The Effects of SNAP Sugary Drink Restrictions on Consumption and Welfare

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Abstract

As of July 2026, 23 states had approved policies that disallow spending of SNAP benefits on sugary drinks. The effects of such restrictions on consumption are unclear, however, since most households can simply switch to buying sugary drinks with non-SNAP funds. Using a difference-in-differences design with nationwide grocery purchase panel data, we estimate that restrictions in the first 10 states reduced the average SNAP household's retail purchases of excluded drinks by 12.4 percent (standard error = 1.0) over the first half of 2026. In states that excluded only some sugary drinks, SNAP households partially substituted to non-excluded drinks. Surveys we carried out before and after implementation show that the restrictions increased SNAP recipients' perceptions of stigma. We combine our estimated consumption reductions with external parameters to model welfare effects given an over-consumption internality and a fiscal externality via public health care spending. In our model, excluding all sugary drinks from SNAP nationwide would provide benefits of about $1.1 billion per year, of which about 70 percent is from reduced health care costs.

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"We shouldn't be subsidizing people to eat poison."
– Robert F. Kennedy Jr. (2025)

"What is the likely impact of banning soda purchase with SNAP dollars, then? Consumers would change the payment method used for buying soda, but likely will not change the amount of soda they buy."
– Diane Schanzenbach (2017)

1 Introduction

Many countries provide much of their social safety net through in-kind transfers—such as housing vouchers, public education, and health insurance subsidies—rather than cash. In the United States specifically, the vast majority of transfers to the non-elderly are in-kind (Currie and Gahvari 2008; Edin and Shaefer 2015; Schmidt, Shore-Sheppard, and Watson 2025). One rationale for in-kind transfers is that they can shift consumption towards goods that policymakers think should be consumed more, and away from goods that they think should be consumed less (e.g., Thurow 1974; Besley 1988; Mulligan and Philipson 2000; Liscow and Pershing 2022; Ambuehl et al. 2025; Chorniy, Finkelstein, and Notowidigdo 2025).

The U.S. Supplemental Nutrition Assistance Program (SNAP) is a classic example of an in-kind program with a stated goal of shifting consumption. SNAP is the second-largest means-tested transfer program in the U.S., providing an in-kind food subsidy to over 40 million Americans at an annual cost of about $100 billion (Hoynes and Schanzenbach 2015; USDA 2025a). Benefits can be used to purchase almost any food or drink sold in stores. When the program was created in 1964, one explicit aim was to increase recipients' food consumption in order to improve their health (Johnson 1964; USDA 2025b). In recent years, however, there has been growing interest in designing SNAP to influence the nutritional quality of food purchases, not just the quantity. Particular attention has focused on sugary drinks, which account for nine percent of SNAP spending (Garasky et al. 2016) and may contribute to health problems such as obesity and type 2 diabetes (Malik et al. 2010; Malik et al. 2013).

Debates over whether to allow SNAP benefits to be used for sugary drinks date back to the program's creation by the Food Stamp Act of 1964.[1] Over the last several decades, state and local politicians have repeatedly tried to restrict the use of SNAP benefits to purchase sugary drinks, but their policies were never approved by the federal government until 2025, when the Trump administration encouraged states to apply for waivers to do so. As of July 2026, 23 states had received federal waivers to remove sugary drinks from SNAP eligibility.

Reducing sugary drink consumption and the associated health harms was an explicit rationale for encouraging states to apply for these waivers, as the above quote from Health and Human Services

1. Although the final version of the law established that benefits could be used for "all items intended for human consumption" except for alcohol, the House version would have prohibited purchases of sugary drinks, among other items (USDA 2025b).

Secretary Robert F. Kennedy indicates. Yet most SNAP recipients spend more on groceries than they receive in SNAP benefits (Trippe and Ewell 2007; Hoynes, McGranahan, and Schanzenbach 2014; Hastings and Shapiro 2018). As a result, standard economic theory predicts that removing sugary drinks from SNAP will not affect purchases: people will simply switch to paying for sugary drinks with their own (non-SNAP) dollars, as the above quote from economist Diane Schanzenbach suggests. However, recent empirical work finds that SNAP benefits increase grocery spending much more than receiving the same amount of cash (Hastings and Shapiro 2018; Song 2022). Hastings and Shapiro (2018) attribute this behavior to mental accounting, where recipients think of SNAP benefits as increasing a separate mental budget to be spent on eligible groceries. This raises the possibility that removing sugary drinks from the list of SNAP-eligible groceries could materially reduce their consumption. These competing theories make it a priori unclear whether and how much these restrictions will actually reduce sugary drink consumption.

This paper estimates the effect of the SNAP sugary drink restrictions on SNAP recipients' consumption of excluded drinks,[2] leveraging the staggered roll-out of the restrictions across 10 states in the first half of 2026. We then combine the estimated consumption effects with external parameters to predict the effects on consumer welfare and public health care spending.

