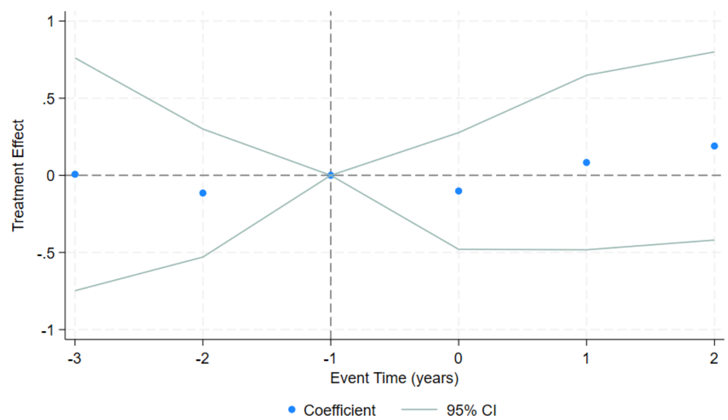
Figure A1.9-(a) presents the results from the CPS-ASEC, where the distribution is bell-shaped with a mean of -0.001 and a standard deviation of 0.008. The vertical red line represents the original DiD estimate of 0.025 for the effect of the BBCE expansion on low-income individuals' SNAP participation rates as a binary outcome (column (1) of Table 1.3). Figure A1.9-(b) presents the results from USDA administrative data, showing the coefficients on the natural log of the number of individuals participating in SNAP (i.e., participation counts). The distribution closely resembles that in Figure A1.9-(a), with a mean of 0 and a standard deviation of 0.021. Again, the original DiD estimate of 0.075 (column (4) of Table A1.9-A), represented by the vertical red line, also falls in the right tail of the distribution. The *p*-values from the two permutation tests are 0.003 and 0.001, respectively.

As another type of falsification test, I estimate the effects of BBCE on SNAP participation among *high*-income individuals as placebo groups and report the event-study graphs in Figure A1.10. I use the same specification as in Figure 1.3-(a) and column (1) of Table 1.3, using a binary outcome in the CPS-ASEC. However, the sample now consists of high-income individuals, defined as the top 10% (Figure A1.10-(a)), top 20% (Figure A1.10-(b)), and top 35% (Figure A1.10-(c)) of individuals based on the predicted FPL ratio, as discussed in Section A1.1.

Using the same y-axis scale as in Figure 1.3-(a) for low-income individuals, Figure A1.10 shows that there is no sharp increase in SNAP participation for any subsample of high-income individuals across different thresholds. Together with the permutation test, these results support the validity of my research design and estimates, indicating that they reflect actual policy effects rather than patterns driven by chance.



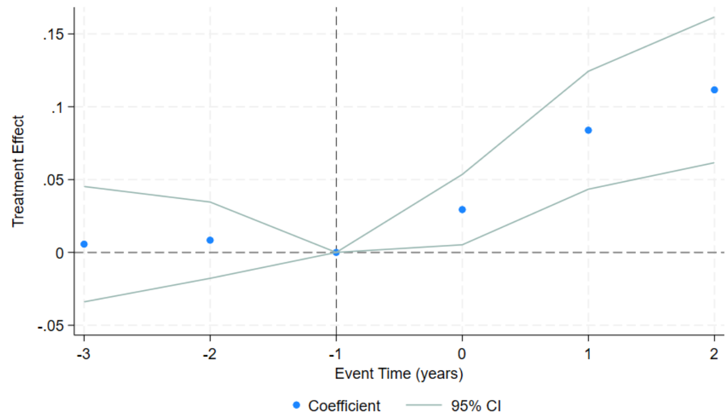
(a) Unemployment Rates (1997-2016)



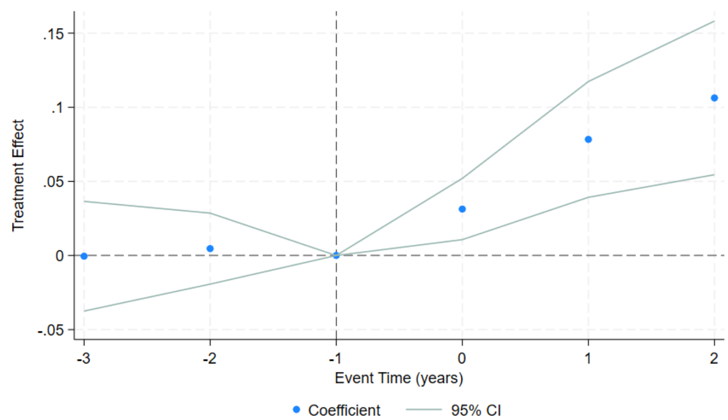
(b) Unemployment Rates (2007-2016)

Figure A1.7: Effect of BBCE Expansion on Unemployment Rates

Notes: Figure A1.7 displays the estimation results of the impact of the BBCE expansion on state unemployment rates using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.2. Each figure shows the effect over the entire data period (Figure A1.7-(a)) and in the restricted period from 2007 onward (Figure A1.7-(b)). Standard errors are clustered at the state level.



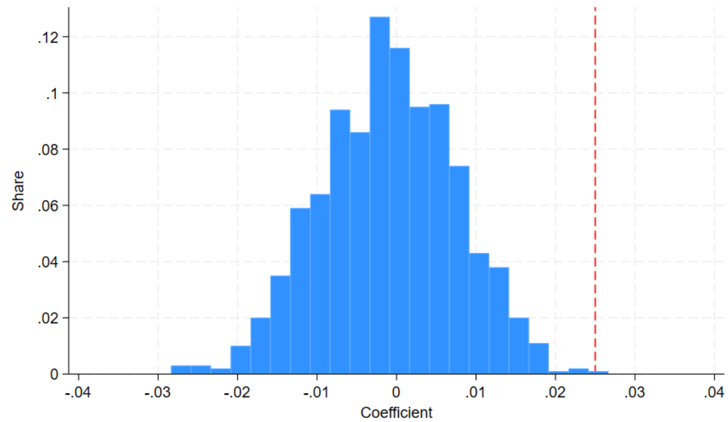
(a) Individuals



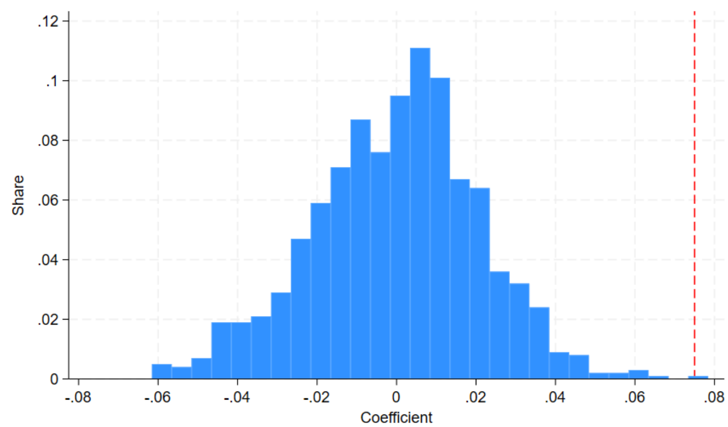
(b) Households

Figure A1.8: Robustness: Effect of BBCE Expansion on SNAP Participation (USDA Data)

Notes: Figure A1.8 displays the estimation results of the impact of the BBCE expansion on SNAP participation using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024). Each figure shows the effect on the natural log of the number of individuals (Figure A1.8-(a)) and households (Figure A1.8-(b)) participating in SNAP based on Equation 1.2, using USDA administrative data from 1997 to 2016. The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. Standard errors are clustered at the state level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.



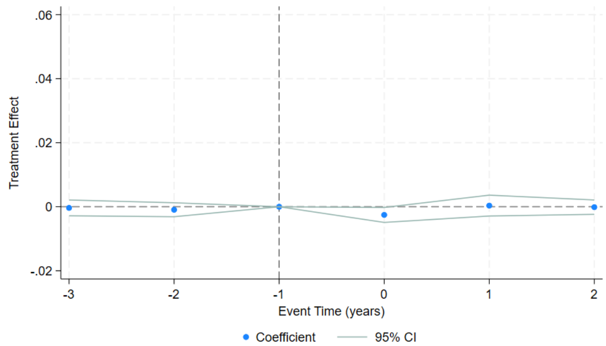
(a) SNAP Participation Rate (binary)



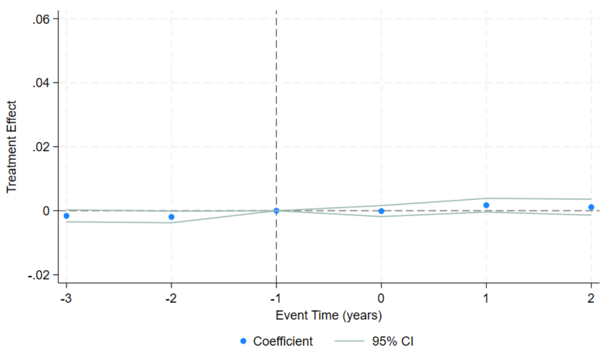
(b) log(SNAP Participation Counts)

Figure A1.9: Permutation Test: Effect of BBCE Expansion on SNAP Participation

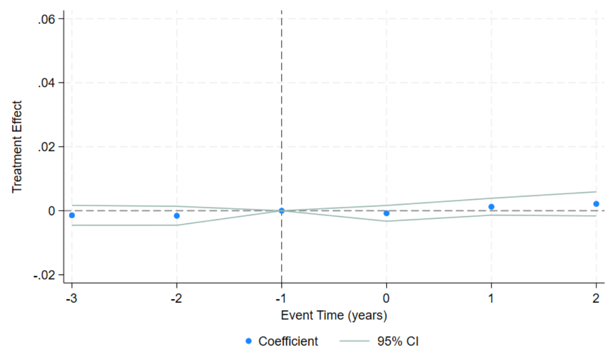
Notes: Figure A1.9 displays the distribution of the average effect of the BBCE expansion on SNAP participation, using the CPS-ASEC (Figure A1.9-(a)) and USDA administrative data (Figure A1.9-(b)). This is done by randomizing the treated states 1,000 times with the adoption dates fixed, using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.1 (Figure A1.9-(a)) and Equation 1.2 (Figure A1.9-(b)). The numbers on the x-axis correspond to the estimates from each replication, and each bar shows the relative frequency within the given interval of estimates. The vertical red lines represent the original DiD estimates for the effect of BBCE on SNAP participation: 0.025 (column (1) of Table 1.3) and 0.075 (column (4) of Table A1.9-A). The *p*-values of the permutation tests are 0.003 and 0.001, respectively.



(a) Top 10%



(b) Top 20%



(c) Top 35%

Figure A1.10: Falsification Test: Effect of BBCE Expansion on SNAP Participation

Notes: Figure A1.10 displays the estimation results of the impact of the BBCE expansion on SNAP participation among high-income individuals using the event-study framework developed by De Chaisemartin and d'Haultfoeuille (2024). Each figure shows the effect on high-income individuals' program participation as a binary outcome based on Equation 1.1. The x-axis represents the number of years since the BBCE expansion (with negative numbers indicating pre-intervention placebo effects). The 95% confidence intervals are represented by light green lines. The sample of high-income individuals is defined as the top 10% (Figure A1.10-(a)), top 20% (Figure A1.10-(b)), and top 35% (Figure A1.10-(c)) of individuals based on the predicted FPL ratio, as discussed in Section A1.1, using the CPS-ASEC from 1997 to 2016. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age, gender, race/ethnicity, marital status, and education level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table A1.9: Robustness: Effect of BBCE Expansion on SNAP Participation (USDA Data)

	(1)	(2)	(3)	(4)
A. Individuals (in %)				
BBCE	0.081***	0.076***	0.081***	0.075***
	(0.019)	(0.018)	(0.019)	(0.018)
CONTROLS				
Individual	N/A	N/A	N/A	N/A
State				
Unemployment	NO	YES	NO	YES
SNAP Policies	NO	NO	YES	YES
Pre-Period Mean	522,613			
B. Households (in %)				
BBCE	0.078***	0.072***	0.078***	0.072***
	(0.019)	(0.018)	(0.019)	(0.018)
CONTROLS				
Individual	N/A	N/A	N/A	N/A
State				
Unemployment	NO	YES	NO	YES
SNAP Policies	NO	NO	YES	YES
Pre-Period Mean	235,064			

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.9 reports the estimation results of the impact of the BBCE expansion on SNAP participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) with a different combination of covariates. Each Panel presents the DiD estimates for the natural log of the number of individuals (Panel A) and households (Panel B) participating in SNAP based on Equation 1.2, using USDA administrative data from 1997 to 2016. Standard errors are clustered at the state level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

Table A1.10: Effect of BBCE Expansion on Underreporting of SNAP Participation

	(1)	(2)	(3)	(4)
BBCE	-0.010	-0.008	-0.010	-0.009
	(0.023)	(0.022)	(0.022)	(0.022)
CONTROLS				
Unemployment	NO	YES	NO	YES
SNAP Policies	NO	NO	YES	YES
Pre-Period Mean		0.941		

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.10 reports the estimation results of the impact of the BBCE expansion on the degree of underreporting of SNAP participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) based on Equation 1.2. Each column presents the DiD estimate for the SNAP participation ratio between CPS-ASEC and USDA administrative data (multiplied by 1,000 as a scaling factor), using a different combination of covariates. The numerator is the reported SNAP participation count of low-income individuals, as defined in Section A1.1, using the CPS-ASEC from 1997 to 2016, with CPS-ASEC person-level weights applied when aggregating to the state-year level. The denominator is the statewide SNAP participation count from USDA administrative data from 1997 to 2016. Standard errors are clustered at the state level. State-level covariates include unemployment rates and four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3.

A1.3 Continuous Treatment

I estimate the same specification using two measures of continuous treatment, rather than a binary indicator, to better capture differences in treatment intensity given variation in income thresholds across BBCE states. The average coefficients from USDA administrative data (column (1)) and the CPS-ASEC (columns (2)-(4)) are presented in Table A1.11.

Panel A shows the estimates for the first treatment measure, which is constructed based on the gross income limit, ranging from 130% to 200% of the FPL. I assign a value of 0 to states and years without the BBCE expansion (i.e., 130% of the FPL) and assign the change in the gross income limit as a percentage of the FPL to treated states after adoption, scaling it to represent the effect of a 10 percentage-point increase in the income limit. Columns (1) and (2) show that a 10 percentage-point increase in the gross income limit (i.e., from 130% to 140% of the FPL) increased SNAP participation by 1.3 percent and 0.4 percentage points (a 2.03% increase), respectively for each data source. With the maximum increase in the income threshold from 130% to 200% of the FPL, the estimates indicate 8.82 and 14.04 percent increases in SNAP participation.

In Panel B, the other continuous treatment measure is based on the share of the predicted SNAP-eligible population as determined by the gross income limit.[39] For example, during the pre-treatment period, the lowest 20% of survey respondents in the CPS-ASEC had predicted family incomes below 130% of the FPL, while the lowest 35% had predicted incomes below 200% of the FPL. I assign a value of 0 to states and years without the BBCE expansion and assign the change in the eligible population share to treated states after adoption, scaling it to represent the effect of a 1 percentage-point increase in the population share.

Panel B shows that a 1 percentage-point increase in the share of the predicted SNAP-eligible population (i.e., from the bottom 20% of the income distribution to 21%) increased SNAP participation by 0.06 percent (column (1)) and 0.2 percentage points (a 0.93% increase) (column (2)). Again, when the eligible population share rises from 130% to 200% of the FPL, SNAP par-

[39]These values are also used to plot the time series of the predicted SNAP-eligible population share in Figure 1.2.

ticipation increases by 8.72 and 14 percent, which are very similar in magnitude to those in Panel A.

Assuming the maximum increase in all BBCE states, the estimates are slightly larger than those in Table 1.3 and Table A1.9 from the binary treatment: 11.7 percent from the CPS-ASEC and 7.2 to 7.5 percent from the USDA data. Since 16 of the 28 BBCE states in my sample adopted the 200% limit, however, the effects across treatment measures remain highly consistent. Applying the same approach to Medicaid and WIC participation in columns (3)–(4) yields estimates from the continuous treatment that similarly align with those from the binary treatment.

Table A1.11: Continuous Treatment: Effect of BBCE Expansion on Program Participation

	log(Part. Count)	Binary Outcomes		
	(1)	(2)	(3)	(4)
	SNAP	SNAP	Medicaid	WIC
A. Gross Income Limit				
$BBCE_{Inc\ Limit}$	0.013***	0.004***	0.003***	0.003***
	(0.003)	(0.001)	(0.001)	(0.001)
Baseline Controls	YES	YES	YES	YES
Pre-Period Mean	522,613	0.211	0.275	0.149
B. Eligible Population Share				
$BBCE_{Pop\ Share}$	0.006***	0.002***	0.001***	0.001***
	(0.001)	(0.0005)	(0.0003)	(0.0004)
Baseline Controls	YES	YES	YES	YES
Pre-Period Mean	522,613	0.211	0.275	0.149

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A1.11 reports the estimation results of the impact of the BBCE expansion on program participation using the difference-in-differences (DiD) estimator developed by De Chaisemartin and d'Haultfoeuille (2024) with continuous treatment measures. Panel A presents the estimates for the first treatment measure, which is constructed based on the gross income limit, ranging from 130% to 200% of the FPL. I assign a value of 0 to states and years without the BBCE expansion (i.e., 130% of the FPL) and assign the change in the gross income limit, expressed as a percentage of the FPL, to treated states after adoption. The measure is scaled to represent the effect of a 10 percentage-point increase in the income limit. Panel B presents the estimates for the second treatment measure, which is constructed based on the share of the predicted SNAP-eligible population as determined by the gross income limit. Specifically, during the pre-treatment period, the lowest 20% of survey respondents in the CPS-ASEC had predicted family incomes below 130% of the FPL; 27% for 160% of the FPL; 28% for 165% of the FPL; 32% for 185% of the FPL; and 35% for 200% of the FPL. I assign a value of 0 to states and years without the BBCE expansion and assign the change in the eligible population share to treated states after adoption. The measure is scaled to represent the effect of a 1 percentage-point increase in the eligible population share. In each panel, column (1) presents the DiD estimates for the natural log of the number of individuals participating in SNAP based on Equation 1.2, while columns (2)-(4) present the DiD estimates for low-income individuals' program participation as a binary outcome based on Equation 1.1. The sample consists of all SNAP participants using USDA administrative data (column (1)) and low-income individuals, as defined in Section A1.1, using the CPS-ASEC (columns (2)-(4)) from 1997 to 2016. In column (4), the sample is restricted to females aged 15 to 39 with children in the household. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied (columns (2)-(4)). Individual-level covariates include age, gender, race/ethnicity, marital status, and education level (columns (2)-(4)). State-level covariates include unemployment rates, four SNAP policy indices (i.e., eligibility, transaction costs, stigma, and outreach), as defined in Section 2.3, and indicators for Medicaid expansion as part of the Affordable Care Act (ACA) (columns (3)-(4)).

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Chapter 2 PRIVATIZING THE DISTRIBUTION OF NUTRITION BENEFITS: EVIDENCE FROM MISSISSIPPI WIC

This chapter is based on joint work with Shreya Bhardwaj (University of Southern California) and Katherine Meckel (University of California, San Diego)

1 Introduction

A central question in economics is whether public services should be provided directly by the government or contracted out to private providers. Proponents argue that private provision can improve efficiency through competition and innovation, while critics emphasize concerns about service quality when important dimensions of performance are difficult to monitor or contract over. As a result, the choice between public provision and contracting out involves fundamental trade-offs between efficiency and equity.

These issues are particularly relevant for in-kind transfer programs, which constitute a major component of anti-poverty policy in the United States and other developed countries. An important design choice is whether goods are distributed directly through government-operated sites or indirectly through vouchers redeemed in private markets. Voucher-based systems may expand consumer choice and reduce costs through private-sector competition, but they may also weaken incentives to maintain service quality and limit access in rural or thin markets. Despite the importance of these trade-offs, there is limited empirical evidence on the effects of distribution systems within U.S. programs, in part because large-scale changes in benefit delivery mechanisms are rare.[40]

In this paper, we study a unique, large-scale shift in benefit distribution in Mississippi's Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). During 2021,

[40]There is more evidence from developing countries, where several large programs have changed their distribution methods in recent decades (Banerjee et al. 2023; Jiménez Hernández and Seira 2022; Muralidharan, Niehaus, and Sukhtankar 2023).

Mississippi transitioned approximately 80,000 WIC participants from receiving food at government-operated warehouses to purchasing WIC-approved products at authorized grocery stores through a county-by-county rollout. Under the prior system, the state procured food directly from wholesalers and distributed it at state-run warehouses; under the new system, authorized retailers were reimbursed at retail prices for participant purchases. We use this shift to study how distribution mechanisms affect participation, targeting, and costs in a large federal nutrition program.

To analyze the effects of this transition, we compile monthly administrative data on WIC participation and costs from the Mississippi State Department of Health (MSDH) and the US Department of Agriculture (USDA), covering FY 2019 to FY 2024. This allows us to observe trends two years before and three years after the reform. Participation data are available at the county level and disaggregated by category (infants, pregnant women, postpartum mothers, and children). Cost data, including food and administrative expenditures, are reported at the state level. We supplement these sources with Vital Statistics birth records, which contain county identifiers and provide an alternative measure of WIC participation based on self-reported usage during pregnancy.

Our empirical strategy relies on difference-in-differences (DiD) models. For county-level WIC participation, we exploit the staggered, county-level rollout of retail distribution, using neighboring states as never-treated controls. Given concerns about bias in standard two-way fixed effects models under staggered treatment, we implement the de Chaisemartin and D'Haultfoeuille (DCDH) estimator to account for heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille 2020; De Chaisemartin and d'Haultfoeuille 2024). For state-level program costs, we use a DiD framework comparing Mississippi before and after the reform to trends in neighboring states. Our preferred specifications control for state-level unemployment rates and Medicaid policies.

We find that the transition to retail distribution led to a sustained 12–14 percent decline in overall WIC participation, with the largest reductions among children (-17 percent). There is no evidence of pre-existing divergent trends, and results remain robust across several specifications, including models that truncate the sample before the 2022 infant formula shortage and control for pandemic-related policy variation. We validate our finding of a decline in participation among pregnant women using birth certificate data.

We next turn to the targeting properties of the reform. Although WIC is a means-tested program, the value of benefits is based solely on household composition. We first document that participation declines were larger among households with smaller food packages, suggesting that the shift may have improved targeting efficiency by discouraging those with lower benefit levels (as in Nichols and Zeckhauser 1982). However, declines were also larger in high-poverty counties, suggesting a reduction in targeting by income level. Additionally, among pregnant women, participation declines were more pronounced among Black, low-educated, and unmarried mothers, historically disadvantaged groups.

We next examine the mechanisms underlying the participation decline. The sharpest declines occurred among children, suggesting that barriers associated with retail distribution deterred experienced participants, as WIC households typically enroll during pregnancy or infancy. This makes it unlikely that the decline was driven by information frictions or enrollment barriers, which primarily affect new applicants. Additionally, per-participant clinic expenditures did not change and clinic procedures remained unchanged, so hurdles associated with quarterly clinic appointments and recertification are unlikely explanations.

Instead, we consider the most likely mechanisms to be related to the shift in how and where participants obtained WIC foods. One factor that changed was the travel burden associated with WIC shopping. However, the transition expanded the number of food sites from 95 distribution centers to 292 grocery stores, reducing the average shopping distance by 1.5 miles (-30 percent). Since shorter travel distances should increase rather than decrease access, travel burden is unlikely to explain the participation decline.

We then examine whether shopping effort and stigma contributed to the participation decline. Surveys of WIC participants consistently identify grocery store shopping—–searching for WIC-approved items, dealing with checkout errors, and stockouts that require multiple trips—–as one of the most burdensome aspects of participation (WIC 2017; Barnes et al. 2023; USDA 2023; Gleason et al. 2021). Checkout-related stigma is also reported, though less frequently.

To compare these experiences with the pre-reform direct distribution model, we draw on both quantitative and qualitative evidence. First, we hypothesize that if shopping effort plays a role in the participation decline, it should operate on both the extensive margin (program participation) as well as the intensive margin (share of food benefits redeemed). This contrasts with fixed costs of participation like travel distance, which should only operate on the extensive margin. We use statewide redemption records from before and after the transition to retail distribution to calculate redemption shares across five consistently measured food categories. Because redemption shares are calculated as a fraction of benefits issued, they already account for the decline in program participation.

Across each of these categories, we find large reductions in the redemption share of benefits issued, often exceeding 50%. The size of these declines rules out the possibility that they are driven by changes in the composition of participants by redemption propensity. We also find evidence that redemption rates gradually increased in the years following the reform, consistent with improvements in grocer stocking practices or administrative processes. Nonetheless, redemption shares remain well below pre-reform levels. Using the same data, we also find that the set of WIC-eligible products exhibits high turnover, suggesting ongoing learning costs.

We supplement this quantitative analysis with qualitative data from two sources: a state-commissioned report on center-based shopping experiences, and hundreds of archived Google reviews of WIC distribution centers. These sources describe a structured, efficient shopping process in which staff played an active role in assisting participants. However, they also highlight limitations, including restricted hours and narrower product variety compared to retail stores.

Taken together, the quantitative and qualitative evidence supports the hypothesis that increased shopping burden contributed to the decline in participation. The structured, one-stop format of distribution centers may have reduced logistical and cognitive barriers relative to the more decentralized and complex process of redeeming benefits in retail settings.

We next evaluate the effects of the reform on program costs. First, we estimate that per-participant expenditures initially rose during the transition but ultimately declined by 32 percent in the post-reform period. This reduction was driven entirely by a 44 percent drop in food-related costs, with no significant change in administrative ("clinic") expenditures. Clinic costs include enrollment, recertification, and the provision of nutrition and health education. Disaggregating food-related costs using state procurement contracts, we find that the decline was attributable entirely to the elimination of state-run warehouses; the cost of the food packages themselves remained stable.

This stability in food benefit costs is somewhat surprising given the structural differences in food procurement between the two models: competitive bidding under direct distribution versus market-based pricing in retail settings. In fact, further employing our redemption data, we find it reflects two offsetting forces: higher per-ounce food prices under retail distribution and the lower redemption rates discussed above. Across eight of nine food categories where we can conduct direct comparisons, we find consistently higher per-ounce prices post-reform. These price gaps exceed standard grocery markups, suggesting that competitive procurement in the prior system substantially lowered costs. That said, changes in product composition complicate a clean comparison. In the ninth category——milk——retail distribution resulted in lower average prices because the prior warehouse model restricted offerings to shelf-stable (ultra-pasteurized) varieties due to limited refrigeration capacity. Overall, our cost analysis sheds light on the trade-offs of direct procurement versus market vouchers for public goods and services.

Our study contributes to several strands of literature on the design and delivery of redistributive transfers. First, our paper is related to a large body of research on incomplete program take-up, which finds that information costs, transaction costs, and provider interactions influence participation (see, for example, Currie 2004; Kleven and Kopczuk 2011; Herd and Moynihan 2018; Finkelstein and Notowidigdo 2019). Recent work highlights the role of procedural hurdles that arise during enrollment and re-certification, such as burdensome application processes, re-certification requirements, and wait times (Deshpande and Li 2019; Wu and Meyer 2023; Homonoff and Somerville 2021). Our findings contribute to this literature by demonstrating that benefit distribution systems can impose frictions that meaningfully affect participation. While this literature focuses on participation at the extensive margin, our unique collection of administrative records allows us to examine effects on the intensive margin as well.

We contribute to the literature on contracting out public service delivery, which examines the effects of shifting government functions to private entities. While privatization can improve efficiency, it may also reduce access when cost-cutting incentives lead to lower-quality service (Hart, Shleifer, and Vishny 1997; Duggan 2004; Kuziemko, Meckel, and Rossin-Slater 2018; Currie and Fahr 2005). Our findings are broadly aligned with this literature, showing that while transferring food provision to private retailers lowered government costs, it also introduced frictions that discouraged participation by diminishing service quality——specifically, the shopping experience.

The remainder of the paper is structured as follows. Section 2 provides background on the WIC program and the transition from direct to retail distribution in Mississippi. Section 3 develops a simple framework to predict how the privatization affects service quality. Section 4 describes the data, and Section 5 outlines the empirical strategy. Section 6.1 presents the main results on participation, and Section 6.2 explores potential mechanisms driving the observed effects. Section 6.3 presents the results on program costs and explores underlying mechanisms. Section 7 concludes.

2 Institutional Background

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a federal assistance program administered by the United States Department of Agriculture (USDA) with the goal of safeguarding the nutritional well-being of low-income women and their young children. Participants include pregnant and postpartum women, as well as infants and children under the age of five. To qualify for WIC, individuals must live in households with annual incomes below 185 percent of the federal poverty line (currently \$51,338 for a family of four) or participate in Medicaid.[41] Participants must also be deemed "at nutritional risk," though this requirement rarely binds (IOM 2000).

WIC benefits are designed to address nutritional deficiencies and improve health outcomes. These benefits include the provision of specific nutritious foods, health screenings, nutrition ed-

[41] Source for income cut-off: https://liheapch.acf.hhs.gov/profiles/povertytables/FY2023/popstate.htm.

ucation, and referrals to other social services. In 2023, WIC served approximately 6.6 million individuals, operating with a program budget of $6.7 billion.[42] Evidence suggests that WIC participation significantly improves outcomes such as infant health, maternal health, breastfeeding rates, and access to other social services (Bitler and Currie 2005; Currie and Rajani 2015; Hoynes, Page, and Stevens 2011; Rossin-Slater 2013).

A distinguishing feature of WIC among US anti-poverty programs its use of fixed-quantity benefits rather than value-based transfers, which are used in programs like the Supplemental Nutrition Assistance Program (SNAP). For instance, a pregnant woman enrolled in WIC is eligible to receive a monthly food package that includes the following quantities: 4 gallons of milk, 36 ounces of breakfast cereal, 48 ounces of whole wheat bread, one dozen eggs, 10 ounces of canned fish, 64 ounces of juice, and a choice between 64 ounces of canned or dried beans or 18 ounces of peanut butter. Tables A2.1 and A2.2 detail the current maximum food packages for women, children, and infants under one year of age.

WIC is a widely used program. In 2021, 39% of infants, 23% of children aged one to four, and 20% of pregnant and postpartum women nationwide participated in the program. Despite its broad reach, participation rates are relatively low, with only 51.2% of eligible individuals enrolling (Gray et al. 2021). Prior work highlights various barriers to participation, including distance to clinics, burdensome documentation requirements, and hurdles involved in the shopping process (Bitler, Currie, and Scholz 2003; Swann 2007; Rossin-Slater 2013; Barnes et al. 2023; Meckel 2020).

2.1 Distribution Systems

Although WIC is primarily federally funded, states have significant autonomy in its administration. Historically, states have chosen among three delivery systems: retail distribution, direct distribution, and home delivery. Under retail distribution, participants use food instruments (paper vouchers or electronic cards) to purchase WIC-approved items at grocery stores. In direct distribu-

[42] Information about WIC program participation and funding is available here: https://www.fns.usda.gov/pd/wic-program.

tion, state programs procure food through competitive bidding and distribute it via government-run warehouses ("distribution centers"). Home delivery involves state-procured food being delivered directly to participants' homes.

Since WIC's inception in 1974, retail distribution has been the predominant model. A 1976 survey found that 65% of WIC clinics used retail distribution, 17% used home delivery, and 7% relied on direct distribution, while 11% employed a mix of methods (Bendick 1976). Early funding formulas incentivized retail distribution by allocating states more administrative funds when food costs were higher, as retail prices exceed wholesale costs.[43] By 2020, only Mississippi continued to use a distribution system other than retail.

2.2 Mississippi WIC and the Transition from Direct to Retail Distribution

In FY 2019, Mississippi WIC served 79,192 participants, representing 1.2% of all WIC participants nationwide.[44] Using U.S. birth certificate data from 2018–2023, Table 2.1 compares WIC participation rates among pregnant women in Mississippi to national rates and examines demographic differences between WIC participants in Mississippi and those nationwide. In Mississippi, 43% of births are to WIC participants——more than 30% higher than the national average of 32%——likely reflecting the state's lower median household income (Engel and Posey 2024). Among pregnant WIC participants in Mississippi, 59% are Black, more than double the national share of 24%. Mississippi's WIC participants are less likely to be married or and more likely to have attended college compared to the national average.

Enrollment in Mississippi WIC takes place at clinics located throughout the state, with approximately one clinic per county.[45] Participants must visit these clinics in person every three months for nutritional counseling and health screenings, and every six months for re-certification.[46] Quarterly clinic visits are required to receive vouchers, which provide food benefits for the next

[43] As noted by Bendick (1976), states could maximize staff salaries by choosing retail distribution.
[44] Source: https://www.fns.usda.gov/pd/wic-program.
[45] Information about Mississippi WIC was collected through communications with MS WIC staff, the MS WIC website, and Simon and Leib (2011).
[46] As an exception, pregnant women are certified for the duration of their pregnancy as well as 6 weeks postpartum.

three months. Under the direct distribution model, benefits were provided as paper vouchers, printed and distributed at the clinic. In contrast, under the retail distribution model, participants receive electronic benefit cards, which are automatically reloaded after each appointment, with the initial card mailed to their home. Failure to redeem benefits for two consecutive months results in automatic removal from the program, and unused benefits do not roll over.

Under the direct distribution model, participants redeemed benefits at one of 96 WIC distribution centers across the state. These centers typically operated on weekday business hours (e.g., 8am to 5pm), although some were open during weekends as well. During the initial months of the COVID-19 pandemic, distribution centers remained open but restricted access to no more than two WIC participants at a time. County health centers, the site of most WIC clinics, remained open during normal business hours.[47]

The Healthy, Hunger-Free Kids Act of 2010 mandated that all state WIC programs transition to electronic benefit transfer (EBT, or "eWIC") as the method for delivering food benefits by October 1, 2020. For Mississippi, this mandate effectively required a shift from direct distribution to a retail-based system. EBT replaces paper vouchers with a plastic smart card that electronically stores a household's WIC benefits and dates between which the benefits can be redeemed. Although the federal deadline for this transition was October 2020, implementation in Mississippi was delayed due to the COVID-19 pandemic.

Mississippi adopted a phased rollout of EBT across counties, detailed in Figure A2.1 and Table A2.3. The process began with a pilot phase in February 2021 in Forrest, Lauderdale, and Lee counties, followed by three additional rollout phases between April and June 2021. Participants began receiving EBT cards during their regularly scheduled tri-monthly clinic appointments starting on their county's rollout start date. As a result, all participants in a given county received EBT cards within the first three months of retail distribution. Correspondingly, warehouses remained operational for three months after each county's rollout began, allowing participants who had received paper vouchers before the transition date to continue redeeming them. Participating stores

[47] Source: https://www.wjtv.com/news/msdh-announces-operation-changes-amid-coronavirus-outbreak/.

in each county were required to start accepting EBT on the rollout start date.

Figure A2.2 maps the 96 WIC distribution centers in FY 2020 and 292 WIC-authorized grocery stores in FY 2022. Counties are shaded by poverty rates (subfigure a) or urbanicity (subfigure b).[48] While distribution centers were relatively evenly spaced——roughly one per county——WIC stores are more concentrated in urban areas like Jackson (the state capital) and the Gulf Coast (including Gulfport and Biloxi), where poverty rates are lower. In contrast, the Mississippi Delta——a largely rural, high-poverty region in the northwest——has much sparser WIC store coverage.

To analyze the types of grocery stores participating in Mississippi's WIC program, we link our WIC store data to historical records on all SNAP-authorized retailers, which include store type classifications.[49] Table A2.4 shows that among Mississippi WIC-authorized grocers in FY 2022, 56.2% were classified as superstores and 39.7% as supermarkets, indicating relatively little variation in store size.

