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Income vs. Asset Tests in the Safety Net: Impacts on Access, Targeting, and Costs in SNAP

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Abstract

Means-tested programs worldwide screen applicants on both income and assets, yet little is known about the comparative effects of relaxing each screen. We provide the first within-program comparison, exploiting the staggered adoption of Broad-Based Categorical Eligibility in SNAP, under which some U.S. states relaxed only the asset test while others also raised the gross income threshold. Using a stacked difference-in-differences design, we find that raising the income threshold increased both eligibility and enrollment by roughly twice as much as relaxing the asset test, with roughly half of the enrollment response coming from inframarginal households who were previously eligible but not enrolled. At the same time, newly eligible households under income expansion were more disadvantaged than those under asset relaxation, and raising the income threshold brought in enrollees who were on some dimensions needier than baseline enrollees. For benefit amounts, average benefits per enrollee remained similar across the two expansions despite the income expansion making many low-benefit households newly eligible, as few of these households actually enrolled. Simulations further suggest that incorporating asset information into the benefit formula could substantially improve the targeting of benefit dollars toward the most disadvantaged households.

JEL classification

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Keywords

SNAP, means-tested transfers, income and asset tests, program eligibility, targeting

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1 Introduction

Social assistance programs are generally not universal and often designed to target households that are socioeconomically disadvantaged. To identify these households, governments around the world typically screen applicants on two financial dimensions: income (capturing current cash flow) and assets (capturing accumulated wealth). The rationale for using both income and asset tests is that economic well-being depends on both current and accumulated resources, and neither dimension alone fully captures a household's economic circumstances. In practice, programs as varied as the United Kingdom's Universal Credit, Australia's Centrelink, Germany's Bürgergeld (Citizen's Benefit), the Netherlands' Bijstand (social assistance program), and Japan's Seikatsu Hogo (Public Assistance) all incorporate both income and asset tests into their eligibility criteria, as do major U.S. programs such as the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Supplemental Security Income (SSI).

However, strict means testing can come at a cost. Asset tests may discourage saving, as households near the asset limit face an incentive to draw down or avoid accumulating wealth to maintain eligibility for benefits.[1] Income tests may discourage work, as eligibility thresholds may create benefit cliffs that penalize households for earning above the limit (Moffitt 2002; Koşar and Moffitt 2017).[2] Both tests also impose administrative burdens, as applicants are required to gather and submit detailed financial documentation, while caseworkers must continuously process, verify, and monitor this information over time (Herd and Moynihan 2018). Recognizing such concerns, several U.S. programs have moved toward relaxing eligibility rules. Since 2000, SNAP has allowed states to expand access by loosening asset and income tests. Furthermore, more than 40 states since 2014 have raised income

1. The empirical evidence on savings disincentives is mixed. Powers (1998) finds that asset tests in AFDC discourage savings, while Sullivan (2006) and Hurst and Ziliak (2006) find no relationship between asset limits in AFDC/TANF and household asset accumulation. More recently, in the context of SNAP, Pirog et al. (2017) document no consistent relationship between relaxed liquid asset limits and household asset ownership. By contrast, Ratcliffe et al. (2016) find that households in states that relaxed the SNAP asset limit are more likely to maintain a bank account with at least $500, but detect no significant effects on aggregate liquid assets, net wealth, or vehicle ownership.

2. A related literature examines the effects of SNAP work requirements (a form of labor-supply-conditioned eligibility), with recent work finding no evidence of extensive-margin changes in employment but suggestive evidence of intensive-margin labor supply responses (Han 2022; Gray et al. 2023).

thresholds for Medicaid to 138% of the Federal Poverty Level (FPL), nine states over the past decade have eliminated asset tests for the Low Income Home Energy Assistance Program (LIHEAP), and in 2025 Congress proposed the SSI Savings Penalty Elimination Act to raise asset limits for SSI from $2,000 to $10,000.

These policy changes raise a question that has received surprisingly little direct attention: does it matter *which* eligibility dimension is relaxed? Income tests and asset tests screen along fundamentally different resource margins, and relaxing a given one opens the door to a different set of households. Understanding which screen, when loosened, does more to expand access, reach the most disadvantaged, and efficiently allocate program resources is central to the design of means-tested programs. Yet, the existing literature offers limited guidance, given that income and asset expansions have almost always been studied either as a bundle or in isolation across different programs and contexts.

This paper directly compares the effects of relaxing the asset test versus raising the income threshold within SNAP, one of the largest U.S. government programs. Beginning in 2000, states gained the discretion to expand SNAP eligibility via Broad-Based Categorical Eligibility (BBCE), and they generally exercised this discretion in one of two ways. Some states only relaxed the asset test, typically by eliminating it entirely (from the federal limit of $2,000-$3,000). Others went further by also raising the gross income threshold, typically from 130% to 185–200% of the FPL.[3] We implement this comparison using a stacked difference-in-differences design that exploits the staggered timing of BBCE adoption across states. By comparing states that relaxed only the asset test and states that additionally raised the income threshold to never- and future-treated states around the time of expansion, we can estimate the effect of each lever (with the incremental effect of raising the income threshold identified as the difference between the two). Our analysis focuses on non-elderly, non-disabled households, who are subject to both the asset test and the gross income threshold under federal SNAP rules and for whom BBCE expansions are most relevant.

Our analysis draws on two complementary data sources. The Survey of Income and Program Participation (SIPP) provides the detailed income, asset, and expense data necessary

3. In addition to the gross income and asset tests, SNAP also includes a net income test, which our analysis incorporates. These tests are discussed in greater detail in a later section.

to build a comprehensive SNAP eligibility calculator, along with hardship and demographic measures for assessing targeting. The SNAP Quality Control (QC) administrative files provide high-accuracy data on actual program enrollment and enrollee characteristics. We proceed in three stages. We first exploit the observed pattern of BBCE adoption to compare the effects of each lever on the number of eligible and enrolled households, the characteristics of eligibles and enrollees, and program costs. Because our eligibility calculator can simulate any combination of income and asset rules, we then conduct counterfactual exercises that evaluate each lever independently against a common baseline, allowing us to generalize beyond the specific bundled adoptions that states happened to choose. Finally, we explore whether asset information could play a different (and potentially more valuable) role as part of the benefit formula rather than purely in assessing eligibility.

Using the observed policy variation, we find that raising the income threshold expanded program access among non-elderly, non-disabled households (the population subject to both eligibility screens under federal rules) substantially more than relaxing the asset test. The income expansion increased the share of eligible households by 4.9 percentage points (46%) and enrollment by 1.8 percentage points (22%), while relaxing the asset test increased eligibility by 2.5 percentage points (23%) and enrollment by 1.2 percentage points (12%). Importantly, this larger effect on access did not come at the expense of targeting. Among eligible households, relaxing the asset test brought in those with significantly fewer food hardships, while raising the income threshold brought in eligible households that were only modestly less disadvantaged. Among enrollees, raising the income threshold brought in enrollees with lower education and higher rates of homelessness, while relaxing the asset test brought in enrollees that were no more disadvantaged than baseline. Despite these differences in access and targeting, average benefit amounts per enrollee were similar across the two expansions — a surprising finding given that the income expansion mechanically brought in households eligible for lower benefits.

Two forces can explain this last pattern. First, although the income expansion made many low-benefit households newly eligible, very few actually enrolled. Second, roughly half of the enrollment increase as a result of the income expansion came from inframarginal households (those who were already eligible but had not previously enrolled) who tended to receive high benefits and were more disadvantaged than the newly eligible population. This inframarginal response, while not precisely estimated, is sizable and qualitatively consistent with prior evidence on BBCE expansions (Anders and Rafkin 2024). For both inframarginal and marginal enrollees, we find recertification to be an important stage at which the enrollment gains materialize.[4] For inframarginal households, the income expansion may reduce these burdens by simplifying eligibility determination at recertification. For marginal households, raising the threshold creates a buffer against income fluctuations that would have previously triggered program exit —– a retention channel that, to our knowledge, has received less attention in prior work.

Our counterfactual simulations allow us to generalize the eligibility and targeting results beyond the specific policy bundles observed in practice. In the observed variation, the effect of raising the income threshold is identified incrementally on top of relaxing the asset test, but a policymaker choosing between the two levers independently faces a different comparison. To address this, we simulate each lever applied independently against a common baseline. Not only do both levers continue to produce statistically significant increases in eligibility, but this exercise strengthens our main finding on targeting, as the newly eligible households under the income expansion are now even more disadvantaged than those under the asset relaxation. Our main patterns remain robust to a further exercise that evaluates both levers over the same set of states and time periods to eliminate differences in economic conditions at the time of adoption. Although we do not undertake a full welfare calculation, our findings on access, targeting, costs, and inframarginal responses taken together point toward the income lever delivering larger welfare gains than the asset lever on the channels we can measure.

Given that relaxing the asset test brings in less disadvantaged households than expanding the income test, assets may be a stronger signal of need than income among the eligible population. This raises the natural question of whether asset information could improve the allocation of benefits, not just the determination of eligibility. Under the current SNAP formula, benefits depend entirely on income net of deductions, and a household's asset holdings are irrelevant once it clears the eligibility threshold. We show that "blended" formulas that

4. Prior work has documented that administrative barriers at recertification, such as paperwork burdens and the frequency of eligibility verifications, reduce continued enrollment (e.g., Kabbani and Wilde 2003; Ribar et al. 2008; Gray 2019; Homonoff and Somerville 2021; Unrath 2021; Wu and Meyer 2023)

add a deemed income flow from assets to the net income concept used in benefit calculation — an approach similar to means-testing rules in the United Kingdom and Australia — generate a markedly stronger association between benefit amounts and food hardship. Collecting asset information is often cited as administratively costly, but we find no significant change in administrative costs per recipient from eliminating the asset test. These simulations suggest that the value of asset information may extend beyond eligibility determination to the calibration of benefit amounts.

Our paper contributes to several literatures. At a high level, we contribute to the literature on the design of means-tested programs. The questions of which financial dimension to screen on and how best to use each in program design arise in any means-tested program that combines income and asset tests, and a long tradition in public economics has studied which observable characteristics to condition on when targeting transfers (Akerlof 1978; Nichols and Zeckhauser 1982) and whether to incorporate asset information into program rules (Feldstein 1987; Golosov and Tsyvinski 2006). Yet, while most safety-net programs screen along several resource dimensions simultaneously, the literature has largely studied each dimension in isolation. We provide the first empirical evidence on whether it matters which resource dimension is relaxed when it comes to access, targeting, and program costs.

Second, we contribute to the empirical literature on eligibility expansions in safety-net programs. Within SNAP, most prior work has examined the combined effects of income and asset expansions under BBCE (e.g., Mulligan 2012; Ziliak 2015; Han 2016; Ganong and Liebman 2018; Dickert-Conlin et al. 2021; Austin et al. 2023; McInerney et al. 2025; Moffitt and Yang 2025; Wang et al. 2026).[5] Studies that do isolate one margin tend to focus on different programs and populations. For example, income expansions have been studied in Medicaid (Yelowitz 1995; Currie and Gruber 1996; Guldi and Hamersma 2023), while asset expansions have been studied in TANF (Powers 1998; Hurst and Ziliak 2006; Sullivan 2006) and SNAP (Ratcliffe et al. 2016; Pirog et al. 2017). We complement this literature by separately identifying the effects of the income and asset levers within a single program, focusing on non-elderly and non-disabled households for whom both tests bind. In doing

5. McInerney et al. (2025) study BBCE's effects on participation among older adults, focusing on the role of asset and net income tests given that their sample does not face a gross income.

so, we also contribute to a growing body of work documenting inframarginal or "woodwork" enrollment responses to eligibility expansions across a range of programs, including health insurance (e.g., Frean et al. 2017; McInerney et al. 2021; Sacarny et al. 2022) and school meals (e.g., Leos-Urbel et al. 2013; Marcus and Yewell 2022). Consistent with Anders and Rafkin (2024), who document such responses for SNAP, we find meaningful inframarginal effects from raising the income threshold, and go further by showing that these gains often materialize at recertification rather than initial application.

Third, our paper contributes to a growing literature on how program design shapes the targeting of transfers. Recent work has shown that features of program administration — including application costs (e.g., Deshpande and Li 2019; Finkelstein and Notowidigdo 2019) and recertification requirements (e.g., Homonoff and Somerville 2021; Unrath 2021) — can act as screening mechanisms that determine not just how many but which households participate (e.g., Wu and Meyer 2023; Rafkin et al. 2025). We show that the choice of eligibility dimension matters for targeting not only through who becomes eligible but also through who actually enrolls, and that these two margins can tell different stories based on the extent of differential take-up and inframarginal responses. Finally, we show that the benefit formula can be a tool for targeting dollars toward the most disadvantaged, complementing a literature that has focused primarily on targeting through the extensive margin of participation.

The remainder of the paper is organized as follows. Section 2 describes the SNAP program and the BBCE policy variation. Section 3 describes our data sources and sample construction, and Section 4 presents the empirical framework and methods. Section 5 reports our main results on access, targeting, and costs using the observed policy variation, and Section 6 disentangles heterogeneity in the enrollment effects and discusses a battery of robustness checks. Section 7 presents counterfactual simulations, and Section 8 concludes.

2 Policy Background

SNAP (formerly the Food Stamp Program) is the largest nutrition assistance program in the United States, serving roughly 42 million individuals per month and paying out nearly $94 billion in benefits in fiscal year (FY) 2024 (USDA 2025). Administered federally by the

USDA and delivered through state agencies, SNAP provides in-kind benefits that can be used for food and beverage purchases. A distinctive feature of SNAP, relative to many other safety net programs, is that eligibility is governed primarily by household income and liquid assets — rather than by age, disability status, or the presence of children. As a result, SNAP is broadly available to all low-resource households in the U.S.[6]

2.1 Federal Eligibility Rules and Benefit Formula

Under baseline federal rules, a household must generally satisfy a gross income test, a net income test, and an asset test to qualify for SNAP. Gross monthly income must be below 130% of the FPL, which varies with household size, while net income (i.e., gross income net of allowable deductions for earnings, dependent care, medical expenses, and shelter costs) must be below 100% of the FPL. Households containing an elderly (age 60+) or disabled member are exempt from the gross income test. For assets, the federal limit was $2,000 for most of our sample period before being raised to the current level of $3,000 (with a higher limit for households containing an elderly or disabled member). This limit does not vary with household size.

Conditional on eligibility, household i's monthly SNAP benefit in month t is given by:

$$B_{it} \;=\; \bar{B}_{h(i)} \;-\; 0.30 \times \underbrace{(\text{Gross Income}_{it} - \text{Deductions}_{it})}_{\text{Net Income}_{it}},$$

where $\bar{B}_{h(i)}$ is the maximum monthly allotment for household size h, based on the USDA's Thrifty Food Plan and is adjusted annually for inflation.[7] A household with zero net income receives the maximum allotment, and benefits then decline at a rate of 30 cents per dollar of net income.[8] A key feature of this formula for our analysis is that benefit amounts depend entirely on net income. Conditional on meeting the asset test for eligibility, a household's

6. Able-bodied adults without dependents (ABAWDs) may face additional work-related requirements, although federal law permits states and localities to waive these requirements under certain conditions.

7. In FY 2024, the maximum monthly allotment for a four-person household was $973. This number reflects the maximum SNAP allotments for the 48 contiguous states and the District of Columbia. Alaska and Hawaii have higher maximum allotments.

8. For instance, a four-person household with net monthly income of $1,000 would receive a monthly benefit of $\$973 - 0.30 \times \$1,000 = \$673$. Benefits reach (approximately) zero when net income exceeds $\bar{B}_h/0.30$, although one- and two-person households are eligible for a small minimum benefit.

asset levels have no bearing on the benefit amount it receives.

2.2 Broad-Based Categorical Eligibility (BBCE)

In addition to the standard eligibility path described above, households can also qualify for SNAP through categorical eligibility, which links SNAP eligibility to participation in other means-tested transfer programs. Historically, categorical eligibility was tied to the receipt of cash assistance programs such as SSI and Aid to Families with Dependent Children (AFDC), which have more stringent financial eligibility limits than SNAP. Some of the central goals of categorical eligibility were to streamline the application process, reduce verification burdens, and improve coordination across programs.

In 1996, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) replaced AFDC with TANF. States now had the discretion to use federal TANF and state maintenance-of-effort (MOE) funds for a wide range of benefits and services, including for non-cash benefits such as informational brochures or referrals. In 2000, USDA regulations formalized the Broad-Based Categorical Eligibility (BBCE) policy, which enabled states to establish categorical eligibility for SNAP on any household where at least one member received or was authorized to receive non-cash TANF- or MOE-funded aid, provided the aid program had an income limit of up to 200% of the FPL. This effectively gave states the ability to expand SNAP eligibility to higher-income and/or higher-asset households.

BBCE has become a critical tool for states to improve SNAP access for households excluded by stringent asset or income tests, particularly working families with higher incomes or modest savings. In practice, states adopted BBCE in one of two distinct ways. Some states relaxed only the asset test, with most eliminating it entirely and a few raising it to \$5,000–\$25,000. Other states both relaxed the asset test and raised the gross income threshold beyond the federal limit of 130% of the FPL, typically to 185% or 200% (though some chose intermediate levels like 160–165%). Nearly all states that raised the gross income threshold also eliminated the net income test; accordingly, our concept of "raising the income threshold" encompasses both changes.[9] It is worth emphasizing that even under the broadest

9. A subset of states that relaxed only the asset test also eliminated the net income test, but this change has limited bite when the gross income threshold remains unchanged. This is because standard and other

BBCE expansions, the benefit formula itself is unchanged: benefits continue to depend on net income as described above. BBCE therefore expands who is *eligible*, but the benefit amount for any given household is determined by the same federal formula.

Figure 1 summarizes the timing and geography of BBCE adoption between January 1996 and December 2019. Panel (a) plots the number of states that either relaxed the asset test only (green circles) or relaxed both the asset and income tests (blue diamonds) over time. States that relaxed only the asset test concentrated their adoptions in the late 2000s and early 2010s (coinciding with the Great Recession), with some of these states removing BBCE programs in later years. The expansions during the Great Recession were spurred by a desire to reduce administrative workloads as SNAP caseloads surged. A 2009 USDA memo explicitly encouraged BBCE adoption, noting it would benefit states "by simplifying policies, by reducing the amount of time states must devote to verifying resources, and by reducing errors" (USDA 2009). In contrast, states that relaxed both tests tended to do so in two distinct waves: nine states adopted these broader expansions in the early 2000s, soon after USDA's authorization of BBCE, and nearly twenty more adopted them in the late 2000s and early 2010s during the Great Recession.

Panel (b) maps states according to the policy bundle adopted at their *initial* BBCE expansion.[10] Under this classification, 18 states adopted asset-only BBCE as their initial policy (green), while 24 states (including the District of Columbia) adopted a broader bundle that concurrently relaxed the asset test and raised the income threshold (blue). The remaining 9 states never implemented BBCE as of January 2020 (yellow). These never-adopting states will form an important component of our comparison group, as we discuss in Section 4. While qualitative accounts suggest that asset-test relaxations were often motivated by reducing documentation burdens for clients and caseworkers (Rosenbaum 2019), there is ultimately little systematic evidence on why certain states adopted particular BBCE designs (asset-only versus broader income expansions, and the choice of specific income limits) (Rachidi and Randolph 2025). That said, we empirically check for potential biases through a

deductions typically reduce net income well below gross income, so few households fail the net income test at 100% FPL while passing the gross income test at 130% FPL.

10. This classification treats initial adoption as the relevant "treatment" for describing the rollout: for example, a state that first relaxed the asset test and later raised the income threshold is classified as "asset-only" in the map, even if it eventually moved into the broader category.

number of exercises, including tests of pre-trends and robustness to time-varying covariates.

3 Sample and Data

This section describes our sample construction and data sources. Our primary data sources include the Survey of Income and Program Participation (SIPP) and the SNAP Quality Control (QC) administrative files. The SIPP provides the detailed income, asset, and expense data necessary to simulate SNAP eligibility under different policy regimes. It also includes rich data on material hardships and demographic characteristics, which can be used to analyze program targeting among eligibles. The QC data offer detailed administrative records on a random sample of actual SNAP recipients, allowing us to analyze program enrollment, targeting among enrollees, and benefit amounts with high accuracy.

3.1 Analysis Sample

Our analysis focuses on non-elderly, non-disabled units, who — following official USDA definitions — have no members aged 60 or above and do not receive government disability payments such as Social Security (OASDI), SSI, or Veterans Disability Compensation. This sample restriction is motivated by the fact that units with elderly or disabled members are exempt from the gross income test under federal SNAP rules, making BBCE expansions less relevant for them. Therefore, unlike prior studies that often examined all households, our focus is onworking-age, able-bodied households, who constitute the majority of SNAP participants and are subject to both the asset test and income threshold under federal rules (each of which can be a binding constraint on eligibility).