Our primary data source is nationwide grocery purchase panel data from Numerator, a market research firm. The Numerator Panel Data records whether each purchase was made with an Electronic Benefit Transfer (EBT) card (a proxy for use of SNAP benefits). Our primary analysis sample consists of a balanced panel of about 15,000 SNAP households that recorded purchases over the 18-month period from January 2025 through June 2026. To measure public opinion, perceived stigmatization, and related views, we surveyed about 9,000 Numerator panelists both in December 2025 (before any restrictions took effect) and again in July 2026. Before accessing any post-policy data, we pre-specified our research design, event study estimators, and survey analyses in a detailed pre-analysis plan.

We estimate that the SNAP restrictions reduced SNAP recipients' retail purchases of excluded drinks by 12.4 percent (standard error = 1.0). This translates into a reduction in sweetened drink consumption of about 404 fluid ounces per covered person per year, or about 34 12-ounce cans. This is not consistent with the standard economic theory described above.

Even if SNAP recipients buy fewer sugary drinks from grocery retailers, they might still substitute to other forms of sugar. We can reject substantial substitution to soda at restaurants or to retail purchases of sugary foods. However, some states exclude only some categories of sugary drinks—for example, soda but not energy drinks or fruit drinks. In the states that exclude only some sugary drink categories, we estimate that SNAP recipients diverted up to 39 percent of the excluded drink calorie reduction to increases in retail purchases of non-excluded sugary drinks and fruit juices. This highlights a useful policy design insight: since sugary drink categories are mod-

2. As detailed in Table 1, all states with restrictions exclude at least some sugary drinks, and most also exclude diet drinks sweetened with low- or no-calorie sweeteners. We define "sugary drinks" as those with added caloric sweeteners and "sweetened drinks" as those with any added sweetener. We use "sugary drink restrictions" as a shorthand for policies excluding either type of drink and "excluded drinks" to denote the drinks covered by a given state's policy.

erately substitutable, continuing to include some sugary drinks in SNAP will naturally undo some of the potential consumption reductions. This is analogous to how substitution across products or leakage across jurisdictions can reduce the gains from corrective taxes.

We carry out seven pre-specified subgroup analyses. We find much stronger effects on retail excluded drink purchases among households that pay for a larger share of their groceries with EBT cards; this is consistent with the restrictions acting through mental accounting. We find suggestive evidence of stronger effects among households whose survey respondent reported perceiving little or no health risk from consuming soft drinks. Across the other subgroup comparisons (including baseline excluded drink consumption, whether survey respondents are diabetic, and whether respondents believe they drink soft drinks more than they should), we find no significant evidence of heterogeneous effects.

Our survey data allow us to glean additional insights not possible from purchase data alone. Support for SNAP sugary drink restrictions was much larger among non-SNAP recipients than SNAP recipients. Difference-in-differences estimates comparing baseline versus endline survey responses in treatment versus control states suggest that the restrictions caused statistically significant 0.10 to 0.17 standard deviation increases in three measures of SNAP recipients' perceptions of stigma: feeling judged when paying with SNAP benefits and perceiving that the current SNAP policies in their state were disrespectful or took away personal freedom. These estimates provide support for concerns that the restrictions would make SNAP recipients feel stigmatized, judged, or disrespected (Stuber and Schlesinger 2006; Barnhill 2011; Schanzenbach 2013; Gundersen 2020; Krobath et al. 2025). We find no significant effect on SNAP recipients' perceived health risks of soft drinks, which does not support a model where the restrictions act through signaling health risk information.

In the final part of the paper, we use our estimated sugary drink consumption reduction as an input into calibrated consumer choice and health models, allowing us to predict the welfare effects of excluding all sugary drinks from SNAP nationwide. We model three distortions relative to the social optimum: fiscal externalities (since governments pay most of SNAP recipients' health care costs), mental accounting (which delivers the effect of excluding sugary drinks from SNAP), and sugary drink consumption internalities (from self-control or imperfect information about health risks). Consistent with such internalities, about 60 percent of SNAP recipients reported on our survey that they drink soft drinks more often than they should, while about 30 percent reported that drinking soda poses very little or no health risk.

These models require additional assumptions plus parameter estimates from other papers, including the magnitude of the sugary drink consumption internality and the effects of sugary drink consumption on health outcomes and health care costs. Many of these external estimates do not involve quasi-experimental variation. The resulting predictions are thus more speculative than our empirical estimates of the effects on consumption. Bearing in mind these caveats, our models can provide useful predictions of the eventual implications of our estimated consumption changes.