3 Conceptual Framework

We develop a simple model to predict how the privatization of benefit distribution in Mississippi WIC affects service quality, defined as amenities that improve the shopping experience for WIC households. We consider two types of providers——a public distribution center and a private retailer——each run by a manager who allocates effort to improve service quality. Our setup is similar to Hart, Shleifer, and Vishny (1997), but differs in an important respect: in their model, both public and private managers serve only program recipients, whereas in our setting the private provider serves both WIC and non-WIC customers. This distinction matters because amenities in the retail environment influence the broader customer base the retailer seeks to attract, not only WIC households.[50]

[48] Data on distribution centers and authorized grocery stores covering FY 2019–2023 were obtained from the Mississippi State Department of Health (MSDH) through an Open Records Request. This dataset includes the name, address, and, for distribution centers, the hours of operation for each location.

[49] The data are publicly available at https://www.fns.usda.gov/snap/retailer/historical-data.

[50] A second difference from Hart, Shleifer, and Vishny (1997) is that managers in our setting take costs as given rather than allocating effort toward cost reduction. Costs at public centers are determined by procurement and staffing rules, while costs at retailers are set by market forces, making it reasonable to treat them as outside managerial control.

3.1 Effort Allocation by Provider Type

We assume that some dimensions of service quality are contractible and fully specified by the government. After fulfilling these requirements, managers devote residual effort E_r to non-contractible aspects of service quality.

Public WIC manager. The public provider serves only WIC households. We assume they allocate a fixed share $\gamma \in (0, 1)$ of residual effort to service amenities (for example, helping participants find their allotted food benefits). The parameter $\gamma \in (0, 1)$ captures lower incentive intensity (e.g., if salaried employees who are not directly rewarded for quality improvements or due to misalignment between federal and state objectives). Assuming effort maps one-for-one into quality, WIC-relevant quality under public provision is therefore:

$$Q^{\text{gov}} = \gamma E_r.$$

Private retailer. Retailers divide residual effort E_r between two categories of amenities:

- **WIC-targeted amenities** (e.g., keeping WIC products in stock, signage, staff training), and
- **General amenities** (e.g., cleanliness, hours, layout).

Let $\omega \in [0, 1]$ denote the share of effort devoted to WIC-targeted amenities. General amenities benefit all shoppers, but only a fraction $\alpha \in (0, 1)$ of general amenities are relevant to WIC shoppers (e.g., cleanliness in WIC aisles vs. the rest of the store). WIC-relevant quality at a retailer is:

$$Q^{\text{retail}}(\omega) = \omega E_r + (1 - \omega)\alpha E_r.$$

3.2 Retailer Optimization

Private retailers are incentivized to provide quality because doing so attracts both WIC and non-WIC customers. Amenities attract both types of customers away from competing retailers and also draw WIC-eligible households into program participation.

Let $\delta \in [0, 1]$ denote the (expenditure-weighted) share of the potential customer base that is WIC-eligible. Let $R_w(\cdot)$ and $R_{nw}(\cdot)$ denote revenue net of cost for each customer type, assuming the entire customer base belonged to that type.[51] Both functions are increasing and concave so additional effort attracts customers at a diminishing rate:

$$R_i'(\cdot) = \rho_i(\cdot) > 0, \qquad R_i''(\cdot) = \rho_i'(\cdot) < 0, \quad i \in \{w, nw\}.$$

The retailer's profit function is given by:

$$\pi(\omega) = (1-\delta)\, R_{nw}((1-\omega)E_r) + \delta\, R_w(\omega E_r + \alpha(1-\omega)E_r) - C_o,$$

where C_o is a fixed cost term.

The retailer chooses ω to maximize $\pi(\omega)$, yielding the first-order condition:

$$(1-\delta)\, \rho_{nw}((1-\omega^*)E_r) = (1-\alpha)\, \delta\, \rho_w(\omega^* E_r + \alpha(1-\omega^*)E_r)\,. \tag{1}$$

This condition equates the marginal loss in revenue from non-WIC customers—arising from reallocating effort away from general amenities (a marginal increase in ω)—with the marginal gain in revenue from WIC-eligible households, who benefit directly from additional WIC-targeted effort. The relative weights on each of these terms depend on the store's potential customer mix (δ) and the spillover rate (α).

When δ is small, WIC households represent a limited share of potential revenue, so the marginal gain from WIC-targeted effort is small. In this case, the first-order condition can be satisfied only at a relatively low ω^*. As δ increases, WIC households contribute more to potential revenue, raising the marginal return to WIC-targeted amenities and increasing the optimal ω^*. By contrast, a higher spillover rate α reduces the value of additional WIC-targeted effort, since general

[51] More precisely, these functions represent revenue under the simplifying assumption that (i) WIC and non-WIC households have equal budgets, and (ii) WIC households spend only their benefits and make no additional non-program grocery purchases. The parameter δ then rescales revenues to reflect the much smaller volume of program relative to non-program expenditures.

amenities already provide value to WIC households. The optimal ω^* therefore declines as α rises.

3.3 WIC-Relevant Service Quality by Provider Type

Under private provision, the total WIC-relevant quality is given as follows:

$$Q^{\text{retail}} = E_r\big[\omega^* + \alpha(1-\omega^*)\big].$$

In contrast, under government provision, as stated above, WIC-relevant quality is given as follows:

$$Q^{\text{gov}} = \gamma E_r.$$

Privatization therefore raises WIC-relevant quality only if $Q^{\text{retail}} > Q^{\text{gov}}$, which requires that potential WIC expenditures at the store (as captured by δ) and/or spillovers from general amenities (α) are large enough that the retailer's optimal effort toward WIC-relevant amenities exceeds γ.

Small δ In our empirical analyses, we provide evidence that δ is very small. In this case, the optimal targeted effort satisfies $\omega^* \approx 0$, implying the following predictions:

- Retailers will offer little or no amenities specifically for WIC shoppers;
- Under privatization, WIC households benefit primarily through spillovers from general amenities, so WIC-relevant effort is approximately $Q^{\text{retail}} \approx \alpha E_r$;
- Privatization improves WIC-relevant service quality only if $\alpha > \gamma$, meaning WIC households must place sufficient value on general amenities relative to the level of service quality provided by the public WIC manager

These implications translate into predictions about participation, our primary outcome of interest. If retail distribution improves the WIC shopping experience, we expect: (i) higher extensive-margin participation, as improved quality raises the value of enrolling and remaining enrolled (e.g., by reducing stigma or shopping hassles); and (ii) higher intensive-margin participation, as the same improvements lower the effective cost of redeeming benefits in the store. Thus, both enrollment and redemption rates should increase when $Q^{\text{retail}} > Q^{\text{gov}}$ (and decrease when $Q^{\text{retail}} < Q^{\text{gov}}$).

4 Primary Data Sources

4.1 WIC Participation

We analyze WIC participation in Mississippi using data obtained from the Mississippi State Department of Health (MSDH), covering the period from January 2019 to September 2024. These data, provided in response to an Open Records Request, report monthly participation counts by county of residence and participant type (pregnant women, postpartum breastfeeding women, postpartum non-breastfeeding women, infants, and children). For comparison, we incorporate state-level monthly participation data from the USDA for Mississippi's neighboring states–Alabama, Arkansas, Louisiana, and Tennessee–covering the same period.[52] These data, which are similarly disaggregated by participant type, serve as a control group for participation trends in Mississippi.

In these data, a participant is defined as an individual who receives WIC food instruments during a given month.[53] Because WIC participants receive food benefits at quarterly WIC clinic appointments, monthly participation totals reflect the number of individuals who attended their most recent appointment.

[52]The data are available here: https://www.fns.usda.gov/pd/wic-program.

[53]From WIC Program Regulation 7 CFR 246.2: "Participation means the sum of the number of persons who have received supplemental foods or food instruments during the reporting period and the number of infants breastfed by participating breastfeeding women (and receiving no supplemental foods or food instruments) during the reporting period." Details are available at https://www.fns.usda.gov/wic/certification-and-monthly-food-benefits-issuance-cycles-and-reporting-monthly-participation-fns-798. We verified this definition with MS and USDA WIC staff.

4.2 Program Costs

We use monthly, state-level cost data for Mississippi WIC and its neighboring states from October 2018 to September 2023 (Figure A2.6). These data, provided by the USDA, categorize WIC program costs into two components: Food Costs and Nutrition Services and Administration (NSA) Costs.[54] Under direct distribution, Food Costs encompassed not only the wholesale cost of food but also expenses related to warehousing and distribution, such as staffing, leasing, and maintaining distribution centers. In contrast, under the retail distribution model, Food Costs reflect only the reimbursements paid to grocery retailers for WIC-approved food.

NSA Costs, which make up the remainder of program expenditures, cover administrative expenses related to clinic operations, including participant certification, nutrition education, breastfeeding support, and staff salaries. They also fund outreach activities, compliance monitoring, and the development of educational materials.

To separate food benefit costs from warehousing expenses under direct distribution, we utilize data from Transparency Mississippi, a state-managed website that provides detailed records of government expenditures, contracts, and payroll.[55] Specifically, we collect purchasing contract data from MSDH for grocery wholesalers during the direct distribution period.[56] These contracts reveal that MSDH procured food through a competitive bidding process, typically involving two or three wholesalers, selecting vendors based on the lowest prices for specified products. Contracts detailed product quality, quantities, packaging, and delivery schedules to WIC distribution centers. We aggregate these payments at the year-month level for the duration of the direct distribution period.

To construct a consistent time series of food benefit costs for Mississippi, we combine contract-derived food cost data from the direct distribution period (pre-February 2021) with USDA-

[54] The data are available here: https://www.fns.usda.gov/pd/wic-program.

[55] Transparency Mississippi is operated by the Mississippi Department of Finance and Administration. See https://www.transparency.ms.gov/about.aspx.

[56] The following contract numbers were provided by MSDH: Supervalu Holding (#8200051302) – Food; Bimbo Food (#8200051274) – Bread; Sunrise (#8200045731) – Produce; MS Fruit (#8200051464) – Produce; Mead Johnson (#8200052008) – Standard infant formula.

reported Food Costs data from the retail distribution period (February 2021 onward). We also use USDA Food Costs data for neighboring states, which used retail distribution throughout.

5 Identification Strategy

To estimate the causal effects of the transition from direct to retail distribution in Mississippi WIC, we employ three complementary identification strategies. The first two strategies rely on variation in the timing of the rollout across counties in Mississippi while incorporating state-level data from neighboring states (Alabama, Arkansas, Louisiana, and Tennessee) as never-treated controls. These strategies differ in their estimation approach: the two-way fixed effects (TWFE) model and the de Chaisemartin and D'Haultfoeuille estimator (DCDH), which addresses biases in TWFE models under heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille 2020; De Chaisemartin and d'Haultfoeuille 2024). The third strategy, applied at the state level, uses a standard difference-in-differences (DiD) framework to compare Mississippi (the treated unit) to neighboring states.

5.1 Two-Way Fixed Effects Estimator

Our primary strategy exploits the staggered rollout of EBT across Mississippi counties, using neighboring states as a "never-treated" control group. We estimate the following model for a given outcome Y_{gt}:

$$Y_{gt} = \alpha + \beta \textbf{\textit{Post}}_{gt} + \gamma_g + \mu_t + \Gamma' Z_{gt} + \epsilon_{gt} \tag{2.1}$$

where g represents a geographic unit–either Mississippi county or surrounding state–and t is year-month. The variable $Post_{gt}$ is an indicator equal to 1 if county g has transitioned to retail distribution by time t (so this variable is always 0 when g represents a neighboring state). γ_g and μ_t are fixed effects for geographic units and time, respectively. Z_{gt} includes state-level controls for unemployment rates and Medicaid policies.[57] The error term ϵ_{gt} is clustered at the level of county or

[57]These Medicaid controls, motivated by the existence of adjunct eligibility between Medicaid and WIC, are indicators for: (1) postpartum coverage extensions, adopted by Louisiana and Tennessee in April 2022, Alabama in January

state-year-month, depending on the level of aggregation of the data. Since EBT benefits were rolled out over a three-month period (with approximately one-third of county residents transitioning per month), we also estimate an alternative specification that splits $Post_{gt}$ into two indicators: one for the first three months after the transition began and another for subsequent months.

To assess treatment dynamics and test for pre-trends, we estimate event-study versions of this specification, replacing $Post_{gt}$ with indicators for event time relative to the transition. Specifically, we include 18 leads and 18 lags, balancing the sample within this symmetric, 36-month window.

Given the potential for bias in two-way fixed effects DiD models with staggered treatment (De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024), we implement the DCDH estimator proposed by De Chaisemartin and d'Haultfoeuille (2024), which accounts for treatment effect heterogeneity and avoids negative weighting issues.

Our identifying assumption is that there are no unobserved, time-varying factors correlated with the timing of the rollout. Under this assumption, β captures the causal effect of the transition on WIC participation in treated counties, relative to other (TWFE) or not-yet-treated (DCDH) counties and neighboring states.

Lastly, we apply the inverse hyperbolic sine transformation to the participation outcome variable to account for differences in magnitude and variation across Mississippi counties and the control states.

5.2 State-Level Difference-in-Differences

For analyses conducted at the state level, we employ a standard DiD framework comparing Mississippi to neighboring states. For an outcome Y_{st}, measured at the state s-month t level, we

2023, and Mississippi in December 2023; (2) work requirements, enforced in Arkansas from June 2018 to November 2021; and (3) expanded eligibility for immigrants within the five-year waiting period, implemented for children and pregnant women in Arkansas in January 2018 and for children in Louisiana in April 2019.

estimate:

$$Y_{st} = \alpha + \beta(\mathit{AfterJan2021}_t \cdot \mathit{MS}_s) + \rho_s + \lambda_t + \Gamma' Z_{st} + \epsilon_{st}, \tag{2.2}$$

where $\mathit{AfterJan2021}_t$ is an indicator equal to 1 for months after January 2021, the month before the start of the rollout in Mississippi, and MS_s is an indicator for Mississippi. State fixed effects (ρ_s) and time fixed effects (λ_t) account for unobserved variation at these levels, while Z_{st} controls for the state-level covariates described above. The error term ϵ_{st} is clustered at the state-date level (i.e., at the level of variation).

We extend this specification by estimating event-study models, replacing $\mathit{AfterJan2021}_t \cdot \mathit{MS}_s$ with event-time indicators. Additionally, in some specifications, we include separate indicators for the rollout period (February–July 2021) and the post-rollout period (August 2021 onward).

6 Results

6.1 Participation

Figure 2.1 compares monthly WIC participation trends in Mississippi to trends in neighboring states. Before Mississippi's transition to retail distribution began in February 2021, participation trends in the state were generally similar to those of its neighbors. However, during the rollout period (indicated by vertical red dashed lines), Mississippi experienced a pronounced decline in participation, while neighboring states' participation remained stable. This divergence persists after the rollout, as Mississippi's participation levels do not return to pre-rollout levels.

Two important events–the COVID-19 pandemic and the 2022 infant formula shortage–could potentially confound our analysis of participation trends. Figure A2.3 overlays participation trends with the timing of these events. Figure A2.3A shades the period of the COVID-19 pandemic, while Figure A2.3B shades the time frame of the 2022 infant formula shortage. These figures show that neither event coincides with the transition to retail distribution, and that they do not appear to be have had important effects on total WIC participation.

Figure A2.4 further examines WIC participation trends by county groups corresponding to each rollout phase. The figures show a sharp decline in participation immediately after the rollout start month for each group, with these declines largely persisting in the post-transition period.

We begin by examining the impact of the transition on WIC participation, leveraging the staggered rollout across Mississippi counties in a difference-in-differences framework (Eq. 2.1). As discussed above, the traditional two-way fixed effects (TWFE) estimator can produce biased estimates in the presence of heterogeneous treatment effects. Visual evidence from the raw data (Figure A2.4) suggests that treatment effects may vary over time (e.g., due to a gradual adjustment to the new scheme) as well as across county groups (e.g., due to differences in WIC offices or retail markets). Consistent with this, a diagnostic test reveals that 18% of the weights in the TWFE model are negative, highlighting the need for alternative estimation methods (De Chaisemartin and d'Haultfoeuille 2024). Therefore, we use the de Chaisemartin and D'Haultfoeuille (DCDH) estimator as our preferred approach.

Figure 2.2, Panel A, presents event-study estimates from the DCDH method, with coefficients plotted relative to the pre-treatment baseline ($t = -1$). The outcome variable is the inverse hyperbolic sine of total WIC participants. The absence of significant pre-treatment trends supports the validity of the parallel trends assumption. Following the implementation of retail distribution, participation declines over the three-month period corresponding to the phased distribution of EBT cards in each county, then stabilizes at a significantly lower level.

Table 2.2, Columns (1)-(3), presents the regression estimates from Eq. 2.1, comparing results across different estimators. Column (1) reports the DCDH estimate, which indicates an 12.7% overall decline in participation, statistically significant at the 1% level. Column (2) provides the TWFE estimate, which is similar in magnitude and significant at the 1% level. In Column (3), distinguishing between the three-month rollout period ("During Rollout") and the post-rollout period ("After Rollout"), the TWFE estimate for the post-rollout period is -13.5%, while the estimate during the rollout is -11.0%.

State-level DiD estimates based on Eq. 2.2 are reported in Columns (4)-(5) of Table 2.2. Column (4) estimates a 13.9% decline in participation, statistically significant at the 1% level. In

Column (5), separating the statewide rollout period (February–July 2021) from the post-rollout period (August 2021 onward), we find a larger post-rollout decline of 17.8%, also significant at the 1% level.

Taken together, these estimates imply that the transition to EBT cards reduced WIC participation by approximately 10,500 to 13,800 participants per month, compared to the pre-rollout mean of 77,665 participants per month.[58]

Robustness Table A2.5 presents robustness checks of our preferred specification. Column (1) shows the baseline county-level estimate of a 12.7% decline in participation. To address potential confounding from the 2022 infant formula shortage, which began in May 2022, Column (2) restricts the sample to data through April 2022, yielding a slightly smaller but statistically similar decline (-10.8%). Column (3) removes control variables, including Medicaid eligibility changes and unemployment rates, producing an estimate nearly identical to the baseline (-12.9%). Column (4) adds controls for the expiration of SNAP Emergency Allotments, which varied by state, and finds a comparable decline (-12.5%).[59] Lastly, Column (5) replaces the transformed outcome with raw participation counts, yielding a similar precise, negative effect (-10,170.4), corresponding to a 13.1% decline.

We use birth certificate data to validate the patterns observed in the administrative records. While administrative data capture all WIC participants, birth certificates are limited to pregnant women but provide detailed demographics and county identifiers for Mississippi and neighboring states. The outcome is a binary indicator for WIC receipt during pregnancy. Treatment status is assigned based on the start of the second trimester (conception + 14 weeks), consistent with evidence that most pregnant women enroll by this time (USDA-FNS 2020). Column (1) of Table A2.7 estimates a 2.9 percentage point (pp) decline in WIC participation (-6.5%), smaller than the 9.7% reduction from administrative data (reported in Table 2.3) but within its confidence interval. This

[58]These figures are calculated using the post-rollout coefficients reported in Table 2.2, Columns (3) and (5), which estimate participation declines of 13.5% and 17.8%, respectively.

[59]Emergency Allotments were introduced nationwide in March 2020 and should be accounted for by date fixed effects. Expiration dates by state are reported in Pukelis (2024) (Figure 3).

attenuation likely reflects the longer participation window in birth certificate data and imprecision in assigning treatment timing, which biases estimates toward zero.

Declines by Participant Type WIC benefit packages are determined primarily by participant type (e.g., pregnant women, postpartum women, infants, and children) rather than adjusted for household income, and there is substantial variation in benefit value across categories. For example, a 2018 report using national data found that, on average, monthly benefits were worth $138.64 for infants and $31.78 for children (USDA 2020).

Table 2.3 presents estimates from administrative data of Eq. 2.1 using the DCDH estimator. The results indicate that children experienced the largest participation decline (-16.9%), followed by women (-10.2%) and infants (-6.9%), with all estimates statistically significant at the 1% level. Among women, postpartum non-breastfeeding participants exhibited the largest decline, although differences across subgroups of women are not statistically significant. These declines suggest a potential relationship between benefit value and participation–if participants weigh the value of their benefits against the costs of participation, we would expect larger declines among those with smaller benefit packages.

Targeting Properties of the Reform Next, we examine heterogeneity in participation effects by socioeconomic status. Table 2.4 presents estimates of participation declines by county poverty level, defined as above or below the median poverty rate (weighted by low-income households). Poorer counties experienced much larger declines, with participation dropping by 17 percentage points in above-median poverty counties compared to 8 percentage points in below-median counties—nearly half as large. This suggests that the reform disproportionately reduced participation in higher-poverty areas, shifting program access away from those with greater financial need.

We also examine heterogeneity by demographic characteristics from birth certificate data. In Table A2.7, participation declines were larger among Black, low-educated, and unmarried mothers. These patterns suggest potential negative targeting effects, as these groups tend to be more socioeconomically disadvantaged, though the differences are generally not statistically significant.

6.2 Mechanisms for Participant Decline

In this section, we evaluate the mechanisms driving the decline in participation. Figure 2.2 shows that participation began declining immediately after the rollout began, with a sharp drop over the first three months. This pattern reflects the staggered issuance of EBT cards: participants received their card after completing their required clinic appointment, meaning roughly one-third transitioned each month. Since WIC participation is recorded for all individuals with active benefit instruments, regardless of redemption, this immediate decline suggests that some pre-reform participants or potential enrollees chose not to attend their scheduled clinic appointments or enroll under the new system. The particularly large decline among children suggests that much of this short-run dropout came from pre-reform enrollees electing not to continue.[60]

Several factors could explain this short-run decline. One possibility is short-term adjustment costs, such as clinic congestion caused by staff training and participant education on new procedures.[61] Another possibility is that some pre-reform participants——particularly those nearing the end of their eligibility, such as households with children——decided that the cost of navigating a new system outweighed the benefits of continued participation.

It is unlikely that participants were unaware of the upcoming transition to retail distribution. The change was widely publicized across the state through news coverage (WLOX News 2020; Jackson Free Press 2020) and social media outreach. The rollout was originally scheduled for 2020 but was postponed due to the COVID-19 pandemic.

However, the long-run stabilization at a lower participation level suggests that costs persisted beyond the transition period. To explore these persistent barriers, Figure A2.8 outlines three stages of WIC participation——initial enrollment, ongoing clinic appointments, and food shopping——along with associated participation barriers at each stage. Arrows indicate whether each barrier increased, decreased, or remained unchanged post-reform, with question marks marking areas of uncertainty.

[60] Initial enrollment in WIC occurs mostly during pregnancy or infancy (Bitler et al. 2023; USDA-FNS 2020).

[61] The increase in clinic ("NSA") costs in Figure 2.3 during the rollout period is generally consistent with this explanation, but it is small ($1.14/participant) and not statistically significant, as reported in Table 2.5.

6.2.1 Initial Enrollment

We consider three potential barriers associated with initial enrollment: lack of information about the program, travel costs, and administrative requirements. While the transition to retail distribution did not alter traditional outreach efforts, it may have influenced program visibility. Increased WIC shopping in grocery stores could have raised visibility, while declining participation may have reduced awareness. However, travel to in-person clinic visits and enrollment requirements——including documentation of income, identity, and eligibility——remained unchanged.

Moreover, the disproportionately larger drop among children——who are generally existing participants——suggests that barriers at initial enrollment are unlikely to drive the participation decline.

6.2.2 3-Month Clinic Appointments

Under retail distribution, WIC clinic requirements—maternal and child health assessments, nutrition education, and referrals—remained unchanged. However, benefit issuance shifted from in-person voucher printing to automatic EBT card loading by a third-party processor.[62] This change may have shortened appointment duration, which would be expected to reduce participation costs and potentially increase, rather than decrease, enrollment.

Despite this, Figure 2.3 shows no long-run change in clinic (NSA) costs, suggesting that the reform had minimal impact on administrative and operational demands at clinics. In Table 2.5, the long-run effect on costs is small ($1.04) and noisy.

6.2.3 Benefit Shopping

Since the transition altered the food distribution model, this is where we expect the most significant changes to occur. Here, we discuss four potential barriers: travel distance, shopping effort, limited food choice, and stigma.

[62]Following each clinic appointment, cards were electronically updated with three months of benefits.

Distance Costs We first examine whether the shift to retail distribution affected travel distance for WIC shopping. To quantify changes, we estimate the distance to the nearest WIC food site—-distribution centers in 2020 or WIC grocery stores in 2022—-for low-income households, using block group centroids as proxies for residential locations. Table A2.8, Column (1) shows that, on average, low-income households experienced a 1.5-mile (29%) reduction in distance, from a baseline of 5.3 miles to the nearest distribution center.[63]

Distance reductions were smaller in high-poverty block groups and rural areas, reflecting the concentration of WIC grocery stores in urban, lower-poverty areas (Figure A2.2). Nevertheless, all area subsets saw improvements: distance declined by 2.3 miles in below-median poverty areas, 1.4 miles in above-median poverty areas, 1.8 miles in urban areas, and 1.3 miles in rural areas.[64] These estimates suggest that travel distance is unlikely to explain the decline in WIC participation following the transition to retail distribution.

However, distance-based measures do not account for trip frequency or the relative locations of WIC food sites and clinics, both of which shape overall travel burdens. While retail distribution allows participants to combine WIC shopping with routine grocery trips, some distribution centers were located near WIC clinics, potentially allowing for combined trips in the pre-reform period. To better assess these trade-offs, we estimate total travel distances over a three-month period, reflecting WIC's quarterly clinic visit requirement. The results, described in Section A2.2, show that total travel distances declined by approximately 50% under retail distribution. This reduction was driven by the increased number of WIC-authorized stores, the ability to consolidate WIC shopping with regular grocery trips, and the fact that clinic and distribution center co-location was rare.[65]

Shopping Effort and Stigma

One thing we heard loud and clear is that shopping with WIC is difficult, time-consuming,

[63] See table footnotes for additional details on the data and methodology.

[64] Similarly, Table A2.9 examines zip codes, which adjust in size based on population density. The likelihood of having a WIC food site in one's zip code increased by 31 percentage points (a 69% increase).

[65] Only 2 of 87 distribution centers shared an address with the nearest WIC clinic, and just 5 were within 250 feet.

and embarrassing. The WIC program dictates precise items, sizes, and brands that make the shopping experience into something like a treasure hunt, which can be both time-consuming and stigmatizing.

— From *In Their Own Words: Parents Help Us Understand Barriers to Accessing WIC* (Code for America 2019)

Mississippi WIC imposes over 100 purchasing restrictions, requiring participants to navigate a complex set of rules on eligible foods, brands, sizes, and ingredients.[66] Unlike SNAP, which allows most grocery items, WIC limits choices to a specific, often-changing list. Participants must adhere to strict guidelines——pasta must be 100% whole wheat and exactly 16 oz.; milk brands vary by store; juice sizes depend on the child's age. Foods are often allocated by weight, requiring participants to select exact combinations to stay within benefit limits. Minor mistakes——such as choosing a 20 oz. jar of peanut butter instead of the allowed 16–18 oz.——can lead to checkout rejection. These complexities pose significant cognitive and logistical burdens, particularly in a state with low literacy and numeracy rates (Leone et al. 2022; NCES 2023).

Importantly, these challenges persist even for experienced participants. Frequent updates to the Approved Product List (APL), limited shelf labeling, and variation across stores require ongoing adaptation (Barnes et al. 2023). Participants rely on a barcode-scanning smartphone app to confirm item eligibility,[67] but checkout rejections still occur when store inventory or systems are not properly synced with the state-maintained list.[68] Moreover, restricted product sizes and formulations often lead to stockouts, requiring participants to visit multiple stores or make repeated trips. These issues intensify as household needs shift——for example, when children turn one and move from formula to standard WIC foods, triggering a new set of restrictions that even mothers who participated in WIC during pregnancy may not have encountered before.

[66] See the Vendor Food Guide for MS: https://msdh.ms.gov/page/resources/11709.pdf. These rules are similar to those used in other state WIC programs.

[67] The app allows users to scan barcodes to verify eligibility against the APL.

[68] See participant reports on Reddit: https://www.reddit.com/r/WIC/comments/1eoup58/foods_werent_covered_by_the_store/.

Indeed, surveys and academic research reveals that WIC participants frequently cite grocery shopping as one of the most burdensome aspects of the program (Leone et al. 2022; Barnes et al. 2023). The 2023 Multi-State WIC Participant Satisfaction Survey found that nearly all participants encountered difficulties, with confusion over eligible items, stockouts, and checkout errors being the most common complaints (USDA 2023). Similarly, a large Texas survey identified WIC shopping as the most challenging aspect of participation, ranking it ahead of customer service and clinic wait times (WIC 2017). In Texas, 62% of participants reported being sent back at checkout due to unauthorized item selection, while 54% encountered stockouts. Checkout rejections and item shortages may also contribute to stigma, with 20% of Texas respondents reporting discomfort at checkout.

To evaluate how retail shopping compares to the experience at WIC distribution centers, we draw on administrative redemption data, official reports, and online reviews. First, we analyze monthly, statewide redemption data from before and after the reform. Differences in the granularity of redemption records across periods (pre vs. post-reform) require that we focus on a subset of food types with consistently interpretable units over time: milk, infant fruits/vegetables/meat, bread, infant cereal, and fish. We estimate redemption shares as the number of units redeemed divided by the maximum monthly package issued per participant. Because some households receive smaller or partial packages (e.g., mothers without food benefits), this measure likely understates true redemption rates——but the bias should be stable over time and not affect comparisons across periods. We compare monthly average redemption shares across two periods: January 2019–September 2019 (pre-reform) to July 2021–June 2022 (post-reform).[69]

If shopping effort (a form of search cost) is substantial under retail distribution, we expect it to reduce redemption rates by lowering the value of benefits conditional on participation. This contrasts with fixed access costs, such as travel time, which should affect extensive margin participation. Indeed, Figure 2.4 shows large drops in redemption shares across all categories, with

[69] These two time periods are defined by data limitations in other months. We truncate the data in mid-2022 to avoid the effects of the formula shortage. However, our results remain nearly unchanged even when we extend the post-reform data up through December 2022, instead of June 2022. See Section A2.4 for details.

several falling by more than 50%. Bread, is particularly challenging to find due to its non-standard 20 oz. size (McLaughlin 2020), shows the largest decline. These declines are too large to be explained by participation losses alone.[70]

Some frictions, like incomplete signage or stocking, may have improved over time as grocers and WIC administrators adjusted to the new regime. If redemption rates respond to these frictions, we might expect gradual recovery after the initial transition. Indeed, Figure A2.6 shows a modest increase in average food costs (inflation-adjusted) in 2023 and 2024, despite flat participation (Figure 2.1). Since participation and prices are held constant, this suggests a partial rebound in redemption rates, though not large enough to reverse the initial sharp declines.

We also use redemption data to proxy for ongoing learning costs in retail settings. Figure A2.10 plots monthly counts of (1) "new" WIC products——defined as Universal Product Codes (UPCs) redeemed for the first time——and (2) "abandoned" products——UPCs never redeemed again.[71] Each month there are between 10 and 50 new or abandoned UPCs. This turnover highlights the dynamic nature of WIC-eligible products and suggests that participants must continually update their knowledge of which items qualify, adding cognitive burden even for long-time users.

We complement this analysis with qualitative evidence on the in-store experience at distribution centers. Figure A2.11 shows photos of a center in Hinds County.[72] These images highlight clear signage, structured product groupings, and multilingual labels, which likely reduced cognitive burden compared to grocery stores. However, they also reveal potential trade-offs, including limited product variety and restricted operating hours.

An MSDH-commissioned report on barriers to WIC participation further supports these observations (MPHI 2023). Site visits to seven distribution centers described efficient, structured

[70]For example, redemption of infant foods——which start from a lower base of about 60%——falls by 40 percentage points to around 20%. Our largest estimate of the post-rollout participation decline is approximately 20% (Table 2.2, column 5). Even under the extreme assumption that those who exited WIC had previously redeemed 100% of their benefits, the remaining 80% of participants would have to redeem just 50% for the combined rate to fall to 60%——still far above the roughly 20% observed in the post period.

[71]We require that we observe at least 2 months of non-redemption before a product can be considered "new" and 2 months of non-redemption after a product is considered "abandoned". We do not observe the reasons for these changes, which may be related to changes in WIC eligibility, grocer stock, or manufacturer specifications.

[72]Additional images: https://uconnmsobesity.weebly.com/jackson-medical-mall.html.

shopping experiences. In five of the seven centers, shopping was noted to take 15 minutes or less, with staff actively assisting participants in selecting items and ensuring compliance with food package rules. Some centers also provided additional services, such as carrying groceries to participants' cars or offering play areas for children.

Google reviews of WIC distribution centers in Mississippi provide another view of participant experiences.[73] On average, WIC distribution centers received a rating of 4.1 out of 5.0 across 754 reviews, comparing favorably to other social service providers in Mississippi (Table A2.13). Figure 2.5A presents a word cloud of text-based reviews, where "helpful," "friendly," "people," and "nice" appear most frequently. Sentiment analysis in Figure 2.5B and Table A2.15 confirms that positive reviews dominate, with only 14.7% of reviews classified as negative.

Three reviews, written after the transition, explicitly compare distribution centers to retail WIC shopping:

> *I miss picking up my WIC [at distribution centers]. Grocery stores are not as easy. WIC is very limited on items at the stores.* (Melissa 2023)

> *The WIC benefits are difficult now, and the little 12 oz they pay for is always unavailable in every store I come to. I hate the program changed because it's no good.* (Tabb 2023)

> *I miss the location being open & the helpful lady that worked there. This location is now closed.* (Butler 2022)

This collection of quantitative and qualitative evidence suggests that increased shopping burden likely contributed to the WIC participation decline. Distribution centers provided a structured, simplified, and supportive environment, while retail shopping likely increased cognitive and logistical burdens, as well as stigma.

[73] We collected reviews of 84 of the 96 WIC distribution centers, supplemented with reviews from social service locations compiled by Li, Shang, and McAuley (2022). Most reviews were written before 2022 and appear to reflect participant experiences. Section A2.3 details methodology and sample statistics.

6.3 Costs

This section examines the impact of the transition to retail distribution on WIC program costs using the state-level difference-in-differences (DiD) framework specified in Eq. 2.2. Event-study estimates for total program costs per participant are presented in Figure 2.3, Panel A, with the rollout period shaded in gray. The results show a temporary increase in costs during the rollout, followed by a sustained decrease in the post-rollout period. While the pre-period estimates exhibit some noise, there is no evidence of differential trends before the transition.

Panel B of Figure 2.3 breaks down the estimates by cost category, showing that the decline in total costs is driven by a decline in the costs of food procurement and distribution ("Food Costs"), which account for 73 percent of total program costs. The costs of clinic operations ("NSA Costs"), which make up the remaining 27 percent, remain relatively stable in the post-rollout period but increase slightly during the rollout, likely reflecting short-term adjustment costs in clinics, such as the purchase and installation of EBT card readers.

Regression estimates for total program costs per participant are reported in Table 2.5, Columns (1)–(2). The transition leads to a decrease of \$5.8 per participant, representing a 6.5 percent reduction relative to the pre-rollout cost in MS, though this estimate is imprecise. Column (2) disaggregates the effects into the rollout and post-rollout periods, finding a temporary increase during the rollout of \$47.0 per participant, followed by a significant decline of \$28.9 per participant in the post-rollout period, both statistically significant at the 1 percent level. The long-run decline represents a 32 percent reduction relative to the pre-rollout mean.

Table 2.5, Columns (3)–(6), provides separate estimates for Food and NSA Costs. As seen in the figures, the changes in per-participant costs are fully explained by changes in Food Costs. The long-run decline of \$30.0 per participant reflects a 44 percent reduction in Food Costs. In contrast, the effects on NSA Costs during the rollout and post-rollout periods are small and imprecise, with an estimated increase of approximately \$1 per participant in both periods.

As noted in the participation results, lower-benefit-value participants were more likely to drop out. This suggests that the changes in costs could reflect compositional as well as causal effects (with higher-value packages over-represented, partially offsetting direct savings). A back-of-the-envelope calculation estimates composition effects increased costs by just 0.6%, confirming minimal impact on savings.[74]

To determine the source of the decline in Food Costs, we distinguish between two components: (1) the direct cost of food benefits and (2) the operational costs of warehousing and distribution under the direct distribution system. To isolate the impact on food benefit costs, we use a constructed measure incorporating direct distribution food contracts from Transparency Mississippi (as described in Section 4.2). Event-study estimates in Figure A2.12 show a temporary increase in per-participant food benefit costs during the rollout, followed by a return to baseline. Table A2.10 confirms that the long-run increase is just $1 per participant and statistically insignificant, indicating that the overall cost decline in Table 2.5 was driven by reductions in warehousing and distribution costs, such as leases, staff, and storage.