3.2 Survey of Income and Program Participation (SIPP) Data

We use data from the SIPP to examine how expanding financial eligibility affects the number, composition, and predicted benefits of eligible households. This nationally representative survey provides detailed information on demographics, economic conditions, and the receipt and amounts of various income sources. Each SIPP panel interviews approximately 25,000–

45,000 households, following them for up to five years. We use data from the 1996, 2001, 2004, 2008, 2014, 2018, 2019, 2020, 2021, and 2022 panels, spanning more than 25 years. The SIPP is uniquely suited for simulating SNAP eligibility because it provides all of the components necessary to comprehensively determine eligibility under different policy regimes: (1) monthly income from taxable and non-taxable sources to calculate gross income; (2) information on liquid and non-liquid assets to determine countable assets; and (3) monthly expenses (medical, child care, shelter) to calculate net income after deductions. The SIPP reports all income sources at the monthly level, aligning with SNAP's reference period, and collects detailed asset and expense information annually.[11]

The most comparable existing estimates of eligibility come from the USDA's official SNAP take-up calculations, which uses CPS ASEC data to simulate the number of eligible households under federal rules only (USDA 2024b). Panel (a) of Figure 2 shows that our SIPP-based eligibility counts under federal rules closely track the USDA's CPS-based counts nationally across fiscal years. The fact that two independent calculations using different survey data produce similar estimates is reassuring and provides a foundation for the additional features of our calculator. In particular, because the SIPP contains the monthly income, asset, and expense data needed to apply state-specific policy rules, we can go beyond the federal baseline and simulate eligibility under actual BBCE rules (as we describe below and in Appendix Section B). Panel (b) illustrates the quantitative importance of doing so. Through the mid-2000s, national eligibility counts under federal rules and federal-plus-BBCE rules are close to each other, as few states had adopted BBCE. But a substantial gap emerged as BBCE adoption accelerated, and in recent years accounting for BBCE increases the count of eligible households by roughly 50%. While the SIPP faces measurement error like all surveys, it is widely considered the most accurate for income reporting (Meyer et al. 2015) and is

11. Alternative datasets fall short on key dimensions. Tax records, which have been widely used to measure income (e.g., Chetty et al. 2014), report only annual taxable income that potentially misses month-to-month shocks and excludes non-taxable sources. These records also cover only those connected to the tax system, potentially missing the lowest-income households who are precisely those eligible for SNAP. Alternatively, Unemployment Insurance wage records provide quarterly earnings data but still miss the monthly variation relevant for SNAP and lack information on non-wage income sources. Other Census surveys like the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) and American Community Survey (ACS) measure income annually rather than monthly and lack the comprehensive asset and expense data necessary for directly simulating eligibility.

perhaps the only dataset containing all necessary components at the appropriate reference period for directly simulating SNAP eligibility.[12]

Methods for Simulating SNAP Eligibility. We define SNAP units at the household level, given that benefit units are based on who purchases and prepares meals together.[13] Gross monthly household income is taken directly from a recoded survey variable. Countable assets are calculated by summing assets held in bank accounts, stocks, other financial institutions, and real estate (excluding the primary residence), plus vehicle values in states with vehicle tests. To calculate net income, we subtract from gross income the standard deduction, earnings deduction, medical expenses, child support paid, and dependent care costs to obtain adjusted income, and then compute the shelter deduction as shelter expenses minus half of adjusted income. Net income equals adjusted income minus the shelter deduction. Asset and expense information are drawn from topical modules in pre-2014 SIPP panels and from core files in later panels.[14] Armed with the relevant values for household gross income, net income, countable assets, and applicable demographic characteristics, we can simulate SNAP eligibility under federal rules, BBCE rules (where relevant), or any combination of hypothetical income and asset rules. Our eligibility simulation also realistically accounts for simplified reporting rules adopted by some states starting in the mid-2000s. Under these rules, SNAP recipients are not required to report changes in their financial circumstances unless gross income exceeds the eligibility limit prior to recertification, which typically occurs every 6 or 12 months. For more details, see Appendix Section B.

12. Moreover, measurement error in the SIPP may be less of a first-order concern for our analysis than it would be for studies focused on *levels* of eligibility. Because our research design relies on difference-in-differences estimates, misreporting of incomes or assets would need to trend differentially between treated and control states to bias our results on eligibility. Additionally, our enrollment outcomes are measured using QC administrative data, which provide a high degree of accuracy in capturing actual program participation.

13. For the purposes of this paper, we treat SNAP assistance units (cases) as equivalent to survey households, though there may be some mismatch between these concepts. For example, Czajka et al. (2015) use linked survey and administrative data for New York and Colorado to estimate that a recipient household has on average 1.06-1.08 SNAP assistance units. These ratios are sufficiently small such that they are unlikely to meaningfully affect our results. That said, we also conduct robustness checks using families (within households) as our units of analysis.

14. In pre-2014 SIPP panels, data on assets and expenses were typically collected once every three interviews, with each interview containing data for four months. For months lacking this information, we impute values from the nearest available interview in which asset and expense information is collected for the same household.

Food Hardship and Demographic Characteristics. The SIPP also collects extensive data on material hardship and demographic characteristics, drawn from topical modules prior to 2014 and from core files in recent years. These measures allow us to assess the types of eligible households targeted by asset versus income expansions. We focus on three consistently measured food hardship indicators aligned with USDA's stated purpose for SNAP "to help [households] afford the nutritious food essential to health and well-being": (1) food bought did not last, (2) could not afford balanced meals, and (3) cut meal size or skipped meals.[15] We also construct a composite measure summing across these three indicators.[16] We further examine income-to-poverty ratio, education, whether the household has earnings, age, household structure, marital status, and race. Finally, as supplementary outcomes, we analyze homeownership status, the presence of savings and retirement accounts, as well as the receipt of various income sources (including earnings, child support, Unemployment Insurance, and TANF).

3.3 Quality Control (QC) Data

Our principal data source on SNAP recipients comes from the 1996-2022 QC files from USDA and Mathematica. The QC records consist of administrative microdata for a 1% random sample of national recipients audited monthly, enabling us to construct repeated cross-sections at the state-month level. A key advantage is that these data are derived from administrative records rather than self-reported survey data, avoiding the measurement errors that may otherwise affect program receipt in surveys (Meyer et al. 2015; Meyer and Mittag 2019).[17] Beyond enrollment, the QC data contain information about the characteristics of enrollees reported on application and recertification forms, including benefit amounts, income sources, and selected demographic and economic characteristics (e.g., case structure,

15. See https://www.fns.usda.gov/snap/supplemental-nutrition-assistance-program.

16. We use these questions to measure food hardship because they are consistently collected in the SIPP throughout our sample period. These measures are a subset of those in the USDA's Household Food Security Survey Module (Bickel et al. 2000) and are similar to those used in prior SIPP-based work on SNAP (Wei and Gundersen 2024).

17. The ideal dataset would be universe SNAP microdata for all states and years. While such microdata exist for certain states at the Census Bureau, their geographic and temporal coverage are incomplete: only several dozen states are available, starting from 2004 for some and the mid-2010s for others. In contrast, the QC files cover all states continuously, enabling us to leverage all available policy variation.

age, education, marital status, race/ethnicity, experiencing homelessness). The data also identify whether each case most recently appeared at initial application or recertification.

3.4 Additional Datasets

For policy information on when and which states implemented BBCE, we primarily rely on the USDA SNAP Policy Database (USDA 2024a). This database contains details on the gross income and asset limits implemented by each state for every month between January 1996 and December 2020. It also contains information on other state-level enrollment policies that we use as covariates, including those related to online applications, call centers, phone interviews, and combined applications with SSI (to name a few). Because this database does not record whether states eliminated the net income test when adopting BBCE, we hand-collect this information from QC technical documentation and other sources. We also bring in state-level unemployment rates from the Bureau of Labor Statistics (used as covariates in our regression specification) and population characteristics from the Census Bureau (used also as covariates and to construct denominators for our eligibility and enrollment share outcomes).

4 Empirical Framework

This section develops the conceptual and empirical tools we use to compare the effects of relaxing the asset test versus raising the income threshold. We start by discussing a conceptual framework that maps each policy lever onto the population of households it brings into eligibility. We then describe our stacked difference-in-differences (DiD) design to empirically isolate the causal effects of each policy lever.

Before turning to the framework, it is useful to ask what determines whether one reform delivers larger welfare gains than another. The welfare gain from an eligibility expansion depends not just on how many households gain eligibility but on who they are and what it costs to enroll them. A reform that expands access more broadly is not necessarily preferable if it brings in households who are less disadvantaged or whose benefits are costlier per dollar of welfare delivered. Conversely, a reform with a smaller effect on eligibility may still dominate if it draws in more disadvantaged households, induces take-up among already-eligible households (who are typically more disadvantaged than the newly eligible), or avoids large behavioral responses outside the program. The comparison therefore depends on how access, targeting, and costs combine in practice, which is what our empirical analyses are designed to measure. Appendix Section C formalizes these ideas in a welfare decomposition with four channels: a newly eligible welfare differential, an inframarginal welfare differential, an administrative cost differential, and a fiscal externality differential. We can speak empirically to ingredients in the first three, and we discuss the fourth when interpreting our findings in the conclusion.

4.1 Conceptual Framework

Suppose that households are characterized by two resource dimensions: income y and assets a, drawn from a joint distribution $F(y, a)$. A household's level of need or disadvantage is captured by a function $h(y, a)$ that is decreasing in both income and assets. Under baseline federal SNAP rules, a household is eligible if its income falls below a threshold $\bar{y}$ and its assets fall below a limit $\bar{a}$.[18] This defines the rectangular eligibility region $E_0 = \{(y, a) : y < \bar{y},\ a < \bar{a}\}$, depicted in Panel (a) of Figure 3.

BBCE allows states to expand eligibility along two dimensions. First, a state can relax (eliminate) the asset test by expanding the eligibility region via the set $\Delta_A = \{(y, a) : y < \bar{y},\ a \geq \bar{a}\}$ shown in Panel (b). These newly eligible households still have incomes below the federal threshold but their assets are now above the federal limit. Because they are drawn from the high-asset region ($a \geq \bar{a}$), they should be on average less disadvantaged than the baseline eligible population. Second, a state can additionally raise the gross income threshold from $\bar{y}$ to some $\bar{y}' > \bar{y}$. Combined with relaxing the asset test, this further expands the eligibility region by adding the set $\Delta_I = \{(y, a) : \bar{y} \leq y < \bar{y}'\}$ shown in Panel (c). This higher-income group is heterogeneous in assets and includes both high-asset households in $\Delta_I^H = \{(y, a) : \bar{y} \leq y < \bar{y}',\ a \geq \bar{a}\}$ and low-asset households in $\Delta_I^L = \{(y, a) : \bar{y} \leq y < \bar{y}',\ a < \bar{a}\}$. In particular, the latter group (e.g., workers without

18. For simplicity, we abstract from the distinction between gross and net income tests and from household-size variation in thresholds. These details are incorporated in our empirical analysis.

savings) may still be quite disadvantaged despite their moderately higher incomes.

This framework motivates two distinct ways to estimate the effects of each policy lever, which differ in what populations they compare and what they identify. The first (and main) approach exploits the "observed" pattern of BBCE adoption. As described in Section 2, some states relaxed only the asset test while others relaxed the asset test *and* raised the income threshold. By comparing asset-only states to untreated states, we can estimate the effect of adding Δ_A to E_0. By then comparing asset-plus-income states to asset-only states, we can estimate the *incremental* effect of adding the full strip $\Delta_I = \Delta_I^L \cup \Delta_I^H$ to a pool that already includes Δ_A. This approach has the advantage of exploiting actual policy variation.

However, the observed variation reflects specific policy bundles that states happened to adopt and the two coefficients are estimated against different comparison pools, meaning this approach may not provide a fully apples-to-apples comparison of the two levers. A policymaker considering whether to relax the asset test *or* raise the income threshold — but not both — faces a different comparison. To address this and generalize our findings to such a scenario, our second approach uses "counterfactual" simulations that apply each policy lever independently against a common baseline. When we simulate raising the income threshold without relaxing the asset test, the newly eligible set is only Δ_I^L (moderate-income households who also have low assets). When we simulate relaxing the asset test without raising the income threshold, the newly eligible set remains Δ_A. Because both coefficients are now estimated against the same baseline (E_0), this approach allows us to draw broader lessons about the comparative effects of each policy lever beyond the specific bundled adoptions we observe in practice. We implement this for eligibility-related outcomes, which we can simulate directly from SIPP microdata.

The framework also generates predictions for the effects of each policy lever under the observed comparison and for how the counterfactual comparison may differ. For the observed comparison, the framework yields clear predictions across three sets of outcomes. For the *number of eligible households*, asset relaxation adds Δ_A while income expansion adds the full strip Δ_I; the relative magnitudes depend on the joint distribution of assets and income in the population, which is an empirical question. For *targeting*, both levers should bring in households that are less disadvantaged than the baseline eligible population (as households in Δ_A have assets above the federal limit, while those in Δ_I have incomes above the federal threshold). However, the comparison between the two groups of newly eligible households is less clear. Households in Δ_I^H are unambiguously less disadvantaged than those in Δ_A, but households in Δ_I^L trade off higher income against lower assets (making the comparison with Δ_A ambiguous). For *benefit amounts*, relaxing the asset test should have little effect on average benefits since the current SNAP formula does not depend on assets. By contrast, raising the income threshold brings in higher-income households, which maps directly into lower benefits.

The counterfactual comparison differs from the observed only through the income lever by capturing Δ_I^L rather than $\Delta_I = \Delta_I^L \cup \Delta_I^H$. For *eligibility*, the counterfactual income expansion should yield a weakly smaller effect, since $\Delta_I^L \subset \Delta_I$. For *targeting*, the counterfactual income expansion excludes the high-asset households in Δ_I^H and should therefore bring in weakly more disadvantaged households on average — implying a larger targeting advantage for the income lever relative to the asset lever. For *benefit amounts*, whether the two approaches diverge depends on whether incomes differ systematically across asset categories within the $(\bar{y}, \bar{y}')$ range.

We note that the observed versus counterfactual approaches also differ in their baseline comparison pools: the observed income-threshold effect is measured against $E_0 \cup \Delta_A$, while the counterfactual is measured against E_0 alone. This could in principle complicate the targeting and benefit comparisons, but it may not have a large effect if Δ_A represents a modest addition to the baseline pool (something we can assess empirically). Finally, these predictions pertain to the eligible population; effects among enrollees additionally depend on selection into take-up and on inframarginal responses among previously eligible households.

4.2 Stacked Difference-in-Differences Design

We implement both the observed and counterfactual comparisons using a stacked DiD design. We adopt this approach given that states adopted BBCE at different times, and recent research has documented biases in standard two-way fixed effects estimators with variation in treatment timing (e.g., Chaisemartin and D'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021). A stacked DiD design enables clean comparisons between treated and untreated states around each treatment date (Cengiz et al. 2019; Deshpande and Li 2019). We classify treated states into two groups: (i) *asset-only* states that relaxed the asset test but did not raise the gross income threshold, and (ii) *asset-plus-income* states that both relaxed the asset test and raised the income threshold.

For each treated state, we construct a control group comprising "never-treated" states (those never adopting BBCE) and "later-treated" states (those adopting BBCE more than three years after the focal treated state). The three-year window matches the post-treatment period of the treated state, eliminating any overlap with the pre-treatment period of the control group. We then stack these state-specific comparisons, yielding 18 stacks corresponding to asset-only states and 22 stacks corresponding to asset-plus-income states.[19] The key identifying assumption is that treated and untreated states would have followed similar trends in outcomes absent treatment, an assumption we can validate by testing for pre-trends and demonstrating robustness to covariates.

Our approach estimates two stacked DiD models of the same form — one using the asset-only stacks and one using the asset-plus-income stacks. Let s index states, g index stacks, and t index year-months. Let $Post_{sgt}$ equal one if state s is the treated state in stack g and year-month t falls in the post-treatment period. Each model takes the form:

$$Y_{sgt} = \alpha_{sg} + \lambda_{tg} + \gamma \cdot Post_{sgt} + \beta X_{st} + \varepsilon_{sgt}, \tag{1}$$

where Y_{sgt} is the outcome, α_{sg} and λ_{tg} are stack-by-state and stack-by-time fixed effects, and X_{st} is a vector of time-varying state covariates.[20] We aggregate observations to the state-month level, in large part because the QC microdata contain only recipient cases.[21]

19. Appendix Table A.1 shows a list of all states and their BBCE policy changes. Note that twenty-four states raised the gross income limit, with all but two also eliminating the net income test. Since we define "raising the income threshold" as both raising the gross income limit and eliminating the net income test, we exclude Montana and South Dakota (which only raised the gross income limit) from our analysis.

20. The fixed effects α_{sg} and λ_{tg} control for time-invariant differences across state-stack combinations and common time shocks within each stack, respectively, ensuring that identification comes from within-stack comparisons of treated versus control states around the treatment date. Covariates include population shares by race/ethnicity, gender, and age group; the unemployment rate; SNAP outreach spending per capita; and indicators for various SNAP policies — i.e., simplified reporting, longer recertification periods (beyond three months), face-to-face interview requirements, call centers, online applications, combined SSI applications, vehicle exclusions from asset tests, TANF transitional benefits, and elimination of the net income test.

21. Since the QC data contain only SNAP recipients, we cannot construct individual-level enrollment indicators, which would require observing both participating and non-participating households.

The coefficient $\hat{\gamma}$ from the asset-only model estimates the effect of relaxing the asset test (corresponding to Δ_A in Figure 3), while $\hat{\gamma}$ from the asset-plus-income model estimates the combined effect of both policy changes. The incremental effect of raising the income threshold (corresponding to Δ_I in Figure 3) is identified as the difference between these two estimates. In practice, we pool the two sets of stacks and interact all terms with a specification indicator $D_g \in \{0, 1\}$ (equal to zero for asset-only stacks and one for asset-plus-income stacks), which is equivalent to estimating the models separately but allows us to directly estimate the difference and its standard error. Standard errors are clustered at the specification-by-state level.

To examine how effects evolve over time and to test whether treated and control states were on parallel trends prior to treatment, we estimate an event-study analog of each model:

$$Y_{sgt} = \alpha_{sg} + \lambda_{tg} + \sum_{k \neq -1} \gamma_k \cdot \mathbf{1}^k_{sgt} + \beta X_{st} + \varepsilon_{sgt}, \tag{2}$$

where $\mathbf{1}^k_{sgt}$ equals one if state s is the treated state in stack g and year-month t falls in the kth year relative to treatment. We pool across months within years to increase power and reduce seasonality bias, using a window of three years pre- and post-treatment ($k \in -3, ..., 2$, with $k = -1$ as the omitted base period).[22] As with the static model, the event-study coefficients from the asset-only model trace out the dynamic path of the asset-test effect, while the difference in coefficients across models traces the incremental effect of raising the income threshold. Pre-treatment differences ($k < 0$) serve as a test of parallel trends.

These empirical designs apply to both the observed and counterfactual comparisons. For the observed comparison, the outcomes reflect actual eligibility, enrollment, or characteristics under the policies states adopted. For the counterfactual comparison, Y_{sgt} reflects simulated eligibility under each policy lever applied independently to a common baseline, as we describe in Section 7. Our key outcomes include the share of non-elderly, non-disabled households that are eligible for SNAP (based on the SIPP) and the share that are enrolled (based

22. We aggregate to the yearly level rather than reporting monthly coefficients because monthly estimates are noisy, particularly for SIPP-based outcomes where sample sizes within state-months are small. We also choose a relatively short post-treatment window (three years) to focus on the immediate effects of the expansions. For targeting outcomes in particular, a longer horizon would risk conflating the composition of the newly eligible or enrolled population with downstream behavioral responses to program participation.

on the QC data).[23] We also analyze average characteristics and benefit amounts among eligibles and enrollees. Regressions are weighted by total non-elderly/disabled households (for share outcomes), eligible households (for characteristics of eligibles), or recipient cases (for characteristics of enrollees).

5 Main Results

We now present results from the observed policy variation, organized around three sets of outcomes: access (eligibility and enrollment), targeting (characteristics of eligibles and enrollees), and costs (benefit amounts and administrative costs). Throughout, we report estimates from equation (1) for time-averaged effects and equation (2) for event-study analogs.

5.1 Impacts on Access

We begin by comparing the effects of each policy lever on the share of non-elderly, non-disabled households that are eligible for SNAP (based on the SIPP) and enrolled in SNAP (based on the QC data). Although the eligibility effects largely reflect the underlying joint distribution of income and assets, they remain substantively important. They determine who has the legal right to apply and shape the population from which take-up and enrollment outcomes subsequently emerge. In practice, "relaxing" the asset test meant eliminating it (from the federal limit of \$2,000-\$3,000) in most states, with the exception of three states that raised but did not eliminate their asset limits (Indiana to \$5,000, Minnesota to \$7,000, and Nebraska to \$25,000). States that additionally raised the gross income threshold did so from 130% FPL to an average of roughly 190% FPL.