Our models predict that excluding all sugary drinks from SNAP nationwide would provide benefits of about $1.1 billion per year. About 70 percent of these benefits come from the predicted health care cost reduction. That health care cost reduction stems from predictions that the restrictions would cause the average adult SNAP recipient to lose 0.27 pounds and would reduce the risk of developing type 2 diabetes over the next 10 years by 2.5 percent, or about 33,000 new cases nationwide. The remaining 30 percent of the benefits come from consumer welfare gains from reduced sugary drink consumption in the presence of an over-consumption internality. This estimate of consumer welfare gains may be overstated, however, as it does not reflect potential losses from increased stigma.

Our paper's primary contribution is to provide a wide-ranging nationwide evaluation of the effects of the controversial SNAP sugary drink restrictions on consumption, health, and welfare. In doing so, our paper relates to the large literature on the nutritional effects of SNAP. This includes work on the impact of SNAP on food insecurity and diet quality; Hastings, Kessler, and Shapiro (2021) provide a recent overview of this literature, as well as evidence that SNAP receipt has little effect on the composition of purchased foods.

As described above, it was a priori unclear whether SNAP restrictions would affect consumption. To further document this, we surveyed 336 researchers in economics and public health before releasing our paper. Those researchers had very divergent prior beliefs about the effects of SNAP restrictions on excluded drink consumption: 29 percent predicted no effect at all, 51 percent predicted effects of 5 percent or less (less than half of our point estimate), and 12 percent predicted effects of 25 percent or more (more than twice our point estimate). Appendix A provides details and additional results.

Consistent with this uncertainty, the existing literature has suggested a range of possible impacts for SNAP sugary drink restrictions. Two prior papers have used randomized experiments with 200–300 households to measure the effects of excluding sugary foods and drinks from a SNAP-like benefit. Harnack et al. (2016) find no statistically significant effect of sugary item restrictions on sugary drink consumption, while Harnack et al. (2024) find a 40 percent reduction; both zero and 40 percent are far outside the 95 percent confidence bounds of our empirical estimate. Two other prior papers have used structural grocery demand models to predict the possible effects of removing sugary drinks from SNAP (Basu et al. 2014; Cox and Harris-Lagoudakis 2026). These two papers predict that excluding sugary drinks from SNAP would reduce consumption by 15 and 22 percent, respectively; both of these predictions are above the 95 percent confidence bound of our empirical estimate. Contemporaneous work by Frisvold and Lozano Rojas (2026) finds that the restrictions reduced soda purchases by 13 percent over the first six months of 2026.

More broadly, our work connects to the theoretical and empirical literature in the opening paragraph on the potential rationale for in-kind transfers as a way to shift consumption, as well as to the behavioral public economics literature evaluating the welfare effects of public policy when consumers have non-standard preferences.[3] In this spirit, our findings may have implications for

3. This includes Gruber and Kőszegi (2001), Carroll et al. (2009), Chetty, Looney, and Kroft (2009), Abaluck and Gruber (2011), Handel (2013), Chetty et al. (2014), Allcott and Taubinsky (2015), Bernheim, Fradkin, and Popov (2015), Taubinsky and Rees-Jones (2018), Goldin and Reck (2020), Rees-Jones and Taubinsky (2020), Allcott et al. (2022), Barahona, Otero, and Otero (2023), and Allcott et al. (2025).

a broader set of means-tested transfer programs that restrict how benefits may be used. Indeed, within the nutrition-assistance landscape, SNAP is unusual in placing relatively few limits on eligible purchases. Other U.S. food programs are more restrictive: the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) confines beneficiaries to a specified package of foods, while the National School Lunch Program (NSLP) and the Child and Adult Care Food Program (CACFP) impose detailed nutrition standards on the meals institutions serve (Hoynes and Schanzenbach 2016; USDA 2024b, 2024c). Analogous attempts to steer consumption also extend to cash assistance. For example, media coverage of recipients accessing Temporary Assistance for Needy Families (TANF) cash benefits at casinos, liquor stores, and strip clubs led Congress in 2012 to require states to prevent benefits from being accessed electronically at such venues (U.S. Congress 2012; U.S. Government Accountability Office 2012).[4] Our findings and modeling approaches may be useful for evaluations of other restrictions on in-kind or cash benefits.

Sections 2–7, respectively, present the policy overview, data, estimated effects on consumption, survey results, modeled effects on consumer welfare and health care spending, and conclusion.

2 Policy Overview

SNAP is implemented by states but funded by the federal government through the U.S. Department of Agriculture (USDA), with a common set of implementation rules that all states must follow. Eligibility is means-tested based on income and asset restrictions, but otherwise nearly universal.