The stability of per-participant food benefit costs post-transition is notable given structural differences between the systems. Competitive bidding under direct distribution may have lowered food benefit costs, while market competition among retailers could have had a similar effect under retail distribution. Additionally, retail distribution offers more choice and allows benefits to be redeemed across multiple shopping trips throughout the month. However, these factors appear to have had minimal impact or offset each other.

In sum, the transition substantially reduced per-participant costs, primarily by eliminating warehousing and distribution expenses, with little evidence that participant composition, clinic operations, or food benefit costs contributed to the overall decline.

[74]We use estimates of food package costs per participant type from USDA (2020, ("post-rebate" costs from Table 4.1)). We multiply these cost estimates by pre-rollout shares of participant types from Table 2.3, generating an average per-participant cost of $36.21. We then adjust participant shares using our estimated participation declines by participant type (Table 2.3) and recalculate the weighted average cost. This generates an adjusted per-participant cost of $36.44, implying that compositional shifts increased per-participant costs by only 0.6%. As a result, the overall cost decline, after adjusting for compositional changes, is 45.0% instead of 44.4%.

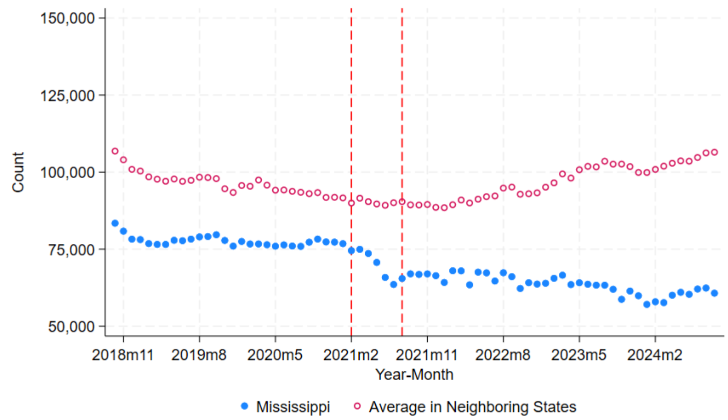
7 Conclusion

This paper provides new evidence on how distribution mechanisms affect participation, targeting, and costs in a large federal nutrition program. We examine the transition of Mississippi's WIC program from a state-run distribution system to a retail-based model. Using a difference-in-differences approach that exploits the staggered rollout across county groups and never-treated neighboring states, we find that this shift significantly reduced participation, particularly among children. Examining the reform's targeting effects, we find that it discouraged take-up among individuals with lower benefit amounts but also among historically disadvantaged groups. We then document a substantial decrease in per-participant program costs, achieved entirely through the elimination of state-run warehouses, rather than changes in costs of administering the program or food benefits themselves.

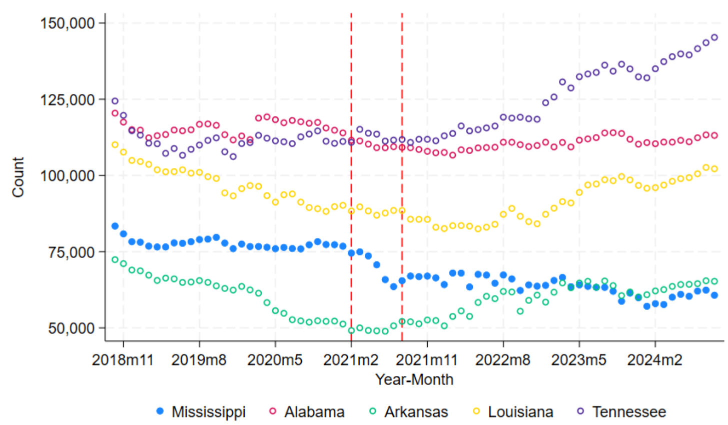
The participation decline was unlikely to be driven by information frictions, administrative hurdles, or travel burdens, as clinic procedures remained unchanged and shopping distances decreased on average. We hypothesize that shopping-related burdens——such as difficulty locating WIC-approved products, checkout errors, and stockouts——were key drivers of the decline. Qualitative evidence from online reviews and state reports provides support for this hypothesis, highlighting the structured and supportive nature of WIC distribution centers in contrast to the more complex and independent shopping environment at grocery stores.

These findings contribute to broader debates on the privatization of public service delivery and the role of transaction costs in shaping access to anti-poverty programs. While retail distribution reduced program costs and expanded the number of food sites, it also introduced new participation barriers and reduced access among disadvantaged groups. Our results highlight the importance of distribution systems as a core component of program design, with direct implications for participation, targeting, and cost-effectiveness.

Figures



(a) MS vs. Neighboring States



(b) By State

Figure 2.1: Time-Series Graphs for WIC Participation

Notes: Figure 2.1 displays time series graphs of WIC participation in Mississippi (MS) and its four neighboring states (i.e., Alabama, Arkansas, Louisiana, and Tennessee). The data are from the USDA, covering the period from October 2018 to September 2024. The numbers on the x-axis correspond to calendar years and months. In both figures, the blue dots represent the number of participants in Mississippi. In Figure 2.1A, the red dots represent the average number of participants in the four neighboring states. In Figure 2.1B, the dots in different colors represent the number of participants in each of the four neighboring states, respectively. The two vertical red dashed lines in each figure indicate February 2021 and August 2021, denoting the rollout period for the transition from direct distribution to retail distribution.

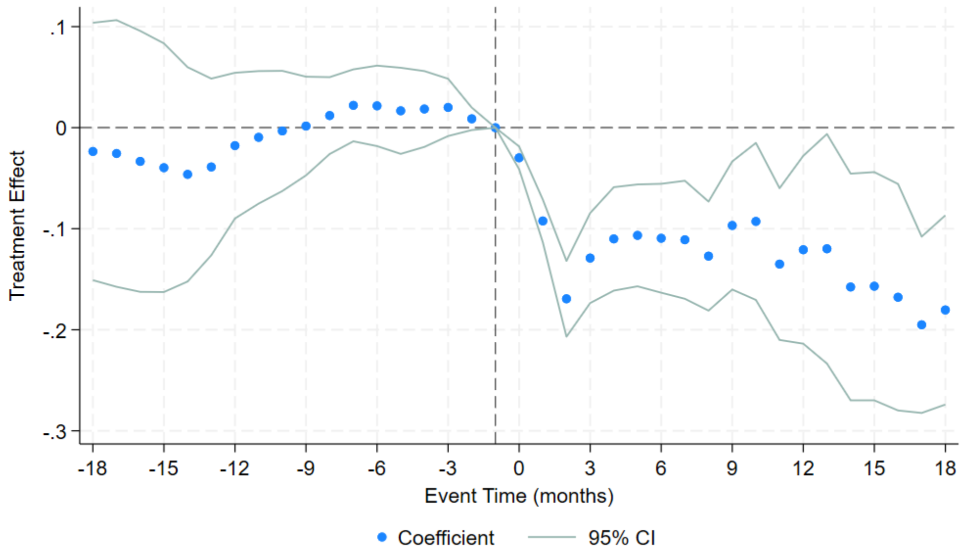
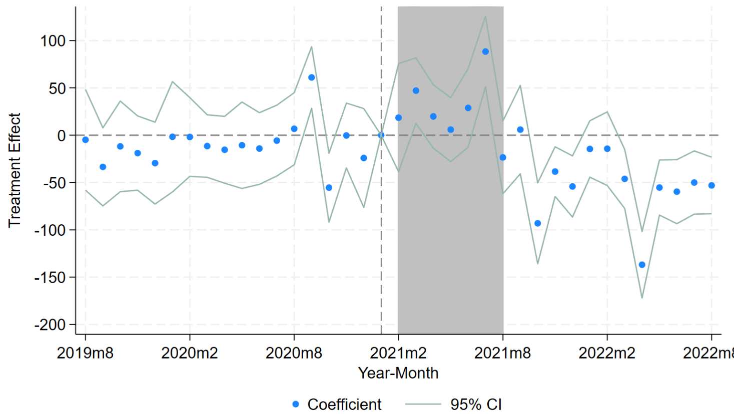
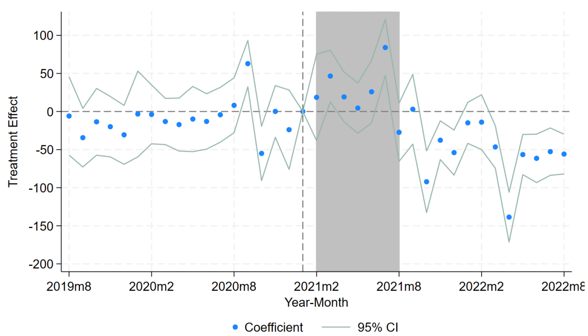


Figure 2.2: Effects of Transition to Retail Distribution on WIC Participation

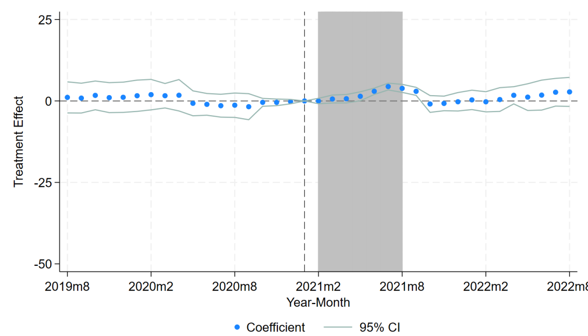
Notes: Figure 2.2 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation using the event study framework of the DCDH estimator based on Eq. 2.1. The outcome is an inverse hyperbolic sine of total WIC participants. The data are from Freedom of Information Act (FOIA) requests for MS and the USDA for the four neighboring states, covering the period from January 2019 to September 2024. The numbers on the x-axis correspond to months past the transition of the distribution system (negative numbers for pre-intervention placebo effects). Standard errors are clustered at the county (for MS) and state level (for neighboring states). The 95% confidence intervals are shown in light green lines. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).



(a) WIC Program Costs (per participant)



(b)-1. Food Procurement and Distribution (73%)

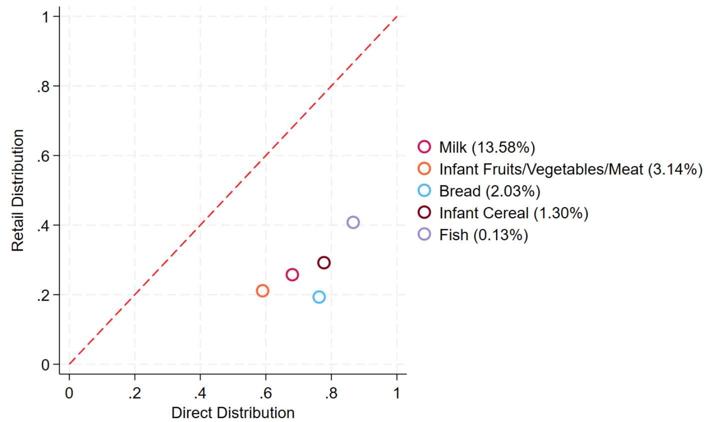


(b)-2. Clinic Operations (27%)

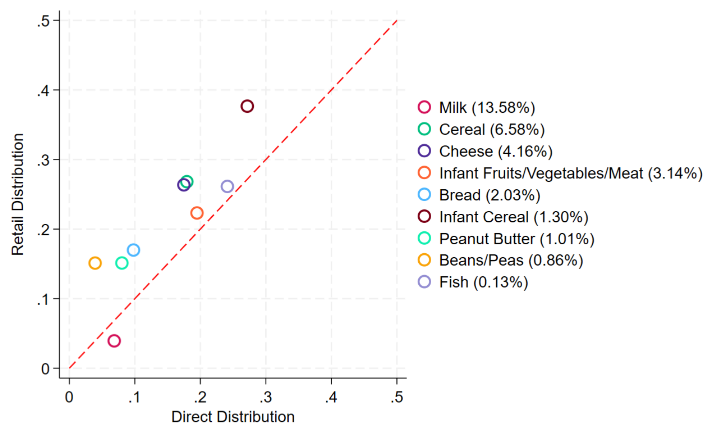
(b) WIC Program Costs by Category (per participant)

Figure 2.3: Effects of Transition to Retail Distribution on WIC Program Costs

Notes: Figure 2.3 presents the estimation results of the impact of the transition from direct distribution to retail distribution in Mississippi on WIC program costs (in 2023 dollars), using the event study framework of the DCDH estimator based on Eq. 2.2. Figure 2.3A shows the results for per-participant total WIC program costs. Figure 2.3B shows the results for per-participant WIC program costs by USDA category: costs for food procurement and distribution (i.e., Food Costs) and clinic operations (i.e., Nutrition Services and Administration, or NSA, Costs). The data are from the USDA, covering the period from October 2018 to September 2023. The numbers on the x-axis correspond to calendar years and months. The gray shade in each figure covers February 2021 to July 2021, denoting the rollout period for the transition from direct distribution to retail distribution. Standard errors are clustered at the state-date level. The 95% confidence intervals are shown in light green lines. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).



(a) Redemption Share

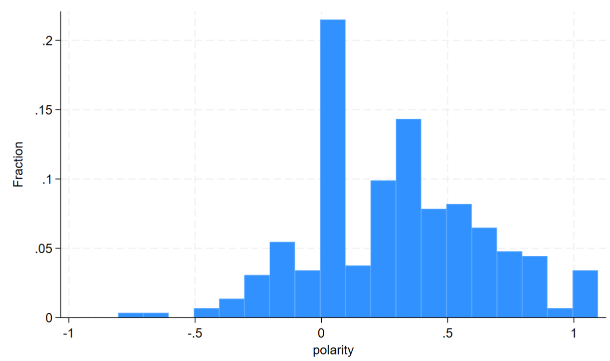


(b) Average Price per Oz.

Figure 2.4: Comparing Direct and Retail Distribution: Redemption Share and Prices

Notes: Figure 2.4 compares food redemption measures in Mississippi WIC during the pre-reform period (January 2019 to September 2019; x-axis) and the post-reform period (July 2021 to June 2022; y-axis). The subfigures display snapshots of the share of food redeemed out of food issued (Figure 2.4A) and average price per unit (i.e., ounce) (in 2023 dollars) (Figure 2.4B). In all figures, each dot represents a different food category, and the red line indicates the 45-degree line. The percentage next to each food category in the legend indicates the post-rebate redemption share for each food category from July 2021 to June 2022. The data are from Freedom of Information Act (FOIA) requests, direct distribution food contracts of Transparency Mississippi, and the USDA, covering the corresponding reference periods. Detailed calculations are provided in the Section A2.4.

(a) Word Cloud



(b) Polarity Score from Sentiment Analysis

Figure 2.5: Google Reviews of WIC Distribution Centers

Notes: Figure 2.5 presents analyses of user-generated ratings and written feedback from Google reviews of WIC distribution centers in Mississippi. The dataset includes reviews for 84 of 96 total centers, all of which had at least one review as of June 3, 2024. Of 754 total reviews, 293 contain text, while 461 are numeric star ratings on a 1-to-5 scale. Additional details on data collection are in Section A2.3. Figure 2.5A shows a word cloud of the 293 text-based reviews, with word size reflecting relative frequency. Common function words—including pronouns, articles, prepositions, conjunctions, and auxiliary verbs—are removed for clarity. Figure 2.5B presents the distribution of polarity scores from sentiment analysis using TextBlob, a Python-based natural language processing tool. Scores range from -1 to 1, where positive values indicate positive sentiment, negative values indicate negative sentiment, and zero indicates a neutral statement.

Tables

Table 2.1: Descriptive Statistics: Birth Certificate

	(1) All	(2) Mississippi
A. Demographics of WIC Births		
Age	27.192	25.673
White	0.673	0.386
Black	0.240	0.587
Other Race	0.055	0.018
No High School Diploma	0.213	0.161
High School Diploma	0.408	0.407
Some College or Above	0.365	0.432
Married	0.292	0.244
Observations	7,008,136	90,271
B. WIC Participation, All Births		
WIC	0.323	0.426
Observations	21,698,419	211,781

Notes: Table 2.1 displays average characteristics for all births from the restricted-use birth certificate data, covering the period from 2018 to 2023. Averages are shown separately for two groups: all births (column (1)) and births in Mississippi (column (2)). In Panel B, WIC participation is defined as having received WIC foods at least once during pregnancy.

Table 2.2: Effects of Transition to Retail Distribution on WIC Participation by Method

	DCDH	TWFE		DiD	
	(1)	(2)	(3)	(4)	(5)
Post	-0.127***	-0.121***		-0.139***	
	(0.033)	(0.031)		(0.023)	
During Rollout			-0.110***		-0.013
			(0.025)		(0.029)
After Rollout			-0.135***		-0.178***
			(0.042)		(0.022)
Geographic Unit	county	county		state	
Pre-Period Mean (MS)	964.059	964.059		77,664.5	
Observations	5,788	5,788		360	

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 2.2 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation using different econometric techniques. The outcomes are the inverse hyperbolic sine of the number of participants. Column (1) shows the DiD estimate using the DCDH estimator based on Eq. 2.1. Columns (2) and (3) show the DiD estimate using the TWFE estimator based on Eq. 2.1. Columns (4) and (5) show the DiD estimate using the DiD estimator based on Eq. 2.2. Columns (3) and (5) show the DiD estimates, separately for the rollout period and the post-rollout period. The data are from the Freedom of Information Act (FOIA) requests for MS (columns (1)-(3)), covering the period from January 2019 to September 2024, and the USDA for the four neighboring states (also for MS in columns (4) and (5)), covering the period from October 2018 to September 2024. Standard errors are clustered at the county (for MS) or state level (for neighboring states). State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table 2.3: Effects of Transition to Retail Distribution on WIC Participation by Type

				Type of Women Participants		
	(1)	(2)	(3)	(4)	(5)	(6)
Type	Women	Infants	Children	Pregnant	Breastfeeding	Postpartum
Post	-0.102***	-0.069***	-0.169***	-0.097***	-0.098**	-0.104***
	(0.026)	(0.025)	(0.046)	(0.035)	(0.040)	(0.037)
Pre-Period Mean (per MS county)	212.651	279.444	464.001	79.496	38.094	95.062
Observations	5,788	5,788	5,788	5,788	5,788	5,788

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 2.3 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation by type, using the DCDH estimator based on Eq. 2.1. The outcomes are the inverse hyperbolic sine of the number of participants. Each column reports the DiD estimates for each type of WIC participant. The data are from the Freedom of Information Act (FOIA) requests for MS and the USDA for the four neighboring states, covering the period from January 2019 to September 2024. Standard errors are clustered at the county (for MS) or state level (for neighboring states). State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table 2.4: Effects of the Transition to Retail Distribution on WIC Participation by Poverty

		Poverty Share	
	(1)	(2)	(3)
	Total	Above Median	Below Median
Post	-0.127***	-0.174***	-0.080**
	(0.033)	(0.041)	(0.034)
Pre-Period Mean (MS)	964.059	756.347	1171.772
Observations	5,788	3,028	3,036

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 2.4 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation by poverty level, using the DCDH estimator based on Eq. 2.1. The outcomes are the inverse hyperbolic sine of the number of participants. Columns (2) and (3) show the DiD estimates for different subsamples on poverty share: MS counties where the share of households below the poverty line is above the median and below the median. The data are from the Freedom of Information Act (FOIA) requests for MS and the USDA for the four neighboring states, covering the period from January 2019 to September 2024. Standard errors are clustered at the county (for MS) or state level (for neighboring states). State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table 2.5: Effects of Transition to Retail Distribution on WIC Program Costs

			Category of Program Costs			
	Total		Food Procurement and Distribution (73%)		Clinic Operations (27%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-5.810		-6.878		1.069**	
	(9.578)		(9.499)		(0.485)	
During Rollout		47.019***		45.877***		1.142
		(11.094)		(10.725)		(0.810)
After Rollout		-28.929***		-29.966***		1.037*
		(7.772)		(7.704)		(0.540)
Pre-Period Mean (MS)	89.536		67.424		22.112	
Observations	300		300		300	

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 2.5 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi on WIC program costs (in 2023 dollars) using the DCDH estimator based on Eq. 2.2. Columns (1) and (2) shows the DiD estimate on per-participant total WIC program costs. Columns (3)-(6) show the estimation results for per-participant WIC program costs by category: costs for food procurement and distribution (i.e., Food Costs) (columns 3 and 4) and clinic operations (i.e., Nutrition Services and Administration, or NSA, Costs) (columns 5 and 6). Columns (2), (4), and (6) show the DiD estimates, separately for the rollout period and the post-rollout period. The data are from the USDA, covering the period from October 2018 to September 2023. Standard errors are clustered at the state-date level. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Appendix

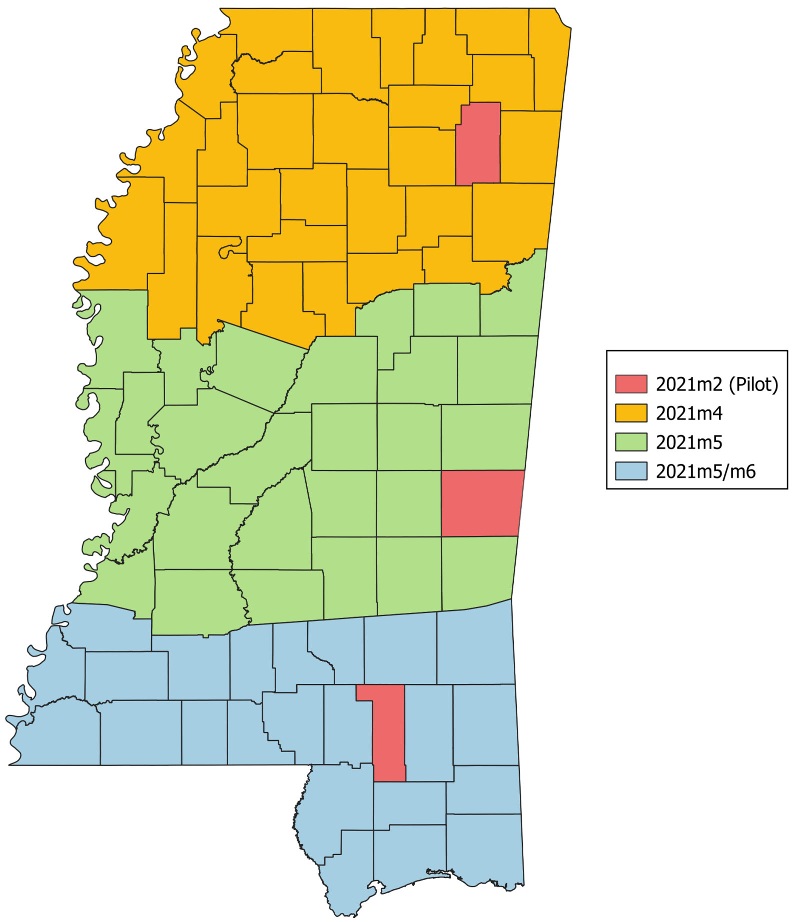
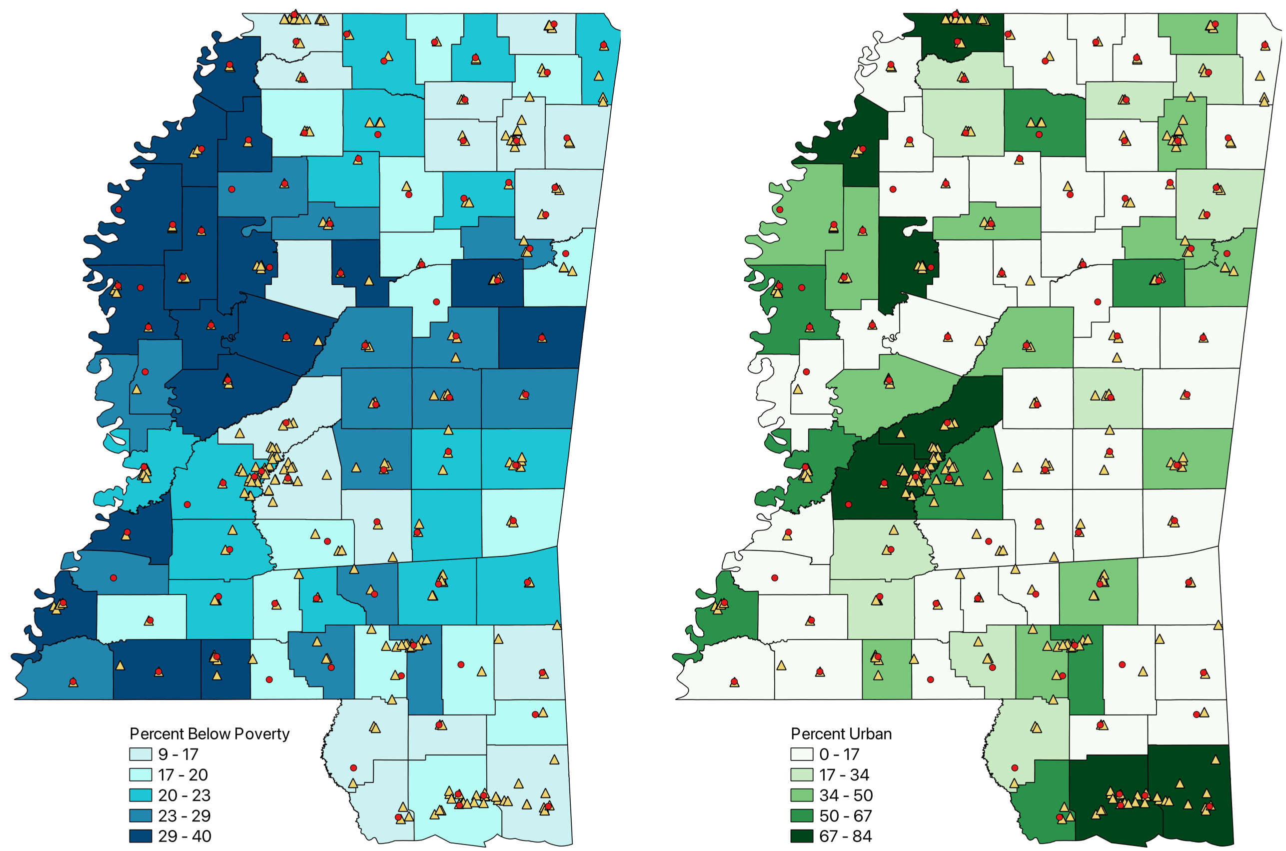


Figure A2.1: Rollout Map of Transition to Retail Distribution

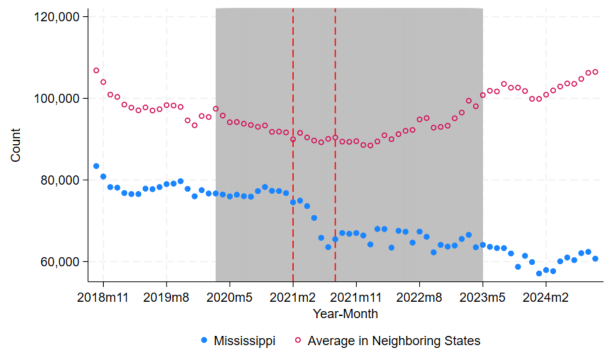
Notes: Figure A2.1 displays a map of Mississippi, illustrating the timing of the transition of WIC distribution system to retail distribution from direct distribution. Each color represents a group of counties based on the month when each county began issuing EBT cards to a third of WIC participants over a three-month period, following the rollout schedule in Table A2.3: Wave 1 (Feb 2021), Wave 2 (Apr 2021), Wave 3 (May 2021), and Wave 4 (May/Jun 2021).
Source: MSDH Vendor management transition plan provided by Food and Nutrition Service.



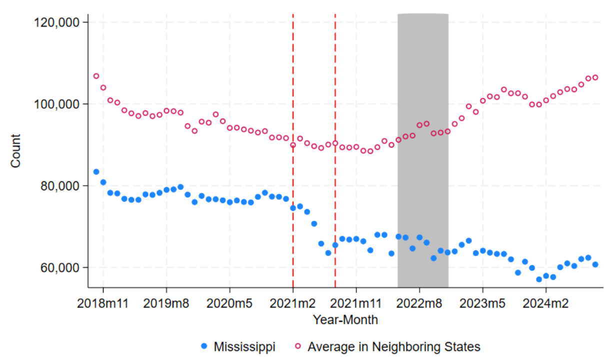
(a) (b)

Figure A2.2: Locations of WIC Distribution Centers and Grocery Stores

Notes: Figure A2.2 displays the geographical distribution of WIC warehouses (red circles) that were active in fiscal year (FY) 2020 and WIC-authorized vendors (yellow triangles) active in FY 2022, illustrating the transition to retail distribution. Map (Figure A2.2A) overlays county-level poverty rates (shown in blue shading), while map (Figure A2.2B) shows the percentage of urban households by county (shown in green shading). We use the coordinates of WIC distribution centers and WIC-authorized stores from the Freedom of Information Act (FOIA) requests.



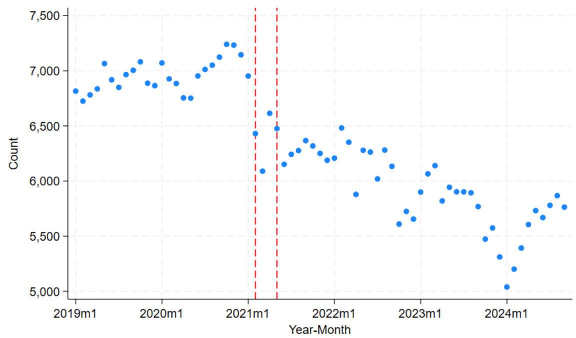
(a) COVID-19 Pandemic



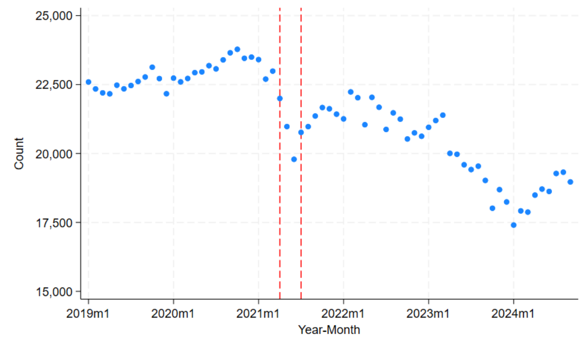
(b) Infant Formula Shortage

Figure A2.3: Time-Series Graphs for WIC Participation with Confounders

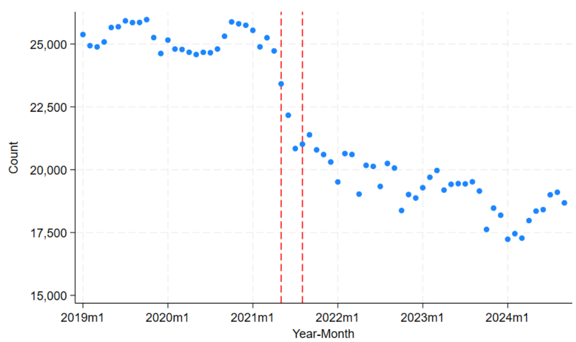
Notes: Figure A2.3 displays time series graphs of WIC participation in Mississippi and its four neighboring states (i.e., Alabama, Arkansas, Louisiana, and Tennessee), accounting for potential confounders: COVID-19 and the infant formula shortage. Each figure shows the trend for WIC participants with specific elements: the gray-shaded period representing the COVID-19 pandemic from March 2020 to May 2023 (Figure A2.3A) and the formula shortage beginning in May 2022 (Figure A2.3B). The data are from the USDA, covering the period from October 2018 to September 2024. The numbers on the x-axis correspond to calendar years and months. In all figures, the blue dots represent the number of participants in Mississippi, while the red dots represent the average number of participants in the four neighboring states. The two vertical red dashed lines in each figure indicate February 2021 and August 2021, denoting the rollout period for the transition from direct distribution to retail distribution.



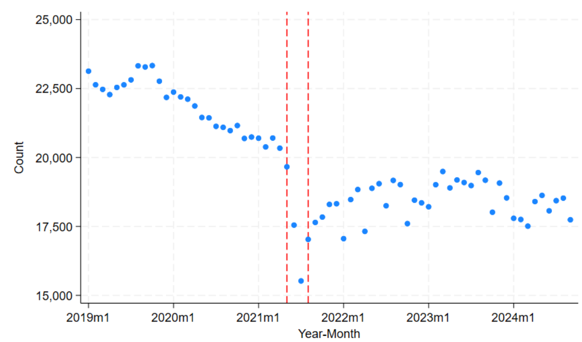
(a) Wave 1 (Feb 2021)



(b) Wave 2 (Apr 2021)



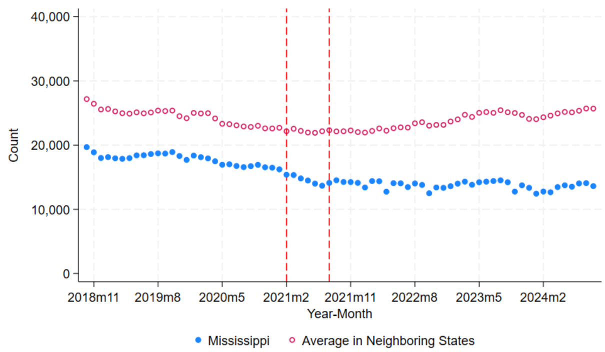
(c) Wave 3 (May 2021)



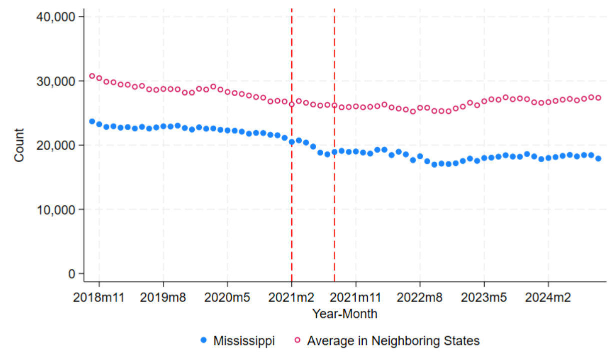
(d) Wave 4 (May/Jun 2021)

Figure A2.4: Time-Series Graphs for WIC Participation by Timing of Transition

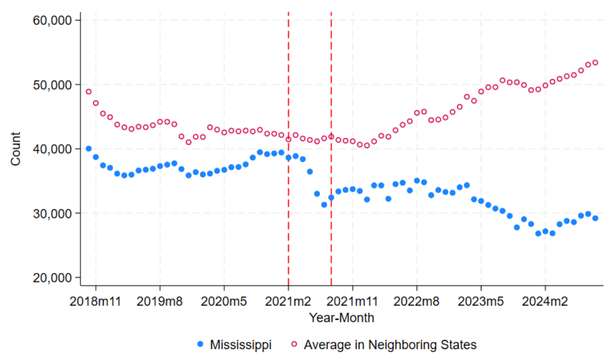
Notes: Figure A2.4 displays time series graphs of WIC participation in Mississippi. Each figure shows the trend for total WIC participants in the counties for each wave, according to the rollout schedule in Table A2.3: Wave 1 (Feb 2021), Wave 2 (Apr 2021), Wave 3 (May 2021), and Wave 4 (May/Jun 2021). The data are from the Freedom of Information Act (FOIA) requests, covering the period from January 2019 to September 2024. The numbers on the x-axis correspond to calendar years and months. The two vertical red dashed lines in each figure denote the three-month rollout period for the transition from direct distribution to retail distribution for each wave.