Figure 4 shows event-study estimates from equation (2). Looking first at eligibility in Panel (a), we find that raising the income threshold increased the share of eligible households by substantially more than relaxing the asset test. Both effects materialize immediately and

23. Denominators (total non-elderly/disabled households) come from the ACS starting in 2000 and the CPS ASEC for earlier years. Numerators come from the SIPP for eligibility and the QC data for enrollment. We use shares rather than raw counts, which vary dramatically by state size. For SIPP-based analyses, we omit five small states (Maine, North Dakota, South Dakota, Vermont, and Wyoming) that are not separately identified in earlier SIPP panels.

remain stable over the post-treatment period, with little to no evidence of differential pre-trends for either policy lever. Averaged over three post-treatment years, relaxing the asset test increased the share of eligible households by 2.5 percentage points, a 23% increase over the baseline mean, while raising the income threshold increased the share by an additional 4.9 percentage points — nearly double the effect of relaxing the asset test alone.[24]

These broad patterns carry over to enrollment in Panel (b), where the increase induced by raising the income threshold again exceeds that from relaxing the asset test. However, the enrollment effects are more gradual, building over the post-treatment period rather than appearing immediately. Averaged over three post-treatment years, raising the income threshold increased enrollment by 1.8 percentage points (a 22% increase), statistically significant at the 5% level. Relaxing the asset test increased enrollment by 1.2 percentage points (a 12% increase), which is not statistically significant at conventional levels, although the effect two years after treatment is marginally significant at the 10% level. Note that the specific magnitudes for enrollment may reflect a combination of forces including differences in the size of the newly eligible population, differential take-up among the newly eligible, and potential inframarginal responses. We discuss these channels in further detail later in this section and in Section 6.

5.2 Impacts on Targeting

Having established that raising the income threshold generated larger effects on the share of households who are eligible and enrolled, we now examine the *types* of households brought in by each policy lever. Comparing the targeting properties of each lever — i.e., the extent to which one brings in more or less disadvantaged households than another — helps shed light on the welfare implications of expanding eligibility through different channels. Table 1 reports time-averaged changes in characteristics among eligible households (Columns 1–4) and enrolled cases (Columns 5–8) from equation (1), with Appendix Figures A.1 and A.2 showing event-study analogs underlying these static estimates.

24. Note that the relatively modest size of the asset-test effect (Δ_A in Section 4.1) also suggests that the difference in baseline comparison pools between the observed and counterfactual approaches is unlikely to materially affect our results.

We start by examining targeting among eligible households. Our primary targeting measures are food hardship indicators from the SIPP.[25] Looking at our composite measure that takes the sum of three separate hardship indicators, we find that both levers brought in eligible households that were less disadvantaged than those eligible under federal rules. However, the reduction in disadvantage was considerably larger from relaxing the asset test: the number of food hardships among eligibles fell by 0.15 (a 25% decline relative to baseline, significant at the 5% level), compared to a decline of 0.07 (11%, not statistically significant) under the income expansion. Across each of the three individual indicators — food did not last, meals were not balanced, and had to skip meals — relaxing the asset test consistently brought in less disadvantaged eligibles than relaxing the income test, with effects that are statistically significant at the 10% level or better. Pre-trends are also flat for each of these measures (Appendix Figure A.1, Panels a–d).

We also assess targeting among eligibles using average income-to-poverty ratio and years of education. The income-to-poverty ratio among eligibles remained essentially unchanged as a result of relaxing the asset test, while it increased by 26.5 percentage points (43% over baseline, significant at the 1% level) as a result of raising the income threshold. This latter result is a largely mechanical effect, since the newly eligible have higher incomes by construction. Years of education of the household head also increased under both levers, but neither estimate is statistically significant and the event study (Appendix Figure A.1, Panel f) shows a suggestive pre-trend for relaxing the asset test.

We next examine targeting among enrolled cases. Food hardship measures are not available in the QC data, so we focus on two outcomes common to both the SIPP and QC data (income-to-poverty ratio and years of education), as well as an indicator for experiencing homelessness (unique to the QC data).[26] For the income-to-poverty ratio, both levers brought in slightly more advantaged enrollees, with similar magnitudes (2.7 pp for asset relaxation, significant at the 5% level, and 0.025 pp for the income expansion, not statistically significant). Notably, the increase under the income expansion was far smaller among

25. Because these food hardship measures are not collected in every interview month, we assign each household's nearest observed value to months in which the measures are not collected.

26. For the two common outcomes between eligibles and enrollees, baseline means are consistently lower among enrollees than among eligibles, reflecting the previously documented pattern that enrollees tend to be a disadvantaged subset of the eligible population (see, e.g., Rafkin et al. 2025).

enrollees than among eligibles (2.5 versus 26.5 pp), suggesting that selection into enrollment substantially attenuated the mechanical effect on eligibles. For years of education, relaxing the asset test produced no significant change (-0.018 years), while raising the income threshold brought in enrollees with significantly lower education (-0.34 years, significant at the 1% level). For homelessness, relaxing the asset test brought in enrollees with slightly lower rates (-0.8 pp) while raising the income threshold brought in enrollees with slightly higher rates ($+0.8$ pp), although neither estimate is statistically significant.

Taken together, the income expansion continued to bring in more disadvantaged enrollees than asset relaxation across multiple measures. However, the results also present a puzzle. Raising the income threshold brought in enrollees who were no less disadvantaged — and on education, significantly *more* disadvantaged — than those at baseline, despite bringing in less disadvantaged eligibles. This is difficult to reconcile with a model in which new enrollees come exclusively from the newly eligible population. One potential explanation is that raising the income threshold also drew in previously eligible but unenrolled (inframarginal) households who tended to be more disadvantaged (see, e.g., Anders and Rafkin 2024). We examine this hypothesis more rigorously in Section 6.

Table 2 provides a broader picture of how the composition of eligibles and enrollees changed along other demographic and economic dimensions, with Appendix Table A.2 reporting an expanded set of characteristics.[27] Among eligibles, relaxing the asset test produced largely insignificant changes across a range of characteristics, although there is (noisy) evidence that it brought in younger and non-white individuals. In contrast, raising the income threshold brought in eligible households that were significantly more likely (at the 5% level) to have earnings ($+10.5$ pp, 16%) and to be married ($+7.1$ pp, 20%), but significantly less likely to have children (-7.2 pp, 11%).[28] Among enrollees, the effects of both levers on demographic characteristics are largely statistically insignificant. For relaxing the asset test, this parallels the null results among eligibles. For the income expansion, however, the null

27. Comparing baseline means across the two populations reinforces the pattern that enrollees are a selected subset of eligibles. Relative to eligible households, enrolled cases are less likely to have earnings, less likely to be married, more likely to have younger household heads, and more likely to be non-white.

28. In terms of supplemental outcomes (Appendix Table A.2), eligible households from the income expansion were also significantly more likely to own a home ($+6.5$ pp, 18%) and to have a retirement account ($+5.2$ pp, 44%).

results are more puzzling: for instance, the significant increase in the share with earnings among *eligibles* does not appear among *enrollees*. This pattern is again potentially consistent with the enrollment response to raising the income threshold being partly driven by inframarginal households whose characteristics offset those of the newly eligible.

5.3 Impacts on Costs

We now turn to the cost implications of each policy lever, focusing on direct costs in the form of benefit amounts and administrative costs. We start by discussing benefit payments, which comprise approximately 90% of total SNAP outlays and are thus the dominant cost margin. Prior studies have often used benefit amounts as measures of targeting, on the logic that higher benefits signal greater disadvantage. In our setting, however, benefit amounts are mechanically linked to income through the benefit formula, making them difficult to interpret as pure targeting measures when one of the policy levers operates by directly changing the income threshold. We therefore analyze benefit amounts from the perspective of program costs, and specifically whether additional benefit dollars under each expansion are directed toward the most disadvantaged households.

Figure 5 shows event-study estimates for average benefit amounts expressed as a share of the maximum for a given household size, which effectively equivalizes across households of different sizes. Among eligibles (Panel a), relaxing the asset test produced no significant change in average benefit amounts. This is consistent with the predictions in Section 4.1, as households in Δ_A have assets above the federal limit but span the same income range as baseline eligibles. Column (1) of Table 3 confirms this pattern, showing that more than three-quarters of newly eligible households under asset-test relaxation would have received benefits between 50–100% of the maximum, and nearly half would have received benefits above 75% — consistent with the average benefit amounts (69% of the maximum) simulated for baseline eligibles. In contrast, Panel (a) of Figure 5 shows that raising the income threshold led to a significant decrease in average benefits among eligibles of 0.14 points as a share of the maximum (a 21% decline). This follows directly from newly eligibles having incomes above the federal limit, thereby feeding into lower benefits through the benefit formula. Column (2) of Table 3 shows that the entire increase in eligibility from the income expansion was concentrated among households who would have received benefits below 50% of the maximum, with nearly 90% of the increase coming from those below 25% of the maximum.

Among enrollees, Panel b of Figure 5 shows small and statistically insignificant changes in average benefits as a result of relaxing the asset test. Mirroring the pattern for eligibles, three-quarters of the enrollment increase came from cases with benefits above 50% of the maximum (Column 5 of Table 3). For raising the income threshold, the decline in average benefits among enrollees was also small and statistically insignificant. This parallels the finding from Section 5.2 that raising the income threshold produced only a small, insignificant increase in the income-to-poverty ratio among enrollees despite a large increase among eligibles. Two forces can explain why the large decline in benefits among eligibles did not carry over to enrollees. First, although raising the income threshold produced a significant enrollment increase of 0.5 pp among those in the lowest benefit category (<25% of maximum), this represented less than 12% of those who became newly eligible in the same category. Thus, very few of the newly eligible low-benefit households actually enrolled. Second, the majority of the enrollment effect came from cases with benefits between 75–100% of the maximum (albeit noisily estimated). Because households in this range are likely to have very low incomes and be eligible even under federal rules, this enrollment response must have come from inframarginal households who were already eligible prior to the expansion.

Finally, a frequently invoked argument for relaxing the asset test (and to some degree the income test) is that doing so may reduce the costs of administration by eliminating documentation and screening requirements. Appendix Figure A.3 shows event-study estimates for total administrative costs per enrolled case under each policy lever.[29] Neither lever produces a statistically significant change in administrative costs, and point estimates represent only 3% and 1% of the baseline means for the asset and income levers, respectively. There is therefore little evidence that, at least in the short run, relaxing either the asset or income test generates meaningful administrative savings.

29. We observe only aggregate administrative costs at the state level and cannot decompose them across subgroups (e.g., elderly/disabled versus non-elderly/disabled). We therefore examine total administrative costs divided by all recipient cases.

6 Enrollment Channels and Robustness Checks

Having established that raising the income threshold generates a larger and statistically significant enrollment response, we now examine the channels through which this expansion increases enrollment. We decompose the enrollment effects by the type of enrollee (newly eligible versus previously eligible) and the stage of the application process at which enrollment occurs (initial application versus recertification). We then conduct a battery of robustness checks on our main access and targeting estimates.

6.1 Decomposing Enrollment Effects of Raising Income Threshold

Recall from Section 5.1 that raising the income threshold increased enrollment by 1.8 percentage points (significant at the 5% level), averaged over three post-treatment years. Yet several features of this enrollment response seem puzzling. For example, the new enrollees were no less disadvantaged (and were on some dimensions more disadvantaged) than baseline (Section 5.2), and the majority of enrollment gains were concentrated among high-benefit cases whose eligibility did not change (Section 5.3). These patterns point to a role for inframarginal responses: enrollment increases among households who were already eligible prior to the expansion. To test this hypothesis, we decompose the enrollment effect of raising the income threshold into responses among marginal cases (newly eligible from the expansion) and inframarginal cases (previously eligible under federal rules).[30] While the federal thresholds are technically 130% FPL for gross income and 100% FPL for net income, we define inframarginal as having gross income below 115% FPL and net income below 85% FPL (and marginal as exceeding at least one), providing a buffer that allows us to estimate pre-trends for the share of marginal cases over a period when essentially no "marginals" were enrolled.

Panel (a) of Table 4 decomposes the share of enrolled cases in income-expansion states into marginal and inframarginal components, with Appendix Figure A.4 showing the event studies. Raising the income threshold produced a significant enrollment increase of 0.8 percentage points among marginal cases (significant at the 1% level). But we also find a

30. An additional reason for focusing on the income expansion is that this decomposition is not feasible for relaxing the asset test, as states that eliminated the asset test often stopped collecting asset information, making it impossible to classify enrollees as marginal or inframarginal on the asset dimension.

sizable inframarginal response of 1.0 percentage point that, while noisily estimated, accounts for more than half the magnitude of the overall enrollment effect. This finding is consistent with prior work documenting inframarginal (or "woodwork") effects of BBCE expansions (Anders and Rafkin 2024), and it implies that the welfare gains from eligibility expansions extend beyond the newly eligible population to include previously eligible households who are induced to participate.. The presence of a large inframarginal response also helps to explain the targeting patterns from Section 5.2. Because inframarginal cases tend to be more disadvantaged than marginal ones, their enrollment can pull average enrollee characteristics toward greater disadvantage even as newly eligible cases are less disadvantaged.[31] One possible explanation for the inframarginal response is that eligibility expansions reduced administrative burdens and made it easier for previously eligible households to maintain enrollment, a channel we discuss further below.

Having established that inframarginal responses are a major component of the enrollment effect, we next ask how these responses operate by examining which stage of the application process they occur at (initial application or recertification). Panel (b) of Table 4 reports the effects of raising the income threshold on enrollment jointly defined by marginal/inframarginal status and application stage. Among inframarginal enrollees, the entire effect (averaged over three years) occurs at recertification. Among marginal enrollees, the increase is roughly evenly split between initial application and recertification. Even when restricting our post-treatment period to one year, which suppresses recertification effects that could mechanically reflect holdovers from earlier initial applications, we continue to find that recertification accounts for the majority of the inframarginal effect and nearly half of the marginal effect (Appendix Table A.3).

The concentration of inframarginal effects at recertification has implications for understanding the mechanisms behind these "woodwork" responses. Prior work has emphasized reductions in information frictions or stigma as drivers of inframarginal enrollment (e.g.,), but these forces are less likely to operate at recertification since recipients at this stage have already overcome informational and stigma barriers to enter the program. Instead, our re-

31. For example, among the states that raised the income threshold in the three years after treatment, inframarginal cases have fewer years of education (11.36 vs. 11.65 years), higher rates of homelessness (7.0% vs. 0.4%), and lower incomes (36% vs. 135% of FPL) compared to marginal cases.

sults point toward reducing hassle/transaction costs (the primary administrative burdens at recertification) as another key driver of inframarginal effects. Although raising the income threshold does not eliminate documentation requirements, it may still reduce ordeals through several channels. First, caseworkers may scrutinize documentation less closely when a household's income is well below the threshold, whereas applications near the threshold are more likely to trigger additional income verification. Second, households with incomes just below the federal threshold may be uncertain whether they will ultimately qualify and may conclude that the application costs are not worth incurring; raising the threshold reduces this uncertainty for a meaningful share of the eligible population.

The fact that a substantial share of the marginal enrollment effect also occurs at recertification highlights a complementary channel. Because incomes among low-income households frequently fluctuate, individuals who initially enrolled under the higher threshold may see their incomes rise above the old federal limit but remain below the new threshold. Under the prior regime, these households would have exited the program at recertification, but they can remain enrolled under the expansion. This points to a previously underemphasized mechanism by which raising the income threshold increases enrollment. It not only brings new households onto the program, but it also allows existing recipients to maintain coverage through periods of income volatility that would have previously disqualified them.

6.2 Robustness Checks

We now assess the sensitivity of our main results to alternative specification and sample choices. These include alternative sets of covariates and control groups, alternative definitions of income and the unit of analysis, and adjustments to the treatment of simplified reporting rules and SIPP panel structure. Appendix Tables A.4 and A.5 report robustness for access and targeting outcomes (respectively) using equation (1), and Appendix Figures A.6–A.9 shows the event-study analogs for selected checks. For targeting, we focus on one primary outcome among eligibles (number of food hardships) and one among enrollees (years of education). Our estimates remain remarkably stable in both magnitude and statistical significance across nearly all of the checks we conduct.

We start by discussing checks that we can do for both the SIPP-based eligibility analyses and the QC-based enrollment analyses. We first show robustness to the presence and choice of covariates. When we exclude all state- and time-varying covariates (except for the net income test indicator, which we retain to isolate the effect of relaxing the asset test), the estimates remain largely unchanged. They also remain stable when we exclude only demographic and economic covariates (e.g., population shares and unemployment rate) or only SNAP-related policy covariates. This suggests that our estimates are not confounded by concurrent changes in state economic conditions or other SNAP policies. We also show robustness to an alternative control group restricted only to never-treated states (excluding later-treated states). We retain the broader control group of never- and later-treated states in our main specification as the more inclusive and less ad hoc choice, but the similarity of results across control groups is reassuring.

Several additional checks address features of the SIPP-based eligibility simulations. First, we show that the eligibility results are similar when we define the unit of analysis at the survey family level rather than the household level, in case some households contain multiple SNAP assistance units. Second, we show robustness to alternative income definitions that exclude lump-sum and miscellaneous sources that may not be captured by the SNAP resource definition. Third, we verify that results hold when we do not allow for continuous eligibility under simplified reporting rules, which is less realistic but avoids assumptions about reporting periods. Fourth, we address concerns about the SIPP's short-panel structure by dropping months at the beginning and end of each panel that may be incomplete by design, and by upweighting observations at the beginning of each panel that may be affected by left-censoring of continuous eligibility spells. Results are unchanged in both cases.

Finally, to assess whether differences in state and temporal coverage between the SIPP and QC results drive any of our findings, we conduct three checks on the QC-based enrollment analyses. First, we omit the five small states excluded from the SIPP analyses (Maine, North Dakota, South Dakota, Vermont, and Wyoming) and find very similar results. This is not surprising given that only two of these states are treated in our sample and small states receive little weight in our population-weighted regressions. Second, we omit the year-months that fall between SIPP panels to align temporal coverage. Third, we align both states and time simultaneously. In all cases, the estimates remain highly stable.

7 Policy Simulations

A key advantage of our eligibility calculator is that it can simulate SNAP eligibility under any combination of income and asset rules, not just the policies states actually adopted. In this section, we leverage this flexibility to conduct various policy simulations. First, we evaluate each lever independently against a common baseline to ask whether our main findings generalize beyond the specific bundles adopted in practice. Second, we verify that these results hold when both levers are evaluated over the same set of states and time periods, eliminating differences in economic conditions at the time of adoption. Because these exercises rely on simulated eligibility, we focus on outcomes among only *eligible* households (share eligible, characteristics of eligibles, and predicted benefit amounts) that we can recompute under alternative policy regimes. Finally, motivated by our finding that asset information is a stronger correlate of need than income among the eligible population, we ask whether incorporating assets into the benefit formula could improve the targeting of benefit dollars.

7.1 Counterfactual Eligibility Simulations

Analysis Setup. Our main exercise asks what effect each lever would have on its own, rather than in the "bundled" form that states adopted. For states in the asset-only group, we simulate eligibility under a regime that eliminates the asset test (rather than the mix of elimination and raising that occurred in practice, where three of the 18 states in this group merely raised the threshold). For states in the asset-plus-income group, we simulate eligibility under a regime that raises the gross income threshold to 200% FPL and eliminates the net income test, *without* simultaneously relaxing the asset test. This allows us to compare the effects of each lever holding the other at its federal baseline. Note that we no longer need to take the difference in coefficients across models to identify the effect of raising the income threshold. Instead, we estimate equations (1) and (2) separately for each group, and the coefficients from each model directly capture the effect of the corresponding lever. The two groups of states and their treatment dates remain as in the observed analysis, as only the simulated policy regime changes.

Results. We start by discussing effects on the share of eligible households. Panel (a) of Figure 6 shows results from the independent-levers exercise. Eliminating the asset test would increase the share of eligible households by 2.8 percentage points (a 26% increase), slightly above the 2.5 point estimate from the observed variation. This difference can be explained by the fact that we now set all states in the asset-only treatment group to uniformly eliminate the asset test (whereas all but three did so using the observed variation). On the other hand, raising the income threshold independently would increase eligibility by 2.4 percentage points (a 23% increase), a smaller estimate than the 4.9 point estimate from the observed variation and in line with our framework's predictions in Section 4.1. Nevertheless, the effects of both levers remain statistically significant at the 5% level.

We next examine effects on targeting, with Columns 1–4 of Table 5 reporting the main pre-post coefficients and Appendix Figure A.10 showing the event-study analogs. Eliminating the asset test would continue to bring in less disadvantaged eligibles than baseline, with estimates for the food hardship measures that are very similar to the observed variation and statistically significant at the 10% level or better. For raising the income threshold, however, the newly eligible households would not significantly different from the baseline population on any targeting measure (except income-to-poverty ratio, which changes mechanically). This is consistent with our framework's prediction that the counterfactual income expansion, which captures only low-asset households, should bring in weakly more disadvantaged households than the observed expansion, which also includes some high-asset households. As a result, the gap in targeting between policy levers widens when we compare them independently.