(a) Women (22%)



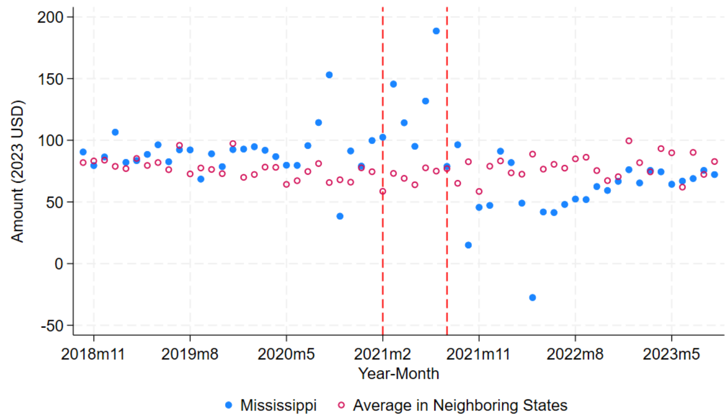
(b) Infants (29%)



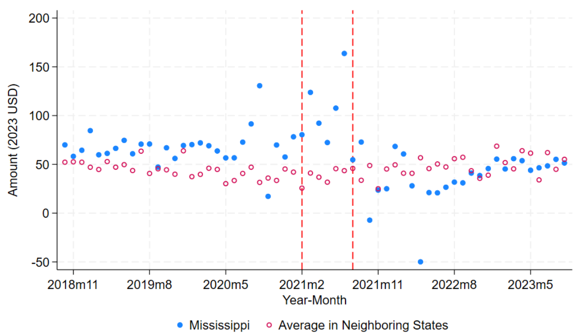
(c) Children (49%)

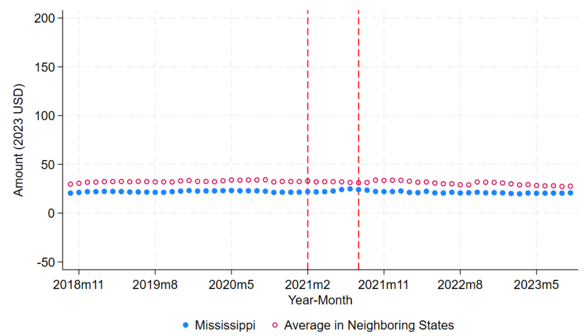
Figure A2.5: Time-Series Graphs for WIC Participation by Type

Notes: Figure A2.5 displays time series graphs of WIC participation in Mississippi and its four neighboring states (i.e., Alabama, Arkansas, Louisiana, and Tennessee). Each figure shows the trend for each type of WIC participants: Women (Figure A2.5A), Infants (Figure A2.5B), and Children (Figure A2.5C). The data are from the USDA, covering the period from October 2018 to September 2024. The numbers on the x-axis correspond to calendar years and months. In all figures, the blue dots represent the number of participants in Mississippi, while the red dots represent the average number of participants in the four neighboring states. The two vertical red dashed lines in each figure indicate February 2021 and August 2021, denoting the rollout period for the transition from direct distribution to retail distribution.



(a) WIC Program Costs (per participant)





(b)-1. Food Procurement and Distribution (73%) (b)-2. Clinic Operations (27%)

(b) WIC Program Costs by Category (per participant)

Figure A2.6: Time-Series Graphs for WIC Program Costs

Notes: Figure A2.6 displays time series graphs of WIC program costs (in 2023 dollars) in Mississippi and its four neighboring states (i.e., Alabama, Arkansas, Louisiana, and Tennessee). Figure A2.6A shows the trend for per-participant total WIC program costs. Figure A2.6B shows the trend for per-participant WIC program costs by category: costs for food procurement and distribution (i.e., Food Costs) and clinic operations (i.e., Nutrition Services and Administration, or NSA, Costs). The data are from the USDA, covering the period from October 2018 to September 2023. The numbers on the x-axis correspond to calendar years and months. In all figures, the blue dots represent the average costs per participant in Mississippi, while the red dots represent the average costs per participant in four neighboring states. The two vertical red dashed lines in each figure indicate February 2021 and August 2021, denoting the rollout period for the transition from direct distribution to retail distribution.

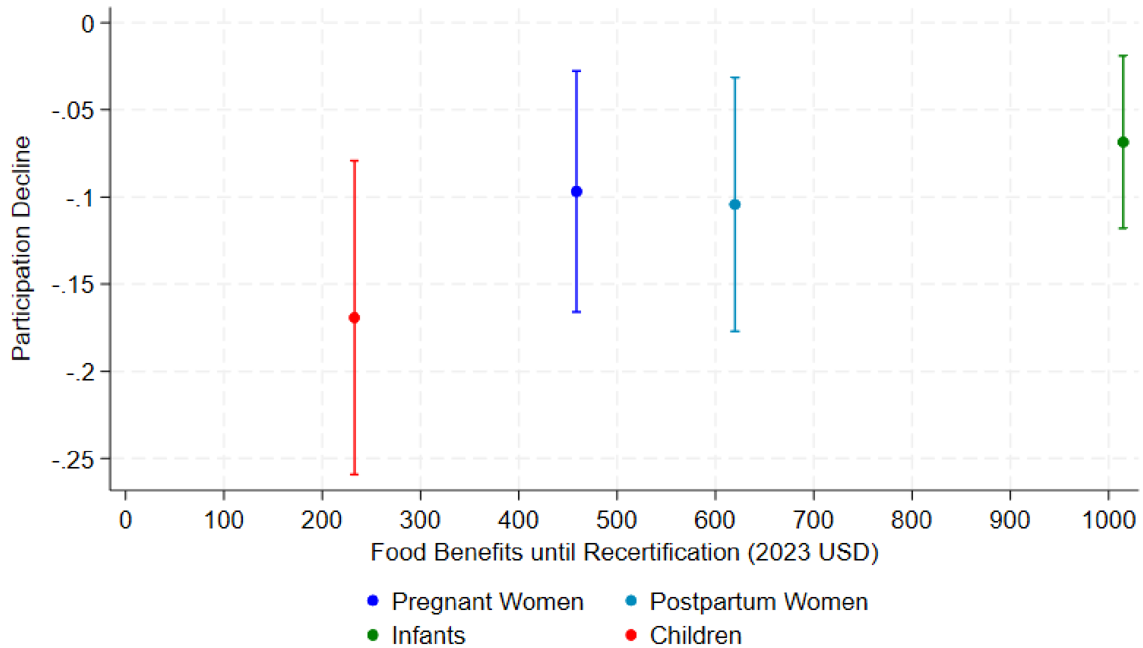


Figure A2.7: Relation between the Decline in Participation and Average Food Package Cost

Notes: Figure A2.7 presents the relation between the impact of the transition from direct distribution to retail distribution in Mississippi on average food package costs (in 2023 dollars) and WIC participation by participant type. The x-axis shows a per-participant WIC pre-rebate food cost in FY 2018 by participant type, which is adjusted for inflation to 2023 dollars. The food cost is calculated as the sum of benefits until the participant's next recertification appointment. The y-axis shows the magnitude of the decline in participation by type using the DCDH estimator based on Eq. 2.1. The outcome is an inverse hyperbolic sine of WIC participants by type. The data are from Freedom of Information Act (FOIA) requests for MS and the USDA for the four neighboring states, covering the period from January 2019 to September 2024. Standard errors are clustered at the state level. The 95% confidence intervals are shown as vertical lines. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

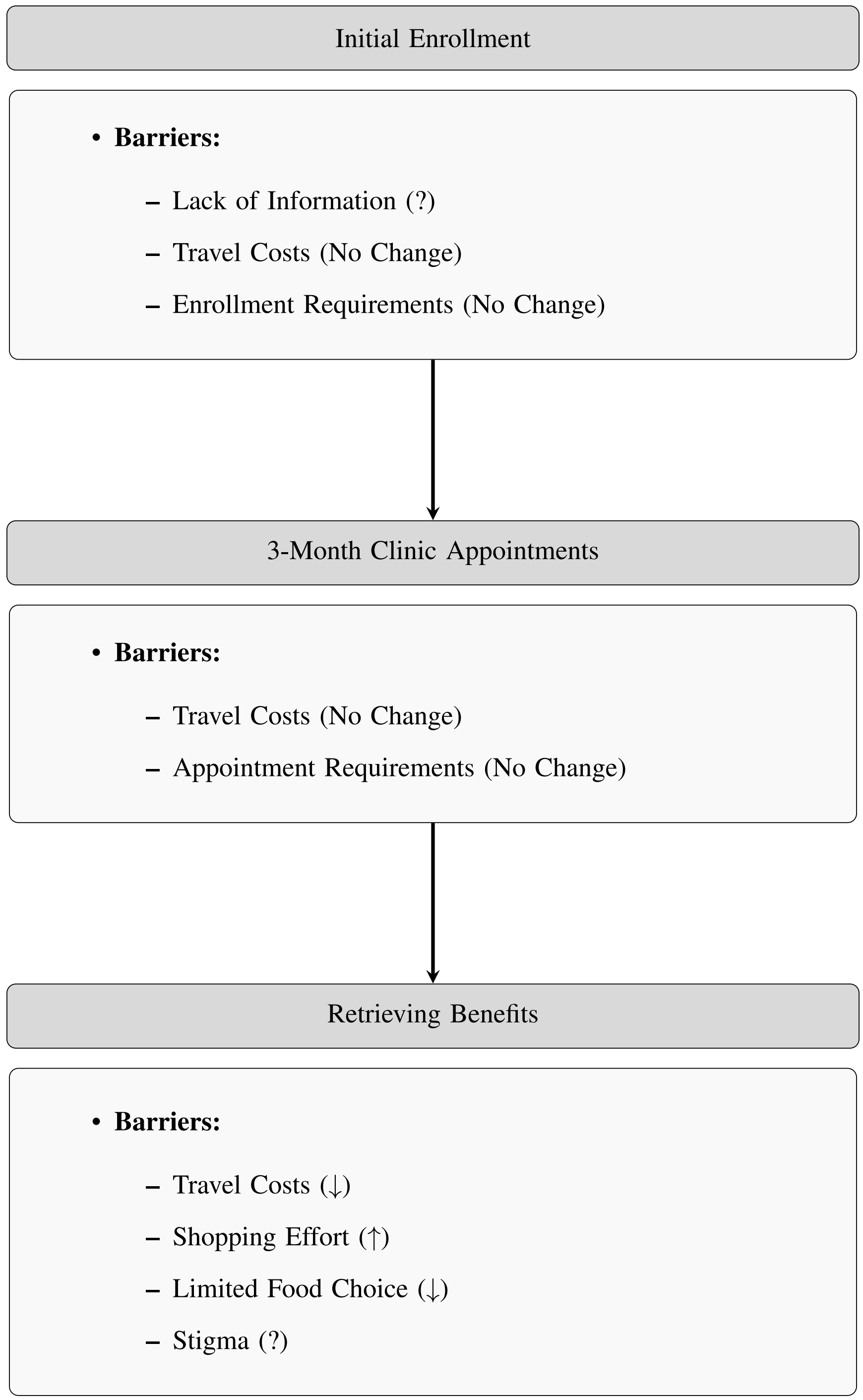


Figure A2.8: Impact of Retail Distribution on Participation Barriers in the WIC Program

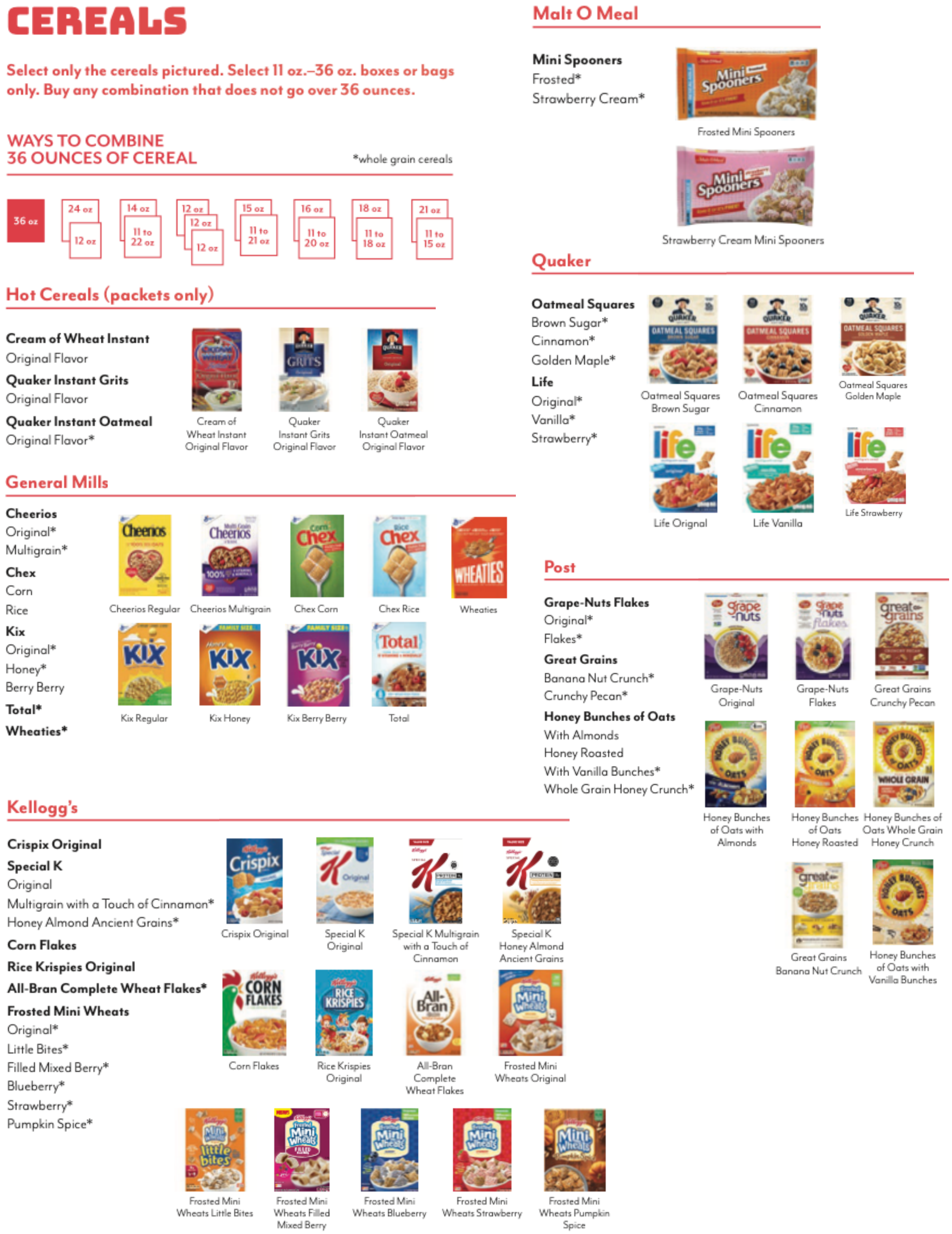


Figure A2.9: MSDH WIC Cereal Redemption Rules

Notes: Figure A2.9 illustrates the brands, flavors, and sizes of cereal combinations eligible for redemption through Mississippi WIC, as detailed on the Mississippi State Department of Health's website.

Source: https://msdh.ms.gov/page/resources/11709.pdf.

Figure A2.10: Time Series Graph for Turnover in WIC-Eligible Products

Notes: Figure A2.10 displays time series graphs of turnover in WIC-eligible products in Mississippi following the transition to retail distribution. We exclude fruits and vegetables, which are largely unrestricted at the product level. "Newly eligible" refers to the number of products that became WIC-eligible for the first time in a given month. "No longer eligible" refers to the number of products that ceased to be eligible after the indicated month. We proxy for newly eligible products using the number of unique product codes newly redeemed each month, defined as those not redeemed for at least two months prior to their first month of observation. Similarly, we proxy for no longer eligible products using the number of unique product codes ceased to be redeemed each month, defined as those not redeemed for at least two months following their last month of observation. The data are from Freedom of Information Act (FOIA) requests from June 2021 to August 2022.

(a) Entryway

(b) Produce

(c) Cheese

(d) Eggs, Rice, and Tortillas

Figure A2.11: Pictures of Jackson WIC Distribution Center

Notes: Figure A2.11 displays photos from the WIC Distribution Center in Jackson County, which was located in the Jackson Medical Mall.

Source: https://uconnmsobesity.weebly.com/jackson-medical-mall.html.

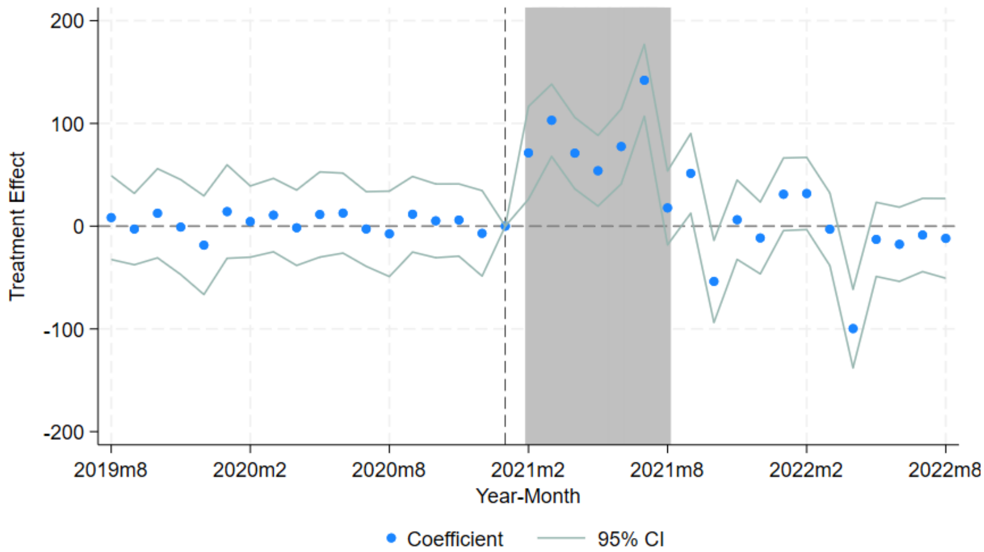


Figure A2.12: Event Study Graphs for WIC Program Costs: Food Benefits

Notes: Figure A2.12 presents the estimation results of the impact of the transition from direct distribution to retail distribution in Mississippi on the costs of per-participant WIC food benefit (in 2023 dollars), using the event study framework of the DiD estimator based on Eq. 2.2. The data are from direct distribution food contracts of Transparency Mississippi as described in Section 4.2. The numbers on the x-axis correspond to calendar years and months. The gray shade in each figure covers February 2021 to July 2021, denoting the rollout period for the transition from direct distribution to retail distribution. Standard errors are clustered at the state-date level. The 95% confidence intervals are shown in light green lines. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table A2.1: Maximum Monthly Allowances in the WIC Food Packages - Children and Women

Type	Children	Women		
Foods	**Food Package IV** A: 12-23 months B: 2-4 years	**Food Package V** A: Pregnant B: Partially Breastfeeding (up to 1 year postpartum)	**Food Package VI** Postpartum (up to 6 months postpartum)	**Food Package VII** Fully Breastfeeding (up to 1 year postpartum)
Juice, single strength	64 fl. oz.	64 fl. oz.	64 fl. oz.	64 fl. oz.
Milk	A: 12 qt. B: 14 qt.	16 qt.	16 qt.	16 qt.
Breakfast cereal	36 oz.	36 oz.	36 oz.	36 oz.
Eggs	1 dozen	1 dozen	1 dozen	2 dozen
Fruits and vegetables (CVB)	$26	A: $47 B: $52	$47	$52
Whole wheat bread	24 oz.	48 oz.	48 oz.	48 oz.
Fish (canned)	6 oz.	A: 10 oz. B: 15 oz.	10 oz.	20 oz.
Legumes, dry or canned and/or	1 lb. dry/64 oz. canned	1 lb. dry/64 oz. canned	1 lb. dry/64 oz. canned	1 lb. dry/64 oz. canned
Peanut butter	18 oz.	18 oz.	18 oz.	18 oz.

Notes: Table A2.1 displays the WIC food packages for women and children as of September 2024.

Source: https://www.fns.usda.gov/wic/food-packages/maximum-monthly-allowances.

Table A2.2: Maximum Monthly Allowances in the WIC Food Packages - Infants

Type	**Fully Formula Fed**		**Partially Breastfed**		**Fully Breastfed**	
Food Packages	**I and III**	**II and III**	**I and III**	**II and III**	**I**	**II**
Foods	A: 0-3 months B: 4-5 months	6-11 months	A: 0-3 months B: 4-5 months	6-11 months	0-5 months	6-11 months
WIC Formula	A: Up to 806 fl. oz. B: Up to 884 fl. oz.	Up to 624 fl. oz.	A: Up to 364 fl. oz. B: Up to 442 fl. oz.	Up to 312 fl. oz.	N/A	N/A
Infant cereal	N/A	8 oz.	N/A	8 oz.	N/A	16 oz.
Baby food fruits and vegetables	N/A	128 oz.	N/A	128 oz.	N/A	128 oz.
Baby food meat	N/A	N/A	N/A	N/A	N/A	40 oz.

Notes: Table A2.2 displays the WIC food packages for infants as of September 2024.

Source: https://www.fns.usda.gov/wic/food-packages/maximum-monthly-allowances.

Table A2.3: Schedule of Transition to Retail Distribution in Mississippi

Rollout Start Date	County
Feb, 2021	Lee, Lauderdale, Forrest
Apr, 2021	De Soto, Marshall, Benton, Tippah, Alcorn, Tishomingo, Tunica, Tate, Coahoma, Quitman, Panola, Lafayette, Union, Prentiss, Pontotoc, Itawamba, Bolivar, Sunflower, Tallahatchie, Yalobusha, Calhoun, Chickasaw, Monroe, Leflore, Grenada, Carroll, Montgomery, Webster, Clay
May, 2021	Washington, Issaquena, Sharkey, Warren, Claiborne, Humphreys, Yazoo, Hinds, Copiah, Holmes, Madison, Rankin, Simpson, Attala, Leake, Scott, Smith, Choctaw, Oktibbeha, Lowndes, Winston, Noxubee, Neshoba, Newton, Jasper, Kemper, Clarke
May/Jun, 2021	Jefferson, Adams, Wilkinson, Franklin, Amite, Lincoln, Pike, Lawrence, Walthall, Jefferson Davis, Covington, Jones, Wayne, Marion, Lamar, Perry, Greene, Pearl River, Stone, George, Hancock, Harrison, Jackson

Notes: Table A2.3 displays the schedule of transition of WIC distribution system to retail distribution from direct distribution in Mississippi. The table records the month in which each county began issuing EBT cards to a third of WIC participants each month over the three months period.

Source: MSDH Vendor management transition plan provided by Food and Nutrition Service.

Table A2.4: Summary Statistics: WIC Stores

	FY 2021		FY 2022	
	(1)	(2)	(3)	(4)
Store Type	Frequency	Share (%)	Frequency	Share (%)
(i) Super Store/Chain Store	152	58.91	164	56.16
(ii) Supermarket	96	37.21	116	39.73
(iii) Large Grocery Store	7	2.71	8	2.74
(iv) Medium Grocery Store	0	0	1	0.34
(v) Combination Grocery/Other	2	0.78	2	0.68
(vi) Convenience Store	1	0.39	1	0.34
TOTAL	258	100	292	100

Notes: Table A2.4 displays the number and share of WIC stores in Mississippi by store type, respectively in fiscal year (FY) 2021 and 2022. When assigning the store type, we follow the SNAP store type definitions of the USDA (https://www.fns.usda.gov/snap/store-definitions): (i) Super Store/Chain Store: Very large supermarkets, "big box" stores, super stores, and food warehouses primarily engaged in the retail sale of a wide variety of grocery and other store merchandise; (ii) Supermarket: Establishments commonly known as supermarkets, food stores, grocery stores and food warehouses primarily engaged in the retail sale of an extensive variety of grocery and other store merchandise; typically has ten or more checkout lanes with registers, bar code scanners, and conveyor belts; (iii) Large Grocery Store: A store that carries a wide selection of all four staple food categories, with food items as primary stock; (iv) Medium Grocery Store: A store that carries a moderate selection of all four staple food categories, with food items as primary stock; (v) Combination Grocery/Other: Primary business is sale of general merchandise but also sell a variety of food products (e.g., independent drug stores, dollar stores, and general stores); and (vi) Convenience Store: Self-service stores that offer a limited line of convenience items and are typically open long hours to provide easy access for customers.

Table A2.5: Robustness Checks: Effects of Transition to Retail Distribution on WIC Participation

	County-Level DCDH				State-Level DiD
	(1)	(2)	(3)	(4)	(5)
	Baseline	Formula Shortage	No Controls	EA	Part. Count
Post	-0.127***	-0.108***	-0.129***	-0.125***	-10170.38***
	(0.033)	(0.024)	(0.034)	(0.035)	(1808.827)
CONTROLS					
Unemployment	YES	YES	NO	YES	YES
Medicaid Policies	YES	YES	NO	YES	YES
Expiration of EA	NO	NO	NO	YES	NO
Pre-Period Mean (MS)	964.059	964.059	964.059	964.059	77,664.5
Observations	5,788	3,360	5,788	5,788	360

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.5 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation with different controls, using the DCDH estimator based on Eq. 2.1 (columns 1-4) and the DiD estimator based on Eq. 2.2 (column 5). The outcomes are the inverse hyperbolic sine of the number of participants (columns 1-4) and participation counts (column 5). Column (1) shows the DiD estimate from our preferred specification, reported in column (1) of Table 2.2. Column (2) shows the DiD estimate using the data prior to the start of the infant formula shortage in May 2022. Columns (3) and (4) shows the DiD estimate with different state-level controls. The data are from the Freedom of Information Act (FOIA) requests for MS and the USDA for the four neighboring states (also for MS in column 5), covering the period from January 2019 to September 2024 (columns 1, 3, and 4), from January 2019 to April 2022 (columns 2), and from October 2018 to September 2024 (column 5). Standard errors are clustered at the county (for MS) or state level (for neighboring states) (columns 1-4) and state-date level (column 5). State-level controls include unemployment rates, Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period), and expiration of Emergency Allotments (EA) in SNAP.

Table A2.6: Effects of Transition to Retail Distribution on WIC Participation with Different Control States

Census Region	South			Midwest		Northeast		West	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Census Division	E.S. Central	W.S. Central	South Atlantic	E.N. Central	W.N. Central	New England	Middle Atlantic	Mountain	Pacific
Post	-0.126***	-0.152***	-0.103***	-0.099***	-0.101***	-0.117***	-0.112***	-0.099***	-0.095***
	(0.022)	(0.032)	(0.029)	(0.019)	(0.020)	(0.023)	(0.037)	(0.020)	(0.019)
Pre-Period Mean (MS)	964.059	964.059	964.059	964.059	964.059	964.059	964.059	964.059	964.059
Number of Control States	3	4	9	5	7	6	3	8	5
Observations	5,719	5,788	6,133	5,857	5,995	5,926	5,719	6,064	5,857

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.6 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi (MS) on WIC participation with different control states (based on Census Divisions), using the DCDH estimator based on Eq. 2.1. The outcomes are the inverse hyperbolic sine of the number of participants. The data are from the Freedom of Information Act (FOIA) requests for MS and the USDA for other states, covering the period from January 2019 to September 2024. Standard errors are clustered at the county (for MS) or state level (for other states). State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table A2.7: Effects of the Transition to Retail Distribution on WIC Participation by Maternal Characteristics

		Maternal Characteristic			
	(1)	(2)	(3)	(4)	(5)
	Total	Hispanic	Black	Low-Educated	Unmarried
Post	-0.029***	-0.032	-0.045**	-0.034**	-0.045***
	(0.010)	(0.041)	(0.018)	(0.016)	(0.016)
Pre-Period Mean (MS)	0.449	0.404	0.610	0.596	0.620
Observations	1,586,464	155,638	459,320	704,169	765,067

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.7 presents the estimation results of the impact of the transition to retail distribution from direct distribution in Mississippi on WIC participation rate by maternal characteristics, using the DCDH estimator based on Eq. 2.3. The outcome is whether the mother has ever received WIC foods during her pregnancy. Column (1) shows the DiD estimate for the entire sample of births. Columns (2)-(5) show the DiD estimates for different subsamples based on maternal characteristics. Low-Educated is defined as having high school diploma or less (column 4). The data are from the restricted-use birth certificate data, covering the period from 2018 to 2023. Standard errors are clustered at the county level. Individual level controls include age (5 groups - Age <20, Age 20-24, Age 25-29, Age 30-34, Age 35+), race (3 groups - white, black, others), marital status (2 groups - married or not), and educational attainment (3 groups - no high school diploma, high school diploma, some college or above). State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

Table A2.8: Change in Distance to Nearest WIC Food Site Following Retail Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Year = 2022	-1.5428***	-2.2930***	-1.7524***	-3.0295***	-1.3319***	-1.7318***
	(0.1192)	(0.1899)	(0.1118)	(0.2137)	(0.2109)	(0.2843)
Year = 2022 × High Pov.		0.9262***		1.5274***		0.5103
		(0.2358)		(0.2470)		(0.3835)
Observations	4,512	4,512	2,058	2,058	2,454	2,454
Dep. Var. Mean (in 2020)	5.27	5.27	2.94	2.94	7.61	7.61
Sample	All	All	Urban	Urban	Rural	Rural

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.8 presents estimates of changes in distance to WIC food sites for low-income households, where food sites are defined as distribution centers in 2020 and WIC stores in 2022. The outcome variable is the distance (in miles) from the population-weighted block group centroid to the nearest WIC food site. A negative coefficient indicates that WIC stores were, on average, closer than distribution centers. To estimate these coefficients, we construct a dataset with two observations for each block group in Mississippi–one for 2020 and one for 2022. We then calculate the distance in miles from the block group population centroid to the nearest warehouse (2020) or WIC store (2022), denoted as $Distance_{bt}$ for block group b and year t. We estimate the regression model $Distance_{bt} = \alpha + \beta Year2022_t + \Gamma_b + \epsilon_{bt}$, where $Year2022_t$ is an indicator for 2022, Γ_b represents block group fixed effects, and standard errors are clustered at the block group level. Odd-numbered columns report estimates for β. Even-numbered columns estimate an extended model: $Distance_{bt} = \alpha + \beta Year2022_t + \delta(Year2022_t \times High\ Pov_b) + \Gamma_b + \epsilon_{bt}$, where $High\ Pov_b$ is an indicator for block groups with a household poverty rate above the median for Mississippi (0.16). Block groups are classified as urban or rural based on the majority of housing units: an urban block group has more than 50% of its housing units classified as urban, while a rural block group has less than 50% classified as urban. All regressions are weighted by the number of households per block group with income below the poverty level. Data on household poverty and urban classification at the block group level are from the 2020 Census.

Table A2.9: Change in Within-Zip Access to WIC Food Sites Following Retail Distribution

	(1)	(2)
Year $= 2022$	0.3086***	0.4190***
	(0.0617)	(0.0936)
Year $= 2022 \times$ High Pov		-0.2199*
		(0.1181)
Observations	832	832
Dep. Var. Mean (in 2020)	0.45	0.45
Sample	All	All

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.9 presents estimates of changes in within-zip access to food distribution sites, defined as distribution centers in 2020 and WIC stores in 2022. we construct a dataset with two observations per zip code in Mississippi–one for 2020 and one for 2022. We define an indicator $Access_{zt}$ for whether a WIC distribution site exists in zip code z in year t. Column (1) estimates the regression model $Access_{zt} = \delta + \upsilon Year2022_t + \Psi_z + \epsilon_{zt}$, where $Year2022_t$ is an indicator for 2022, Ψ_z represents zip code fixed effects, and standard errors are clustered at the zip code level. Column (2) estimates an extended model: $Access_{zt} = \delta + \upsilon Year2022_t + \rho(Year2022_t \times High\ Pov_z) + \Psi_z + \epsilon_{zt}$, where $High\ Pov_z$ is an indicator for zip codes with a household poverty rate above the median for Mississippi (0.19). All regressions are weighted by the number of individuals per zip code with income below the poverty level. Data on household poverty per zip code are from the 2015-2019 American Community Survey 5-Year Estimates.

Table A2.10: Effects of Transition to Retail Distribution on WIC Program Costs: Food Benefits

	(1)	(2)
Post	21.706***	
	(8.207)	
During Rollout		85.491***
		(11.342)
After Rollout		1.302
		(5.647)
Pre-Period Mean (MS)	45.535	
Observations	355	

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A2.10 presents the estimation results of the impact of the transition from direct distribution to retail distribution in Mississippi on the costs of per-participant WIC food benefit (in 2023 dollars), using the DiD estimator based on Eq. 2.2. Column (1) shows the DiD estimate on per-participant WIC food benefit costs. Column (2) shows the DiD estimates, separately for the rollout period and the post-rollout period. The data are from direct distribution food contracts of Transparency Mississippi as described in Section 4.2. Standard errors are clustered at the state-date level. State-level controls include unemployment rates and Medicaid policies (i.e., postpartum coverage extensions, work requirements, and expansion of eligibility for immigrants within the five-year waiting period).

A2.1 Birth Certificates

We use restricted-use Vital Statistics Natality data from 2018 to 2023, which covers the universe of U.S. births and provides detailed information on birth outcomes (e.g., gestation weeks and birth weight), maternal characteristics (e.g., county and state of residence, and demographics), and whether the mother received WIC foods during pregnancy.

To assign treatment timing, we define the start of the second trimester (14 weeks from conception), as the majority of pregnant women join WIC by this time (USDA 2020). Using reported birth dates and gestation weeks, we calculate each mother's estimated conception date and add 14 weeks. We restrict the sample to births between July 2017 and June 2023 to ensure a balanced panel around the timing of treatment.

Our empirical approach follows the difference-in-differences framework used in previous analyses. Specifically, we estimate the following specification for individual birth outcomes Y_{ict} in county c and year-month t of the start of the second trimester:

$$Y_{ict} = \alpha + \beta \textit{Post}_{ct} + \gamma X_i + \delta_c + \mu_t + \epsilon_{ict} \tag{2.3}$$

where $Post_{ct}$ is an indicator for whether WIC recipients in county c are receiving EBT benefits in year-month t. X_i includes individual controls: maternal age (five groups: $<$20, 20–24, 25–29, 30–34, 35+), race (White, Black, Other), marital status (married or not), and educational attainment (no high school diploma, high school diploma, some college or higher). δ_c and μ_t are county and date fixed effects, and ϵ_{ict} is the error term, clustered at the county level.

A2.2 3-Month Travel Distance Calculations

We estimate the travel burden associated with WIC participation by modeling the total travel distance for grocery shopping and WIC-related trips among WIC and non-WIC participants living in the same areas over a three-month period. We focus on three months because WIC participants in Mississippi are required to attend clinic appointments at this interval. Table A2.11 outlines the framework used to model travel distance under both direct and retail distribution. Using this framework, we estimate the change in WIC-associated travel burden following the transition to retail distribution.

The unit of analysis is the Census tract, with household locations (WIC and non-WIC) proxied by tract centroids. For simplicity, we assume all households make one routine grocery shopping trip per month. Unlike non-WIC participants, WIC participants must visit a clinic every three months to receive benefits and nutrition education. Additionally, they must collect WIC foods either from state-run distribution centers (direct distribution) or from authorized grocery stores (retail distribution). Our framework accounts for two ways in which WIC participants may consolidate these trips:

- **Joint Clinic-Food Pickups**: For one of the three required WIC food pickups over a three-month period, participants may combine their clinic visit with a trip to a WIC food site (distribution center or WIC store), rather than making a separate trip from home. In these cases, we assume they travel the shorter of:
  - The distance from the WIC clinic to the WIC food site.
  - The distance from home to the WIC food site.
- **WIC Shopping at Usual Grocery Stores**: Under retail distribution, WIC participants whose nearest WIC-authorized store is also their usual grocery store do not require additional trips for WIC food pickup.[75] In these cases, no additional travel burden is assigned beyond what

[75] For each Census tract, we proxy the "usual grocery store" as the nearest SNAP-authorized superstore or supermar-

a non-WIC participant would incur.

To calculate the additional travel burden associated with WIC participation, we use the letter labels in Table A2.11 to represent the distances described in Table A2.12. The total WIC-associated travel burden is given by:

$$3[B] + [C] + [D] + 2[E] - 3[A]$$

Since $3[A] = 3[B]$, these terms cancel, leaving:

$$[C] + [D] + 2[E]$$

We compute this sum separately for direct and retail distribution. Distances are weighted by the number of families below 185% of the federal poverty level (FPL) in each Census tract to align with WIC eligibility criteria. We perform these calculations for fiscal years (FY) 2019, 2020, and 2021 for the direct distribution system, and FY 2021 and 2022 for the retail distribution system.

Total WIC-related travel distance was 18.3 miles in 2020 under direct distribution and 8.6 miles in 2022 under retail distribution. Overall, WIC participants traveled approximately half as far for WIC benefits under retail distribution compared to direct distribution. This reduction was driven by: (1) the greater number of WIC-authorized grocery stores compared to distribution centers; and (2) the ability to combine WIC purchases with routine grocery shopping at SNAP-authorized stores.

ket. We use SNAP-authorized store data as a proxy for a grocery store census, as nearly all supermarkets participate in SNAP (Dong et al. 2024). We then determine whether this store is also WIC-authorized. Data on SNAP stores, including addresses and store types, can be found here: https://www.fns.usda.gov/snap/retailer/historical-data.

Table A2.11: Framework for Calculating 3-Month Travel Burden Associated with WIC Participation

		(1) **Direct Distribution**	(2) **Retail Distribution**	(3)
Household	**Trip Purpose**		**NOT Usual Store**	**Usual Store**
Non-WIC	[A] Routine Grocery Shopping (1/month)	Home to Grocery Store		
WIC	[B] Routine Grocery Shopping (1/month)	Home to Grocery Store		
	[C] WIC Clinic Visit (1/quarter)	Home to WIC Clinic		
	[D] WIC Food Shopping (1/quarter) (Joint Clinic-Food Pickup)	Shorter distance of: (i) WIC Clinic to WIC Distribution Center or (ii) Home to WIC Distribution Center	Shorter distance of: (i) WIC Clinic to WIC Grocery Store or (ii) Home to WIC Grocery Store	None
	[E] WIC Food Shopping (2/quarter)	Home to WIC Distribution Center	Home to WIC Grocery Store	None

Table A2.12: Calculation of 3-Month Travel Distance (in miles)

Dist. System	Starting Point - Destination	(1) FY 2019	(2) FY 2020	(3) FY 2021	(4) FY 2022
Both	[C] centroid - health dept	5.993			
Direct	[D]-(i) health dept - dist center	1.650	1.662	2.024	
	[D]-(ii) centroid - dist center	5.453	5.464	5.804	
	Total Travel Distance	**18.307**	**18.342**	**19.358**	
Retail	[D]-(i) health dept - store			1.853	1.399
	[D]-(ii) centroid - store			4.170	3.928
	Total Travel Distance			**10.081**	**8.579**

Notes: Table A2.12 reports the calculation of total travel distances at three-month interval under each distribution system. The distances are calculated for fiscal years (FY) 2019, 2020, and 2021 for the direct distribution system, and FY 2021 and 2022 for the retail distribution system. For each distribution system, first two rows show the minimum distance for each combination of a starting point to the destination, and the last row shows the total travel distance. We calculate the average of minimum distances for 878 Census tracts in Mississippi by weighting them with the number of families below 185% of poverty level in each Census tract, which is the income criteria used to determine WIC eligibility. Total travel distances are derived by calculating [C] + min{[D]-(i), [D]-(ii)} + 2 × [D]-(ii) where the notations [C], [D]-(i) and [D]-(ii) (equivalent to [E]) are the same as in Table A2.11.

A2.3 Google Reviews

We construct a dataset of Google reviews for WIC distribution centers and other service locations using two sources: (1) manual collection of reviews for WIC distribution centers in Mississippi, and (2) a database of Google reviews compiled by Li, Shang, and McAuley (2022). The first dataset provides a comprehensive review of WIC distribution centers, while the second allows comparison with other locations offering social service.

A2.3.1 Manual Collection of Reviews of WIC Distribution Centers

Our dataset consists of manually collected star ratings and written reviews for WIC distribution centers in Mississippi. Of the 96 WIC distribution centers identified in administrative data (detailed in Section 4), we found 84 centers with at least one Google review as of June 2024. For example, the Forrest County WIC Distribution Center has 49 reviews with an average rating of 4.4 out of 5: https://g.co/kgs/dJUSWyM.

In total, we collected 754 reviews, of which 293 contain text, while 461 are numeric star ratings only (on a 1-to-5 scale, with 5 stars as the highest rating and 1 star as the lowest). Among the 84 centers with reviews, 57 (68%) have at least one text review. Since all WIC distribution centers closed in 2024, nearly all reviews were submitted before 2022, when the transition to retail distribution was completed.

Table A2.13 presents summary statistics for the dataset. The average star rating is 4.1 out of 5.0 across all distribution centers. To analyze review content, we generate a word cloud (Figure 2.5A) from the 293 text-based reviews, where word size represents relative frequency. Table A2.14 lists the 10 most frequently used words among 5,712 total words, excluding pronouns, articles, prepositions, conjunctions, auxiliary verbs, local adverbs (e.g.,“there”), and emphatic adverbs (e.g., “very”).

While word frequency analysis provides useful insights, it may not fully capture overall sentiment, particularly in cases where negations (e.g., “not nice,” “never open,” or “no help”) reverse the intended meaning. For example, a simple word count might misclassify "nice" in "not nice" as a positive sentiment. To address this, we conducted a sentiment analysis using TextBlob, a Python-based natural language processing tool. TextBlob assigns each review a polarity score ranging from -1 to 1, where: positive values indicate positive sentiment; negative values indicate negative sentiment; and zero indicates a neutral sentiment.

Table A2.15 summarizes descriptive statistics for sentiment scores, while Figure 2.5B presents a histogram of polarity scores, illustrating the distribution of positive, negative, and neutral reviews.

A2.3.2 Google Reviews data from Li, Shang, and McAuley (2022)

The second source of data is the Google Local Database compiled by Li, Shang, and McAuley (2022). Based on Google Maps data as of September 2021, it includes business ratings, addresses, and names drawn from the full sample of Google Reviews. Whereas our manually collected data include WIC distribution centers only, this dataset enables comparison of WIC distribution center ratings with those of other locations in Mississippi. However, this database only covers subset of businesses on Google Maps. Specifically, the authors exclude text reviews shorter than five words or containing more than 10 images and remove users with only one review or whose image URLs have expired (Li, Shang, and McAuley 2022).

Table A2.16 reports average ratings (ranging from 1 to 5), number of locations, and number of reviews used in the calculations. The number of WIC distribution centers in this database is about one-third of those in our manually collected dataset, but the average rating is similar (4.3 vs. 4.1 in our data).

In addition to WIC distribution centers, we collect reviews for food banks, social service organizations, and county health departments. Most businesses categorized under "social services" are government offices administering social benefits (e.g., Medicaid, SNAP, welfare, and child support), though the category also includes community centers, religious charities, and similar establishments. The footnotes to Table A2.16 detail the exact text searches used to identify each category.

Table A2.13: Summary Statistics for Google Reviews of WIC Distribution Centers

Avg Rating	# of Dist. Centers	# of Reviews
4.056	84	754

Notes: Table A2.13 summarizes our manually collected dataset of Google reviews for WIC distribution centers in Mississippi. Ratings range from 1 to 5, with higher values indicating more positive reviews.

Table A2.14: Top 10 Words from Google Reviews of WIC Distribution Centers

Rank	Word	Frequency	Share of Words (%)
1	always	49	0.9474
2	helpful	48	0.9281
3	friendly	46	0.8894
4	people	40	0.7734
5	nice	38	0.7347
6	wic	34	0.6574
7	staff	28	0.5414
8	get	28	0.5414
9	place	25	0.4834
10	clean	24	0.4640

Notes: Table A2.14 presents the 10 most frequently used words from a total of 5,712 unfiltered word counts in Google reviews of WIC distribution centers in Mississippi. Common function words—–including pronouns, articles, prepositions, conjunctions, auxiliary verbs, local adverbs (e.g., “there”), and emphatic adverbs (e.g., “very”)—–are excluded. The dataset consists of 293 manually collected Google reviews for 84 WIC distribution centers, gathered in June 2024.

Table A2.15: Sentiment Analysis of Google Reviews of WIC Distribution Centers

(a) Descriptive Statistics

Measure	Value
Mean	0.276
Median	0.3
SD	0.339
N	293

(b) Number of Google Reviews by Sentiment

	Frequency	Share (%)
Positive	197	67.24
Neutral	53	18.09
Negative	43	14.68
TOTAL	293	100

Notes: Table A2.15 presents sentiment analysis results for Google reviews of WIC distribution centers in Mississippi, using polarity scores from TextBlob. The polarity score ranges from -1 to 1, where positive values indicate positive sentiment, negative values indicate negative sentiment, and zero indicates a neutral statement. Table A2.15A reports summary statistics for polarity scores, while Table A2.15B categorizes reviews into positive, neutral, and negative sentiment groups. The dataset consists of 293 manually collected Google reviews for 84 WIC distribution centers, gathered in June 2024.

Table A2.16: Google Ratings for Locations Providing Social Services in Mississippi

Category	(1) WIC Dist. Center	(2) Food Bank	(3) Social Service Org.	(4) County Health Dept.
Average Rating	4.321	4.612	3.835	2.773
# of Locations	26	20	178	10
# of Reviews	532	749	4,597	74

Notes: Table A2.16 reports the average ratings of locations in Mississippi using the Google review database of Li et al. (2021). The numeric star ratings range from 1 to 5. We identify individual WIC distribution centers using our administrative data on these locations. For the other categories of establishments, we perform string searches of their name or Google-assigned category. In Column (2), "Food bank" locations are classified as those with "food bank" or "food pantry" in the category field. In Column (3), "Social services" locations are classified as those with "social services" in the category field. In Column (4), "County health department" locations are classified as those with "health department" in the name field.

A2.4 Food Redemption

We use detailed monthly records on food benefits redemptions from Mississippi WIC to construct two outcomes: redemption share and average price per ounce, as shown in Figure 2.4.

A2.4.1 Data Source

- **Freedom of Information Act (FOIA) requests**
  - **(1) Pre-reform period (2019m1-2021m7)**

    * Data structure: by (granular) food category, agency (w/ county info), year-month

    * Information used: units redeemed

    * Calculated variables: total units redeemed (i.e., numerator of redemption rates)

    * Potential issues:

    (i) Food category is not as detailed as the post-reform data since the data is not at the UPC-level (already collapsed to one value for each food category, agency, and year-month).

    → We use this classification of food categories for both the pre- and post-reform periods.

    (ii) The meaning of the variable 'units redeemed' provided by the agency is not consistent with the data.

    → We tried to figure out its meaning after cross-checking with possible candidates (e.g., oz, lb) and the participation counts.

    → Still, these limits imply that we cannot calculate a pre-reform redemption rate for food categories with different product types (which cannot be separate), with different units (which are not convertible and thus not comparable), and with varying package sizes (which cannot be converted to units redeemed).

  - **(2) Post-reform period (2021m2-2022m12)**

    * Data structure: by UPC, vendor (omitted from the table), year-month

    * Information used: units redeemed, UPCs

    * Calculated variables: total units redeemed (i.e., numerator of redemption rates), average

price per unit, and turnover in UPCs

* Cross-check: The monthly series of the total amount redeemed track the food costs with rebates from the USDA administrative data well.

- **Direct distribution food contracts of Transparency Mississippi**

  * Data period: pre-reform period only

  * Information used: contract information (i.e., size, number of cases, price per case) for each food product

  * Calculated variables: total units redeemed (only for unit conversions) and average price per unit

  * Detailed information about the contracts used:

  (i) Whole grains (bread/rice/tortilla)

  - Contract: 8200038842, Bimbo Bakeries (bread from here) (FY 2019-2020)

  - Contract: 8200038954, SuperValu Foods (rice and tortilla from here)

  (ii) All other food categories

  - Contract: 8200038954, SuperValu Foods

- **USDA administrative data**

  * Data period: both pre- and post-reform periods

  * Information used: monthly participation count by type (same data used for participation analyses) and rebates

  * Calculated variables: total units issued (i.e., denominator of redemption rates) and share of post-rebate redemption amount (shown in the legend of the figures)

- **WIC food packages** (FY 2019; accessed using wayback machine)

  * Data period: both pre- and post-reform periods

  * Information used: units issued to each participant type

* Calculated variables: total units issued (i.e., denominator of redemption rates)

* Potential issues: We assume that the federal package did not change over time during the data period and the package for MS is equivalent to the federal one.

* Source:

https://web.archive.org/web/20201017214826/https://wicworks.fns.usda.gov/resources/snapshot-food-packages-women-infants-and-children

https://web.archive.org/web/20201019233341/https://wicworks.fns.usda.gov/resources/snapshot-infant-food-packages

A2.4.2 Construction of the variables

We perform each calculation below separately for each food category and, where applicable, for each year-month. Since we have UPC-level data for food redemption during the post-reform period—which is more detailed than both the contracts and, more importantly, the food redemption data from the pre-reform period—we ensure consistency across time by following the pre-reform classification.[76]

- **Redemption share**

  * Formula: (a) total units redeemed / (b) total units issued

  * Derivation:

  (i) calculate (a) and (b) by summing up all of the respective values in each year-month during the reference period

  (ii) divide (a) by (b)

  - **(1) Total units redeemed: pre-reform period**

    * Source: warehouse food redemption data through FOIA requests and food contracts of Transparency Mississippi (only for unit conversion)

    * Derivation:

[76]Specifically, we combine different types of milk (e.g., whole, reduced-fat) and yogurt into a single category, as these are examples of milk substitutions.

(i) add the amount redeemed across agencies for each year-month

(ii) multiply the value from (i) by "package size" from the contracts (this step is not needed for the post-reform period)

(iii) convert the unit if applicable

– **(2) Total units redeemed: post-reform period**

* Source: vendor food redemption data through FOIA requests

* Derivation:

(i) add the values of upc quantity (which incorporates each item count and exchange size in the unit provided) across vendors and UPCs for each year-month

(ii) convert the unit if applicable

– **(3) Total units issued: both pre- and post-reform periods**

* Source: USDA administrative data and WIC federal package

* Derivation:

(i) for each participant type, multiply the units issued to each type by the number of participants

(ii) add the product term across all types

(iii) convert the unit if applicable

- **Average price per unit** (ounce) (in 2023 dollars)

– **(1) Pre-reform period**

* Source: contracts from Transparency Mississippi

* Derivation: weighted average of price per unit across products (weight = number of cases) and inflation-adjusted to 2023 dollars

– **(2) Post-reform period**

* Source: vendor food redemption data through FOIA requests

* Formula: (c) total amounts redeemed (in $) / (d) total units redeemed (in ounces)

* Derivation:

(i) calculate (c) and (d) by summing up all of the respective values in each year-month during the reference period

(ii) divide (c) by (d)

(iii) adjusted to 2023 dollars

Time series for redemption shares: Figure A2.13

- Redemption share: total units redeemed / total units issued
- Numbers in legend: post-rebate share of total redemptions for each food category
  (= redemption amount for each category / post-rebate total redemption amount)
  (all amounts in 2023 dollars)
- Full markers (2019m1 - 2020m6, 2021m8 - 2022m12): actual redemption share
- Grey area (2020m7 - 2021m7): We do not calculate the redemption shares for this period due to limited information on WIC food contracts under direct distribution.

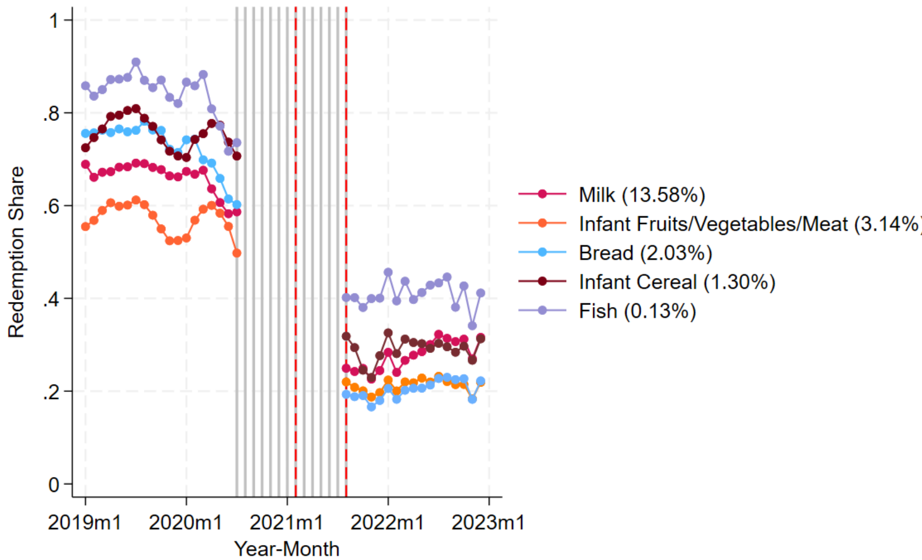


Figure A2.13: Time-Series Graph for WIC Food Redemption Share

Notes: Figure A2.13 displays time series graphs of WIC food redemption shares in Mississippi, defined as the share of quantity redeemed out of quantity issued. The two vertical red dashed lines indicate February 2021 and August 2021, denoting the rollout period for the transition from direct distribution to retail distribution. The gray area indicates the period from July 2020 to July 2021, during which we do not calculate the redemption shares, due to limited information on WIC food contracts under direct distribution. The percentage next to each food category in the legend indicates the post-rebate redemption share for each food category from July 2021 to June 2022. The data are from Freedom of Information Act (FOIA) requests, direct distribution food contracts of Transparency Mississippi, and the USDA, covering the period from January 2019 to December 2022. Detailed calculations are provided in the Section A2.4.

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Chapter 3 IMMIGRATION ENFORCEMENT AND SAFETY NET PARTICIPATION IN MIXED-STATUS HOUSEHOLDS

1 Introduction

Immigration enforcement policies regulate non-citizens' ability to enter, work, reside, and remain in the United States. Although these policies are formally directed at non-citizens who lack legal authorization or otherwise violate immigration laws, their effects may extend to U.S. citizens, particularly those in mixed-status households with non-citizens at risk of deportation. For these households, enforcement may increase economic vulnerability while also raising the perceived costs of interacting with government institutions, including safety net programs. As a result, program participation may depend on both economic circumstances and deportation risk within the household, making responses across programs an empirical question.

This paper investigates how mixed-status households respond to immigration enforcement when deciding whether to participate in major U.S. safety net programs. Specifically, I examine the effects of state-level E-Verify mandates, an employment-based immigration enforcement policy, on participation in three programs—Medicaid, the Supplemental Nutrition Assistance Program (SNAP), and Supplemental Security Income (SSI)—among Hispanic citizens living with individuals facing deportation risk. These mandates require employers to electronically verify workers' legal status using federal databases, facilitating the identification of undocumented workers in ordinary workplaces.

Although these mandates primarily aim to reduce unauthorized employment, they can also generate broader spillover effects across populations and domains. In the labor market, E-Verify may negatively affect even native workers if reductions in firm hiring exceed declines in labor market competition from undocumented immigrants (Chassamboulli and Palivos 2014; Chassamboulli and Peri 2015; Albert 2021; Lee, Shin, and Topa 2026). At the same time, heightened awareness of immigration status and government scrutiny may increase fear even among individuals not directly subject to deportation (i.e., chilling effects). Hispanics, in particular, tend to have closer connections to undocumented immigrants than non-Hispanics, as approximately three-quarters of the nation's unauthorized immigrant population is Hispanic (Passel and Cohn 2009). Moreover, these chilling effects may be particularly pronounced in mixed-status households with members facing deportation risk.

Even among anti-poverty programs, Medicaid, SNAP, and SSI have distinct features. As the largest means-tested transfer program in the U.S., Medicaid offers public health insurance for low-income individuals. SNAP provides nutrition benefits to supplement the food budgets of needy families. SSI is the largest cash welfare program, supporting disabled children, working-age adults, and the elderly. Because these programs differ in their benefit structures, immediacy of need, and degree of institutional interaction, the balance between economic need and deportation risk may vary across programs. Examining these differences is therefore important for understanding the broader consequences of immigration enforcement and the reach of the social safety net.

Applying for or renewing benefits under all three programs involves extensive interaction with government authorities. Applicants must provide detailed information for each household member, including name, date of birth, marital status, race and ethnicity, address, employer, income, disability status, pregnancy status, participation in U.S. military service, incarceration status, Social Security number (SSN), and citizenship or immigration documentation. Such requirements can reveal undocumented status or deportation risk, leading some eligible individuals in mixed-status households to decline benefits to minimize interactions with authorities and avoid placing family members at a greater risk of deportation.

The primary data sources are the 2001–2019 March Supplements of the Current Population Survey (CPS), known as the Annual Social and Economic Supplement (ASEC), and the American Community Survey (ACS). Both are household surveys that provide detailed demographic and economic information on individuals and their households. These data allow me to identify Hispanic citizens (ages 18–64) as the sample of interest, as well as Hispanic non-citizens in their households, who serve as a proxy for undocumented immigrants (Passel and Cohn 2009; Pope

2016; Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2018; Cascio, Lewis, and Zhang 2024) or, more broadly, individuals facing a higher risk of deportation (Immigration Policy Center 2010; Patler and Jones 2025).

I use a difference-in-differences (DID) framework to compare the outcomes for Hispanic citizens living with Hispanic non-citizens before and after the enactment of E-Verify mandates, relative to states without mandates. To address potential bias in ordinary least squares (OLS) regressions with two-way fixed effects (Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024; De Chaisemartin and d'Haultfoeuille 2024), I use the alternative DID estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). The identifying assumption is that no state-specific time-varying factors are correlated with the enactment of E-Verify mandates.

Changes in program participation may arise not only from changes in take-up among eligible individuals but also from changes in the size of the eligible population. I therefore first examine whether household income changed after the mandates, as income affects both demand for welfare programs and program eligibility. I find that E-Verify mandates reduced household income among Hispanic citizens living with Hispanic non-citizens by 4.5%, driven by adverse spillovers to natives in the labor market. This economic hardship following the mandates suggests increased program need and eligibility among mixed-status families.

Based on this, I next examine the effects of E-Verify mandates on program participation among Hispanic citizens living with Hispanic non-citizens. The policy effects differ substantially across programs. I find an average increase of 0.3 percentage points in SSI participation and 0.1 percentage points in SNAP participation. Although the average effect on SNAP participation is imprecisely estimated, the increase in SSI participation is both statistically and economically significant, with more pronounced effects immediately after enactment.

In contrast, Hispanic citizens in mixed-status households were 8.4 percentage points less likely to participate in Medicaid after the enactment of E-Verify mandates, representing a 41.4% decline relative to the pre-treatment mean. The decline in household income suggests that this reduction in Medicaid participation reflects lower take-up rather than reduced eligibility. This result is robust to the inclusion of alternative control variables and to revisions by the Census Bureau designed to correct for potential underreporting of program participation (Meyer, Mok, and Sullivan 2009). I interpret this finding as causal because I find no evidence of differential pre-trends prior to the enactment of E-Verify mandates.

When mixed-status households decide whether to enroll in a program, increased demand and eligibility arising from adverse income shocks may counteract concerns about the deportation of family members. Program characteristics and additional analyses suggest that the differential effects of E-Verify mandates across programs may reflect differences in the relative importance of these competing forces by programs. For example, SSI provides larger cash benefits without spending restrictions, which may be particularly valuable for individuals with disabilities during economic hardship. In contrast, as a form of health insurance, Medicaid may heighten concerns about government surveillance because benefit use requires more frequent interactions with institutions. Moreover, because Medicaid primarily insures against future health care needs, individuals may be more willing to delay enrollment, especially given the availability of alternative enrollment pathways.

The substantial decline in Medicaid participation motivates an examination of alternative sources of health insurance coverage following the mandates. The decline in Medicaid enrollment was accompanied by only limited substitution from public to private insurance, increasing the share of uninsured Hispanic citizens. Enrollment in non-employer-sponsored insurance (non-ESI) among Hispanics rose by 0.9 percentage points, which is substantially smaller than the 8.4 percentage-point decline in Medicaid participation. The decline in household income is also consistent with incomplete substitution from Medicaid to private insurance.

Finally, I examine chilling effects, or spillovers of deportation fear, as a potential mechanism underlying the decline in Medicaid participation following the mandates. I conduct three heterogeneity analyses, all of which show larger declines in Medicaid participation among groups more likely to experience fear. Consistent with this mechanism, the decline in Medicaid participation is concentrated among Hispanic citizens living with Hispanic non-citizens, while the effect is much smaller among those not living with Hispanic non-citizens. In addition, the decline is larger among Hispanic citizens (i) living with Hispanic non-citizens at higher deportation risk (i.e., non-Cuban and non-Salvadoran) and (ii) residing in states without sanctuary cities during the period of E-Verify enactment, where cooperation with U.S. Immigration and Customs Enforcement (ICE) is more limited.[77] [78] These findings suggest that fear within mixed-status households may lead individuals to forgo benefits to protect household members at risk of deportation.

This study contributes to two strands of the literature. First, it relates to research examining the adverse effects of E-Verify mandates on the labor market outcomes of undocumented immigrants and, in some cases, native workers (Amuedo-Dorantes and Bansak 2014; Orrenius and Zavodny 2015; Churchill 2021; Bansak and Raphael 2001; Bohn, Lofstrom, and Raphael 2015; Ayromloo, Feigenberg, and Lubotsky 2020). In contrast, this paper focuses on program participation and health insurance coverage among citizens. In doing so, it extends the literature by showing that immigration enforcement can generate broader spillover effects beyond directly targeted outcomes and populations.[79]

The second strand of the literature examines chilling effects associated with increased deportation risk among non-citizens across a range of domains, including program participation and spillover effects on citizens.[80] Prior evidence suggests that deportation risk is negatively associated with participation in the Special Supplemental Nutrition Program for Women, Infants, and

[77]This is because Cubans are more likely to have political refugee status, and Salvadorans are more likely to receive temporary protection from deportation, allowing them to remain and work legally in the U.S. I provide additional details in Section 6.

[78]As additional supporting evidence, following the approach of Alsan and Yang (2024), I find an approximately 40% to 50% increase in Google searches related to deportation after E-Verify enactment.

[79]Related work also finds that intensified immigration enforcement increased the likelihood of living in poverty (Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2018) and negatively affected child development and human capital accumulation in mixed-status families (Amuedo-Dorantes and Lopez 2017; Amuedo-Dorantes, Churchill, and Song 2022).

[80]Extensive qualitative and correlational evidence also documents that fear of deportation limits undocumented immigrants' activities across various domains, including enrollment in government programs (Artiga and Ubri 2017), participation in the Census survey (Karpman, Zuckerman, and Gonzalez 2020), engagement with school authorities (Murillo 2017) and police (Muchow and Amuedo-Dorantes 2020), and use of health care services (Núñez and Heyman 2007; Heyman, Núñez, and Talavera 2009). Experimental evidence also suggests that immigration-related cues reduce Latino citizens' willingness to seek health care (Pedraza, Nichols, and LeBrón 2017).

Children (WIC) among mixed-status mothers (Vargas and Pirog 2016) and Medicaid participation among children of non-citizens (Watson 2014). Relative to Watson (2014), this paper uses policy variation from employment-based immigration enforcement, focuses on older samples, and expands the analysis to broader measures of safety net participation and health insurance coverage.

As the study most closely related to this paper, Alsan and Yang (2024) provide the first causal evidence that Secure Communities, a form of police-based enforcement, reduced SNAP and SSI enrollment among Hispanic citizens.[81] My work complements their findings by examining a different form of immigration enforcement and extending the analysis to Medicaid participation and health insurance coverage. In addition, while their analysis emphasizes spillovers through broader Hispanic communities, my sample focuses on citizens living with individuals facing deportation risk, highlighting the role of families in program participation decisions. Most importantly, this paper examines the multifaceted effects of immigration enforcement through the joint dynamics of household income and participation across multiple programs. Because Medicaid participation declines despite reductions in household income that increase program demand and eligibility, the results provide stronger evidence that chilling effects can discourage program take-up.

The remainder of the paper proceeds as follows. Section 2 describes institutional background on E-Verify mandates and safety net programs. Section 3 describes the datasets, and Section 4 provides my empirical strategies. Section 5 presents the results on household income, program participation, and other health insurance coverage. Section 6 discusses chilling effects as a potential explanation for the decline in Medicaid participation, and Section 7 concludes.

2 Institutional Background

This section provides institutional details on E-Verify mandates, the policy of interest (Section 2.1), and the three safety net programs that serve as the main outcome variables (Sections 2.2

[81] Relatedly, more intensive Secure Communities enforcement is associated with lower TANF participation among Latino U.S.-born citizens (Pedraza and Zhu 2015).

and 2.3). Table A3.1 summarizes the key features of these programs. Following the main results on program participation (Section 5.2), Section 5.3 provides a more detailed comparison of the programs in terms of benefits and enrollment procedures to help interpret the findings.

2.1 E-Verify Mandates

Interior immigration enforcement programs targeting undocumented immigrants have intensified over the past few decades. Most of these reforms were designed to reduce undocumented immigration either by increasing the costs of unauthorized employment for both employers and undocumented workers (i.e., employment-based enforcement) or by enhancing cooperation between local law enforcement and federal immigration authorities (i.e., police-based enforcement).

E-Verify mandates are employment-based immigration enforcement measures introduced to reduce unauthorized employment. The mandates originated from the Immigration Reform and Control Act (IRCA) of 1986, which required all newly hired employees to complete the Employment Eligibility Verification Form (I-9) and made it illegal to knowingly employ individuals unauthorized to work in the U.S. E-Verify mandates require employers to screen employees for their work eligibility through the E-Verify system, an internet-based program that compares information from the I-9 form with records maintained by the Social Security Administration (SSA) and the Department of Homeland Security (DHS). If a worker's eligibility cannot be not confirmed, the employer receives a "tentative no confirmation" notice, indicating that the worker must resolve the mismatch within eight federal government workdays. Employers are required to dismiss workers who fail to resolve the issue.

The first E-Verify mandate was enacted in Georgia in 2006. Since then, more than 20 states have adopted E-Verify mandates, although the specific provisions vary across states. Table 3.1 lists all states that enacted some form of E-Verify mandates through 2019 and notes that Rhode Island revoked its legislation in January 2011.[82] To illustrate the spread of the policy across states, Figure 3.1 presents maps of states with E-Verify mandates in 2006, 2009, 2012, and 2016. As

[82]The results remain robust when excluding Rhode Island, the only state that revoked a previously enacted mandate. In this specification, the treatment variation comes solely from mandate enactment.

of 2019, 22 states—Alabama, Arizona, Colorado, Florida, Georgia, Idaho, Indiana, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Nebraska, North Carolina, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, and West Virginia—had enacted at least one type of the mandates.

E-Verify mandates increase employment verification, making undocumented workers more likely to be identified at hiring and therefore face non-hiring or job loss. These policies can also generate broader spillover effects across populations and domains. In the labor market, they may adversely affect even native workers if reduced hiring by firms outweighs the decline in competition from undocumented workers through general equilibrium effects (Chassamboulli and Palivos 2014; Chassamboulli and Peri 2015; Albert 2021), particularly among Hispanic natives who are more likely to share observable characteristics with undocumented immigrants (Passel and Cohn 2009; Lee, Shin, and Topa 2026). As a result, reduced employment opportunities may worsen financial conditions and increase demand for welfare programs among Hispanic citizens.

At the same time, E-Verify can make undocumented status more visible and salient in everyday life, contributing to a broader shift in the enforcement environment through heightened perceptions of government scrutiny. Although the policy is formally limited to employment verification, individuals may believe that information is shared with immigration enforcement agencies such as U.S. Immigration and Customs Enforcement (ICE), amplifying fear beyond the policy's legal scope.[83] This perceived risk may also spill over to individuals who are not directly subject to enforcement, especially those living in mixed-status households. Consequently, eligible individuals may avoid public programs out of concern that participation could expose household members facing a higher risk of deportation.

[83] Unlike police-based enforcement measures (e.g., 287(g), Secure Communities, omnibus immigration laws), failures in the employment verification process are less likely to directly result in deportation. However, because employment is a routine part of daily life, immigration status is verified more frequently in workplaces than during interactions with law enforcement or in contexts involving serious crimes.

2.2 Medicaid

Medicaid is a joint federal-state public health insurance program for children, adults, pregnant women, people with disabilities, and seniors who qualify based on low income or other criteria, providing coverage to nearly one-third of Americans (96 million in 2019).[84] As the largest in-kind transfer program in the U.S., Medicaid offers health coverage to eligible individuals either free of charge or at reduced medical costs. The program generally pays healthcare providers directly for medical services, although recipients may sometimes be required to pay small copayments. Established in 1965 as part of the "War on Poverty", Medicaid eligibility and coverage have expanded over time, particularly for pregnant women and children with higher incomes. Most notably, the Affordable Care Act (ACA) in 2014 extended Medicaid eligibility to all U.S. citizens and qualified residents with family incomes at or below 138% of the Federal Poverty Level (FPL).

Enrollment in Medicaid is not automatic, even for eligible individuals. Applications can be submitted through the Health Insurance Marketplace or directly to state Medicaid agencies by mail, in person, or online. To verify identity and determine eligibility, applicants must provide extensive information about all household members, regardless of whether they are applying for benefits. This information includes each household member's name, date of birth, marital status, race, Hispanic/Latino/Spanish origin, address, employer, income, disability status, pregnancy status, military service history, incarceration status, Social Security number (SSN), and citizenship or immigration documentation, which may reveal the undocumented status of household members or expose them to deportation risk.

Although Medicaid provides more affordable healthcare coverage than private insurance, whether employer-sponsored or privately purchased, increased interaction with the government may discourage individuals closely connected to undocumented immigrants from participating, even when eligible. This disincentive may be particularly strong in mixed-status households with undocumented immigrants or other non-citizens at risk of deportation. In addition, Medicaid ben-

[84]Total enrollment counts for Medicaid and the Children's Health Insurance Program (CHIP) are available at: https://www.medicaid.gov/medicaid/data-and-systems/downloads/macbis/2019-race-etncity-data-brf.pdf.

eficiaries in most states must renew their coverage annually to maintain benefits. Depending on the state, coverage is either renewed automatically or requires the submission of renewal forms and updated documentation verifying continued eligibility. To account for state-level differences in Medicaid policy, I include controls for Medicaid expansion and immigrant-related public health insurance policies in the main specification discussed in Section 4.

2.3 SNAP, SSI

In addition to Medicaid, the Supplemental Nutrition Assistance Program (SNAP) and Supplemental Security Income (SSI) are major means-tested safety net programs designed to support low-income families in the U.S. SNAP is the largest food assistance program in the U.S. and aims to improve food security and access to healthy diets among individuals in need. In fiscal year (FY) 2019, SNAP provided food benefits to approximately 35.7 million individuals.[85] Benefits are distributed through an Electronic Benefit Transfer (EBT) card, which can only be used to purchase groceries at authorized retailers. SSI is the largest cash assistance program for disabled children, working-age adults, and seniors with limited income or resources. As of December 2019, SSI covered approximately 8.0 million recipients, with disabled individuals accounting for 86% of all beneficiaries.[86]

Like Medicaid and other safety net programs, SNAP and SSI require applicants to provide detailed personal information about all household members. As a result, enrollment in these programs may involve the perceived risk of disclosing household members without legal status or those at risk of deportation. If individuals believe that the benefits from these programs do not outweigh this risk, they may choose to forgo participation to protect vulnerable family members. Since SNAP and SSI participation may also depend on the generosity of state transfer policies toward immigrants, I control for whether states provide food and cash assistance to lawful permanent residents in the main specification.

[85] The information is available at: https://fns-prod.azureedge.us/sites/default/files/resource-files/Characteristics2019-Summary-1.pdf.

[86] The statistics are from the Social Security Office of Retirement and Disability Policy: https://www.ssa.gov/policy/docs/chartbooks/fast_facts/2020/fast_facts20.html.

3 Data

3.1 CPS-ASEC

For the empirical analysis, I use individual-level data from the March Supplements of the Current Population Survey (CPS), also known as the Annual Social and Economic Supplement (ASEC) (Flood et al. 2023), covering the years 2001–2019. The CPS is a household survey conducted jointly by the U.S. Census Bureau and the Bureau of Labor Statistics that provides detailed information on individuals and households. The CPS is particularly well suited for this analysis because it identifies household composition and contains rich demographic information, including citizenship status, as well as information on insurance coverage, for a nationally representative sample.