Turning to benefit amounts, Panel (b) of Figure 6 shows that the patterns from the observed variation persist: eliminating the asset test would bring in eligibles with benefits similar to baseline, while raising the income threshold would bring in eligibles with significantly lower average benefits. The decline in average benefits under the income expansion is somewhat larger than in the observed variation, likely because we set the threshold uniformly at 200% FPL whereas the thresholds under the observed variation ranged between 160% to 200% FPL across states.

Robustness to Harmonizing Timing. In the observed variation, asset-only states and asset-plus-income states adopted BBCE at different times. Specifically, asset-only states largely adopted during a single wave in the mid-to-late 2000s, while asset-plus-income states largely adopted in two waves, one in the early 2000s and another in the late 2000s (Figure 1, Panel a). To assess whether our findings hold when both levers are evaluated over the same time period (with the same economic and political conditions), we pool all 40 BBCE states and estimate equations (1) and (2) twice: once simulating that every state eliminated only the asset test at its actual BBCE adoption date, and once simulating that every state raised only the income threshold to 200% FPL at that same date. This yields estimates of each independent lever applied to the same set of states at the same times.

Panels (c)–(d) of Figure 6 and Columns 5–8 of Table 5) report the results. On access, the income lever's effect on eligibility would grow substantially larger under harmonized timing, reflecting the greater weight placed on the Great Recession period when income losses pushed more households below the threshold. On targeting, eliminating the asset test would continue to bring in less disadvantaged eligibles than raising the income threshold, though the gap narrows somewhat as the income expansion now brings in eligibles who are modestly less disadvantaged than baseline. This is consistent with the Great Recession drawing in households experiencing temporary income losses rather than chronic deprivation. On benefit amounts, the patterns from the independent-levers exercise continue to hold. In summary, our main findings from the observed variation (particularly with regard to targeting and benefit amounts) generalize to counterfactual settings that evaluate each lever independently and over common time periods, suggesting that the patterns we document reflect more general properties of how each eligibility dimension shapes the population that gains access to the program.

7.2 Benefit Simulations

Our counterfactual simulations confirm that relaxing the asset test brings in less disadvantaged households than raising the income threshold, suggesting that assets are a stronger correlate of need than income among the eligible population. A natural follow-up question is whether this screening advantage could be leveraged not just in terms of determining eligibility but also in the benefit formula. Under the current SNAP formula, benefits depend solely on net income, and a household's asset holdings have no bearing on its benefit amount conditional on eligibility. If assets are indeed more strongly associated with hardship among eligible households, then incorporating them into the benefit formula could improve how well benefit dollars are directed toward the most disadvantaged.[32]

We explore this question using SNAP-eligible households from the SIPP in fiscal year 2022, regressing food hardship measures on benefit amounts (expressed as a share of the maximum for a given household size) under the current formula and four alternatives that incorporate asset information. Under the current formula, benefits are based on net income (gross income minus deductions) with a benefit reduction rate of 30%. Our alternative formulas modify the income concept used in this calculation while maintaining the same benefit reduction rate. In two "blended" formulas, we convert 5% or 10% of household assets into an annualized income flow and add it to net income, yielding an adjusted income measure that reflects both resources. This approach is conceptually similar to the means-testing rules used in basic social assistance programs in the United Kingdom and Australia.[33] In two "asset-only" formulas, benefits depend solely on a deemed income equal to 5% or 10% of assets divided by 12. To ensure comparability, we adjust the maximum benefit amount under each formula so that total benefit expenditures are held constant across each formula.

Table 6 reports the results. Under the current income-based formula, the association between benefit amounts and food hardship is weak: moving from zero to the maximum benefit is associated with an increase of 0.09 in the number of food hardships (out of 3), which is not statistically significant. Across the three individual food hardship measures, only "food did not last" is significantly associated with benefit amounts. Incorporating assets into the benefit calculation strengthens this relationship. Under the blended formula that adds 10% of assets to net income, moving from zero to the maximum benefit is associated with an increase of 0.25 food hardships (roughly 34% of the mean among eligible households) and the

32. Prior studies have found that both income and assets are strongly associated with food hardship (e.g., Gundersen and Gruber 2001; Leete and Bania 2010). Our analysis builds on this literature by examining the relative magnitudes of these associations.

33. See the UK's Universal Credit savings rules (https://www.gov.uk/guidance/universal-credit-money-savings-and-investments) and Australia's income and assets tests (https://www.servicesaustralia.gov.au/sites/default/files/2024-12/co029-2501.pdf).

association is statistically significant for all three individual food hardship measures. The relationship becomes even stronger under the asset-only formulas: under the 10% deemed-income specification, moving from zero to the maximum benefit is associated with an increase of 0.85 food hardships, and each individual food hardship measure shows an increase of 20 percentage points or more.

These results suggest that the role of asset information in means-tested programs need not be limited to determining eligibility. Even holding the set of eligible households and total program costs fixed, incorporating assets into the benefit formula could substantially improve the targeting of benefit dollars toward the most disadvantaged households. Combined with our main finding that asset tests are more effective than income tests at screening less disadvantaged households from eligibility, our findings point to a broader role for asset information in the design of safety net programs than is reflected in the current policy trend toward eliminating asset tests (and, with them, the collection of asset information).

8 Conclusion

This paper provides the first within-program comparison of the effects of relaxing asset tests versus raising income thresholds in a means-tested transfer program. Exploiting the staggered adoption of Broad-Based Categorical Eligibility (BBCE) across U.S. states, we use a stacked difference-in-differences design to separately identify the effects of each policy lever on SNAP access, targeting, and costs among non-elderly, non-disabled households.

We find that raising the income threshold increases both eligibility and enrollment by more than relaxing the asset test, a finding that holds under both the observed policy variation and counterfactual simulations that evaluate each lever independently against a common baseline. On targeting, relaxing the asset test consistently draws in less disadvantaged eligibles than raising the income threshold, and among enrollees this pattern is reinforced by inframarginal responses that pull the composition of new enrollees under the income expansion toward greater disadvantage. On costs, average benefit amounts per enrollee are similar across the two expansions despite the mechanical prediction that income expansions should reduce average benefits. This reflects the low take-up rate among newly eligible low-benefit households and the high benefits received by inframarginal enrollees. Finally, our benefit formula simulations show that incorporating asset information into the benefit calculation generates a substantially stronger association between benefits and food hardship, suggesting that asset information has value in program design beyond its current role as an eligibility screen.

Our findings on access, targeting, and costs collectively point toward the income lever delivering larger welfare gains than the asset lever. However, this comparison omits two potentially important channels through which the policies may affect welfare: inframarginal responses to relaxing the asset test (which we cannot measure because asset-eliminating states stop collecting asset information) and fiscal externalities from behavioral responses, such as labor supply and savings, to relaxing the income or asset tests. For these unmeasured channels to change the welfare ranking between the two policy levers, they would have to be both large and disproportionately favorable to the asset lever. The existing literature on behavioral responses to SNAP income and asset limits broadly suggests modest and often inconclusive effects, providing little reason to expect fiscal externalities that differ enough across the two policy levers to reverse our conclusion. We therefore see the welfare ranking suggested by our measured channels as reasonably credible, while recognizing that directly estimating the unmeasured channels remains a useful direction for future work.

These findings have practical implications for the design of means-tested programs. For policymakers choosing between relaxing asset tests and raising income thresholds, the evidence we present favors the latter on the channels we can measure. At the same time, asset information can sharpen the targeting of benefit dollars conditional on eligibility. More broadly, our results suggest that the effects of screening along a particular resource dimension depend on where in program design that information is deployed, and that the current debate over asset tests may benefit from shifting focus toward how best to use asset information within the structure of transfer programs.

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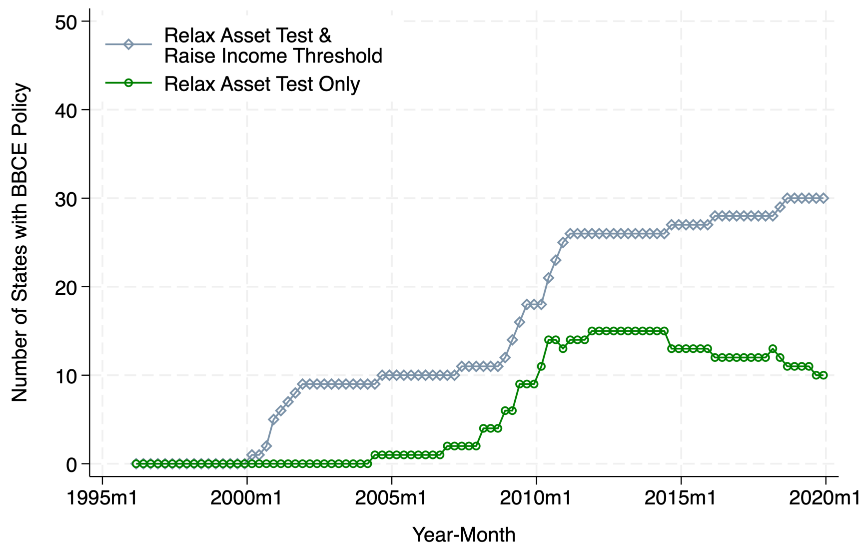
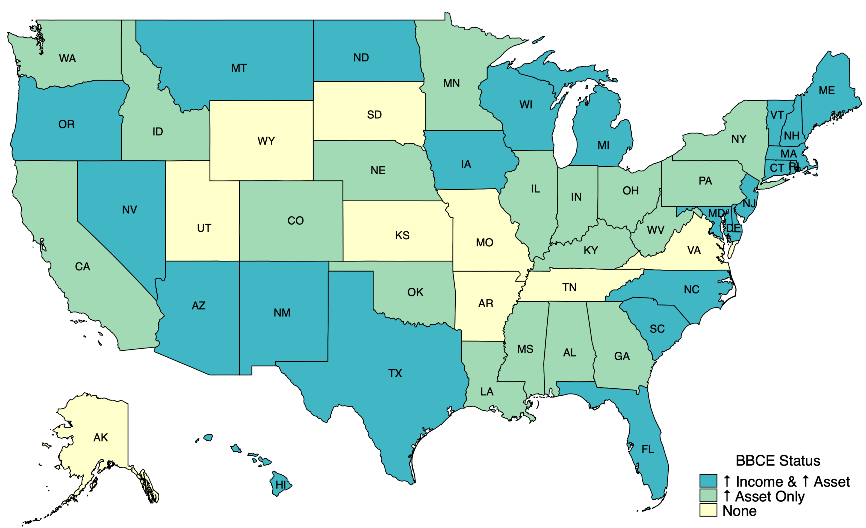
Figures and Tables

Figure 1: BBCE Variation Over Time and Across States (1996-2019)

(a) Adoption Over Time



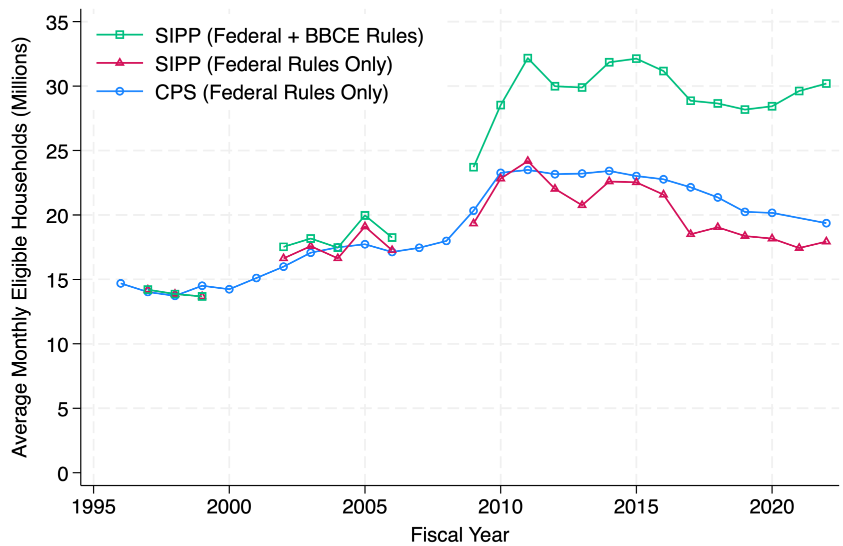
(b) Adoption Across States



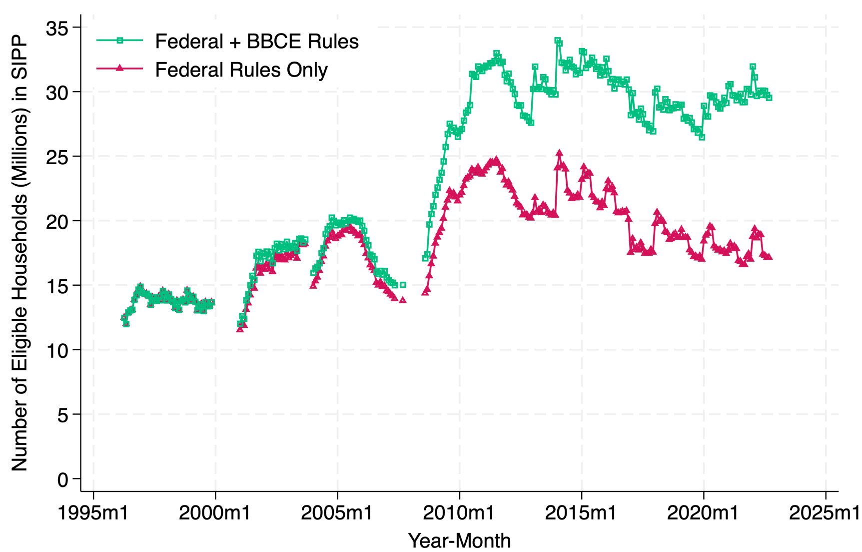
Notes: Panel (a) shows the number of states relaxing the asset test (green circles) or raising the income threshold (gray diamonds) as part of their BBCE policy in a given year-month between January 1996 and December 2019. Panel (b) shows a map of the U.S. that classifies states into three groups based on the changes that comprised each state's initial BBCE expansion: both raising the income threshold and relaxing the asset test (blue), relaxing the asset test only (green), and no BBCE expansion.

Figure 2: Comparison of Eligibility Counts Across Data Sources

(a) CPS vs. SIPP: Average Monthly Counts (by Fiscal Year)

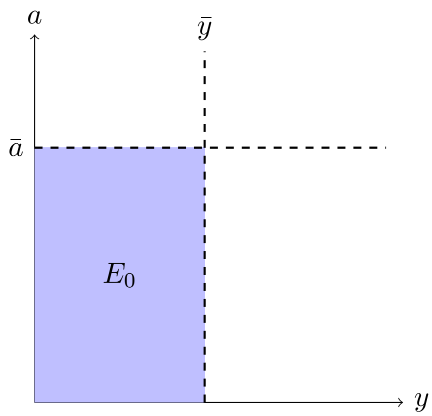


(b) SIPP: Monthly Counts

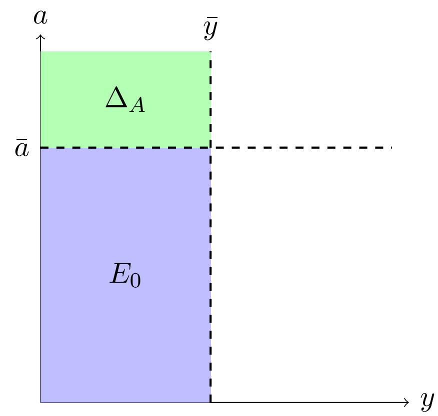


Notes: Panel (a) compares average monthly counts of all SNAP-eligible households (in millions) by fiscal year across three series: SIPP under federal plus BBCE rules (green circles), SIPP under federal rules only (red triangles), and CPS-based USDA estimates under federal rules (blue squares). Panel (b) plots monthly SIPP-based counts under federal rules only (red triangles) and federal plus BBCE rules (green circles).

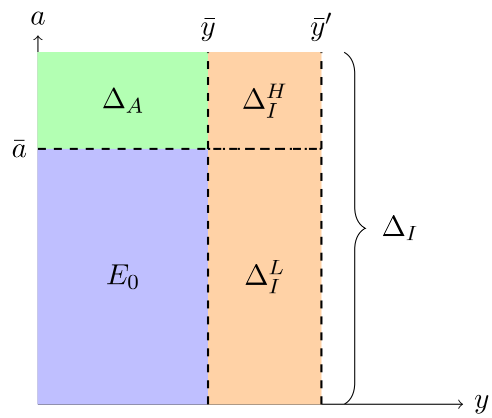
Figure 3: Eligibility Regions Under Alternative Policy Regimes



(a) Federal Baseline



(b) Relax Asset Test

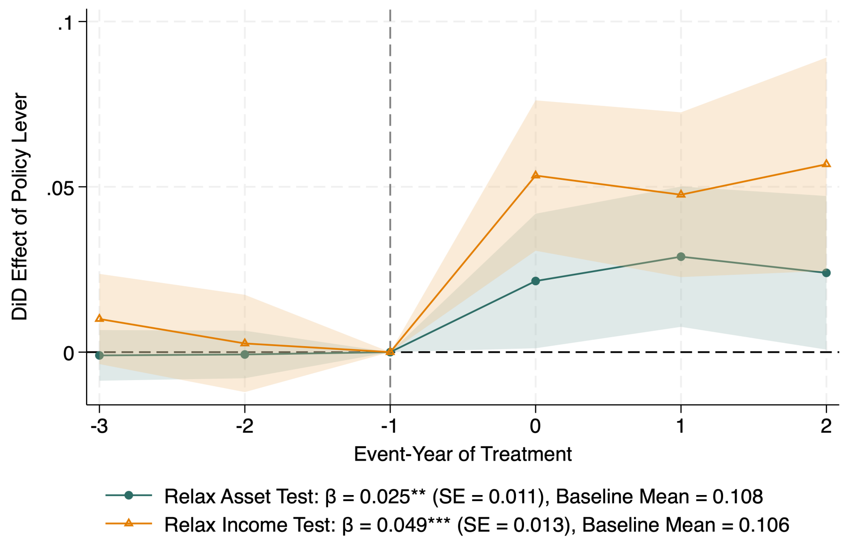


(c) Relax Asset Test + Raise Income Threshold

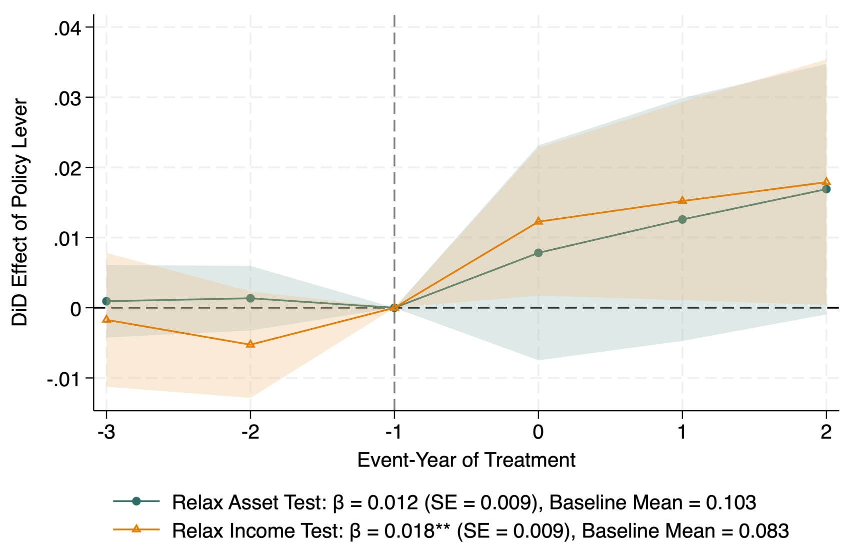
Notes: Each panel depicts the set of households eligible for SNAP as a function of household income (y, horizontal axis) and assets (a, vertical axis). Panel (a) shows the baseline federal eligibility region E_0, defined by an income threshold $\bar{y}$ and an asset limit $\bar{a}$. Panel (b) shows the expanded region after relaxing the asset test, with the newly eligible set Δ_A (green) comprising households with income below $\bar{y}$ but assets above $\bar{a}$. Panel (c) shows the further expansion after additionally raising the income threshold from $\bar{y}$ to $\bar{y}'$, with the incrementally eligible set Δ_I (orange) comprising households with income between $\bar{y}$ and $\bar{y}'$. The set Δ_A is uniformly high-asset, while Δ_I spans the full asset distribution: Δ_I^L contains low-asset households and Δ_I^H contains high-asset households.