I use the CPS-ASEC to examine changes in Medicaid participation as well as other forms of insurance coverage. Because E-Verify mandates are labor market enforcement measures, these outcomes provide a more comprehensive assessment of the policy's effects through changes in labor market conditions. For example, Ayromloo, Feigenberg, and Lubotsky (2020) show that E-Verify mandates reduced employment not only among probabilistically undocumented immigrants but also among documented Hispanics. As a result, the policy may have affected access to employer-sponsored insurance (ESI), as documented by Churchill (2021), which can function as a substitute for Medicaid. In addition, income changes resulting from shifts in employment may affect Medicaid eligibility and health insurance decisions more broadly.

To focus on individuals whose program participation decisions are more likely to be directly affected by E-Verify mandates, I restrict the sample to Hispanic citizens ages 18–64 living with Hispanic non-citizens. Hispanics are more closely connected to undocumented immigrants than other groups, as approximately 76% of the unauthorized immigrant population in the U.S. is Hispanic (Passel and Cohn 2009). In addition, Hispanic non-citizens are commonly used as a proxy for undocumented immigrants in prior empirical work because no dataset contains both legal status and the relevant outcome variables (Passel and Cohn 2009; Pope 2016; Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2018; Cascio, Lewis, and Zhang 2024).[87] More broadly, focusing on non-citizens may be appropriate in this context because deportation risk extends beyond undocumented immigrants. For example, lawful permanent residents account for roughly one in ten deportations, and many remain unaware that their legal status does not fully protect them from removal (Immigration Policy Center 2010; Patler and Jones 2025).

I combine individual-level data from the CPS-ASEC with state-level data on E-Verify mandate enactment dates from National Conference of State Legislatures (2024). A potential concern when using survey-based measures of program participation is underreporting (Meyer, Mok, and Sullivan 2009). This may generate discrepancies between actual take-up and reported participation, especially if policy changes differentially affect respondents' willingness to disclose program participation. To address this concern, I use data edited by the Census Bureau in the preferred specifications, which assigns Medicaid coverage to some respondents who did not report such coverage during the interview.[88] I also present results using the original reported values without the revision procedure to show that my estimates are robust to this adjustment.

Another potential threat to survey responses, and thus to the composition of sample, is the misreporting of Hispanic ethnicity or citizenship, as well as non-response to government surveys due to chilling effects. I directly examine potential compositional changes in the sample after presenting the estimation equation in Section 4.

3.2 ACS

Next, I use individual-level data from the American Community Survey (ACS) (Ruggles et al. 2024) for the reference years 2001–2019. As a repeated cross-sectional dataset covering

[87]The recent work of Cascio, Lewis, and Zhang (2024) shows that citizenship-based proxies better track changes in the share of legal Mexicans than alternative proxies, including those used by Borjas (2017). This approach is also consistent with evidence that Hispanics account for roughly three-quarters of the unauthorized population (Passel and Cohn 2009), while approximately 40% of noncitizens are estimated to be unauthorized based on estimates from the Census Bureau and the Department of Homeland Security (Hoefer, Rytina, and Baker 2012; Acosta, Larsen, and Grieco 2014).

[88]Specifically, the new variable is coded as "Yes" for children under age 21 whose householder or spouse is reported to have coverage, as well as for individuals who reported coverage through the Indian Health Service or indicated having "other government health care" or "other insurance" in a follow-up question about previously unreported health insurance coverage.

approximately 1% of the U.S. population, the ACS provides detailed demographic, economic, and program participation information for a nationally representative sample.

Compared to the CPS-ASEC, the ACS offers a substantially larger sample size. Because my analyses rely on identifying individuals whose household members face a higher risk of deportation, a large sample is essential for achieving sufficient statistical power. However, insurance variables, including Medicaid coverage, are available in the ACS beginning in 2008, after the first E-Verify mandates were enacted in 2006. Therefore, I use the CPS-ASEC for analyses of insurance coverage and the ACS for all other analyses, including the effects on SNAP and SSI participation and household income.

To maintain consistency across analyses, the ACS sample also consists of Hispanic citizens (ages 18–64) living with Hispanic non-citizens, which serves as a proxy for undocumented populations or individuals facing higher deportation risk, as discussed in Section 3.1. As a robustness check, the Appendix presents estimates using both CPS-ASEC and ACS data, separately for the full sample period and for the 2008–2019 period after excluding early-treated states.

3.3 Descriptive Statistics

Panel A of Table 3.2 presents descriptive statistics for health insurance variables from the CPS-ASEC, for the full sample (column 1) and separately by E-Verify enactment status (columns 2 and 3). I first classify insurance into public and private insurance. Because the sample is restricted to individuals ages 18–64, public insurance largely corresponds to Medicaid, as Medicare coverage among the elderly is excluded. I further divide private insurance into employer-sponsored insurance (ESI), provided through firms or unions, and non-ESI private insurance, which includes plans purchased directly from private insurers.

Comparing columns 2 and 3 by policy adoption reveals distinct participation patterns across insurance types. Medicaid participation rates are lower among Hispanic citizens in states with E-Verify mandates, for both the edited and reported values. Among private insurance categories, individuals in treated states are less likely to participate in ESI but more likely to hold non-ESI coverage.

Panel B of Table 3.2 reports descriptive statistics for other ACS variables, including SNAP and SSI participation and household income. In contrast to Medicaid, Hispanic citizens in states with E-Verify exhibit slightly higher participation rates in SNAP and SSI. Average household and labor income are both 13% lower among Hispanic citizens in treated states.

As the primary outcome, Figure 3.2-(a) presents time-series graphs for Medicaid participation among Hispanic citizens living with Hispanic non-citizens. With calendar years on the x-axis, the figure compares Medicaid participation rates between states that enacted E-Verify mandates at some point during the 2001–2019 sample period (i.e., treated states) and states that never enacted the mandates (i.e., never-treated states). Compared with the trends for non-Hispanics in Figure 3.2-(b), the gap between treated and never-treated states for Hispanics in Figure 3.2-(a) widens after the first E-Verify mandates were enacted in 2006 and grows further beginning in 2014, as participation rises in both groups but more rapidly in never-treated states.

4 Empirical Strategy

4.1 Estimation Framework

This section describes the empirical strategy used to estimate the causal effects of E-Verify mandates on program participation among Hispanic citizens living with Hispanic non-citizens, with a particular focus on Medicaid participation. My identification exploits variation in the timing of E-Verify mandate enactment across states, using a difference-in-differences (DID) framework. In particular, I implement the estimator developed by De Chaisemartin and d'Haultfoeuille (2024) (DCDH), which is robust to heterogeneous treatment effects. The main specification is:

$$Y_{ist} = \alpha + \beta E\text{-}Verify_{st} + \Omega' X_i + \Gamma' Z_{st} + \kappa_s + \lambda_t + \varepsilon_{ist} \tag{3.1}$$

Y_{ist} is the outcome variable for individual i in state s and year t. The primary outcome is Medicaid participation, and other outcomes include SNAP and SSI participation, health insurance coverage, and household income. $E\text{-}Verify_{st}$ is a treatment variable equal to 1 if an E-Verify mandate was enacted in state s by the end of year t, and 0 otherwise.[89] κ_s and λ_t denote state and year fixed effects, which control for time-invariant state characteristics and time-varying shocks common across states. In all specifications, standard errors are clustered at the state level to account for within-state correlation, and person-level weights from the CPS-ASEC or ACS are applied.

My research design relies on the assumption that, conditional on covariates, treatment and control states would have experienced the same trends in outcomes in the absence of E-Verify mandates (i.e., the parallel trends assumption). Under this assumption, β captures the effect of E-Verify enactment on Hispanic citizens living with Hispanic non-citizens. This assumption may be violated if there are time-varying factors correlated with the timing of E-Verify adoption. For example, treatment and control states may have experienced differential changes in economic conditions or differentially adopted other immigration-related policies affecting program participation or insurance coverage.[90] Such correlations between E-Verify adoption and potential confounders could threaten the causal interpretation of the results.

I take several approaches to address threats to the parallel trends assumption. First, I estimate event study versions of Equation 3.1 to examine dynamic treatment effects and pre-intervention placebo effects.[91] Next, I consider specifications with alternative sets of covariates to account for potential confounding factors. I control for individual-level covariates (X_i), including age, gender, race, marital status, and education level. I also include state-level covariates (Z_{st}), including police-based enforcement measures, Medicaid expansion, and immigrant program generosity, most

[89] I define treatment timing based on enactment dates because E-Verify mandates, unlike some other immigration policies, are relatively free from unavoidable implementation delays. Consistent with this, previous studies find that behavioral responses begin upon enactment of the mandates (Ayromloo, Feigenberg, and Lubotsky 2020). Nevertheless, the results remain robust to alternative approaches to assigning treatment timing.

[90] In particular, state economic conditions may be correlated with E-Verify adoption, while weaker economic conditions tend to increase program participation. States also differ in their political stance toward welfare programs. Although part of this reflects persistent political preferences captured by state fixed effects, political factors can also vary over time and influence the adoption of other policies potentially correlated with E-Verify. For example, states adopting E-Verify may be more likely to implement other immigration enforcement policies or less likely to adopt immigrant-inclusive health policies that affect program participation. Failing to account for these policies could bias the estimated effects of E-Verify adoption in either direction.

[91] To construct a balanced state-year panel, I estimate dynamic effects up to $t = 4$ and pre-treatment effects (i.e., placebos) up to $t = -5$.

of which capture other immigration-related policies adopted across states during the study period.[92]

Specifically, I control for police-based immigration enforcement programs, including 287(g) agreements and Secure Communities, which may affect outcomes for undocumented immigrants as well as Hispanic citizens.[93] During the period of E-Verify adoption, states also expanded access to public health insurance and safety net programs for low-income families and immigrants. Accordingly, I control for Medicaid expansion under the Affordable Care Act (ACA) and immigrant-related health policies by including indicators for whether states provided: (i) Medicaid to lawful permanent resident (LPR) pregnant women during their first five years of LPR status; (ii) public health insurance to LPR adults during the five-year bar; (iii) Medicaid to unauthorized immigrant pregnant women through either a state-funded program or the Children's Health Insurance Program (CHIP) option; (iv) food assistance to LPR adults during the five-year bar; and (v) cash assistance to LPRs, similar in value to SSI benefits, even after five years of LPR status (Urban Institute 2022).

In the main specification, I include all individual- and state-level controls discussed above, including changes in these relevant policies, and the results remain robust to their inclusion. However, since I cannot include state-by-year fixed effects, other state-specific time-varying changes coinciding with E-Verify enactment might remain uncontrolled. To address this concern, I also present results for *non*-Hispanic citizens in the Appendix, as they are less likely to be directly affected by E-Verify mandates but still reflect unobserved state-level time-varying factors.

The literature on potential bias in ordinary least squares (OLS) regressions with two-way fixed effects (TWFE) has grown rapidly, particularly when (i) treatment timing varies across units and (ii) treatment effects are heterogeneous across units and over time (Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024; De

[92]Since some states adopted E-Verify around the Great Recession (Table 3.1), differential labor market effects across states could have generated downstream effects on program participation. I do not include state-level unemployment rates in the main specification to avoid introducing a potentially endogenous control, although the results remain robust to their inclusion. To further address this concern, Figure A3.1 shows no strong evidence of differential trends in unemployment rates associated with E-Verify adoption, either before or after implementation. This remains true even when restricting the sample to the post-2008 years, during which unemployment rates fluctuated more substantially. Overall, this evidence alleviates concerns that the economic downturn confounds the estimated effects of E-Verify.

[93]Because these policies were implemented at the local level, I follow Churchill (2021) and the Urban Institute's coding scheme, under which a state is classified as treated if some or all counties with large immigrant populations implemented the policy.

Chaisemartin and d'Haultfoeuille 2024). Hence, I implement the DCDH estimator to account for potential heterogeneity across states adopting E-Verify mandates over time.[94] Unlike other alternative DID estimators, DCDH allows treatment to both 'turn on' and 'turn off,' as in the case of E-Verify mandates.[95]

4.2 Compositional Changes

This section presents several analyses to assess whether the results reflect behavioral responses rather than compositional changes arising from migration or survey reporting in response to E-Verify mandates. Because E-Verify mandates are enforced at the state level, Hispanic citizens, particularly those living with or closely connected to individuals at higher risk of deportation, may have moved out of states adopting these mandates. If such migration occurred selectively, changes in the composition of Hispanic citizens within each state could partly explain the estimated effects of the mandates.

A related concern is that E-Verify mandates may affect survey participation or reporting behavior. Under U.S. law, failure to respond to government surveys or intentionally providing false information can result in fines.[96] Nevertheless, immigration enforcement programs such as E-Verify mandates could still affect individuals' willingness to participate in surveys or accurately report demographic information, including citizenship and Hispanic ethnicity, for household members.

Overall, I do not find strong evidence that cross-state migration, survey non-response, or

[94] A diagnostic test proposed by De Chaisemartin and d'Haultfoeuille (2020) shows that 25% of the weights in the TWFE model are negative when using the CPS-ASEC, and this share increases to 30% when using the ACS. Because negative weighting under heterogeneous treatment effects may generate substantial bias, I use the DCDH estimator as the preferred approach. The parameter of interest is defined as a weighted average of treatment effects among groups switching into and out of treatment. Specifically, states adopting E-Verify mandates are compared with states that remained untreated in both periods, rather than with states already treated in both periods, while states revoking mandates are compared with states that remained treated in both periods. The key identifying assumption is that if a group switches treatment status from being untreated (treated) to treated (untreated) between any two periods, there exists another group that remains untreated (treated) in both periods, referred to as a 'stable' group. Additionally, as with other DID estimators, the parallel trends assumption between switcher and stable groups is required.

[95] To the best of my knowledge, this is the only heterogeneity-robust DID estimator to date that remains valid when treatment timing varies across groups and some groups later opt out of the treatment.

[96] Information on the legal authority requiring participation in the survey is available at https://www.census.gov/programs-surveys/acs/respond/faqs.html.

misreporting in response to E-Verify mandates generated compositional changes in my sample. Table A3.2 shows that E-Verify mandates had no statistically significant effect on the share of citizens (column 1) or Hispanic citizens (column 2) among all survey respondents ages 18–64, nor on the probability of living with Hispanic non-citizens (column 3). The mandates also did not significantly affect the probability that Hispanic citizens living with Hispanic non-citizens moved across states within the past year (column 4), providing more direct evidence against migration responses. The remaining columns (5–8) examine whether E-Verify mandates affected observable characteristics in the sample and find no statistically significant effects.

Together, these findings suggest that E-Verify mandates had limited effects on the composition and observable characteristics of survey respondents in my sample.[97] All event study graphs for the results discussed in this section are presented in Figure A3.2.

5 Results

This section presents the main results on program participation and health insurance coverage following the enactment of E-Verify mandates. Changes in program participation can be decomposed into two components: (i) changes in eligibility and (ii) changes in take-up among eligible individuals. Accordingly, I first examine the effects on income to assess how the mandates affected program need and eligibility before turning to their effects on participation.

5.1 Effects on Household Income

Figure 3.3 and Table 3.3 present the estimation results for household income. Income variables are adjusted to 2019 dollars, and weights are adjusted for the number of household members in the sample. The DID estimate in column 1 of Table 3.3 indicates that E-Verify mandates reduced total household income by $3,402 (a 4.5% decline) among Hispanic citizens living with Hispanic

[97]However, prior studies find that E-Verify mandates reduced the share of likely undocumented immigrants (Churchill 2021) and the share of the immigrant population that is young and male (Chalfin and Deza 2020), which could affect the characteristics of undocumented immigrants in mixed-status families. To the extent that those who out-migrate are more likely to experience deportation-related fear, the estimates based on the remaining population may represent an upper bound of the true policy effects if compositional changes due to migration are present.

non-citizens.

As a form of labor market immigration enforcement, the decline in household income likely reflects adverse labor market effects on Hispanic citizens. Column 2 reports an estimate for household labor income, defined as the sum of wage and salary earnings across all household members, which accounts for 86.6% of total household income in my sample. The estimated \$3,508 decline in household labor income (a 5.3% decline) suggests that the reduction in total household income was primarily driven by lower earnings from employment. Consistent with this interpretation, column 3 shows a small but statistically significant 1.7 percentage-point decline in the employment rate of Hispanic citizens living with Hispanic non-citizens.[98]

In Table A3.3, I present several robustness checks by comparing alternative specifications for household income that differ in the unit of analysis, control variables, and winsorization. Panel A reports DID estimates using the individual-level sample with weights adjusted for the number of household members. Although the estimated declines in household income are slightly smaller than the baseline estimates (columns 1 and 4), they remain robust to excluding controls (columns 2 and 5) and to winsorizing income values at the 2.5th and 97.5th percentiles (columns 3 and 6), which replaces extreme values at each tail of the distribution. Panel B reports estimates obtained after collapsing the data to the household level. All estimates remain similar to those in Panel A, confirming a 3–5% decline in household income. Event study graphs for income variables without controls or weights are presented in Figure A3.3.

In sum, Hispanic citizens experienced worsening financial conditions following the enactment of E-Verify mandates. Because income is a primary eligibility criterion for safety net programs, the resulting decline in income likely expanded both program demand and eligibility.

[98]This is consistent with prior empirical evidence that E-Verify mandates reduced employment among probabilistically documented Hispanics (Ayromloo, Feigenberg, and Lubotsky 2020). As a mechanism underlying these spillover effects on native workers, prior studies document general equilibrium effects of immigration in the labor market using dynamic search-and-matching models (Chassamboulli and Palivos 2014; Chassamboulli and Peri 2015; Albert 2021). In the context of E-Verify mandates, reduced job creation by firms can outweigh the decline in labor market competition from undocumented immigrants despite their substitutability, particularly for Hispanic natives who are more likely to share observable characteristics with undocumented populations (Passel and Cohn 2009; Lee, Shin, and Topa 2026).

5.2 Effects on Program Participation

Table 3.4 presents the main results on the effects of E-Verify mandates on program participation among mixed-status families using the DCDH estimator across three programs: Medicaid, SNAP, and SSI. Consistent with the role of safety net programs during periods of economic hardship, columns 2 and 3 of Table 3.4 show that SNAP and SSI participation among Hispanic citizens living with Hispanic non-citizens increased by approximately 0.1 and 0.3 percentage points, respectively, following the enactment of E-Verify mandates. Relative to the pre-treatment participation rates, these estimates correspond to increases of 0.8% and 15.8%, although the estimate for SNAP is not statistically significant.

In contrast, column 1 shows that Medicaid participation among Hispanic citizens living with Hispanic non-citizens declined by an average of 8.4 percentage points following the enactment of E-Verify mandates. This estimate corresponds to a 41.4% decline relative to the pre-treatment mean. As discussed in Section 5.1, the decline in household income following the mandates implies increased eligibility for safety net programs.[99] Therefore, the decline in Medicaid participation indicates lower take-up among Hispanic citizens.

The underlying identifying assumption is that there are no state-specific time-varying factors correlated with the enactment of E-Verify mandates. To assess this assumption, Figures 3.4 and 3.5 present the main event study estimates of the effects of E-Verify mandates on participation in the three programs. I set period $t = -1$ as the reference period. In each period, the blue dots represent DID estimates exploiting spatial and temporal variation in the enactment of E-Verify mandates, along with 95% confidence intervals. Because the pre-treatment estimates are close to

[99] As a complementary exercise, I estimate the effects on Medicaid eligibility by constructing indicator variables based on state-year Medicaid income eligibility limits and federal poverty guidelines. The estimates show that E-Verify mandates increased Medicaid eligibility among Hispanic citizens by approximately 25%, with statistical significance at the 1% level. A key limitation is that the ACS does not contain sufficient information to precisely construct Modified Adjusted Gross Income (MAGI) for Medicaid eligibility. As a result, reported household income in the ACS likely exceeds Medicaid MAGI, causing the constructed eligibility measure to understate the probability of being income-eligible for Medicaid. The wider confidence intervals in the pre-periods, which are specific to this outcome, also suggest additional noise in the constructed eligibility measure due to sensitivity around the eligibility cutoff. Nevertheless, unless the discrepancy between ACS income and Medicaid MAGI is systematically related to E-Verify adoption, this measurement error should not substantially bias the estimates. Overall, the eligibility results are consistent with the decline in income and suggest that income losses translated into increased Medicaid eligibility.

zero and statistically insignificant in nearly all periods, the results are consistent with the parallel trends assumption and support a causal interpretation of the estimates.

The post-treatment estimates in Figure 3.4 show that Hispanic citizens living with Hispanic non-citizens reduced Medicaid enrollment over time, consistent with the average decline reported in column 1 of Table 3.4. Although the average and dynamic effects on SNAP participation in Figure 3.5-(a) are modest, Figure 3.5-(b) shows a statistically and economically significant increase in SSI participation immediately following the enactment of E-Verify mandates. This increase of 0.6 percentage points corresponds to a 36.9% increase relative to the pre-treatment mean.

Across all three programs, the results remain robust to excluding controls and weights (Figure A3.4 and Table A3.4). To further assess robustness, Table A3.5 reports estimates using both the CPS-ASEC (Panel A) and the ACS (Panel B). I also estimate the effects separately for the full period from 2001 to 2019 (columns 1–3) and for the restricted period from 2008 to 2019 (columns 4–6), excluding early-treated states that enacted mandates before 2008.[100] This comparison helps assess the extent to which the data source or restricting the analysis to later variation affects the estimates.[101] The sharp decline in Medicaid participation and the increase in SSI participation, concentrated in the short run, remain stable across data sources and sample periods.

As a placebo analysis, I examine program participation responses among non-Hispanic citizens, who are less likely to be directly affected by E-Verify mandates.[102] Table A3.6 presents estimates separately for the full sample and for White and Black populations. Nearly all estimates have the same sign as the main results in Table 3.4. Both the decline in Medicaid participation (columns 1–3) and the increase in SSI participation (columns 7–9) are concentrated among Whites (columns 2 and 8). Although the 0.1 percentage-point increase in SSI participation is much smaller

[100]This is because insurance variables, including Medicaid coverage, became available in the ACS only in 2008, as discussed in Section 3.2. The early-treated states are Colorado, Georgia, North Carolina, Arizona, and Oklahoma.

[101]The estimates from the ACS in Panel B have smaller standard errors because of the larger sample size, which improves statistical significance, particularly for SSI. Restricting the sample period to later years (columns 4–6) makes the coefficients more negative, leading to smaller increases or larger declines across all programs. Overall, the estimates for SNAP are noisier than those for the other two programs, with larger differences in pre-treatment means and different signs for the DID coefficients.

[102]The purpose of this analysis is to test for other state- and time-varying changes that may differentially affect program participation and not be fully captured by the baseline controls.

in magnitude (a 4.1% increase) than the 15.8% increase in the main sample, the average effect on SSI participation among non-Hispanics remains non-negligible and statistically significant, unlike the minimal effect on Medicaid participation.

Nevertheless, the event study graphs for non-Hispanic citizens in Panel A of Figure A3.5, which use the same y-axis scale as Figures 3.4 and 3.5 for the main sample, show different dynamic treatment effects for Medicaid and SSI participation. The figures provide no evidence of a significant decline in Medicaid participation (Figure A3.5-A(a)) and, more importantly, no immediate increase in SSI participation (Figure A3.5-A(c)), in contrast to the main event study results for Hispanics (Figure 3.5-(b)). These patterns are also absent in Panel B for low-educated non-Hispanic citizens, who are more demographically similar to the sample of Hispanic citizens. Overall, the results for non-Hispanic citizens suggest that E-Verify enactment led to a substantial decline in Medicaid participation and a short-run increase in SSI participation primarily among Hispanic citizens with family members more likely to face deportation risk.

Lastly, to examine how individuals differentiate across programs in their enrollment decisions, Table A3.7 presents estimates for joint program participation outcomes. I first examine whether the increase in SSI participation and the decline in Medicaid participation are concentrated among the same individuals. The DID estimates show that the increase in SSI participation (column 2) following E-Verify mandates is concentrated among Medicaid participants (column 3). At the same time, the sharp decline in Medicaid participation (column 1) is driven primarily by non-SSI participants (column 6), while participation among SSI recipients increases slightly (column 3).

These results suggest that individuals increasing SSI participation are unlikely to account for the overall decline in Medicaid participation. One possible explanation is that SSI recipients, who are individuals with disabilities, may be more vulnerable to economic hardship because of challenges in labor market participation. As a result, eligible individuals may have stronger incentives to enroll in safety net programs, particularly immediately following adverse income shocks.

Further dividing the decline in Medicaid participation among non-SSI participants (column

6) by SNAP participation, roughly one-third of the estimated decline is concentrated among SNAP participants (column 7), while the remaining two-thirds is concentrated among those not participating in SNAP (column 8). Thus, the decline in Medicaid participation is concentrated primarily among individuals not participating in other programs, suggesting limited substitution into other safety net programs.

5.3 Interpretation of the Results

In response to E-Verify mandates, I find a short-run increase in SSI participation, no significant effect on SNAP participation, and a substantial decline in Medicaid participation among Hispanic citizens living with Hispanic non-citizens (Section 5.2). In this section, I examine how comparisons with related studies and institutional differences across programs help explain these heterogeneous responses, even though all three programs target low-income populations.

An enforcement policy creates two offsetting forces on program participation: (i) increased demand and eligibility arising from adverse income shocks and economic hardship (Section 5.1), and (ii) reduced take-up due to heightened perceived risk for family members facing deportation (i.e., chilling effects). As a result, participation may increase in some programs and decline in others depending on the relative strength of these competing forces, which might vary with enforcement mechanisms and program characteristics.

I first compare my estimates with those in the related literature. As the study most closely related to this paper, Alsan and Yang (2024) show that the activation of Secure Communities—an example of police-based enforcement that strengthens cooperation between local law enforcement and federal immigration authorities—led to a 2.1 percentage-point decline in SNAP participation (a 10% decline) and a 1.7 percentage-point decline in SSI participation (a 30% decline) among Hispanic citizens. Following E-Verify mandates, I also find a notable decline in Medicaid participation, consistent with the broader pattern of reduced participation under Secure Communities, alongside limited effects on SNAP participation and a short-run increase in SSI participation.

These differences likely reflect the distinct effects of policies within the same category of immigration enforcement that operate through different enforcement mechanisms. Because Secure Communities more directly increases deportation risk than E-Verify mandates—which operate through the hiring margin and extend to other domains—chilling effects may dominate other forces, including the income channel, and reduce program take-up. This interpretation is consistent with the limited or smaller adverse effects of Secure Communities on natives' labor market outcomes and income (East et al. 2023; Alsan and Yang 2024) relative to E-Verify mandates, as discussed in Section 5.1.

Next, I examine whether differences across programs and evidence from additional analyses can account for why E-Verify mandates generated different participation responses. Table A3.1 compares the three programs across several dimensions related to benefits and enrollment procedures. Panel B focuses on benefit characteristics, including type, amount, and redemption. Unlike in-kind transfers with usage restrictions—such as medical services under Medicaid and food benefits under SNAP—cash assistance through SSI can be spent freely and may therefore be more attractive, especially during periods of income decline.[103] Although SNAP functions as a near-cash benefit, SSI also provides substantially larger transfers. As of December 2019, the average monthly SSI benefit was $566 per participant, compared to $121 for SNAP.

Medicaid differs from SNAP and SSI in that it provides access to affordable medical care rather than direct, regularly distributed (near-)cash benefits. As a result, it may be more likely to evoke perceptions of surveillance and concerns about personal information being disclosed or shared, since recipients must interact more frequently with institutions to verify coverage when using medical services (Pedraza, Nichols, and LeBrón 2017). In addition, because Medicaid primarily insures against medical expenditures and future health risks, individuals with greater ability to absorb health-related expenses or less immediate demand for medical care may be more willing to forgo enrollment.

The next two analyses support this interpretation. First, to examine how the decline in Medicaid enrollment varies with the ability to afford medical expenditures, Table A3.8 presents

[103] Prior work finds that the cash-equivalent value of SNAP benefits is nearly full in some cases, while other estimates suggest it is about 80 percent for the average distorted recipient (Moffitt 1989; Whitmore 2002).

estimates for joint Medicaid participation and indicators for three household income groups. In each column, the outcome is an indicator equal to one if an individual participates in Medicaid and belongs to a given income tercile: bottom 33 percent, middle 33 percent (33–66 percent), and top 33 percent. The DID estimates show that the decline in Medicaid participation is not concentrated among the lowest-income households (column 2), where the effect is small and statistically insignificant. Instead, the declines are larger and statistically significant among households in the middle and upper parts of the income distribution (columns 3–4). This pattern suggests that the overall decline in Medicaid participation is driven primarily by higher-income households, which may be better able to absorb medical costs, while lower-income households remain more reliant on Medicaid benefits.

Second, the heterogeneity analysis by age in Table A3.9 shows differential responses based on expected demand for program benefits, especially medical care. Since older individuals are more likely to use both routine and unexpected medical care, younger individuals may find it relatively easier to forgo enrollment. Consistent with these life-cycle considerations, E-Verify mandates had a larger negative effect on Medicaid participation among younger individuals (ages 18–34) (column 1), with a 10.5 percentage-point decline, compared to older individuals (ages 35–64) (column 2), who experienced a 4 percentage-point decline. As complementary evidence, younger individuals instead tend to retain SNAP (column 3) and SSI benefits (column 5) as more direct forms of income support, potentially due to limited resources or liquidity constraints, although the estimates are not statistically different across age groups.

Panel C of Table A3.1 offers another explanation for heterogeneous responses across programs by suggesting that flexibility in enrollment may delay or deter participation. Compared with Medicaid and SNAP, SSI offers fewer alternative enrollment pathways beyond standard applications through program offices or online systems. In contrast, Medicaid allows more flexible, need-based enrollment through special enrollment periods and health care providers (e.g., hospitals and community-based organizations), which may reduce the cost of delayed enrollment.[104]

[104]For example, presumptive eligibility provides immediate temporary coverage based on preliminary information. Medicaid also allows retroactive coverage of pre-application expenses, conditional on meeting eligibility criteria.

In short, the relative importance of the income channel and chilling effects as competing forces may vary across immigration enforcement mechanisms and program characteristics. When individuals reduce participation in anti-poverty programs despite economic hardship, they may be more likely to forgo Medicaid than SNAP or SSI because Medicaid involves more frequent institutional interactions, addresses less immediate needs, and allows greater flexibility in enrollment timing. Given the nontrivial role of the income channel—evidenced by the short-run increase in SSI participation—the substantial decline in Medicaid enrollment still points to the importance of chilling effects as an underlying mechanism, as discussed in Section 6.

5.4 Robustness and Heterogeneity

As the primary outcome, I conduct additional robustness checks on Medicaid participation. First, I directly examine potential measurement error and misreporting in Medicaid participation. If there is endogenous underreporting of Medicaid participation in response to E-Verify mandates, the estimates may capture both actual declines and changes in reporting behavior. To address this concern, I compare estimates using two measures of Medicaid participation in the CPS-ASEC: the self-reported measure and an alternative measure edited by the Census Bureau that assigns Medicaid coverage to some respondents who did not report such coverage in the survey, as discussed in Section 3.1.

Panel A of Table A3.10 shows that the baseline results using the Census Bureau's edited Medicaid measure remain robust across different sets of controls, with DID estimates ranging from declines of 6.9 to 8.4 percentage points. Using the same controls as in Panel A, Panel B reports DID estimates based on the reported Medicaid measure before the Census Bureau's revision. The estimates in Panel B are very similar to those in Panel A, with the pre-treatment mean slightly lower by 1.1 percentage points. These findings suggest that the estimates are not substantially affected by changes in reporting of Medicaid participation in response to E-Verify mandates.[105]

[105] In unreported results, the estimates also remain robust after excluding observations where reported and imputed Medicaid participation values do not match, suggesting that E-Verify mandates had limited effects on the reporting of Medicaid participation.

I conduct a permutation test as a falsification exercise to assess the impact of E-Verify mandates on Medicaid participation, ensuring that the findings reflect actual policy effects rather than spurious correlations. In this test, I randomly reassign states as treated or untreated while holding the original enactment dates of E-Verify mandates fixed, as shown in Table 3.1. Under these counterfactual assignments, the average estimated effect of E-Verify mandates on Medicaid participation should be approximately zero.

I repeat the reassignment procedure 1,000 times and plot the counterfactual DID estimates as a histogram in Figure A3.6. The resulting distribution is bell-shaped, with a mean of 0 and a standard deviation of 0.031. The vertical red line represents the original DID estimate of -0.084 for the effect of E-Verify mandates on Medicaid participation among Hispanic citizens living with Hispanic non-citizens (column 1 of Table 3.4). The p-value from the permutation test is 0.016, indicating that the finding is unlikely to have arisen by chance.

Next, I examine heterogeneous effects of E-Verify mandates by mandate scope in the labor market and by demographic characteristics of Hispanic citizens. Column 2 of Table A3.11 presents estimates of declines in Medicaid participation by mandate type using the TWFE estimator.[106] The decline in Medicaid participation is larger under Universal E-Verify mandates, with a decrease of 5.3 percentage points, compared to a 4.9 percentage-point decline under Public mandates. These differential effects are consistent with the larger labor market impacts of Universal mandates documented in prior studies (Amuedo-Dorantes and Bansak 2014; Churchill 2021), as employment verification is required across all sectors under Universal mandates.[107]

Lastly, Table A3.12 presents results from three heterogeneity analyses based on demo-

[106]Unlike the other analyses in this paper, I use the TWFE estimator to estimate the effects of Universal and Public E-Verify mandates separately without dropping treated states. This approach makes the estimates comparable to those from a specification using a single treatment variable that combines both types of mandates. As an additional robustness check, column 1 of Table A3.11 reports the average DID estimate using the TWFE estimator instead of the DCDH estimator. The TWFE estimate indicates a 5 percentage-point decline in Medicaid participation at the 5% significance level (a 24.5% decline), smaller in magnitude than the DCDH estimate of an 8.4 percentage-point decline (a 41.4% decline). As discussed in Section 4.1, the high share of negative weights, ranging from 25% to 30%, highlights the need for alternative estimation methods. Accordingly, I use the DCDH estimator as the preferred approach throughout the paper.

[107]Nevertheless, because the effect of Public E-Verify mandates on Medicaid participation remains non-negligible, I combine Universal and Public mandates into a single treatment variable in all other analyses.

graphic characteristics. When dividing the sample by age, as discussed in Section 5.3, the larger negative effects of E-Verify mandates on younger individuals (ages 18–34) (column 1), relative to older individuals (ages 35–64) (column 2), suggest differential responses based on expected health care use and demand for Medicaid benefits. When dividing the sample by education, Hispanic citizens without a high school degree (column 3) exhibit a larger decline in Medicaid participation than those with higher educational attainment (column 4). Finally, the adverse effect of E-Verify mandates on Medicaid participation among Hispanic citizens is substantial for both males and females relative to pre-treatment means, although the DID estimates in percentage-point terms are slightly larger for females (column 6) than for males (column 5).

5.5 Effects on Health Insurance Coverage

Previous results show that E-Verify mandates reduced Medicaid participation among Hispanic citizens living with Hispanic non-citizens, while providing limited evidence of comparable declines in SNAP or SSI participation. Before turning to spillovers from deportation fear as a primary mechanism underlying the decline in Medicaid participation (Section 6), I examine whether part of the decline reflects substitution toward other forms of health insurance.

Table 3.5 shows how the mandates affected insurance coverage among Hispanic citizens living with Hispanic non-citizens across different types of insurance, in addition to Medicaid. I find no significant effect on overall private insurance coverage (column 2) and a 2.9 percentage-point decline in employer-sponsored insurance (ESI) (column 3), a component of private insurance.

In contrast, I find a 0.9 percentage-point increase in the share of Hispanic citizens with other types of private insurance excluding ESI (non-ESI) (column 4). This increase suggests some substitution from Medicaid to private insurance, alongside reduced access to ESI due to employment declines (Section 5.1). However, this substitution appears limited, as reflected in the 8.6 percentage-point increase in the uninsured rate (column 1). The decline in household income (Section 5.1) may help explain why many individuals did not transition from Medicaid to private insurance, particularly non-ESI coverage, as higher premiums may have made such coverage unaffordable and left some individuals uninsured.