Figure 4: Effects on Share of Eligible Households and Enrolled Cases

(a) Effects on Share Eligible



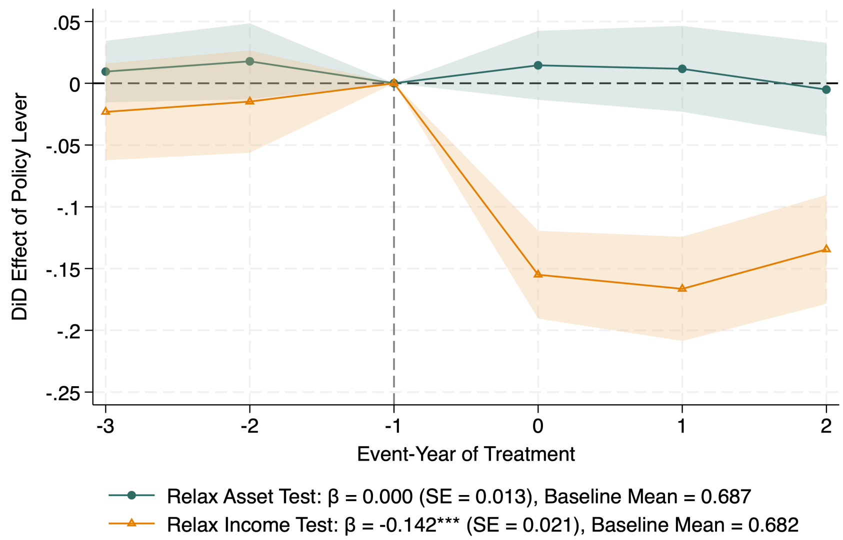
(b) Effects on Share Enrolled



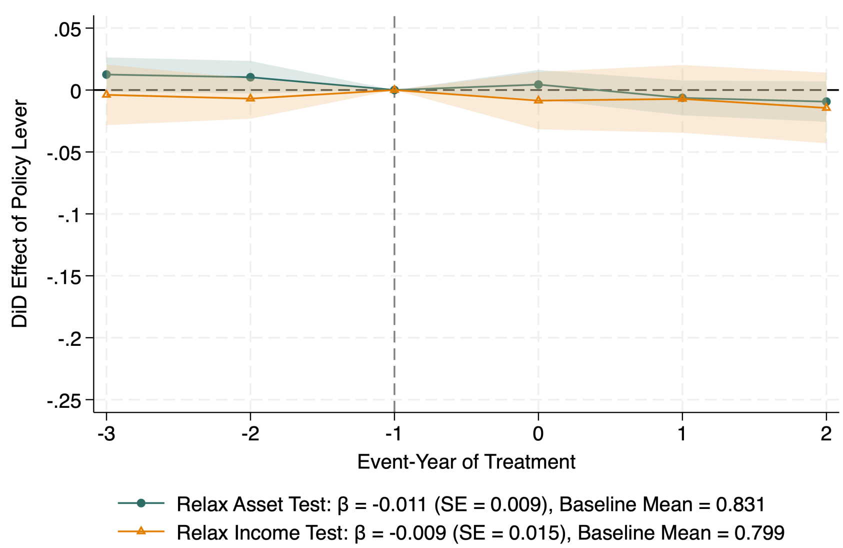
Notes: This figure shows event-study estimates from equation (2) of the effects of relaxing the asset test (green circles) and raising the income threshold on top of relaxing the asset test (orange triangles) on the share of non-elderly, non-disabled households that are eligible for SNAP (Panel a, SIPP) and enrolled in SNAP (Panel b, QC data). The x-axis denotes event-years relative to the initial BBCE expansion, with $k = -1$ as the omitted base period. The control group includes never-treated and later-treated states. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level. The β coefficients, standard errors, and baseline means in the legend correspond to the static estimates from equation (1).

Figure 5: Effects on Average Benefit Amounts (as a Share of Maximum)

(a) Among Eligible Households

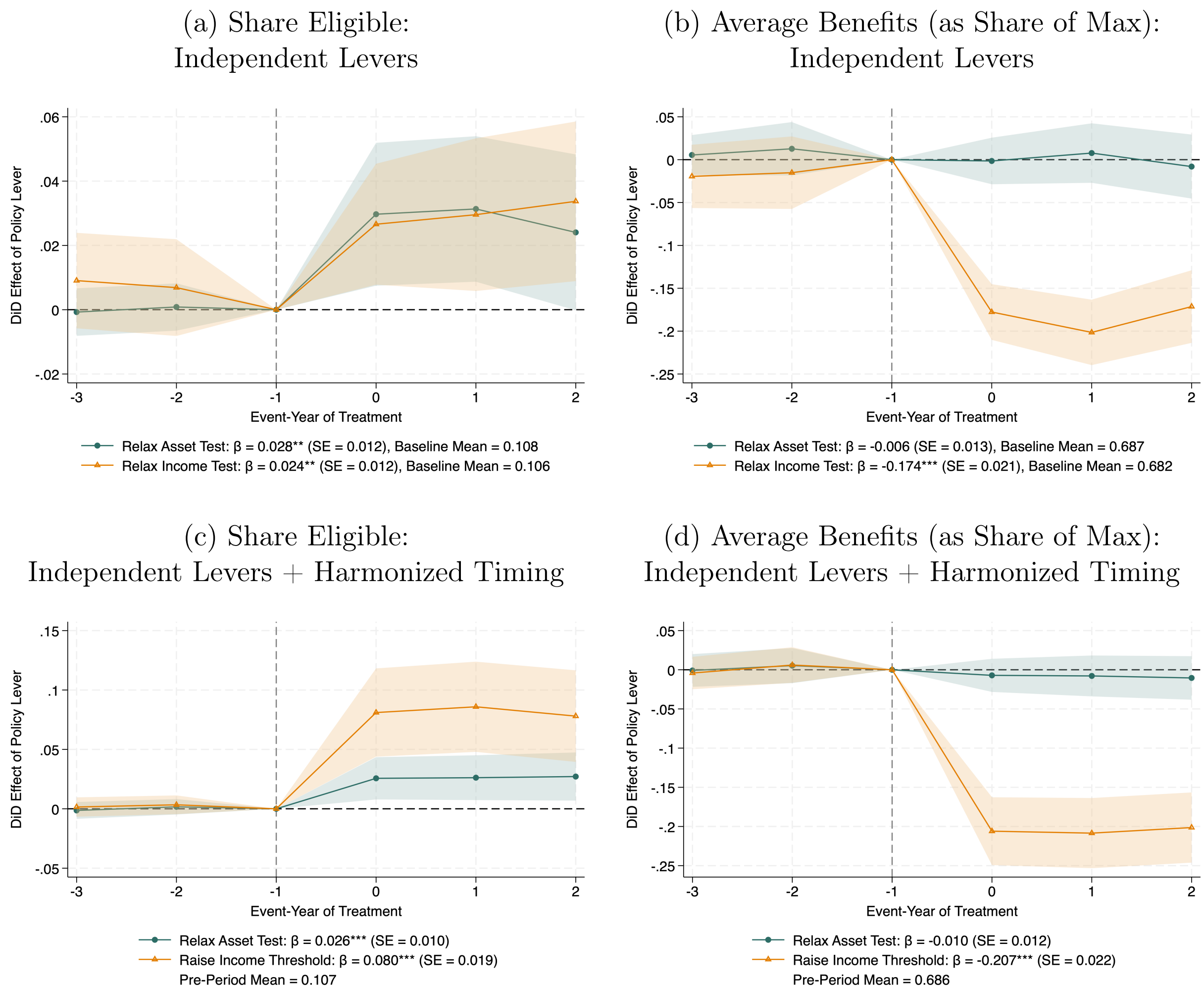


(b) Among Enrolled Cases



Notes: This figure shows event-study estimates from equation (2) of the effects of relaxing the asset test (green circles) and raising the income threshold (orange triangles) on average SNAP benefit amounts as a share of the maximum allotment for a given household size. Panel (a) shows effects among eligible households (SIPP) and Panel (b) shows effects among enrolled cases (QC data). The sample is restricted to non-elderly, non-disabled households. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level. The legend reports static estimates from equation (1).

Figure 6: Counterfactual Effects on Share and Average Benefits of Eligible Households



Notes: This figure shows event-study estimates from equation (2) using counterfactual eligibility simulations from the SIPP. In Panels (a)–(b), each lever is applied independently: the asset group simulates eliminating the asset test without changing the income threshold, and the income group simulates raising the threshold to 200% FPL and eliminating the net income test without relaxing the asset test. States and treatment dates are as in the observed analysis. In Panels (c)–(d), both levers are additionally evaluated over a common set of states and time periods by pooling all 40 BBCE states at their actual adoption dates. Panels (a) and (c) show effects on the share of eligible households, and Panels (b) and (d) show effects on average simulated benefit amounts as a share of the maximum. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Table 1: Average Changes in Targeting Characteristics Among Eligible Households and Enrolled Cases

Outcomes	Among Eligible Households				Among Enrolled Cases			
	Relax Asset Test	Raise Income Threshold	Diff. in Coeffs.	Baseline Means†	Relax Asset Test	Raise Income Threshold	Diff. in Coeffs.	Baseline Means†
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Food Hardships	-0.150**	-0.066	-0.085	0.585, 0.585				
	(0.071)	(0.064)						
Food Did Not Last	-0.075*	-0.056**	-0.019	0.263, 0.263				
	(0.039)	(0.028)						
Meals Not Balanced	-0.039*	-0.011	-0.028	0.206, 0.222				
	(0.020)	(0.025)						
Skipped Meals	-0.037*	0.001	-0.039	0.115, 0.100				
	(0.021)	(0.020)						
Avg. Income-to-Poverty Ratio	-0.000	0.265***	-0.265	0.602, 0.612	0.027**	0.025	0.002	0.422, 0.433
	(0.022)	(0.039)			(0.011)	(0.022)		
Years of Education (Head)	0.039	0.080	-0.042	11.811, 11.703	-0.018	-0.342***	0.323	11.233, 11.455
	(0.114)	(0.143)			(0.030)	(0.114)		
Homeless					-0.008	0.008	-0.017	0.046, 0.026
					(0.007)	(0.007)		
Observations (State-Months)	55,296				55,296			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

†Baseline means listed for asset-only and asset-plus-income treated states, respectively

Notes: This table shows pre-post estimates from equation (1) of changes in average characteristics among eligible households (Columns 1–4, SIPP) and enrolled cases (Columns 5–8, QC data). Column (1): effect of relaxing the asset test. Column (2): incremental effect of raising the income threshold. Column (3): difference in coefficients. Column (4): baseline means in treated states over three pre-treatment years (asset-only states, asset-plus-income states). Columns (5)–(8) follow the same structure for enrolled cases. The sample is restricted to non-elderly, non-disabled households. Observations are at the state-month level, weighted by the number of eligible households (Columns 1–4) or enrolled cases (Columns 5–8). Standard errors, clustered at the state level, are in parentheses.

Table 2: Average Changes in Other Characteristics Among Eligible Households and Enrolled Cases

Outcomes	Among Eligible Households				Among Enrolled Cases			
	Relax Asset Test (1)	Raise Income Threshold (2)	Diff. in Coeffs. (3)	Baseline Means† (4)	Relax Asset Test (5)	Raise Income Threshold (6)	Diff. in Coeffs. (7)	Baseline Means† (8)
Has Earnings	-0.001	0.105***	-0.106	0.625, 0.650	0.016	-0.010	0.026	0.410, 0.451
	(0.033)	(0.031)			(0.011)	(0.023)		
Head Aged <40	0.021	-0.012	0.032	0.561, 0.564	-0.001	-0.011	0.009	0.707, 0.727
	(0.020)	(0.028)			(0.010)	(0.012)		
Single-Person Unit	-0.023	0.008	-0.031	0.219, 0.220	0.014	0.007	0.007	0.308, 0.290
	(0.038)	(0.025)			(0.017)	(0.020)		
Has Children	0.004	-0.072**	0.077	0.679, 0.682	-0.007	-0.016	0.010	0.705, 0.724
	(0.040)	(0.032)			(0.017)	(0.019)		
Married	0.012	0.071**	-0.059	0.365, 0.361	0.002	-0.006	0.008	0.110, 0.122
	(0.045)	(0.028)			(0.010)	(0.015)		
White	-0.036	0.021	-0.057	0.451, 0.409	0.063	0.031	0.032	0.333, 0.291
	(0.026)	(0.028)			(0.070)	(0.061)		
Observations (State-Months)	55,296				55,296			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

†Baseline means listed for asset-only and asset-plus-income treated states, respectively

Notes: This table follows the same methodology as Table 1 but reports additional demographic and economic characteristics. See Table 1 notes for details.

Table 3: Heterogeneity in Effects on Share of Eligible Households and Enrolled Cases by Benefit Amount

	Share of Eligible Households				Share of Enrolled Cases			
Outcomes	Relax Asset Test (1)	Raise Income Threshold (2)	Diff. in Coeffs. (3)	Baseline Means† (4)	Relax Asset Test (5)	Raise Income Threshold (6)	Diff. in Coeffs. (7)	Baseline Means† (8)
Main Estimates	0.025**	0.049***	-0.023	0.108, 0.106	0.012	0.018**	-0.007	0.103, 0.083
	(0.011)	(0.013)			(0.009)	(0.009)		
Share w/ Benefits 0-25% of Max	0.000	0.043***	-0.043	0.013, 0.013	0.001	0.005***	-0.004	0.003, 0.004
	(0.004)	(0.006)			(0.001)	(0.001)		
Share w/ Benefits 25-50% of Max	0.005	0.007***	-0.002	0.020, 0.020	0.001	0.002	-0.001	0.009, 0.008
	(0.004)	(0.002)			(0.001)	(0.001)		
Share w/ Benefits 50-75% of Max	0.007**	0.000	0.007	0.019, 0.020	0.003**	0.002	0.002	0.017, 0.014
	(0.003)	(0.003)			(0.001)	(0.002)		
Share w/ Benefits 75-100% of Max	0.012	-0.002	0.014	0.056, 0.053	0.006	0.010	-0.004	0.075, 0.057
	(0.008)	(0.008)			(0.008)	(0.007)		
Observations (State-Months)	55,296				55,296			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

†Baseline means listed for asset-only and asset-plus-income treated states, respectively

Notes: This table shows pre-post estimates from equation (1) of the effects of relaxing the asset test and raising the income threshold on the share of eligible households (Columns 1–4, SIPP) and enrolled cases (Columns 5–8, QC data), decomposed by benefit amount as a share of the maximum allotment. The sample is restricted to non-elderly, non-disabled households. Observations are at the state-month level, weighted by the number of non-elderly, non-disabled households. Standard errors, clustered at the state level, are in parentheses.

Table 4: Heterogeneity in Enrollment Effects of Raising Income Threshold

Outcomes	Raise Income Threshold (1)	Baseline Mean (2)
A. By Respondent Type		
Share Enrolled: Inframarginal	0.010	0.079
	(0.008)	
Share Enrolled: Marginal	0.008***	0.004
	(0.002)	
B. By Application Stage & Respondent Type		
Share Enrolled: Initial Application & Inframarginal	0.000	0.034
	(0.005)	
Share Enrolled: Initial Application & Marginal	0.004***	0.002
	(0.001)	
Share Enrolled: Recertification & Inframarginal	0.010	0.045
	(0.008)	
Share Enrolled: Recertification & Marginal	0.004***	0.002
	(0.001)	
Observations (State-Months)	55,296	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows pre-post estimates from equation (1) of the effects of raising the income threshold (on top of relaxing the asset test) on enrollment, decomposed by respondent type and application stage. Inframarginal cases have gross income below 115% FPL and net income below 85% FPL; marginal cases exceed at least one threshold. Panel A decomposes by respondent type; Panel B further decomposes by application stage. The sample is restricted to non-elderly, non-disabled cases (QC data). Observations are at the state-month level, weighted by the number of enrolled cases. Standard errors, clustered at the state level, are in parentheses.

Table 5: Counterfactual Changes in Targeting Characteristics Among Eligible Households

Outcomes	Independent Levers				Independent Levers + Harmonized Timing			
	Relax Asset Test (1)	Raise Income Threshold (2)	Diff. in Coeffs. (3)	Baseline Mean (4)	Relax Asset Test (5)	Raise Income Threshold (6)	Diff. in Coeffs. (7)	Baseline Mean (8)
Number of Food Hardships	-0.151**	-0.018	-0.133	0.585	-0.170**	-0.122	-0.048	0.585
	(0.071)	(0.063)			(0.066)	(0.084)		
Food Did Not Last	-0.075*	-0.035	-0.040	0.263	-0.084**	-0.072*	-0.012	0.263
	(0.039)	(0.028)			(0.036)	(0.040)		
Meals Not Balanced	-0.039**	0.006	-0.044	0.212	-0.041**	-0.020	-0.021	0.212
	(0.020)	(0.025)			(0.017)	(0.022)		
Skipped Meals	-0.037*	0.011	-0.048	0.110	-0.045**	-0.030	-0.015	0.110
	(0.021)	(0.020)			(0.022)	(0.027)		
Avg. Income-to-Poverty Ratio	0.009	0.322***	-0.313	0.606	0.015	0.339***	-0.324	0.606
	(0.022)	(0.037)			(0.018)	(0.039)		
Years of Education (Head)	0.050	-0.128	0.178	11.772	0.155	0.073	0.082	11.772
	(0.116)	(0.162)			(0.144)	(0.171)		
Observations (State-Months)	55,296				55,296			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows pre-post estimates from equation (1) using counterfactual eligibility simulations from the SIPP. Columns (1)–(4) show the results of the independent-levers exercise, where Column (1) simulates eliminating the asset test and Column (2) simulates raising the income threshold to 200% FPL (without relaxing the asset test), each against the federal baseline. Column (4) reports baseline means for asset-only and asset-plus-income treated states. Columns (5)–(8) show the results of additionally harmonizing treatment timing, pooling all 40 BBCE states and simulating each lever at each state's actual adoption date. Observations are at the state-month level, weighted by the number of counterfactually eligible non-elderly, non-disabled households. Standard errors, clustered at the state level, are in parentheses.

Table 6: Association between Hardship Incidence and Simulated Benefit Amount Under Alternative SNAP Benefit Formulas, Fiscal Year 2022

	Number of Food Hardships (1)	Food Did Not Last (2)	Meals Not Balanced (3)	Skipped Meals (4)
Original Net Income	0.092	0.056**	0.013	0.023
	(0.070)	(0.028)	(0.028)	(0.023)
Original Net Income + (0.05 × Assets)	0.198***	0.102***	0.044	0.052**
	(0.068)	(0.027)	(0.027)	(0.023)
Original Net Income + (0.1 × Assets)	0.254***	0.125***	0.062**	0.068**
	(0.069)	(0.027)	(0.028)	(0.022)
0.05 × Assets	0.848***	0.383***	0.252***	0.214***
	(0.109)	(0.044)	(0.038)	(0.045)
0.1 × Assets	0.853***	0.373***	0.269***	0.211***
	(0.090)	(0.039)	(0.029)	(0.038)
Mean of Dependent Var.	0.751	0.290	0.270	0.191
Mean of Indep. Var. (Share of Max Benefit)		0.670		
Observations (Households)		13,727		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows regression estimates of indicators for hardship incidence on simulated SNAP benefit amounts (as a share of the maximum benefit for a given household size) under alternative SNAP benefit formulas. The sample includes households without an elderly or disabled member who are eligible for SNAP in Fiscal Year (FY) 2022. Regressions control for demographic characteristics such as the household head's age, race, and years of education, as well as household size, number of children, and state-fixed effects. In each alternative net income formula, the maximum benefit amount is scaled up or down to keep total hypothetical benefit expenditures (among all eligibles in FY 2022) the same. Specifically, the adjustment factor is 1.029 for the first alternative formula, 1.045 for the second, 0.673 for the third, and 0.689 for the fourth.

Online Appendix for "Income vs. Asset Tests in SNAP: Impacts on Access, Targeting, and Costs"

Jeehoon Han and Derek Wu

June 4, 2026

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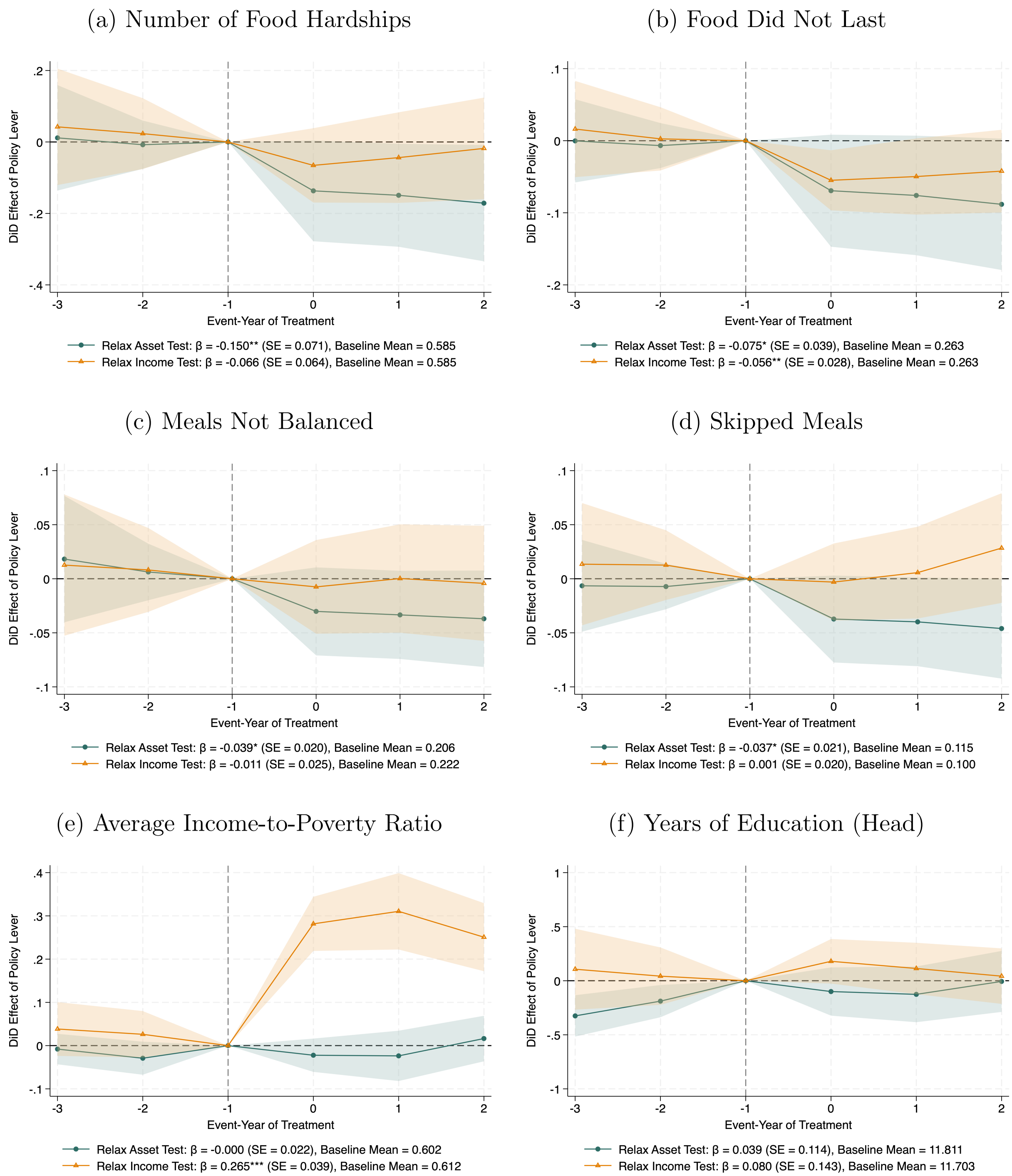
A Supplemental Figures and Tables

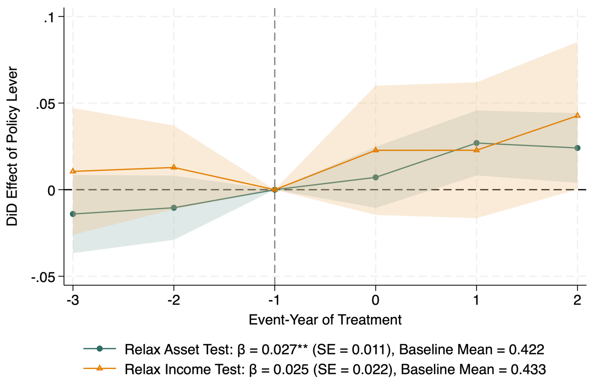
Figure A.1: Event-Study Effects on Targeting Characteristics of Eligible Households



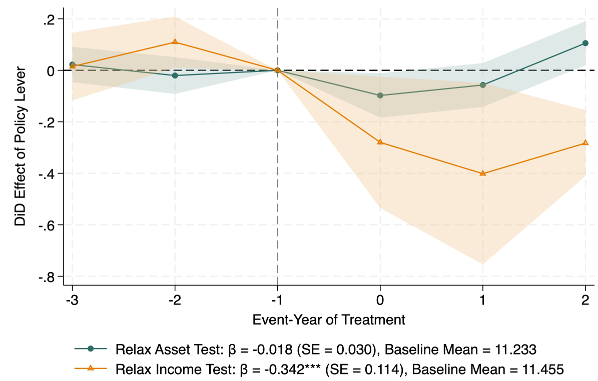
Notes: This figure shows event-study estimates from equation (2) of the effects of relaxing the asset test and raising the income threshold on targeting characteristics among eligible non-elderly, non-disabled households. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.2: Event-Study Effects on Targeting Characteristics of Enrolled Cases

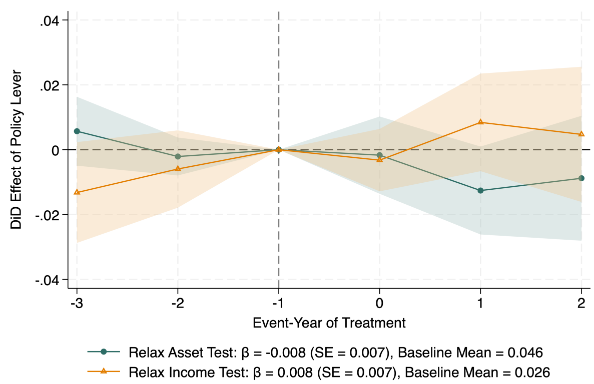
(a) Average Income-to-Poverty Ratio



(b) Years of Education (Head)

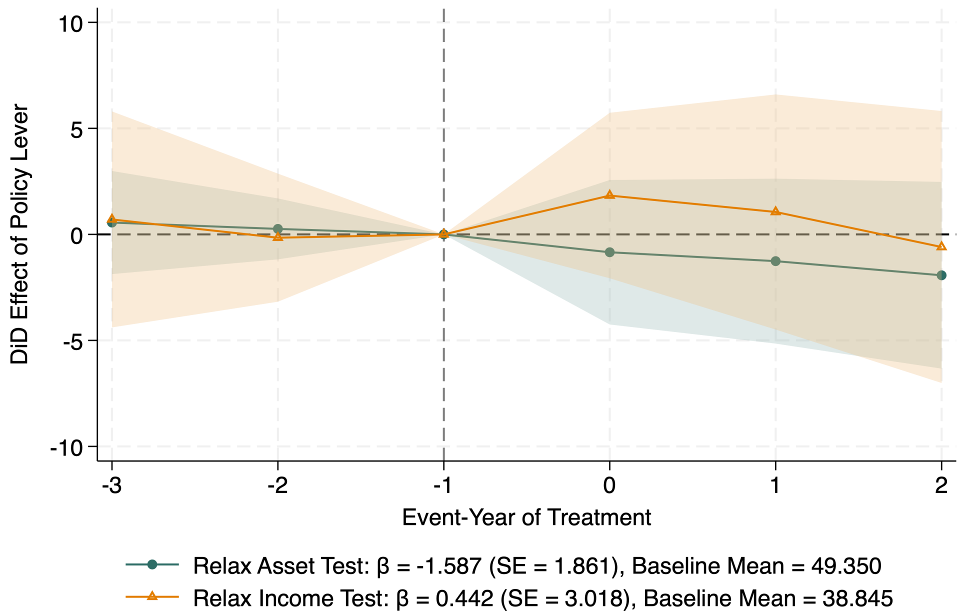


(c) Homeless



Notes: This figure shows event-study estimates from equation (2) of the effects of relaxing the asset test (green circles) and raising the income threshold (orange triangles) on targeting characteristics among enrolled non-elderly, non-disabled cases (QC data). Observations are weighted by the number of enrolled cases. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

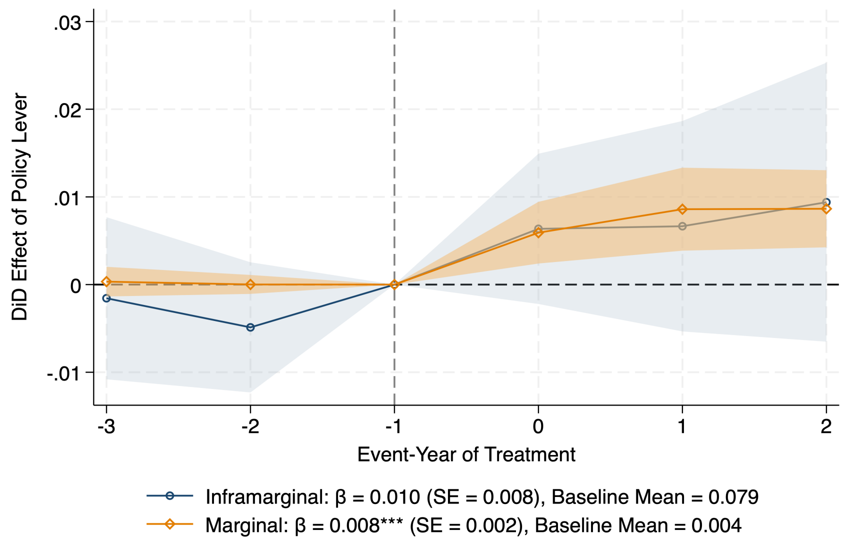
Figure A.3: Event-Study Effects on Administrative Costs Per Recipient



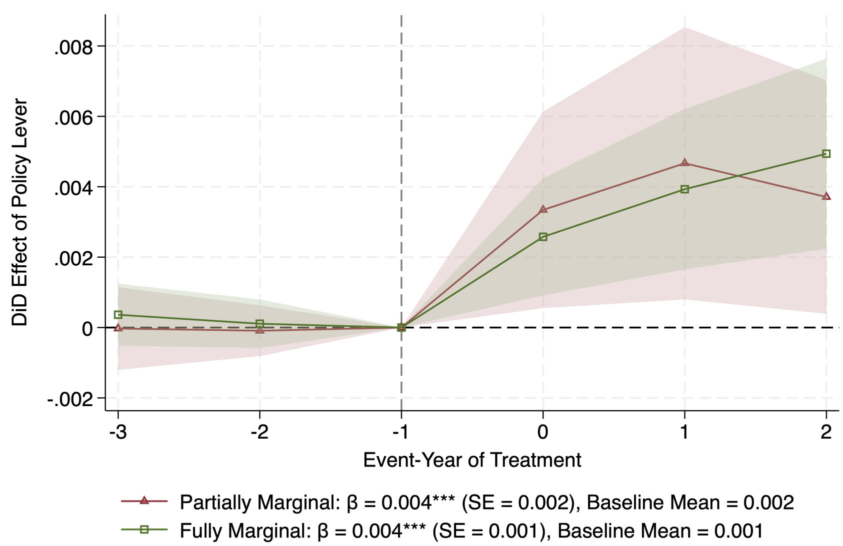
Notes: This figure shows event-study estimates from equation (2) of the effects of relaxing the asset test (green circles) and raising the income threshold (orange triangles) on total administrative costs per SNAP-enrolled case (in dollars). Administrative costs are observed at the state level and divided by all recipient cases, as we cannot decompose them across subgroups. Observations are weighted by total enrolled cases. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.4: Inframarginal vs. Marginal Enrollment Effects of Raising Income Threshold

(a) Effects on Inframarginal vs. Marginal Recipients



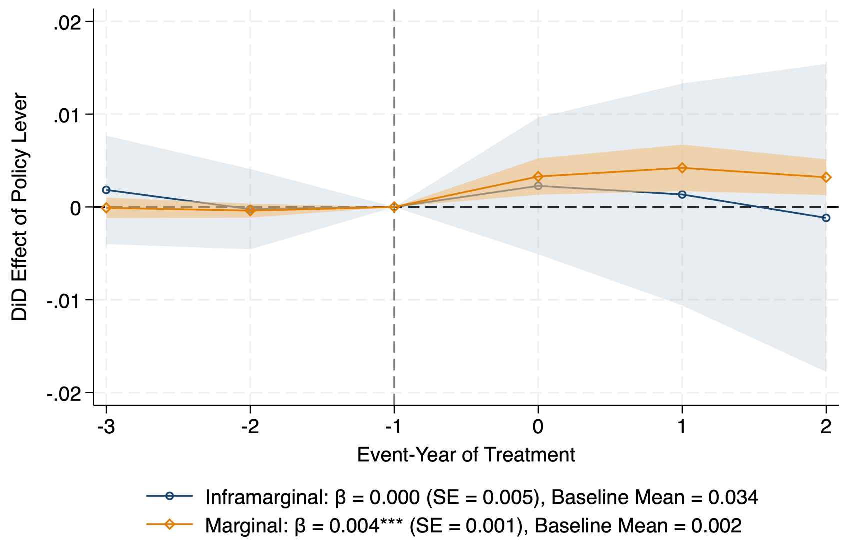
(b) Effects on Different Types of Marginal Recipients



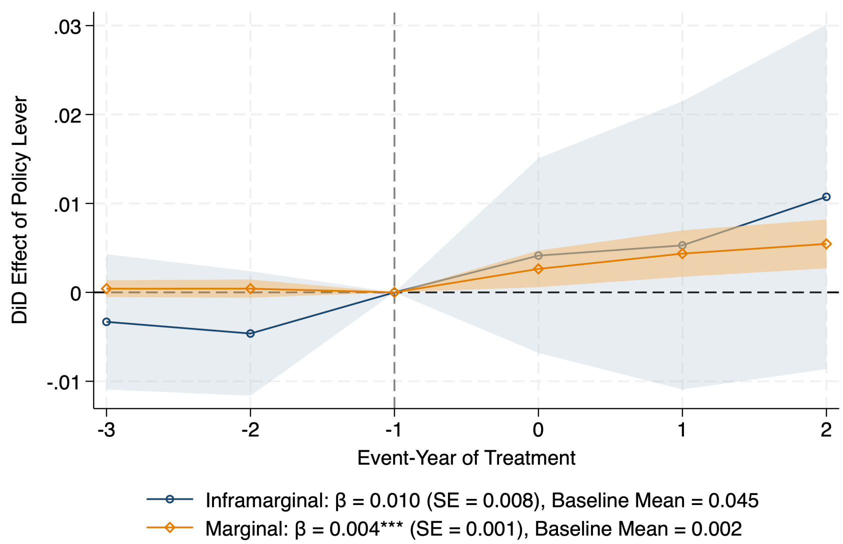
Notes: This figure shows event-study estimates from equation (2) of the effects of raising the income threshold (on top of relaxing the asset test) on the share of non-elderly, non-disabled households enrolled in SNAP (QC data). Panel (a) decomposes the effect into inframarginal cases (gross income below 115% FPL and net income below 85% FPL) and marginal cases (the complement). Panel (b) further decomposes marginal cases into partially marginal (exceeding one threshold) and fully marginal (exceeding both). Observations are weighted by the number of non-elderly, non-disabled households. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.5: Inframarginal vs. Marginal Enrollment Effects of Raising Income Threshold by Application Stage

(a) Initial Application



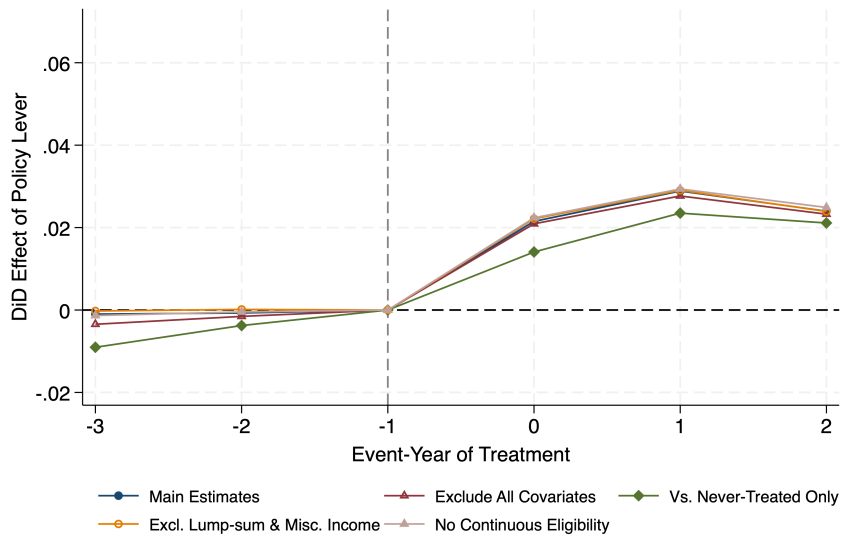
(b) Recertification



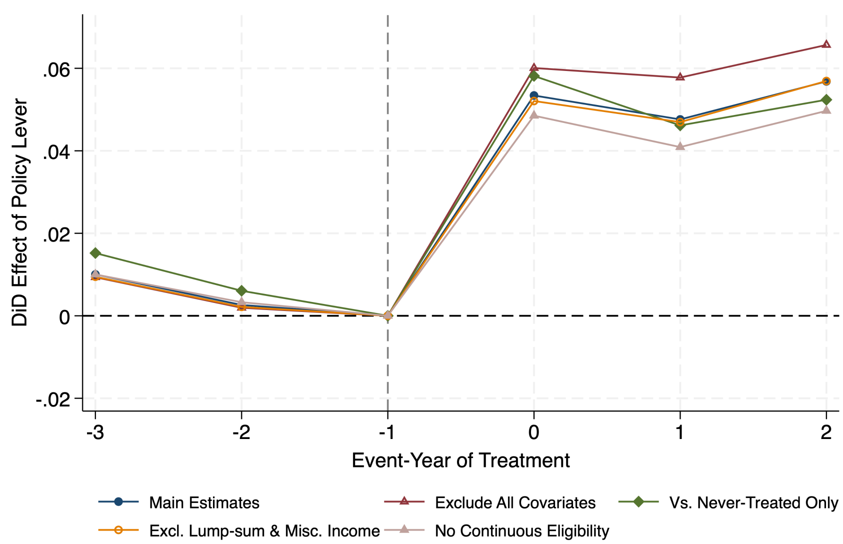
Notes: This figure shows event-study estimates from equation (2) of the effects of raising the income threshold (on top of relaxing the asset test) on enrollment among inframarginal (navy circles) and marginal (orange diamonds) non-elderly, non-disabled cases (QC data). Panel (a) restricts to initial application; Panel (b) restricts to recertification. See Figure A.4 notes for definitions of inframarginal and marginal cases. Observations are weighted by the number of non-elderly, non-disabled households. Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.6: Selected Robustness Checks for Effects on Share of Eligible Households

(a) Effects of Relaxing Asset Test



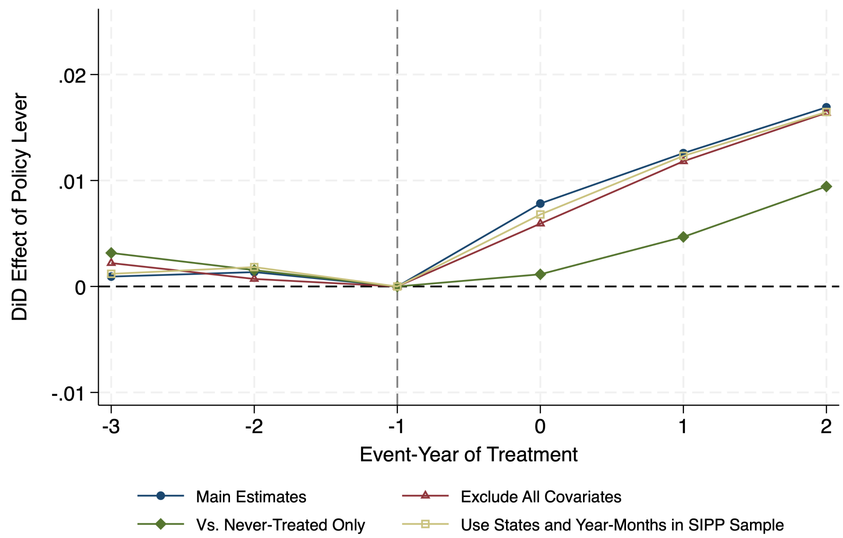
(b) Effects of Raising Income Threshold



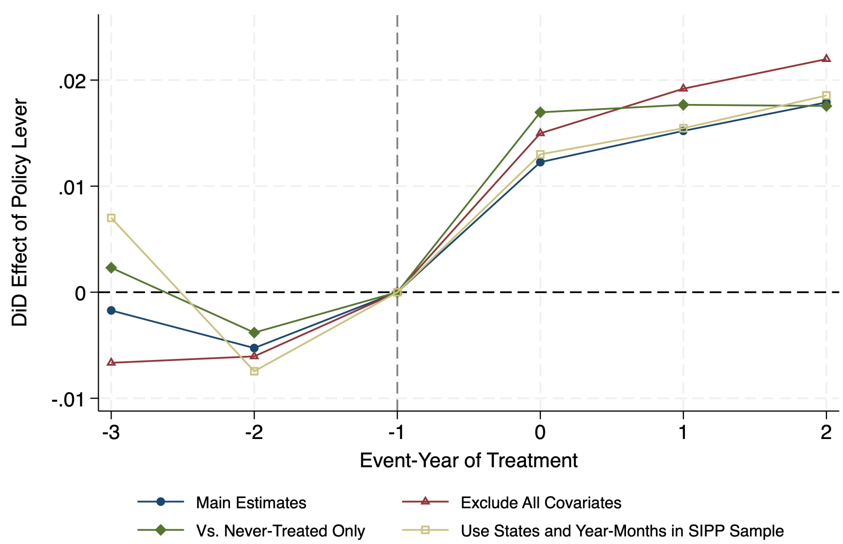
Notes: These figures show selected robustness checks for the event-study effects on the share of eligible non-elderly, non-disabled households (SIPP). Panel (a) shows the effects of relaxing the asset test, and Panel (b) shows the effects of raising the income threshold. The main estimates (solid circles) are compared with: excluding all covariates (triangles), never-treated control states only (diamonds), family-level units (squares), excluding lump-sum/miscellaneous income (hollow circles), and no continuous eligibility under simplified reporting (small triangles).