6 Mechanism: Chilling Effects

In this section, I examine whether chilling effects, or fear among Hispanics, help explain the decline in Medicaid participation among Hispanic citizens living with Hispanic non-citizens. As the term “chilling” suggests, an icy policy environment can affect program participation even among eligible individuals (Watson 2014). Although they are not themselves subject to deportation, individuals may worry about exposing household members who face deportation risk. Consequently, they may become reluctant to interact with the government or participate in government-related programs such as Medicaid to avoid drawing attention to household members with undocumented status or deportation risk.

I conduct three heterogeneity analyses based on deportation risk to support this mechanism. First, I estimate the policy effect on Medicaid participation among Hispanic citizens who are *not* living with Hispanic non-citizens and compare it to the estimate for the original sample. As discussed in Section 3.1, Hispanic non-citizens serve as a proxy for undocumented populations or individuals facing higher deportation risk. If individuals are concerned about the deportation of family members, I should observe a larger reduction in Medicaid participation among those living with Hispanic non-citizens.

Figure A3.7 and column 2 of Table 3.6 show that Medicaid participation among Hispanic citizens not living with Hispanic non-citizens declined by only 1 percentage point following the enactment of E-Verify mandates, and the estimate is statistically insignificant. In contrast, the average effect for the original sample is an 8.4 percentage-point decline (column 1). These results suggest that the adverse effect on Medicaid enrollment is concentrated primarily among individuals living with Hispanic non-citizens. This provides strong evidence that spillovers from deportation fear lead individuals to worry about the potential deportation of household members facing deportation risk, causing them to forgo benefits from government programs, particularly Medicaid.

The remaining two heterogeneity analyses in Table 3.7, motivated by Alsan and Yang

(2024), further investigate fear as a key channel underlying lower Medicaid enrollment. One analysis compares DID estimates across subsamples defined by the Hispanic origin of non-citizens, which serves as a proxy for deportation risk. Among Hispanics, Puerto Ricans cannot be deported because they are U.S. citizens, while Cubans face a lower deportation risk because they are more likely to have political refugee status. In addition, El Salvador is one of the countries whose nationals have been eligible for Temporary Protected Status (TPS) in the U.S. since 2001.[108] Because TPS allows beneficiaries to remain and work legally in the U.S., Salvadorans with TPS are protected from deportation. If Hispanic citizens are reluctant to participate in Medicaid because of concerns about the deportation of household members, I would expect to observe a larger decline in Medicaid participation when Hispanic non-citizens in the household face greater deportation risk.

In columns 1 and 2 of Table 3.7, I divide Hispanic non-citizens into two groups—Cubans or Salvadorans, and all others—since using Hispanic non-citizens as a proxy for undocumented immigrants already excludes Puerto Ricans. The decline in Medicaid participation is larger among Hispanic citizens living with Hispanic non-citizens other than Cubans or Salvadorans (column 2) than among those living with Cuban or Salvadoran non-citizens (column 1). This pattern is consistent with greater fear among the former group, whose household members are more likely to face deportation risk.

The final analysis estimates separate effects depending on whether states had sanctuary jurisdictions that limited cooperation with U.S. Immigration and Customs Enforcement (ICE) during the period of E-Verify enactment. For example, sanctuary jurisdictions may prohibit police officers or city employees from questioning individuals about their immigration status or from detaining individuals beyond their release date at the request of immigration authorities. Because sanctuary jurisdictions may reduce fear of immigration enforcement spillovers, states with sanctuary cities may exhibit smaller declines in Medicaid participation if deportation fear affects program take-up.

In columns 3 and 4 of Table 3.7, I divide the sample based on whether states had sanctuary

[108] The information is available on the U.S. Citizenship and Immigration Services website: https://www.uscis.gov/humanitarian/temporary-protected-status/temporary-protected-status-designated-country-el-salvador.

cities, using the list of jurisdictions in US Immigration and Customs Enforcement (2017) as a time-invariant proxy for attitudes toward undocumented immigrants.[109] Consistent with the previous results, the decline in Medicaid participation is concentrated among Hispanic citizens in states without sanctuary cities (column 4), who are more likely to experience deportation fear, relative to those in states with sanctuary cities (column 3). Thus, the results from all three heterogeneity analyses are consistent with chilling effects. Event study graphs for the subsamples used in the latter two analyses are presented in Figure A3.8.[110]

Lastly, following Alsan and Yang (2024), I examine whether Google searches related to deportation increased after the enactment of E-Verify mandates, as shown in Figure A3.9.[111] Since Google search data begin in 2004, unlike other datasets, I report several event study figures using different state samples. To maintain a balanced state-year panel, Figures A3.9-(b) and (c) exclude states with earlier enactments in 2006 and 2007, while Figure A3.9-(a) includes all states using an unbalanced panel. Across specifications, the roughly 40% to 50% increase in deportation-related searches suggests that fear of deportation may contribute to lower Medicaid participation among mixed-status families.

7 Conclusion

This paper examines the effects of E-Verify mandates on participation in safety net programs in mixed-status households, focusing on Medicaid, SNAP, and SSI. The policy requires employers to verify workers' legal status and may generate spillover effects beyond undocumented immigrants and the labor market. In particular, E-Verify may negatively affect even native workers if reductions in firm hiring exceed declines in labor market competition from undocumented immi-

[109] Specifically, the estimates are reported separately for the 9 E-Verify states with at least one sanctuary city (column 3) and the 14 E-Verify states without sanctuary cities (column 4). The 28 states without E-Verify mandates through 2019 are included in both specifications as the control group.

[110] The event study estimates for individuals whose non-citizen family members are of Cuban or Salvadoran origin (Figure A3.8-A(a)) are less stable in the pre-treatment period, potentially because of the smaller sample size. Except for this subgroup, all graphs show near-zero estimates before mandate enactment and larger post-enactment declines in Medicaid participation among groups more likely to fear the deportation of family members.

[111] The construction of the normalized search intensity for eight deportation-related terms using Google Trends data (2004–2019) is detailed in the figure notes.

grants, while heightened government scrutiny may also generate chilling effects among individuals not directly subject to deportation.

Using a difference-in-differences (DID) framework that exploits spatial and temporal variation in policy enactment, I estimate the effects of E-Verify mandates on Hispanic citizens living with Hispanic non-citizens facing higher deportation risk. I first document a decline in household income following the mandates, driven by adverse spillovers in the labor market. Despite the resulting increase in program demand and eligibility, I find a short-run increase in SSI participation, no significant effect on SNAP participation, and a substantial decline in Medicaid participation. These differential policy effects across programs align with differences in benefit characteristics and enrollment flexibility.

Further analyses support chilling effects as a plausible explanation for the decline in Medicaid participation. Although citizens themselves are not at risk of deportation, they may forgo public benefits to avoid exposing family members facing deportation risk. Consistent with this mechanism, Hispanic citizens living with Hispanic non-citizens, particularly those at higher deportation risk, experienced larger declines in Medicaid participation.

This paper has several policy implications. First, the findings highlight the importance of understanding the indirect or unintended effects of social reforms. Immigration enforcement programs are primarily intended to reduce undocumented immigration and protect U.S. citizens. However, if citizens are also adversely affected through spillover effects, as suggested by my findings, a more careful evaluation is needed to fully account for the broader consequences of these policies.

Second, the results suggest that participation decisions in safety net programs depend not only on economic need, but also on fears associated with immigration enforcement. The increase in SSI enrollment was only temporary, while Medicaid enrollment even declined despite lower household income, suggesting limited program coverage during periods of economic hardship and increased program demand. Non-participation in welfare programs can restrict access to affordable and timely health care and financial assistance, leaving disadvantaged families vulnerable to substantial losses. Future research on program participation decisions will be important for expanding program coverage and improving the welfare of low-income households.

Figures and Tables

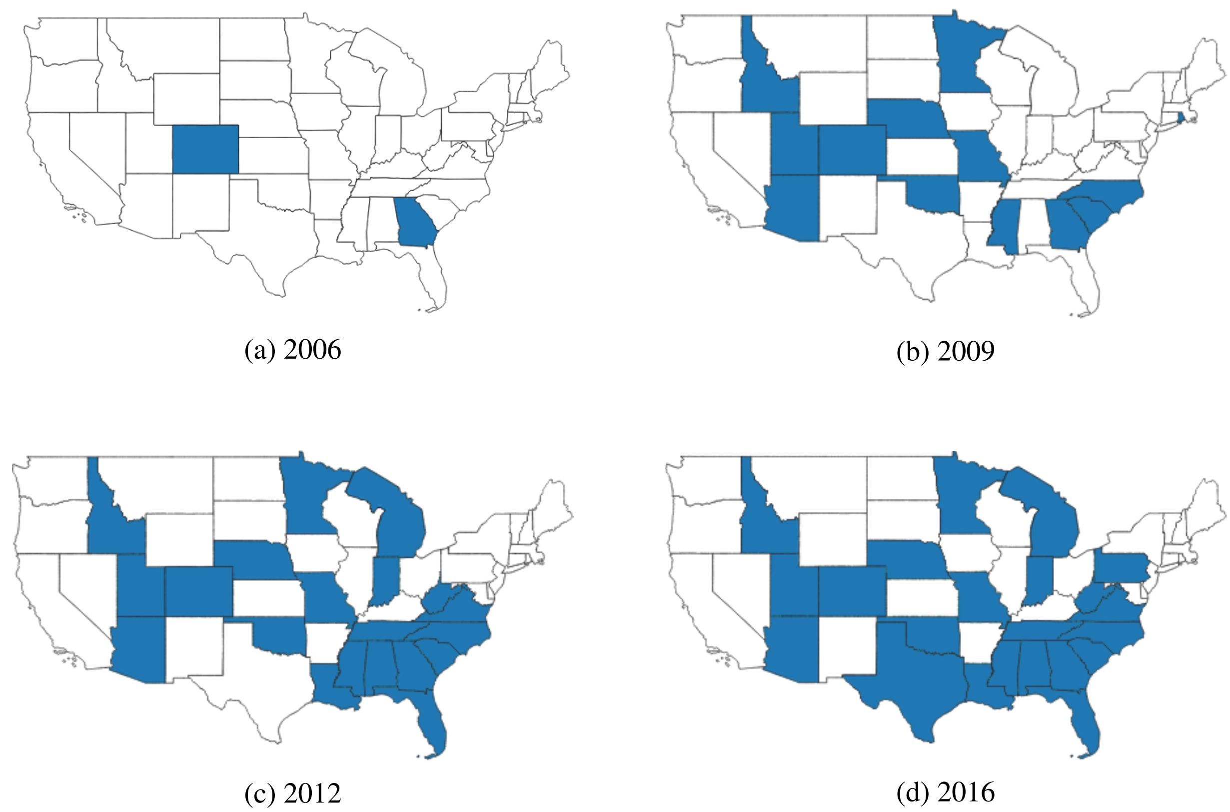
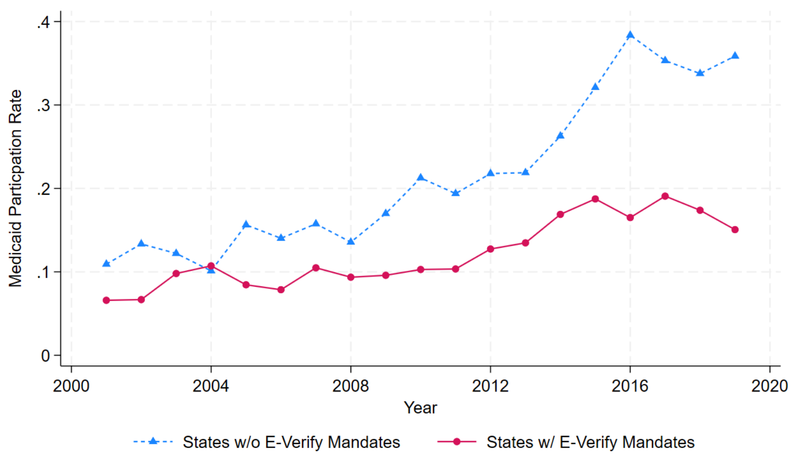
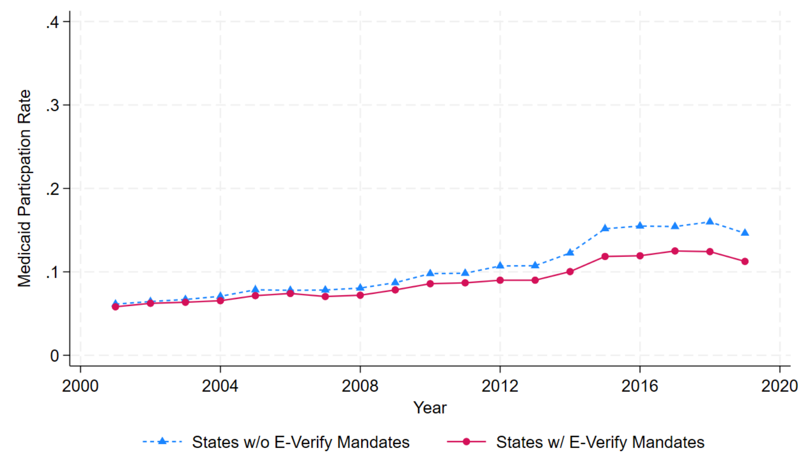


Figure 3.1: Enactment of E-Verify Mandates by State and Year

Notes: Figure 3.1 presents maps of states with E-Verify mandates by year, including both universal and public E-Verify mandates.
Source: State Immigration Policy Resource from Urban Institute



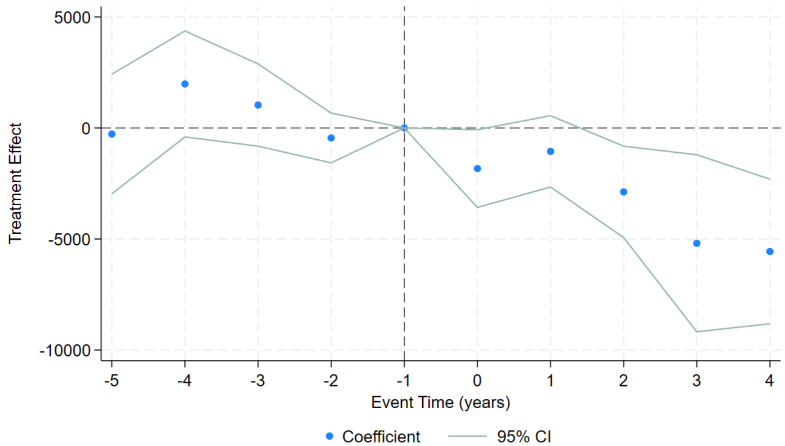
(a) Hispanics



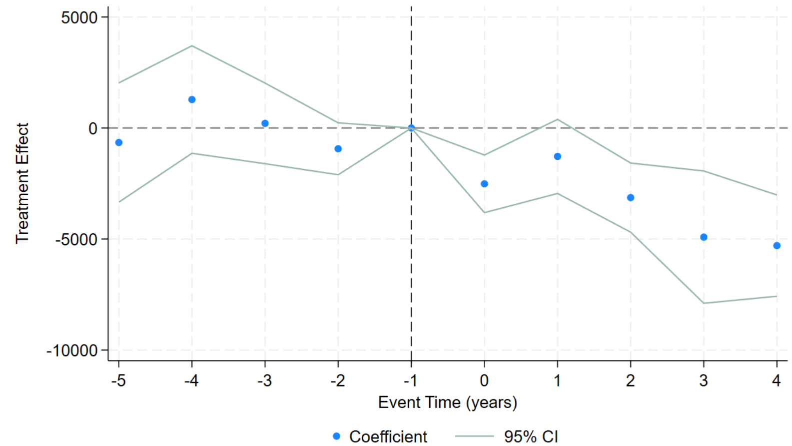
(b) Non-Hispanics

Figure 3.2: Time Series Graphs of Medicaid Participation Rate

Notes: Figure 3.2 shows trends in Medicaid participation rates over time. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens in Figure 3.2-(a), while Figure 3.2-(b) presents corresponding trends for non-Hispanic citizens, using the CPS-ASEC from 2001 to 2019. The data on Medicaid coverage, edited by the Census Bureau, are used to address concerns about underreporting of program participation. The values on the x-axis correspond to calendar years. The blue line shows the average Medicaid participation rate in states that never enacted E-Verify mandates during the sample period (i.e., never-treated states), while the red line shows the corresponding rate in states that enacted E-Verify mandates during the sample period (i.e., treated states).



(a) Total Income



(b) Labor Income

Figure 3.3: Effect of E-Verify Mandates on Household Income

Notes: Figure 3.3 presents event study estimates of the effects of E-Verify mandates on household income (Figure 3.3-(a)) and household labor income (Figure 3.3-(b)), using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Household (labor) income is defined as the total annual (labor) income of all household members and is inflation-adjusted to 2019 dollars. The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the ACS from 2001 to 2019. Standard errors are clustered at the state level. ACS person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

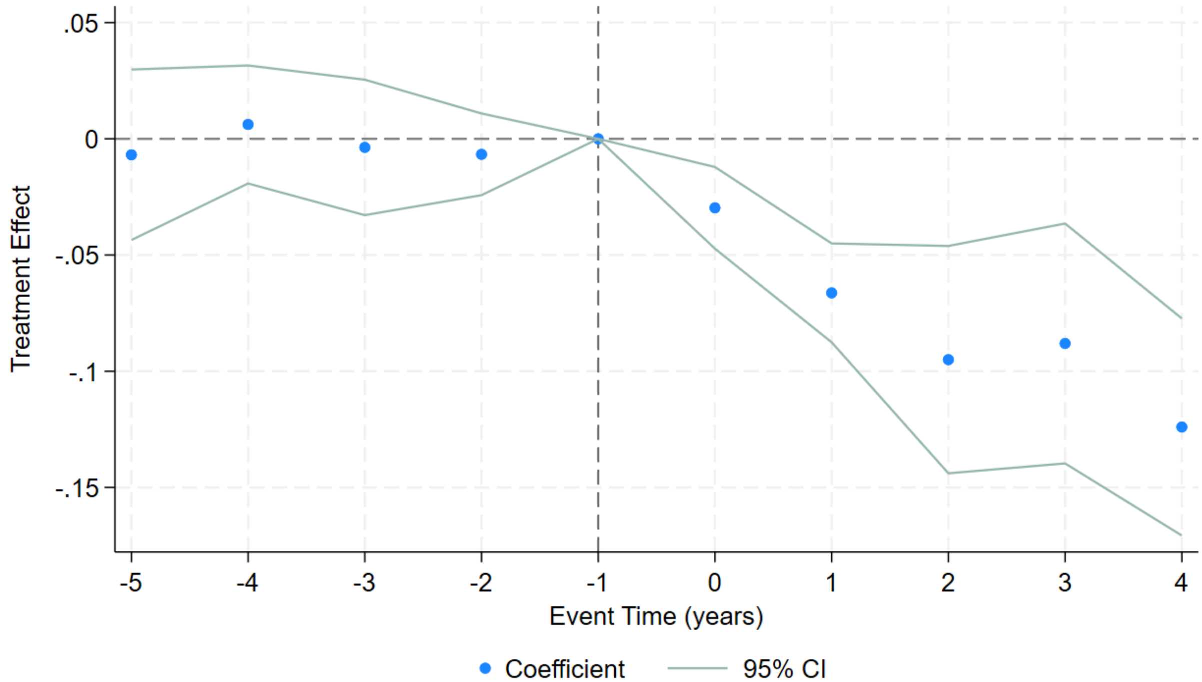
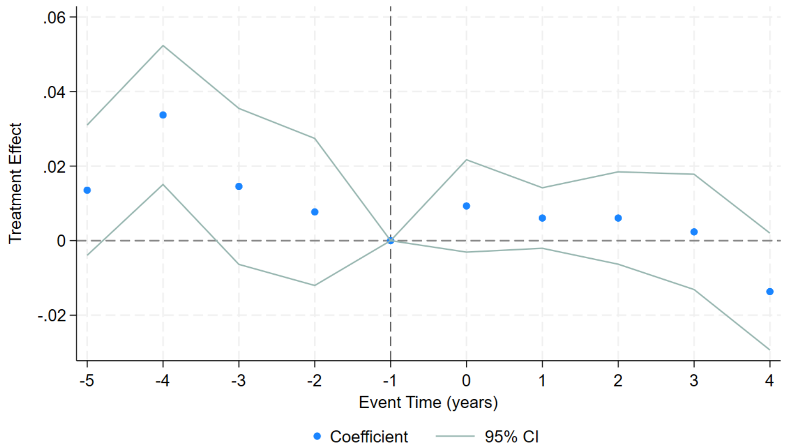
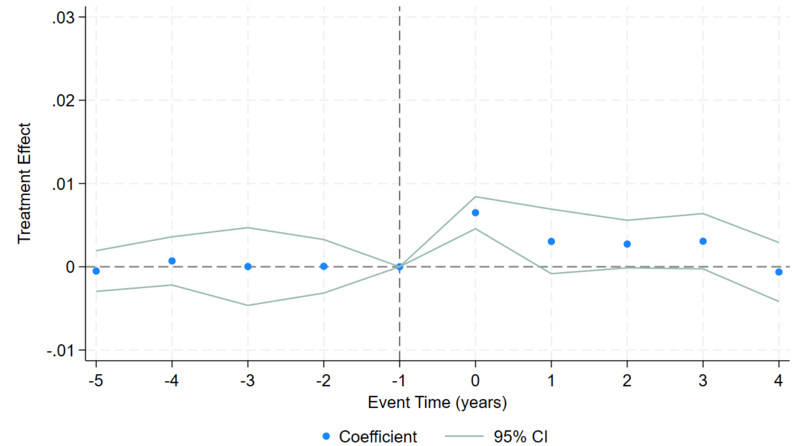


Figure 3.4: Effect of E-Verify Mandates on Medicaid Participation

Notes: Figure 3.4 presents event study estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. The data on Medicaid coverage, edited by the Census Bureau, are used to address concerns about underreporting of program participation. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.



(a) SNAP



(b) SSI

Figure 3.5: Effect of E-Verify Mandates on SNAP and SSI Participation

Notes: Figure 3.5 presents event study estimates of the effects of E-Verify mandates on participation in the Supplemental Nutrition Assistance Program (SNAP) (Figure 3.5-(a)) and the Supplemental Security Income (SSI) program (Figure 3.5-(b)), using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the ACS from 2001 to 2019. Standard errors are clustered at the state level. ACS person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table 3.1: Enactment Dates for E-Verify Mandates (2001–2019)

State	Enactment Date	Scope of the Mandate
Alabama	Jun 2011	Universal
Arizona	Jul 2007	Universal
Colorado	Jun 2006	Public
Florida	Jan 2011	Public
Georgia	Apr 2006	Public
	May 2011	Universal
Idaho	May 2009	Public
Indiana	May 2011	Public
Louisiana	Jul 2011	Universal
Michigan	Jun 2012	Public
Minnesota	Jan 2008	Public
Mississippi	Mar 2008	Universal
Missouri	Jul 2008	Public
Nebraska	Apr 2009	Public
North Carolina	Aug 2006	Public
	Jun 2011	Universal
Oklahoma	May 2007	Public
Pennsylvania	Jul 2012	Public
Rhode Island	Mar 2008	Public
South Carolina	Jun 2008	Public
	Jun 2011	Universal
Tennessee	Jun 2011	Universal
Texas	Jun 2015	Public
Utah	Mar 2008	Public
	Mar 2010	Universal
Virginia	Apr 2010	Public
West Virginia	Mar 2012	Public

Notes: Universal mandates require employment verification in all sectors, while public mandates apply only to the public sector. Rhode Island repealed its mandate in January 2011.
Source: National Conference of State Legislatures (2024)

Table 3.2: Descriptive Statistics

	(1) Entire Sample	(2) States w/o Mandates	(3) States w/ Mandates
A. CPS-ASEC			
Any Insurance	0.661	0.706	0.596
Medicaid (Edited)	0.194	0.236	0.133
Medicaid (Reported)	0.183	0.225	0.123
Any Private Insurance	0.492	0.503	0.477
ESI	0.433	0.447	0.413
Non-ESI	0.059	0.056	0.063
Observations	43,857	26,715	17,142
B. ACS			
SNAP	0.182	0.166	0.206
SSI	0.018	0.018	0.018
HH Total Income	75,579.36	79,615.22	70,180.12
HH Labor Income	65,400.36	68,962.06	60,635.45
Observations	495,125	301,238	193,887

Notes: Table 3.2 reports descriptive statistics for the main outcome variables. Column 1 presents means for the full sample of Hispanic citizens (ages 18–64) living with Hispanic non-citizens. Columns 2 and 3 report means for individuals in states that never enacted E-Verify mandates during the sample period (i.e., never-treated states) and those that did (i.e., treated states), respectively. Panel A presents means for insurance coverage variables using the CPS-ASEC from 2001 to 2019. Insurance coverage (row 1) is classified into public and private insurance (row 4), where public insurance is nearly equivalent to Medicaid because the sample is restricted to individuals ages 18–64. Reported Medicaid participation (row 3) is based on respondents' answers in the survey, whereas Medicaid participation (row 2) uses data by the Census Bureau, which assign Medicaid coverage to some respondents who did not report such coverage during the interview to address concerns about underreporting of program participation. Private insurance (row 4) is further divided into employer-sponsored insurance (ESI) (row 5) and non-employer-sponsored insurance (non-ESI) (row 6). Panel B presents means for SNAP participation, SSI participation, and household income (in 2019 dollars) using the ACS from 2001 to 2019. Household (labor) income is defined as the total annual (labor) income of all household members and is inflation-adjusted to 2019 dollars. All variables except household income are binary.

Table 3.3: Effect of E-Verify Mandates on Household Income and Employment Status

	(1) Total Income	(2) Labor Income	(3) Employed
E-Verify	-3402.004***	-3507.563***	-0.017***
	(1127.283)	(837.791)	(0.004)
CONTROLS			
Individual	YES	YES	YES
State			
ACA	YES	YES	YES
Enforcement	YES	YES	YES
SafetyNet	YES	YES	YES
Pre-Period Mean	76,217.35	65,970.77	0.687

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 3.3 reports DID estimates of the effects of E-Verify mandates on household income and employment status (binary), using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Household (labor) income is defined as the total annual (labor) income of all household members and is inflation-adjusted to 2019 dollars. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the ACS from 2001 to 2019. Standard errors are clustered at the state level. ACS person-level weights are adjusted for the number of household members in the sample in columns 1–2, while unadjusted ACS person-level weights are used in column 3. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table 3.4: Effect of E-Verify Mandates on Program Participation

	(1) Medicaid	(2) SNAP	(3) SSI
E-Verify	-0.084***	0.001	0.003**
	(0.017)	(0.005)	(0.001)
CONTROLS			
Individual	YES	YES	YES
State			
ACA	YES	YES	YES
Enforcement	YES	YES	YES
SafetyNet	YES	YES	YES
Pre-Period Mean	0.202	0.173	0.018

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 3.4 reports DID estimates of the effects of E-Verify mandates on participation in safety net programs, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Each column presents the estimate for participation in Medicaid (column 1), the Supplemental Nutrition Assistance Program (SNAP) (column 2), and the Supplemental Security Income (SSI) program (column 3). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens using the CPS-ASEC (column 1) and the ACS (columns 2–3) from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC (column 1) and ACS (columns 2–3) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table 3.5: Effect of E-Verify Mandates on Health Insurance Coverage

	(1) Any	(2) Private	(3) ESI	(4) Non-ESI
E-Verify	-0.086***	-0.020	-0.029**	0.009
	(0.023)	(0.017)	(0.014)	(0.008)
CONTROLS				
Individual	YES	YES	YES	YES
State				
ACA	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES
Pre-Period Mean	0.662	0.486	0.436	0.049

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 3.5 reports DID estimates of the effects of E-Verify mandates on health insurance coverage, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Column 1 presents the estimate for having any type of insurance (either public or private). Column 2 presents the estimate for private insurance. Column 3 presents the estimate for employer-sponsored insurance (ESI). Column 4 presents the estimate for other types of private insurance, excluding ESI (non-ESI). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table 3.6: Heterogeneous Effects of E-Verify Mandates on Medicaid Participation: Living with Hispanic Non-Citizens

	Living with Hispanic Non-Citizens [p-value = 0.000]	
	(1) Yes (Table 3.4 col. 1)	(2) No
E-Verify	-0.084***	-0.010
	(0.017)	(0.011)
CONTROLS		
Individual	YES	YES
State		
ACA	YES	YES
Enforcement	YES	YES
SafetyNet	YES	YES
Pre-Period Mean	0.202	0.161

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 3.6 reports DID estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), separately for individuals living with Hispanic non-citizens and those who are not. The sample consists of Hispanic citizens ages 18–64, using the CPS-ASEC from 2001 to 2019. Column 1 presents the estimate for Hispanic citizens living with Hispanic non-citizens. Column 2 presents the estimate for Hispanic citizens not living with Hispanic non-citizens. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

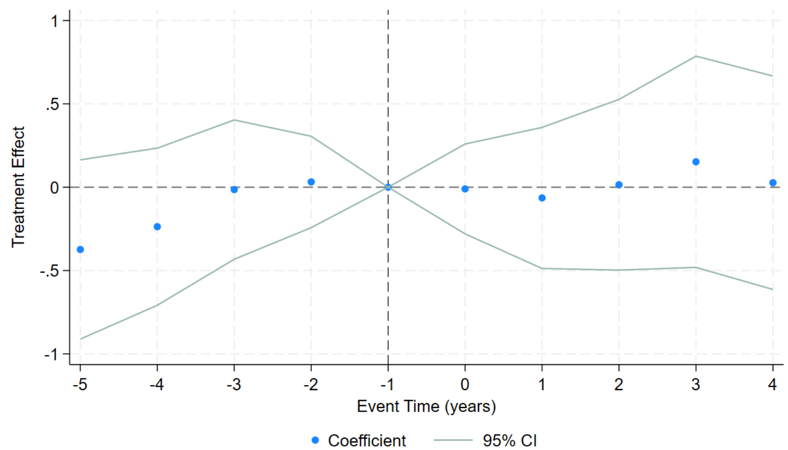
Table 3.7: Heterogeneous Effects of E-Verify Mandates on Medicaid Participation: Hispanic Origin of Non-Citizens and Sanctuary Cities

	Hispanic Origin of Non-Citizens : Cuban or Salvadoran [p-value = 0.026]		Existence of Sanctuary Cities [p-value = 0.000]	
	(1)	(2)	(3)	(4)
	Yes	No	Yes	No
E-Verify	-0.017	-0.088***	-0.008	-0.115***
	(0.026)	(0.019)	(0.018)	(0.018)
CONTROLS				
Individual	YES	YES	YES	YES
State				
ACA	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES
Pre-Period Mean	0.170	0.205	0.226	0.209

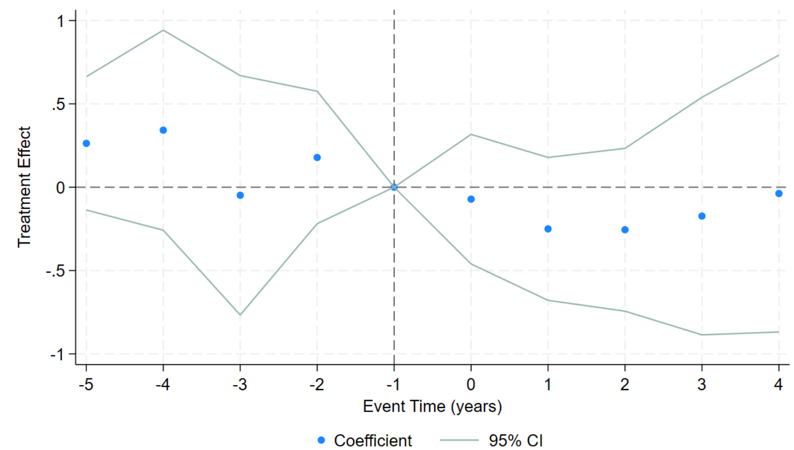
Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table 3.7 reports DID estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Columns 1 and 2 present the estimates after splitting the sample based on the Hispanic origin of non-citizens in the household. Column 1 presents the estimate for Hispanic citizens living with Hispanic non-citizens who have a lower deportation risk (i.e., Cubans or Salvadorans). Column 2 presents the estimate for Hispanic citizens living with Hispanic non-citizens who are neither Cuban nor Salvadoran. Columns 3 and 4 present the estimates based on whether states had sanctuary cities during the period of E-Verify enactment. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Appendix



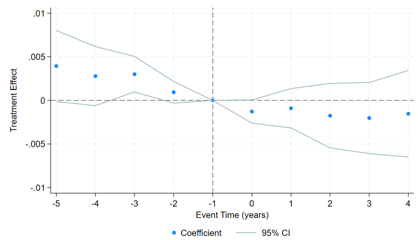
(a) 2001–2019



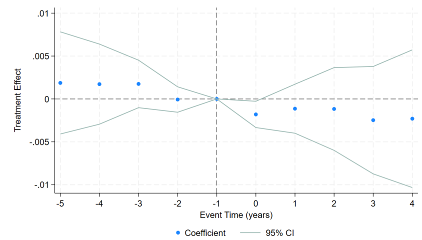
(b) 2008–2019

Figure A3.1: Effect of E-Verify Mandates on Unemployment Rates

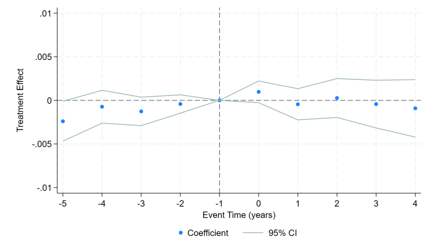
Notes: Figure A3.1 presents event study estimates of the effects of E-Verify mandates on state unemployment rates, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Each figure shows the effect over the full data period from 2001 to 2019 (Figure A3.1-(a)) and the restricted period from 2008 to 2019 (Figure A3.1-(b)). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. Standard errors are clustered at the state level.



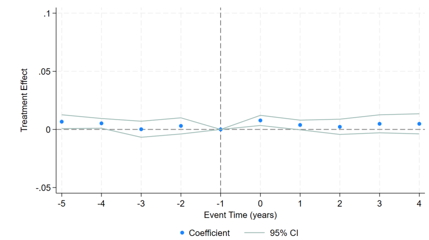
(a) Citizen



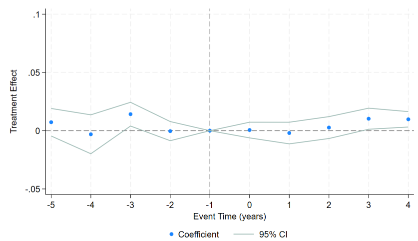
(b) Hispanic Citizen



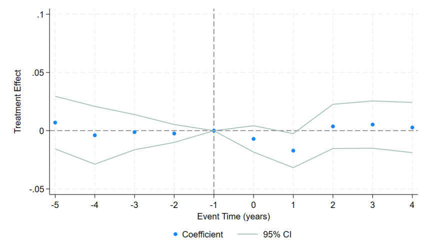
(c) Living w/ Hisp. Non-Citizen



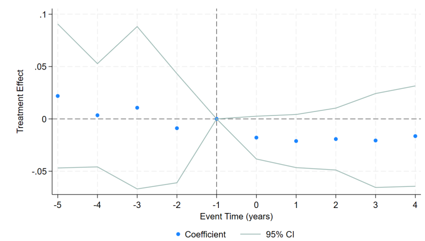
(d) Moved b/w States



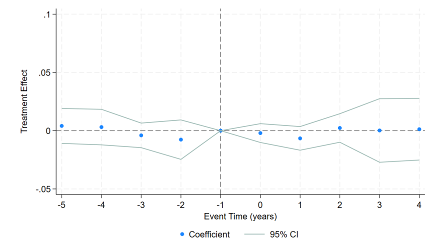
(e) Age (18–34)



(f) Male



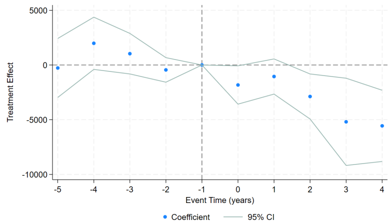
(g) Number of Children



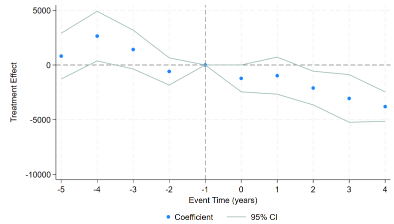
(h) Married

Figure A3.2: Effect of E-Verify Mandates on Migration and Composition

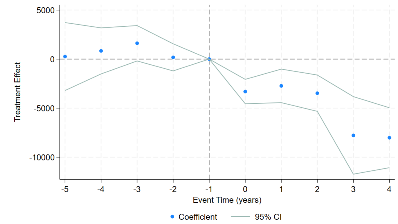
Notes: Figure A3.2 presents event study estimates of the effects of E-Verify mandates on migration and changes in sample composition, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of all survey respondents ages 18–64 (Figures A3.2-(a), (b), and (c)) and Hispanic citizens ages 18–64 living with Hispanic non-citizens (Figures A3.2-(d), (e), (f), (g), and (h)), using the ACS from 2001 to 2019. Standard errors are clustered at the state level. ACS person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups), some of which are excluded depending on the outcome. State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.