Figure A.7: Selected Robustness Checks for Effects on Share of Enrolled Cases

(a) Effects of Relaxing Asset Test



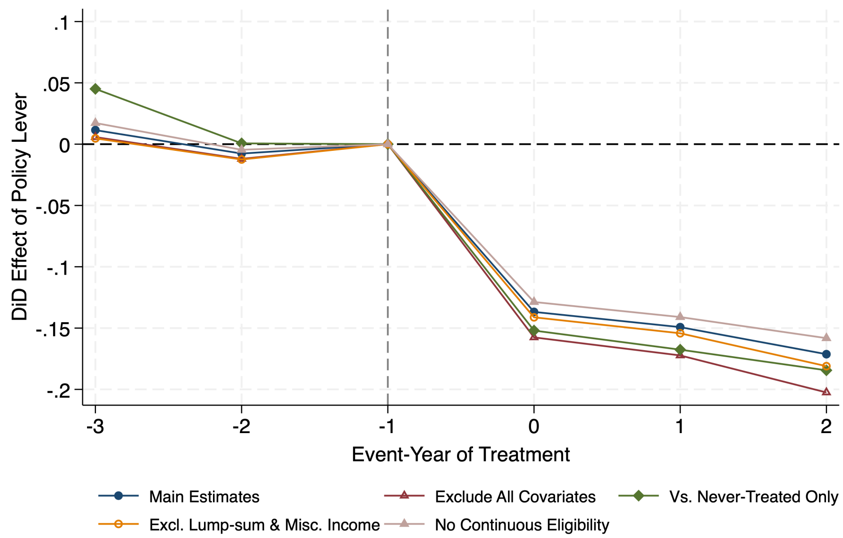
(b) Effects of Raising Income Threshold



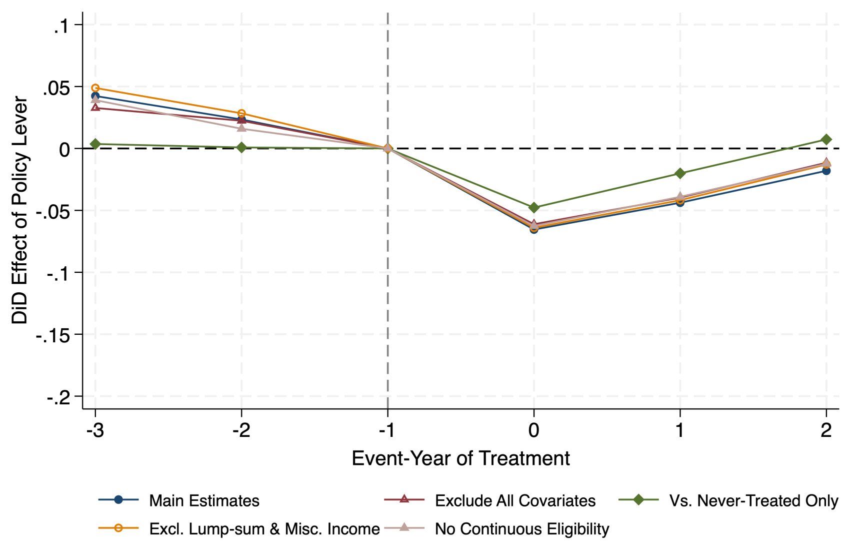
Notes: These figures show selected robustness checks for the event-study effects on the share of enrolled non-elderly, non-disabled cases (QC data). Panel (a) shows the effects of relaxing the asset test, and Panel (b) shows the effects of raising the income threshold. The main estimates (solid circles) are compared with: excluding all covariates (triangles), never-treated control states only (diamonds), and restricting to the states and year-months in the SIPP sample (squares).

Figure A.8: Selected Robustness Checks for Effects on Number of Food Hardships Among Eligibles

(a) Effects of Relaxing Asset Test



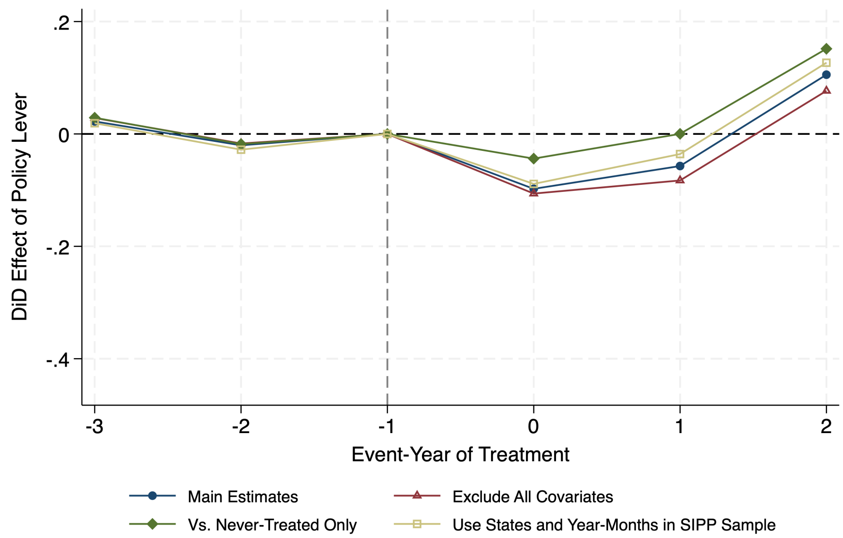
(b) Effects of Raising Income Threshold



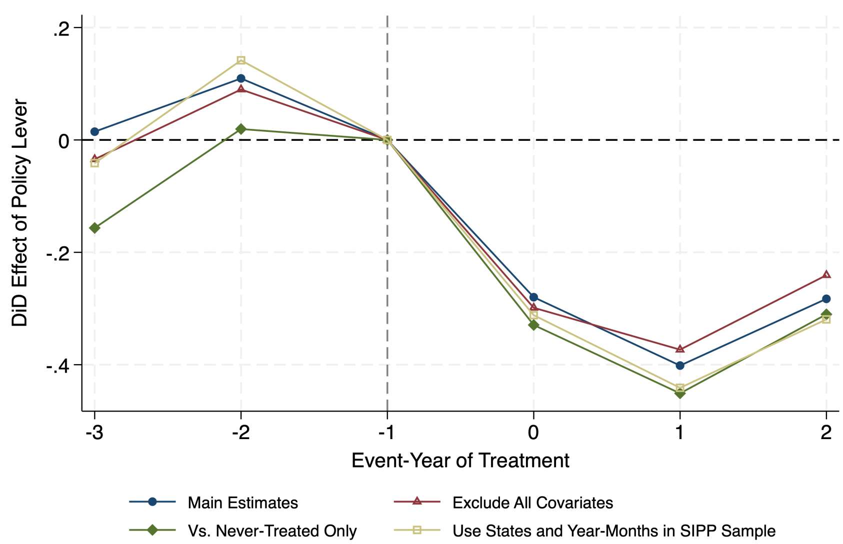
Notes: These figures show selected robustness checks for the event-study effects on the total number of food hardships among eligible non-elderly, non-disabled households (SIPP). Panel (a) shows the effects of relaxing the asset test, and Panel (b) shows the effects of raising the income threshold. The main estimates (solid circles) are compared with: excluding all covariates (triangles), never-treated control states only (diamonds), first-observed food hardship in each panel (squares), excluding lump-sum/miscellaneous income (hollow circles), and no continuous eligibility under simplified reporting (small triangles).

Figure A.9: Selected Robustness Checks for Effects on Years of Education Among Enrolled Cases

(a) Effects of Relaxing Asset Test



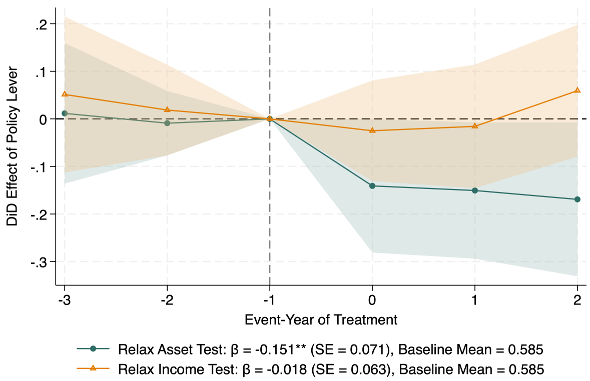
(b) Effects of Raising Income Threshold



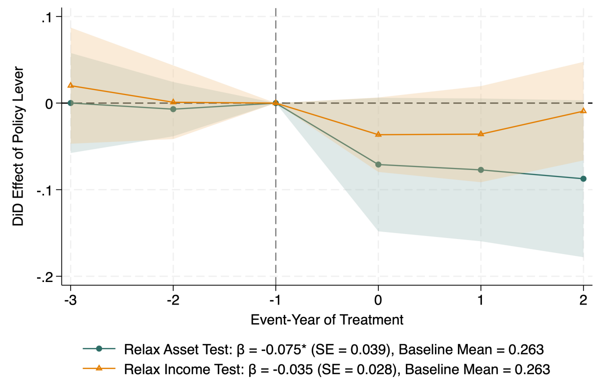
Notes: These figures show selected robustness checks for the event-study effects on average years of education of the case head among enrolled non-elderly, non-disabled cases (QC data). Panel (a) shows the effects of relaxing the asset test, and Panel (b) shows the effects of raising the income threshold. The main estimates (solid circles) are compared with: excluding all covariates (triangles), never-treated control states only (diamonds), and restricting to the states and year-months in the SIPP sample (squares).

Figure A.10: Event-Study Effects on Targeting Characteristics of Eligible Households (Counterfactual – Independent Levers)

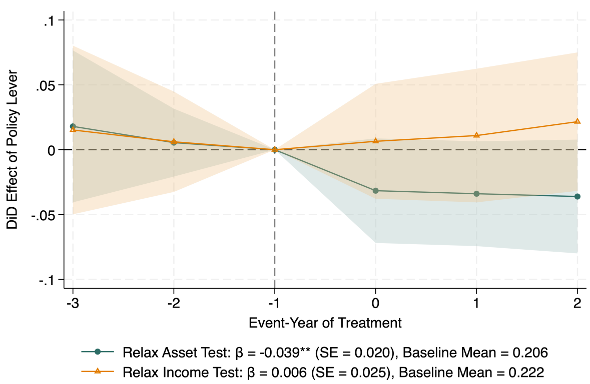
(a) Number of Food Hardships



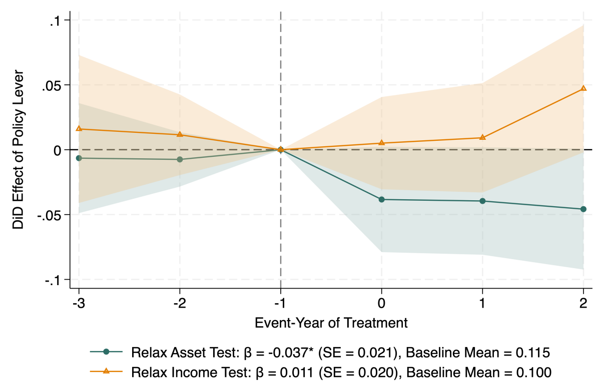
(b) Food Did Not Last



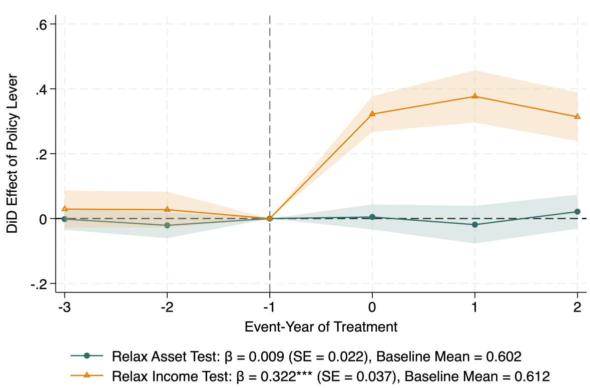
(c) Meals Not Balanced



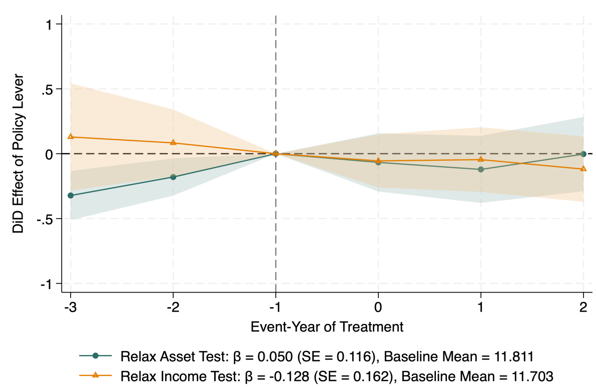
(d) Skipped Meals



(e) Average Income-to-Poverty Ratio



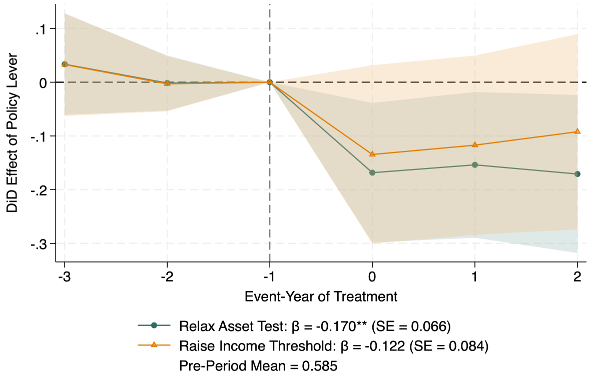
(f) Years of Education (Head)



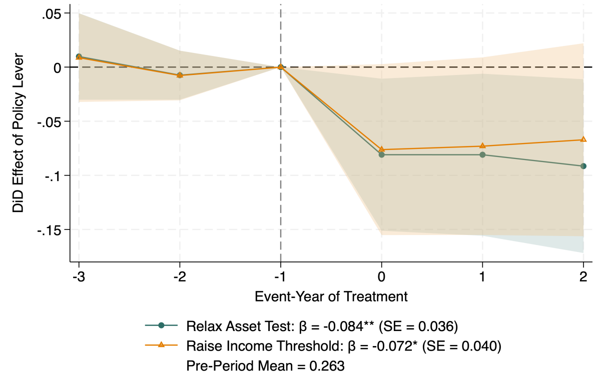
Notes: This figure shows event-study estimates from equation (2) using counterfactual eligibility simulations (independent-levers exercise) of the effects of eliminating the asset test versus raising the income threshold to 200% FPL on targeting characteristics among counterfactually eligible non-elderly, non-disabled households (SIPP). Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Figure A.11: Event-Study Effects on Targeting Characteristics of Eligible Households
(Counterfactual – Independent Levers + Harmonized Treatment Timing)

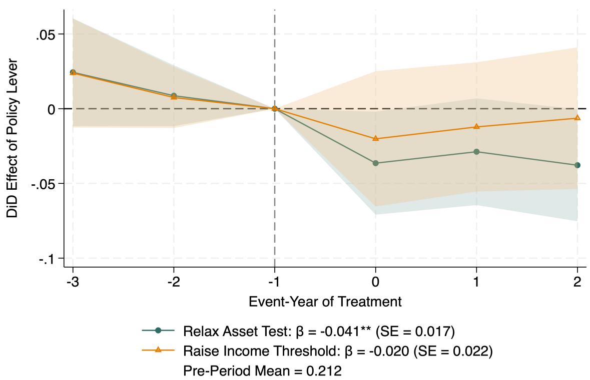
(a) Number of Food Hardships



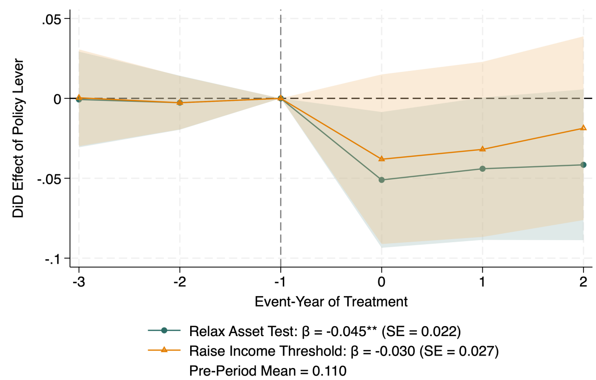
(b) Food Did Not Last



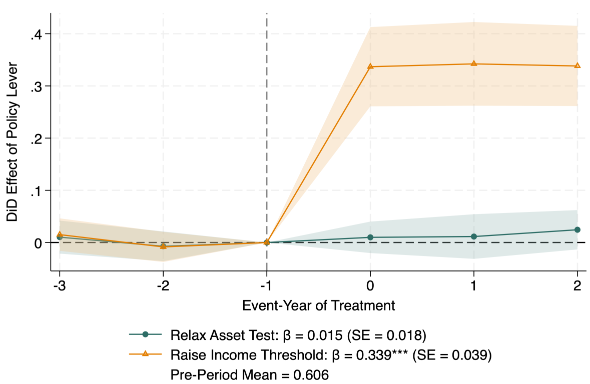
(c) Meals Not Balanced



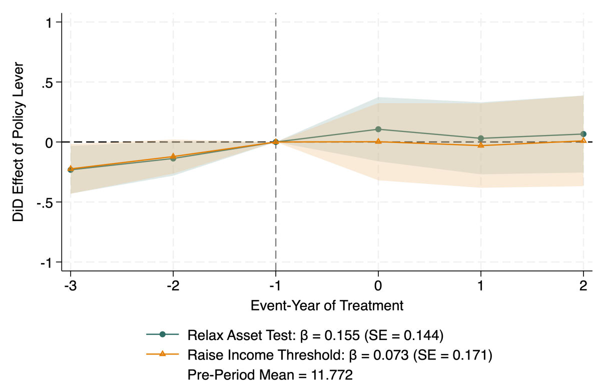
(d) Skipped Meals



(e) Average Income-to-Poverty Ratio



(f) Years of Education (Head)



Notes: This figure shows event-study estimates from equation (2) using counterfactual eligibility simulations (independent levers + harmonized treatment) of the effects of eliminating the asset test versus raising the income threshold on targeting among counterfactually eligible non-elderly, non-disabled households (SIPP). Shaded regions represent 95% confidence intervals. Standard errors are clustered at the state level.

Table A.1: List of New Resource Thresholds Under Initial BBCE Expansion by State (for Non-Elderly/Disabled), 1996-2019

State	**Date**	**Asset Limit**	**Gross Inc. (% FPL)**	**Net Inc. Test Elim.**	**State**	**Date**	**Asset Limit**	**Gross Inc. (% FPL)**	**Net Inc. Test Elim.**
Alabama	Feb 2010	Eliminated	–	Yes	Montana	Mar 2009	Eliminated	185%	–
Alaska	–	–	–	–	Nebraska	Oct 2011	$25,000	–	–
Arizona	Jun 2007	Eliminated	185%	Yes	Nevada	Apr 2009	Eliminated	200%	Yes
Arkansas	–	–	–	–	New Hampshire	May 2009	Eliminated	185%	Yes
California	Jul 2009	Eliminated	–	Yes	New Jersey	Apr 2010	Eliminated	185%	Yes
Colorado	Mar 2011	Eliminated	–	–	New Mexico	Apr 2010	Eliminated	165%	Yes
Connecticut	Jul 2009	Eliminated	185%	Yes	New York	Jan 2008	Eliminated	–	Yes
Delaware	Feb 2000	Eliminated	200%	Yes	North Carolina	Jul 2010	Eliminated	200%	Yes
District of Columbia	Apr 2010	Eliminated	200%	Yes	North Dakota	Oct 2000	Eliminated	200%	–
Florida	Jul 2010	Eliminated	200%	Yes	Ohio	Oct 2008	Eliminated	–	Yes
Georgia	Mar 2008	Eliminated	–	Yes	Oklahoma	Jun 2009	Eliminated	–	–
Hawaii	Oct 2010	Eliminated	200%	Yes	Oregon	Dec 2000	Eliminated	185%	Yes
Idaho	Jun 2009	Eliminated	–	–	Pennsylvania	Oct 2008	Eliminated	–	Yes
Illinois	Mar 2010	Eliminated	–	Yes	Rhode Island	Apr 2009	Eliminated	185%	Yes
Indiana	Jan 2018	$5,000	–	Yes	South Carolina	Apr 2001	Eliminated	200%	Yes
Iowa	Jan 2011	Eliminated	160%	Yes	South Dakota	–	–	–	–
Kansas	–	–	–	–	Tennessee	–	–	–	–
Kentucky	Jun 2010	Eliminated	–	Yes	Texas	Sep 2001	$5,000	165%	Yes
Louisiana	Jun 2010	Eliminated	–	–	Utah	–	–	–	–
Maine	Sep 2000	Eliminated	200%	Yes	Vermont	Jan 2009	Eliminated	185%	Yes
Maryland	Mar 2001	Eliminated	200%	Yes	Virginia	–	–	–	–
Massachusetts	Nov 2001	Eliminated	200%	Yes	Washington	May 2004	Eliminated	–	Yes
Michigan	Oct 2000	Eliminated	200%	Yes	West Virginia	Oct 2008	Eliminated	–	Yes
Minnesota	Dec 2006	$7,000	–	Yes	Wisconsin	Jul 2004	Eliminated	200%	Yes
Mississippi	Jun 2010	Eliminated	–	–	Wyoming	–	–	–	–
Missouri	–	–	–	–					

Notes: This table lists, for each state, the initial year/month of BBCE expansion and the changes to the asset limit, gross income limit, and net income test that were part of the initial expansion (updated through December 2019). We focus on policies relevant for non-elderly/disabled units. In a small minority of cases where a state had different BBCE policy changes for units with and without children, we take the more generous policy change (typically assigned to units with children).

Table A.2: Average Changes in Other Characteristics (Supplemental) Among Eligible Households and Enrolled Cases

	Among Eligible Households				Among Enrolled Cases			
Outcomes	Relax Asset Test	Raise Income Threshold	Diff. in Coeffs.	Baseline Means†	Relax Asset Test	Raise Income Threshold	Diff. in Coeffs.	Baseline Means†
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Home	-0.014	0.065**	-0.079	0.322, 0.369				
	(0.025)	(0.033)						
Has Savings Account	0.041	0.008	0.033	0.233, 0.226				
	(0.031)	(0.035)						
Has Retirement Account	-0.025	0.052**	-0.077	0.141, 0.118				
	(0.023)	(0.022)						
Head Aged <26	0.039	-0.005	0.044	0.110, 0.131	-0.004	-0.001	-0.003	0.321, 0.318
	(0.025)	(0.027)			(0.008)	(0.008)		
Head Aged 26-39	-0.018	-0.007	-0.012	0.451, 0.433	0.003	-0.009	0.012	0.387, 0.409
	(0.027)	(0.031)			(0.008)	(0.013)		
Head Aged 40-59	-0.021	0.012	-0.032	0.439, 0.436	0.001	0.011	-0.009	0.293, 0.273
	(0.020)	(0.028)			(0.010)	(0.012)		
Has Wage Earnings	-0.009	0.102***	-0.112	0.587, 0.608	-0.003	-0.015	0.012	0.375, 0.411
	(0.024)	(0.032)			(0.013)	(0.027)		
Has SE Earnings	0.013	0.002	0.010	0.162, 0.160	0.019***	0.006	0.012	0.039, 0.041
	(0.021)	(0.019)			(0.006)	(0.006)		
Has Child Support	-0.014	-0.019	0.005	0.120, 0.112	-0.013	-0.002	-0.012	0.121, 0.122
	(0.019)	(0.019)			(0.009)	(0.009)		
Has UI	-0.001	0.011	-0.013	0.049, 0.053	0.013***	0.008	0.005	0.035, 0.044
	(0.011)	(0.010)			(0.005)	(0.006)		
Has TANF/GA	0.041***	-0.008	0.049	0.086, 0.080	0.051***	0.012	0.038	0.285, 0.242
	(0.010)	(0.018)			(0.011)	(0.017)		
Observations (State-Months)	55,296				55,296			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

†Baseline means listed for asset-only and asset-plus-income treated states, respectively

Notes: This table follows the same methodology as Table 1 but reports supplemental characteristics including savings/retirement account ownership, detailed age groups, and income source receipt. See Table 1 notes for details.

Table A.3: Heterogeneity in Enrollment Effects of Raising Income Threshold
(Post-Treatment Period: 1 Year)

Outcomes	Raise Income Threshold (1)	Baseline Mean (2)
A. By Respondent Type		
Share Enrolled: Inframarginal	0.009	0.079
	(0.007)	
Share Enrolled: Marginal	0.007***	0.004
	(0.002)	
B. By Application Stage & Respondent Type		
Share Enrolled: Initial Application & Inframarginal	0.004	0.034
	(0.003)	
Share Enrolled: Initial Application & Marginal	0.004***	0.002
	(0.001)	
Share Enrolled: Recertification & Inframarginal	0.005	0.045
	(0.006)	
Share Enrolled: ecertification & Marginal	0.003***	0.002
	(0.001)	
Observations (State-Months)	55,296	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table follows the same methodology as Table 4 but restricts the post-treatment period to one year after the initial BBCE expansion, which suppresses recertification effects that could mechanically reflect holdovers from earlier initial applications. See Table 4 notes for more details.

Table A.4: Robustness Checks for Effects on Share of Eligible HHs and Enrolled Cases

Outcomes	Share of Eligible Households		Share of Enrolled Cases	
	Relax Asset Test (1)	Raise Income Threshold (2)	Relax Asset Test (3)	Raise Income Threshold (4)
Main Estimates	0.025**	0.049***	0.012	0.018**
	(0.011)	(0.013)	(0.009)	(0.009)
Exclude All Covariates	0.025**	0.057***	0.011	0.023**
	(0.011)	(0.012)	(0.009)	(0.011)
Exclude Demographic/Economic Covariates	0.028***	0.046***	0.013	0.023**
	(0.010)	(0.013)	(0.010)	(0.011)
Exclude Policy Covariates	0.023**	0.060***	0.010	0.019**
	(0.011)	(0.012)	(0.009)	(0.009)
Control States: Never-Treated Only	0.022*	0.049***	0.003	0.019***
	(0.013)	(0.013)	(0.008)	(0.007)
Family-Level	0.025**	0.051***		
	(0.011)	(0.012)		
Excl. Lump-Sum and Misc. Income	0.025**	0.048***		
	(0.011)	(0.013)		
No Continuous Eligibility	0.026**	0.042***		
	(0.011)	(0.013)		
Keep Incomplete Months	0.028**	0.048***		
	(0.012)	(0.013)		
Adjust SIPP Left-Censoring	0.025**	0.049***		
	(0.011)	(0.013)		
Use States in SIPP Sample			0.011	0.018**
			(0.010)	(0.009)
Use Year-Months in SIPP Sample			0.011	0.017**
			(0.009)	(0.008)
Use States and Year-Months in SIPP Sample			0.011	0.017**
			(0.009)	(0.008)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports pre-post estimates from equation (1) of the effects of relaxing the asset test and raising the income threshold on the share of eligible households (Columns 1–2, SIPP) and enrolled cases (Columns 3–4, QC data) under alternative specifications. Standard errors, clustered at the state level, are in parentheses.

Table A.5: Robustness Checks for Targeting Among Eligible HHs and Enrolled Cases

Outcomes	Number of Food Hardships Among Eligible Households		Years of Education Among Enrolled Cases	
	Relax Asset Test (1)	Raise Income Threshold (2)	Relax Asset Test (3)	Raise Income Threshold (4)
Main Estimates	-0.150**	-0.066	-0.018	-0.342***
	(0.071)	(0.064)	(0.030)	(0.114)
Exclude All Covariates	-0.174***	-0.053	-0.037*	-0.319***
	(0.059)	(0.053)	(0.022)	(0.117)
Exclude Demographic/Economic Covariates	-0.152**	-0.044	-0.018	-0.335***
	(0.069)	(0.060)	(0.029)	(0.122)
Exclude Policy Covariates	-0.180***	-0.099*	-0.039	-0.318***
	(0.061)	(0.058)	(0.025)	(0.106)
Control States: Never-Treated Only	-0.168**	-0.034	0.016	-0.308**
	(0.081)	(0.061)	(0.047)	(0.121)
Excl. Lump-Sum and Misc. Income	-0.154**	-0.067		
	(0.069)	(0.061)		
No Continuous Eligibility	-0.143**	-0.058		
	(0.070)	(0.065)		
Keep Incomplete Months	-0.150**	-0.066		
	(0.071)	(0.064)		
Adjust SIPP Left-Censoring	-0.147**	-0.065		
	(0.071)	(0.063)		
Use States in SIPP Sample			-0.016	-0.349***
			(0.032)	(0.115)
Use Year-Months in SIPP Sample			-0.001	-0.362***
			(0.035)	(0.114)
Use States and Year-Months in SIPP Sample			0.002	-0.368***
			(0.037)	(0.114)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports pre-post estimates from equation (1) of the effects of relaxing the asset test and raising the income threshold on the total number of food hardships among eligibles (Columns 1–2, SIPP) and years of education among enrollees (Columns 3–4, QC data) under alternative specifications. Standard errors, clustered at the state level, are in parentheses.

B Construction of SNAP Eligibility Measures (SIPP)

Household Units and Demographics

We define SNAP units at the household level. Although SNAP benefit units are technically based on individuals who purchase and prepare meals together, this information is not observed in the SIPP data. Instead, many key economic variables are collected at the household level. Therefore, we use the household as the unit of analysis.

Using demographic characteristics of household members, we classify households into two groups: those with elderly or disabled members, and those without. A household is considered to have an elderly member if it includes at least one individual aged 60 or older. A household is considered to have a disabled member if it includes at least one individual receiving Social Security, SSI, veterans' benefits, or other disability benefits from the U.S. military, the federal government, state or local governments, or the Railroad Retirement System.

Household Income and Resources

Gross Income. Gross income is calculated as the sum of money income from all sources across all household members and is observed in all months.

Countable Assets. Countable assets are calculated by summing assets held in bank accounts, stocks, other financial institutions, and real estate (excluding the primary residence), plus vehicle values in states with vehicle tests. For vehicles, we use the reported values of the first three vehicles, provided their primary use is neither for business purposes nor for transporting a disabled person (such vehicles are excluded from SNAP resource calculations):

- In states using the federal vehicle test, we count the portion of each vehicle's value exceeding $4,650 toward household resources.
- In states that exempt amounts higher than the SNAP standard auto exemption, we count the portion of the first vehicle's value exceeding $12,000 as a resource, while applying the standard $4,650 threshold to the second and third vehicles.
- In states that exclude at least one, but not all, vehicles from SNAP asset calculations, we exclude the first vehicle and apply the $4,650 threshold to the second and third vehicles.

Net Income. To calculate net income, we first estimate adjusted income by subtracting the following from gross income:

- Standard deduction (varies with household size and changes over time),
- Earnings deduction (20% of household earnings),
- Medical expenses (out-of-pocket expenses above $35 for households with elderly or disabled members),

- Child support paid,
- Dependent care costs (capped at $175 per child before fiscal year 2009).

We then compute the shelter deduction as shelter expenses (rent or mortgage payments plus utilities) minus half of adjusted income. Net income is calculated as adjusted income minus the shelter deduction.

In SIPP panels prior to 2014, expense and asset information was collected roughly once every three waves (with each wave covering four months). For months in which such information is unavailable, we impute missing values using the nearest observed value for the same household within the panel. If no information is available for a given household, the observation is excluded from the analysis.

Simulating SNAP Eligibility Rules

Using household gross income, net income, countable assets, and relevant demographic characteristics, we simulate SNAP eligibility under federal rules, BBCE rules, and hypothetical income and asset rules.

Federal Rules

- Gross income limit: 130% of the FPL for households without elderly or disabled members; no gross income limit for households with elderly or disabled members.
- Net income limit: The statutory net income limit is 100% of the FPL. However, because SNAP benefits are a function of net income, there exists an effective net income threshold at which benefits become zero. We apply the more restrictive of (1) the statutory net income limit of 100% of the FPL and (2) the effective net income limit.
- Traditional categorical eligibility: Households in which all members receive cash welfare are treated as SNAP-eligible under the federal rules, regardless of other economic characteristics.

BBCE Rules

States have flexibility in setting income and asset limits for SNAP under BBCE, provided the associated aid program has an income limit of up to 200% of the FPL. In practice, some states relaxed only the asset test, with most eliminating it entirely and a few raising it to $5,000–$25,000. Other states both relaxed the asset test and raised the gross income threshold beyond the federal limit of 130% of the FPL, typically to 185% or 200%. Nearly all states that raised the gross income threshold also eliminated the net income test. A few states changed income or asset limits over time or eliminated BBCE altogether.

We apply state-month–specific BBCE gross income and asset limits. For states that removed the net income limit, we apply the effective net income limit, as defined above.

Exclusions Based on Observed Characteristics

Among financially eligible households, we exclude a small fraction that do not meet non-financial eligibility criteria. In particular, we consider the following households as ineligible:

- Households with SSI recipients in California prior to June 2019,
- Households in which all adults are non-citizens and no disabled members or children are present,
- Households in which all adults are full-time students with no disabled members or children.

Simplified Reporting Rules

Our simulation incorporates simplified reporting (SR) rules, implemented by some states beginning in the mid-2000s. Under these rules, SNAP recipients are not required to report changes in financial circumstances unless their gross income exceeds the eligibility limit prior to recertification (typically every 6 or 12 months). This effectively removes net income and asset tests for a fixed certification period for households that maintain gross income eligibility. To operationalize this policy, we leverage the panel structure of the SIPP data:

1. For each month, we determine whether (1) the household resides in a state with simplified reporting and (2) the household is gross-income eligible.
2. If both conditions are met, we evaluate two additional criteria:
   - The household's SNAP eligibility status k months earlier, where $k \in \{1, ..., 5\}$ for most households, and $k \in \{1, ..., 23\}$ for households with an elderly or disabled member, no earnings, and residence in a state with a median certification period of 24 months or longer.
   - Whether the household was continuously gross-income eligible over the past k months.

If both criteria are satisfied, we assign SNAP eligibility in the current month, regardless of the household's current net income or asset levels.

C Welfare Decomposition

We begin by presenting a simple model that organizes the comparison between income and asset expansions. The model builds on the welfare frameworks in Finkelstein and Notowidigdo (2019) and Anders and Rafkin (2024) — both of which study a single policy lever — by extending the comparison to two policy levers within a single program. Our goal is not to derive an optimal eligibility rule, but rather to clarify the primitives that determine a welfare ranking of the two reforms.

Setup. Let E_0 denote the set of households eligible under federal SNAP rules, and let $j \in \{I, A\}$ index two reforms: raising the income threshold (I) and relaxing the asset test (A). Let M_j denote the set of households made *newly* eligible by reform j. Each household i is characterized by two primitives: (1) $u_i \geq 0$, the dollar-equivalent social value of the household's enrollment (which embeds both the benefit dollars received and a welfare weight reflecting its need) and (2) b_i, the benefit amount the household would receive if enrolled (which determines the fiscal cost of its enrollment). Both are household-specific and do not depend on the reform, which instead changes which households become eligible and how take-up responds.

A reform can increase enrollment through two channels. The first is enrollment among newly eligible households. Let p_{ij} denote the probability that newly eligible household $i \in M_j$ takes up under reform j. The second is an increase in take-up among households already eligible (inframarginal) under baseline rules. Let δ_{ij} denote the increase in take-up probability for household $i \in E_0$ induced by reform j. The total enrollment effect of reform j is thus:

$$\Delta N_j = \underbrace{\int_{M_j} p_{ij}\, di}_{\text{newly eligible enrollment}} + \underbrace{\int_{E_0} \delta_{ij}\, di}_{\text{inframarginal enrollment}} . \tag{C.1}$$

Utility, Costs, and Welfare. The gross social value of enrollment under reform j aggregates contributions across the two channels:

$$\Delta U_j = \underbrace{\int_{M_j} p_{ij}\, u_i\, di}_{\text{value of newly eligible enrollees}} + \underbrace{\int_{E_0} \delta_{ij}\, u_i\, di}_{\text{value of inframarginal enrollees}} . \tag{C.2}$$

The fiscal cost replaces u_i with b_i and adds administrative costs (ΔAC_j) and the net fiscal externality from behavioral responses (ΔFE_j):

$$\Delta C_j = \underbrace{\int_{M_j} p_{ij}\, b_i\, di}_{\text{benefit \$s for newly elig. enrollees}} + \underbrace{\int_{E_0} \delta_{ij}\, b_i\, di}_{\text{benefit \$s for inframarg. enrollees}} + \Delta AC_j + \Delta FE_j. \tag{C.3}$$

Including fiscal externalities in ΔC_j follows standard practice in the Marginal Value of Public Funds (MVPF) literature (Hendren and Sprung-Keyser 2020) and parallels the treatment of fiscal externalities in (Anders and Rafkin 2024).[1] Net social welfare is the gross social value of enrollment minus the fiscal cost:

$$\Delta W_j \equiv \Delta U_j - \Delta C_j. \tag{C.4}$$

We use this additive surplus form rather than the MVPF ratio ($\Delta U_j/\Delta C_j$) because surplus decomposes cleanly into separate channels, which helps to organize the empirical comparison.

Decomposing the Comparison. Substituting equations (C.2) and (C.3) into equation (C.4) and differencing between the income and asset reforms yields:

$$\begin{aligned}
\underbrace{\Delta W_I - \Delta W_A}_{\text{total net welfare differential}} &= \underbrace{\int_{M_I} p_{iI}\,(u_i - b_i)\,di \;-\; \int_{M_A} p_{iA}\,(u_i - b_i)\,di}_{\text{(a) newly eligible welfare differential}} \\
&\quad + \underbrace{\int_{E_0} \delta_{iI}\,(u_i - b_i)\,di \;-\; \int_{E_0} \delta_{iA}\,(u_i - b_i)\,di}_{\text{(b) inframarginal welfare differential}} \qquad \text{(C.5)} \\
&\quad - \underbrace{(\Delta AC_I - \Delta AC_A)}_{\text{(c) admin cost differential}} \;-\; \underbrace{(\Delta FE_I - \Delta FE_A)}_{\text{(d) fiscal externality differential}}\;, \qquad \text{(C.6)}
\end{aligned}$$

where the income expansion is preferred to the asset relaxation when $\Delta W_I - \Delta W_A > 0$ (and vice-versa). Each of the four terms in equation (C.6) corresponds to a different channel through which the two reforms can deliver different welfare effects.

Term (a), the newly eligible welfare differential, captures whether one reform pulls in more newly eligible households with higher net welfare per capita. The income and asset levers may differ in several respects on this margin, including the size of the newly eligible population (the integration domains M_I vs M_A) and differences in household-level utilities u_i and benefit amounts b_i between M_I and M_A. An analysis of targeting sheds light on the comparison of u_i's; if one lever pulls in newly eligible households who are more disadvantaged on average, then that increases its welfare advantage. Lower average benefit amounts likewise contribute by reducing the fiscal cost of newly eligible enrollment.[2] Take-up probabilities p_{ij} further shape how the newly eligible population translates into actual enrollment, with potentially different implications across reforms.

Term (b), the inframarginal welfare differential, captures the comparison across reforms of enrollment responses among households who were already eligible under baseline rules. Such responses can arise if reforms reduce documentation burdens, decrease information frictions, or reduce stigma costs. If a lever induces a larger inframarginal response (and those inframarginals are particularly disadvantaged), then this term reinforces that lever's welfare

1. We treat u_i as a reduced-form welfare value that does not distinguish between the various drivers of inframarginal enrollment, including the stigma versus information distinction emphasized in Anders and Rafkin (2024). Under their decomposition, u_i is close to zero for stigma-driven enrollees (by the envelope theorem) and positive for information-driven enrollees.

2. In SNAP, where benefit amounts decline mechanically with income, the income lever necessarily pulls in households who would receive lower b_i.

advantage. Term (c) captures differences in administrative costs, which reflect the burden of processing and verifying applications and recertifications. Finally, Term (d) captures fiscal externalities reflecting behavioral responses outside of the program, including labor-supply responses to income thresholds and savings responses to asset limits.

Our empirical analyses can speak to many (though not all) of these channels. For Term (a), our analyses of access, targeting, and benefit amounts (among both eligibles and enrollees) collectively address most of the relevant components. For Term (b), we can decompose the income lever's enrollment effect into marginal and inframarginal components, but we cannot do the same for the asset lever because states that eliminate the asset test typically stop collecting asset information from applicants. For Term (c), we report administrative cost effects directly, though these are likely small relative to total benefits paid out and unlikely to be a primary driver of the overall comparison. Term (d) is beyond the scope of our empirical analyses, though we include it here for completeness.