(a) Main

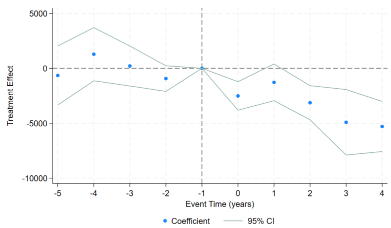


(b) No Controls

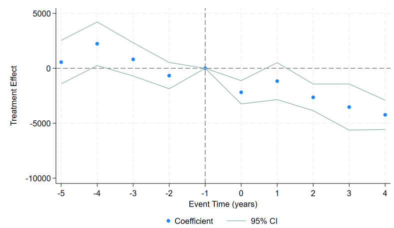


(c) No Weights

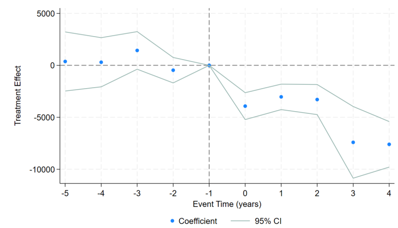
A. Total Income



(a) Main



(b) No Controls

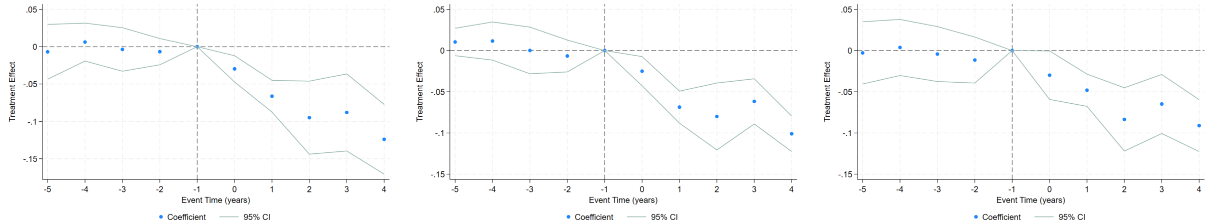


(c) No Weights

B. Labor Income

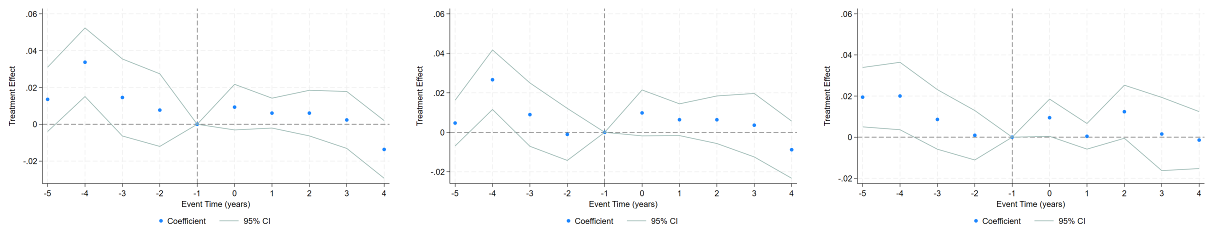
Figure A3.3: Effect of E-Verify Mandates on Household Income: Robustness - Controls and Weights

Notes: Figure A3.3 presents event study estimates of the effects of E-Verify mandates on household income (Panel A) and household labor income (Panel B) using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), under alternative control variables and weighting schemes. Household (labor) income is defined as the total annual (labor) income of all household members and is inflation-adjusted to 2019 dollars. The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the ACS from 2001 to 2019. Standard errors are clustered at the state level. ACS person-level weights are applied after adjusting for the number of household members in the sample. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.



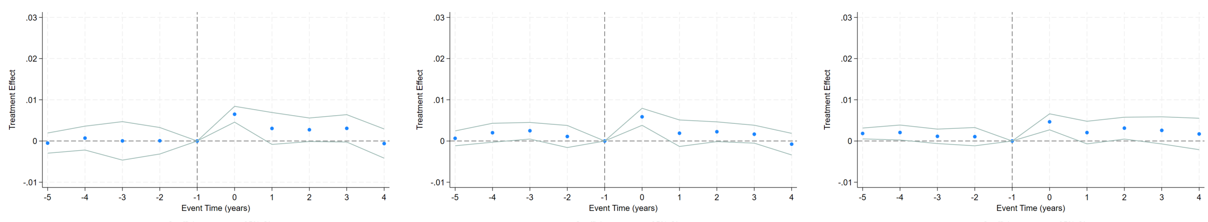
(a) Main (b) No Controls (c) No Weights

A. Medicaid



(a) Main (b) No Controls (c) No Weights

B. SNAP

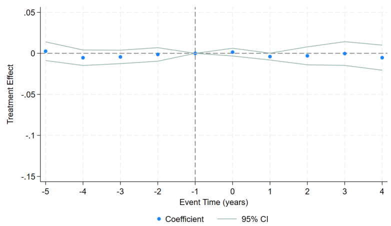


(a) Main (b) No Controls (c) No Weights

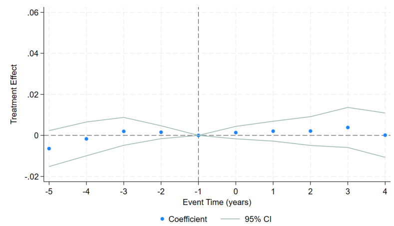
C. SSI

Figure A3.4: Effect of E-Verify Mandates on Program Participation: Robustness - Controls and Weights

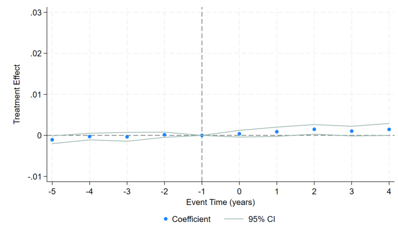
Notes: Figure A3.4 presents event study estimates of the effects of E-Verify mandates on participation in Medicaid (Panel A), the Supplemental Nutrition Assistance Program (SNAP) (Panel B) and the Supplemental Security Income (SSI) program (Panel C) using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), under alternative control variables and weighting schemes. The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC (Panel A) and the ACS (Panels B and C) from 2001 to 2019. Standard errors are clustered at the state level. CPS -ASEC (Panel A) and ACS (Panels B and C) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.



(a) Medicaid

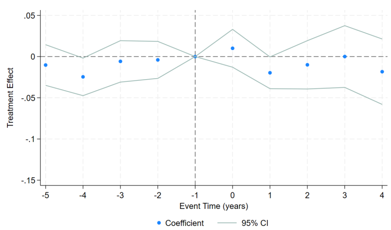


(b) SNAP

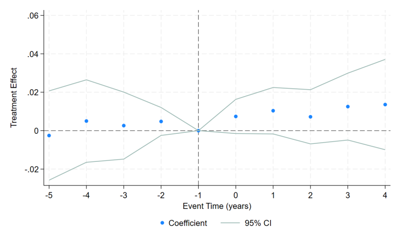


(c) SSI

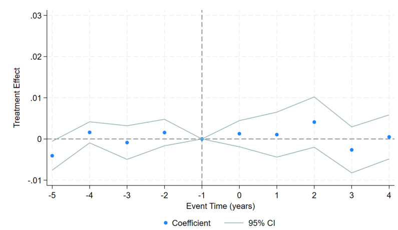
A. Non-Hispanic Citizens



(a) Medicaid



(b) SNAP



(c) SSI

B. Low-Educated Non-Hispanic Citizens

Figure A3.5: Effect of E-Verify Mandates on Program Participation of Non-Hispanic Citizens

Notes: Figure A3.5 presents event study estimates of the effects of E-Verify mandates on program participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of non-Hispanic citizens (Panel A) and individuals without a high school degree (Panel B) ages 18–64, using the CPS-ASEC (Figure A3.5-(a)) and the ACS (Figures A3.5-(b) and (c)) from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC (Figure A3.5-(a)) and ACS (Figures A3.5-(b) and (c)) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

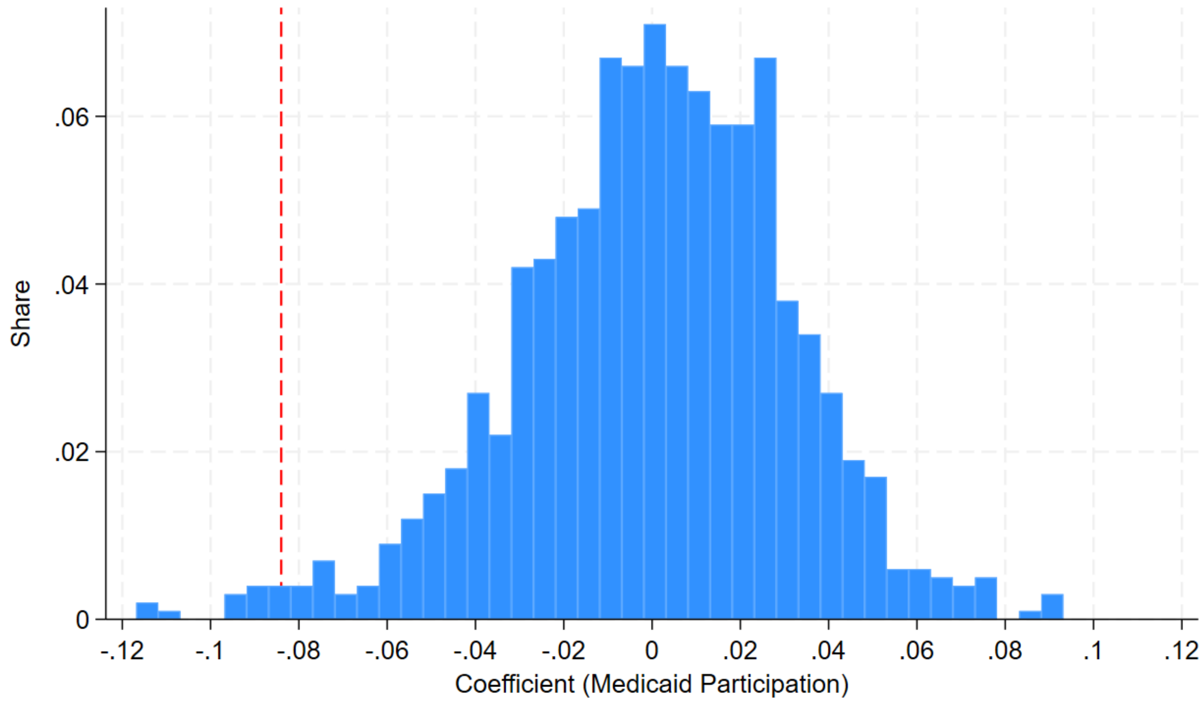


Figure A3.6: Effect of E-Verify Mandates on Medicaid Participation: Permutation Test

Notes: Figure A3.6 presents the distribution of estimated effects of E-Verify mandates on Medicaid participation among Hispanic citizens living with Hispanic non-citizens from a permutation test. The test randomly assigns treatment status to states 1,000 times while holding enactment dates fixed as in Table 3.1, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024) under the specification in column 1 of Table 3.4. The values on the x-axis correspond to the estimated effects from each replication, and each bar shows the relative frequency within a given interval. The vertical red line indicates the original DID estimate of -0.084 for the effect of E-Verify mandates on Medicaid participation (column 1 of Table 3.4). The permutation test yields a p-value of 0.016.

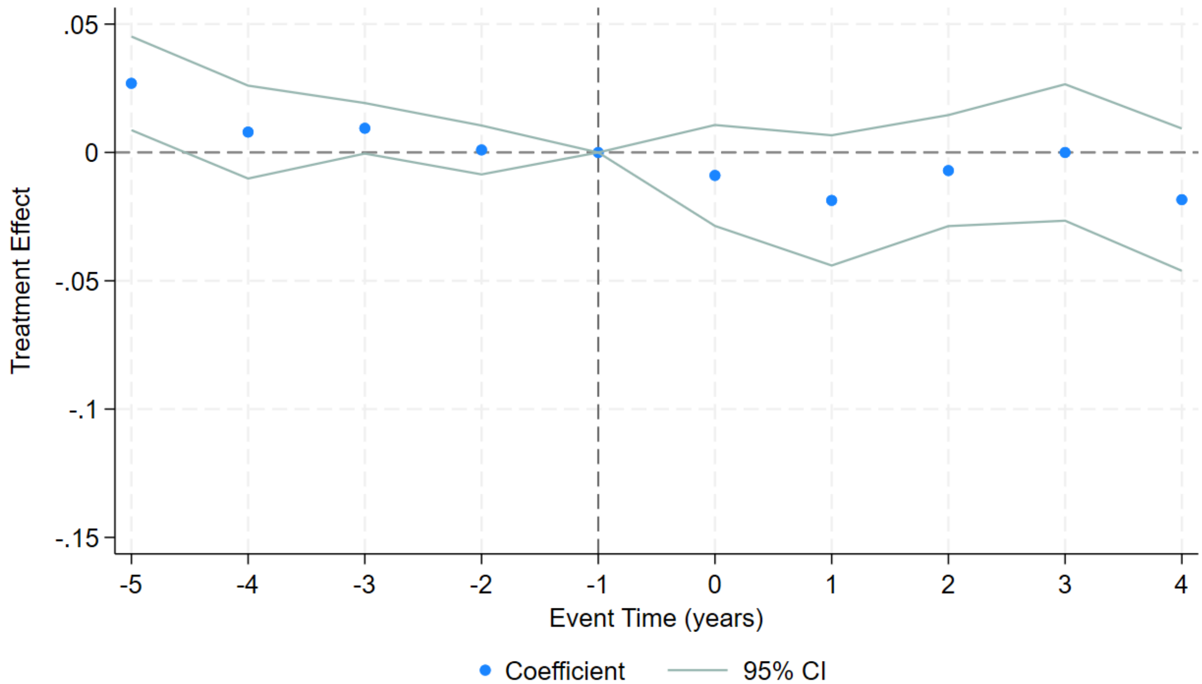
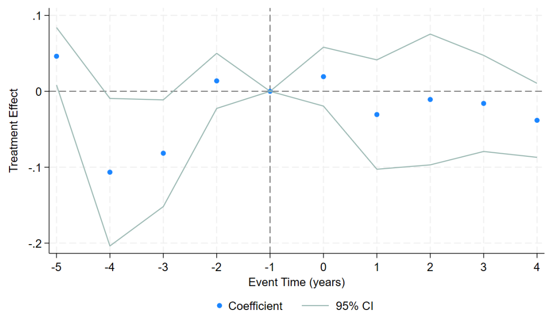
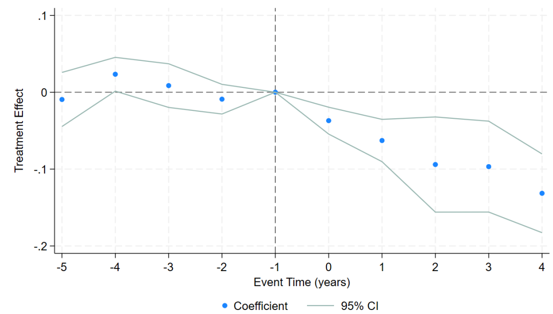


Figure A3.7: Heterogenous Effects of E-Verify Mandates on Medicaid Participation: Not Living with Hispanic Non-Citizens

Notes: Figure A3.7 presents event study estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 who are not living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

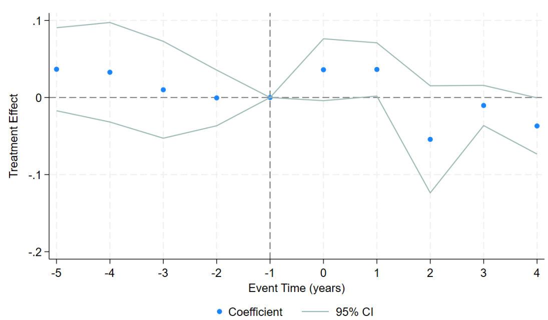


(a) Yes

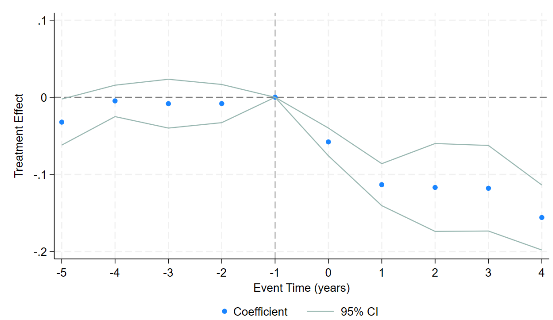


(b) No

A. Existence of Cuban or Salvadoran Non-Citizens in Household



(a) Yes

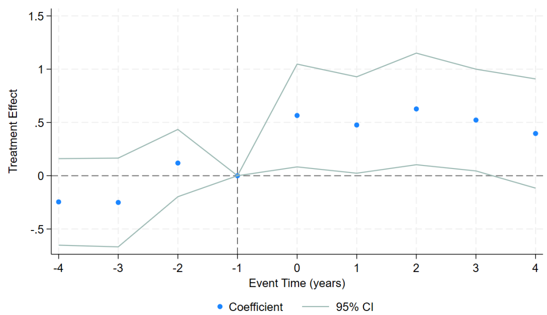


(b) No

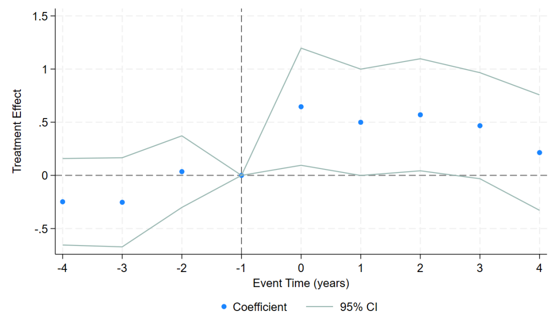
B. Existence of Sanctuary Cities

Figure A3.8: Heterogenous Effects of E-Verify Mandates on Medicaid Participation: Hispanic Origin of Non-Citizens and Sanctuary Cities

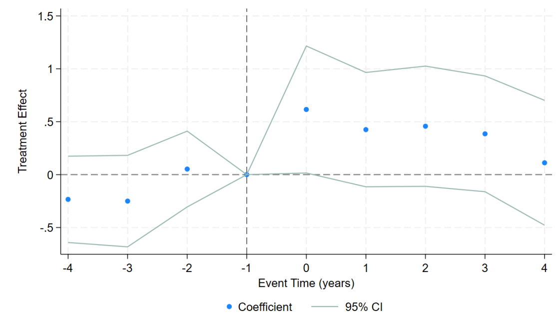
Notes: Figure A3.8 presents event study estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Figure A3.8-A presents the results after splitting the sample based on whether individuals are living with Hispanic non-citizens who have a lower deportation risk (i.e., Cubans or Salvadorans). Figure A3.8-B presents the results based on whether states had sanctuary cities during the period of E-Verify enactment. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.



(a) All States



(b) States with Post-2006 Enactment



(c) States with Post-2007 Enactment

Figure A3.9: Effect of E-Verify Mandates on Deportation-Related Searches in Google Trends

Notes: Figure A3.9 presents event study estimates of the effects of E-Verify mandates on deportation-related searches in Google Trends data (2004–2019), using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The values on the x-axis correspond to years relative to the enactment of E-Verify mandates, with negative values indicating pre-treatment placebo effects. The 95% confidence intervals are represented by light green lines. The outcome variable is the inverse hyperbolic sine of the normalized intensity of deportation-related searches at the state-year level, constructed following the approach of Alsan and Yang (2024). I first collect the normalized search share (out of 100) for eight deportation-related terms in both English and Spanish: deportation, deportacion, immigration, inmigracion, immigration lawyer, abogados de inmigracion, undocumented, and indocumentado. I then add the search intensities for these terms and normalize the demeaned total by the search intensity of "deportes" (sports) and "telenovelas" (soap opera), which are commonly searched within the Hispanic community. Standard errors are clustered at the state level. State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.1: Comparison of Safety Net Programs

	Medicaid	SNAP	SSI
A. Basic			
Eligibility	Low-income; pregnant women, children, seniors (65+), disabled	Low-income	Low-income blind or disabled, seniors (65+)
Administration	Federal, state-administered	Federal (U.S. Dept. of Agriculture (USDA)), state-administered	Federal (Social Security Administration (SSA))
Funding	Federal and state	Mostly federal	Federal
B. Benefit			
Type	Health insurance	Food assistance	Cash
Amount	Not fixed (service-based)	~\$120 (per person/month in 2019)[a]	~\$560 (per person/month in 2019)[b]
Distribution	As services are used; direct payments to providers	Monthly; coupon → Electronic Benefit Transfer (EBT) card	Monthly; direct deposit or debit card
C. Enrollment			
Regular	State Medicaid agency, online	State SNAP office, online	SSA field office, phone, online
Special	Presumptive eligibility, retroactive coverage	Expedited service (fast processing for very low-income HHs)	Limited (cf. emergency advance payments, presumptive disability)
Recertification	Typically 12 months	Every 6–12 months (with interim reporting)	Not fixed (financial: 1–6 years[c]; disability: 3–7 years[d])

a Average SNAP benefit amount (from state-level monthly data): https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

b Average SSI benefit amount: https://www.ssa.gov/policy/docs/chartbooks/fast_facts/2020/fast_facts20.html

c SSI financial redetermination: https://www.ssa.gov/ssi/text-redets-ussi.htm

d SSI Continuing Disability Review (CDR): https://www.ssa.gov/ssi/text-cdrs-ussi.htm

Table A3.2: Effect of E-Verify Mandates on Migration and Composition

	(1) Citizen	(2) Hispanic Citizen	(3) Living w/ Hisp. Non-Citizen	(4) Moved b/w States	(5) Age (18–34)	(6) Male	(7) # Child	(8) Married
E-Verify	-0.002	-0.002	-0.000	0.005	0.005	-0.002	-0.019	-0.001
	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.006)	(0.012)	(0.007)
CONTROLS								
Individual	YES	YES	YES	YES	YES	YES	YES	YES
State								
ACA	YES	YES	YES	YES	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.898	0.107	0.083	0.011	0.583	0.520	0.992	0.466

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.2 reports DID estimates of the effects of E-Verify mandates on migration and changes in sample composition, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The sample consists of all survey respondents ages 18–64 (columns 1–3) and Hispanic citizens ages 18–64 living with Hispanic non-citizens (columns 4–8), using the ACS from 2001 to 2019. Column 1 shows the estimate for whether the respondent is a U.S. citizen. Column 2 shows the estimate for whether the respondent is a Hispanic citizen. Column 3 shows the estimate for whether the respondent is living with Hispanic non-citizens. Column 4 shows the estimate for whether the respondent moved to a different state compared to the previous year. Columns 5–8 show estimates for the corresponding demographic characteristics. Standard errors are clustered at the state level. ACS person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups), some of which are excluded depending on the outcome. State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.3: Effect of E-Verify Mandates on Household Income: Robustness - Unit, Controls, and Winsorization

	Total Income			Labor Income		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Individual-Level						
E-Verify	-3402.004***	-2298.01***	-2798.589***	-3507.563***	-2809.064***	-3185.145***
	(1127.283)	(619.470)	(827.522)	(837.791)	(557.472)	(722.595)
Baseline Controls	YES	NO	YES	YES	NO	YES
Winsorized (at 2.5%)	NO	NO	YES	NO	NO	YES
Pre-Period Mean	76,217.35	76,217.35	74,546.09	65,970.77	65,970.77	64,437.29
B. Household-Level						
E-Verify	-3555.849***	-2388.383***	-2991.303***	-3794.110***	-3008.601***	-3464.757***
	(1163.512)	(621.993)	(861.227)	(872.050)	(553.483)	(735.865)
Baseline Controls	YES	NO	YES	YES	NO	YES
Winsorized (at 2.5%)	NO	NO	YES	NO	NO	YES
Pre-Period Mean	75,785.53	75,785.53	74,108.27	65,713.95	65,713.95	64,173.45

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.3 reports DID estimates of the effects of E-Verify mandates on household income, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Household (labor) income is defined as the total annual (labor) income of all household members and is inflation-adjusted to 2019 dollars. Panel A report estimates using the individual-level sample with weights adjusted for the number of household members in the sample, while Panel B report estimates obtained after collapsing the data to the household level. In columns 3 and 6, the income variables are winsorized at the 2.5th and 97.5th percentile values by replacing extreme values at each tail of the distribution. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the ACS from 2001 to 2019. Standard errors are clustered at the state level. In Panel A, ACS person-level weights are adjusted for the number of household members in the sample, while Panel B uses household-level weights. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.4: Effect of E-Verify Mandates on Program Participation: Robustness - Controls and Weights

	Medicaid			SNAP			SSI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
E-Verify	-0.084***	-0.069***	-0.066***	0.001	0.003	0.005	0.003**	0.002**	0.003**
	(0.017)	(0.010)	(0.013)	(0.005)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
Baseline Controls	YES	NO	YES	YES	NO	YES	YES	NO	YES
Weighted	YES	YES	NO	YES	YES	NO	YES	YES	NO
Pre-Period Mean	0.202	0.202	0.192	0.173	0.173	0.176	0.018	0.018	0.019

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.4 reports DID estimates of the effects of E-Verify mandates on participation in safety net programs, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), under alternative control variables and weighting schemes. Each column presents an estimate for participation in Medicaid (columns 1–3), the Supplemental Nutrition Assistance Program (SNAP) (columns 4–6), and the Supplemental Security Income (SSI) program (columns 7–9). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens using the CPS-ASEC (columns 1–3) and the ACS (columns 4–9) from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC (columns 1–3) and ACS (columns 4–9) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.5: Effect of E-Verify Mandates on Program Participation: Robustness - Data Sources

	2001–2019			2008–2019		
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	SSI	Medicaid	SNAP	SSI
A. CPS-ASEC						
E-Verify	-0.084***	-0.041	0.005*	-0.100***	-0.054**	0.002
	(0.017)	(0.026)	(0.003)	(0.018)	(0.022)	(0.004)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.202	0.127	0.013	0.243	0.150	0.013
B. ACS						
E-Verify	N/A	0.001	0.003**	-0.070***	-0.019*	0.003***
	N/A	(0.005)	(0.001)	(0.011)	(0.011)	(0.001)
Baseline Controls	YES	YES	YES	YES	YES	YES
Pre-Period Mean	N/A	0.173	0.018	0.242	0.204	0.019

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.5 reports DID estimates of the effects of E-Verify mandates on program participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), across alternative data sources and sample periods. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC (Panel A) and the ACS (Panel B) from 2001 to 2019 (columns 1–3) and from 2008 to 2019 (columns 4–6). Standard errors are clustered at the state level. CPS-ASEC (Panel A) and ACS (Panel B) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.6: Effect of E-Verify Mandates on Program Participation of Non-Hispanic Citizens

	Medicaid			SNAP			SSI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	White	Black	All	White	Black	All	White	Black
E-Verify	-0.002	-0.005	0.000	0.002	0.001	0.002	0.001**	0.001***	0.000
	(0.004)	(0.003)	(0.008)	(0.003)	(0.003)	(0.006)	(0.0005)	(0.0004)	(0.0012)
CONTROLS									
Individual	YES	YES	YES	YES	YES	YES	YES	YES	YES
State									
ACA	YES	YES	YES	YES	YES	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.093	0.077	0.178	0.094	0.073	0.221	0.025	0.022	0.049

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.6 reports DID estimates of the effects of E-Verify mandates on participation in Medicaid (columns 1–3), the Supplemental Nutrition Assistance Program (SNAP) (columns 4–6), and the Supplemental Security Income (SSI) program (columns 7–9), using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The sample consists of non-Hispanic citizens ages 18–64, using the CPS-ASEC (columns 1–3) and the ACS (columns 4–9) from 2001 to 2019. Columns 1, 4, and 7 present estimates for all non-Hispanic citizens, while the remaining columns present estimates separately for non-Hispanic Whites (columns 2, 5, and 8) and non-Hispanic Blacks (columns 3, 6, and 9). Standard errors are clustered at the state level. CPS-ASEC (columns 1–3) and ACS (columns 4–9) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.7: Effect of E-Verify Mandates on Joint Program Participation

	Baseline		Medicaid & SSI			Medicaid & No SSI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medicaid	SSI	Medicaid & SSI	Medicaid & SSI & SNAP	Medicaid & SSI & No SNAP	Medicaid & No SSI	Medicaid & No SSI & SNAP	Medicaid & No SSI & No SNAP
E-Verify	-0.084***	0.005*	0.005*	0.003	0.002	-0.089***	-0.028**	-0.060***
	(0.017)	(0.003)	(0.003)	(0.002)	(0.002)	(0.015)	(0.012)	(0.008)
Baseline Controls	YES	YES	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.202	0.013	0.012	0.004	0.008	0.189	0.061	0.129

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.7 reports DID estimates of the effects of E-Verify mandates on joint participation in Medicaid, the Supplemental Nutrition Assistance Program (SNAP), and the Supplemental Security Income (SSI) program, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Columns 1 and 2 present estimates for participation in a single program as the baseline estimates (columns 1 and 3 in Panel A of Table A3.5). Columns 3 and 6 present estimates for joint participation in Medicaid and SSI (column 3) and Medicaid without SSI (column 6). Columns 4–5 and 7–8 further decompose these groups by SNAP participation, reporting joint participation in Medicaid, SSI, and SNAP (column 4), Medicaid and SSI without SNAP (column 5), Medicaid without SSI but with SNAP (column 7), and Medicaid without either SSI or SNAP (column 8). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.8: Effect of E-Verify Mandates on Joint Medicaid Participation and Household Income

	Baseline	Medicaid & Income Group Indicators (ascending)		
	(1)	(2)	(3)	(4)
	Medicaid	Medicaid & Bottom 33%	Medicaid & Middle 33%	Medicaid & Top 33%
E-Verify	-0.084***	-0.005	-0.043***	-0.036***
	(0.017)	(0.013)	(0.006)	(0.006)
CONTROLS				
Individual	YES	YES	YES	YES
State				
ACA	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES
Pre-Period Mean	0.202	0.107	0.059	0.036

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.8 reports DID estimates of the effects of E-Verify mandates on joint Medicaid participation and household income outcomes, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). Column 1 presents the baseline estimate for Medicaid participation (column 1 of Table 3.4). Columns 2–4 present estimates for joint Medicaid participation and indicators for household income groups. In each column, the outcome corresponds to an indicator equal to one if an individual participates in Medicaid and belongs to a given income tercile: bottom 33 percent (column 2), middle 33 percent (33–66 percent) (column 3), and top 33 percent (column 4). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.9: Heterogeneous Effects of E-Verify Mandates on Program Participation: Age

	Medicaid [p-value = 0.017]		SNAP [p-value = 0.370]		SSI [p-value = 0.605]	
	(1)	(2)	(3)	(4)	(5)	(6)
	18–34	35–64	18–34	35–64	18–34	35–64
E-Verify	-0.105***	-0.040***	0.006	-0.001	0.003**	0.002*
	(0.026)	(0.009)	(0.005)	(0.005)	(0.001)	(0.001)
CONTROLS						
Individual	YES	YES	YES	YES	YES	YES
State						
ACA	YES	YES	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.234	0.155	0.186	0.156	0.013	0.024

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.9 reports DID estimates of the effects of E-Verify mandates on program participation by age, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC (columns 1–2) and the ACS (columns 3–6) from 2001 to 2019. Each pair of columns presents estimates for individuals ages 18 to 34 (columns 1, 3, and 5) and 35 to 64 (columns 2, 4, and 6). Standard errors are clustered at the state level. CPS-ASEC (column 1–2) and ACS (columns 3–6) person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.10: Effect of E-Verify Mandates on Medicaid Participation: Robustness - Underreporting and Controls

	(1)	(2)	(3)	(4)	(5)
A. Edited Values					
E-Verify	-0.069***	-0.076***	-0.077***	-0.083***	-0.084***
	(0.010)	(0.012)	(0.013)	(0.016)	(0.017)
Pre-Period Mean	0.202	0.202	0.202	0.202	0.202
B. Reported Values					
E-Verify	-0.066***	-0.073***	-0.072***	-0.079***	-0.079***
	(0.009)	(0.011)	(0.011)	(0.015)	(0.015)
Pre-Period Mean	0.191	0.191	0.191	0.191	0.191
CONTROLS					
Individual	NO	YES	YES	YES	YES
States					
ACA	NO	NO	YES	YES	YES
Enforcement	NO	NO	NO	YES	YES
SafetyNet	NO	NO	NO	NO	YES

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.10 reports DID estimates of the effects of E-Verify mandates on Medicaid participation, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024), under alternative sets of covariates. Panel A presents estimates for Medicaid participation edited by the Census Bureau to address concerns about underreporting of program participation, while Panel B presents estimates based on respondents' reported Medicaid participation prior to the edits. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.11: Effect of E-Verify Mandates on Medicaid Participation by Mandate Type

	(1)	(2)
E-Verify	-0.050**	
	(0.020)	
Universal E-Verify		-0.053***
		(0.020)
Public E-Verify		-0.049**
		(0.021)
CONTROLS		
Individual	YES	YES
State		
ACA	YES	YES
Enforcement	YES	YES
SafetyNet	YES	YES
Pre-Period Mean	0.202	0.202

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.11 reports DID estimates of the effects of E-Verify mandates on Medicaid participation by mandate type, using the TWFE estimator. Column 1 presents the estimate for all E-Verify mandates. Column 2 presents separate estimates for universal and public E-Verify mandates. The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

Table A3.12: Heterogeneous Effects of E-Verify Mandates on Medicaid Participation: Demographic Characteristics

	Age [p-value = 0.017]		Education [p-value = 0.061]		Gender [p-value = 0.646]	
	(1)	(2)	(3)	(4)	(5)	(6)
	18–34	35–64	$<$ HS	$\geq$ HS	Male	Female
E-Verify	-0.105***	-0.040***	-0.140***	-0.075***	-0.075***	-0.087***
	(0.026)	(0.009)	(0.029)	(0.019)	(0.018)	(0.018)
CONTROLS						
Individual	YES	YES	YES	YES	YES	YES
State						
ACA	YES	YES	YES	YES	YES	YES
Enforcement	YES	YES	YES	YES	YES	YES
SafetyNet	YES	YES	YES	YES	YES	YES
Pre-Period Mean	0.234	0.155	0.240	0.184	0.174	0.233

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Notes: Table A3.12 reports DID estimates of the effects of E-Verify mandates on Medicaid participation by demographic characteristics, using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024). The sample consists of Hispanic citizens ages 18–64 living with Hispanic non-citizens, using the CPS-ASEC from 2001 to 2019. Columns 1 and 2 present estimates for individuals ages 18 to 34 and 35 to 64, respectively. Columns 3 and 4 present estimates for individuals with less than a high school degree and those with a high school degree or higher, respectively. Columns 5 and 6 present estimates by gender. Standard errors are clustered at the state level. CPS-ASEC person-level weights are applied. Individual-level covariates include age (in 5 groups), gender, race, marital status, and education level (in 5 groups). State-level covariates include indicators for Medicaid expansion under the Affordable Care Act (ACA), local-level police-based immigration enforcement, and immigrant-related policies concerning Medicaid, food assistance, and cash assistance.

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