Until 2025, USDA policies prohibited states from limiting what SNAP benefits could buy beyond the standard federal rules that disallowed alcohol and hot prepared foods (USDA 2026a). Over the last two decades, several jurisdictions applied for waivers to exclude use of SNAP benefits for certain foods deemed unhealthy but were denied by the federal government out of concerns about stigma, administrative complexity, and unclear benefits.[5] However, in April 2025, the Trump administration encouraged states to request waivers from the standard rules to disallow sugary drinks and processed foods (Kennedy and Rollins 2025). Health and Human Services Secretary Robert F. Kennedy, Jr., advocated for the policy, saying that "For years, SNAP has used taxpayer dollars to fund soda and candy—products that fuel America's diabetes and chronic disease epidemics. These waivers help

4. Comparable measures appear abroad: Brazil has barred Bolsa Família recipients from registering on licensed online-betting platforms, though the measure has since been contested in court (Reuters 2024; Supremo Tribunal Federal 2024; Ministério da Fazenda, Secretaria de Prêmios e Apostas 2025), and Australia has "quarantined" a share of welfare payments for recipients in designated communities onto cards that cannot be used for alcohol, tobacco, pornography, or gambling, or withdrawn as cash (Australian National Audit Office 2018; Services Australia 2026).

5. For example, in 2003, Minnesota applied for a waiver to exclude candy, soda, and other "junk" foods from SNAP; the USDA, under President George W. Bush, denied the request, arguing that defining junk food was too arbitrary and that restrictions would stigmatize recipients (Skorburg 2004). In 2010, New York City, under Mayor Michael Bloomberg, applied for a waiver to exclude sugary drinks from SNAP; the USDA, under President Barack Obama, denied the waiver, arguing that implementation would be too complex and the effects on obesity were unclear (McGeehan 2011). Maine applied for a waiver to exclude sugary drinks and candy from SNAP in 2015 and again 2017; the USDA, first under President Obama and later under President Donald Trump, rejected both requests (Shepard 2018). Policymakers from both sides of the aisle have explored other avenues toward restricting SNAP, including through acts of Congress, but again without success (Paarlberg et al. 2018).

put real food back at the center of the program and empower states to lead the charge in protecting public health" (HHS 2025).

As of July 2026, 23 states had received waivers to disallow spending of SNAP funds on certain foods or drinks. Perhaps reflecting the policy's association with the Trump Administration's "Make America Healthy Again" movement, restrictions have been disproportionately adopted by Republican states. 20 of the 23 states with waivers voted for Donald Trump in the 2024 presidential election, while 17 of the 28 states without waivers voted for Kamala Harris; see Appendix B.

Table 1 presents more information on restrictions in the 23 states that ever received waivers, including implementation date and excluded categories. All 23 states exclude regular soda, but states vary in what other items are excluded: for example, 18 exclude diet soda, 12 exclude fruit drinks, and 15 exclude candy.

Ten states implemented restrictions between January and June 2026; these are the "treatment" states in our difference-in-differences (DiD) analyses below. The remaining 40 states plus D.C. make up our DiD control group. As of July 2026, 11 of our control states have implementation dates after June 2026, while two had waivers scheduled for implementation after June 2026 that were vacated by a lawsuit in late June 2026. Three of our treatment states (Iowa, Nebraska, and West Virginia) also had their waivers vacated in late June 2026 by the same lawsuit.[6]

The earliest restrictions had effective dates of January 1, 2026. In principle, retailers have a 90-day grace period before enforcement begins. In practice, while data on enforcement are not available, our perusal of state websites and news media suggests that the restrictions were communicated to recipients and retailers and actively implemented from their effective dates (e.g., Aldridge 2026; Batzlaff 2026; Indiana Family and Social Services Administration 2026; Utah Department of Workforce Services 2026; Woodruff 2026). For example, Figure 1 shows pictures of signage in grocery stores informing shoppers of the restrictions around the time that the restrictions were implemented. This suggests restrictions were implemented sufficiently early in our study period for their initial effects to be observable in our data.

An important consideration for the potential effects of these restrictions is that SNAP benefits are typically inframarginal: for most households, they cover only part of a household's grocery spending. For example, according to the 2024 SNAP Quality Control data, the average monthly benefit amount was $353, while according to the 2023 Consumer Expenditure Survey, the average SNAP recipient household spent $505 per month on groceries. More directly, Hastings and Shapiro (2018) report that average SNAP-eligible spending in non-SNAP months exceeds average SNAP benefits in SNAP months for 94 percent of recipients. Hoynes, McGranahan, and Schanzenbach (2014) report that food at home spending exceeds SNAP benefits for 84 percent of recipients. Trippe and Ewell (2007) report that food spending exceeds SNAP benefits by at least 10 percent for 78 percent of SNAP recipients. In the Numerator purchase data described below, in 97.9 percent of household-months, SNAP households have at least one retail grocery transaction paid with non-

6. SNAP recipients in those five states sued to block the restrictions (Higham 2026). A federal court ruled for the plaintiffs on procedural grounds on June 22, 2026, vacating the waivers in those five states, though the states could apply for new waivers.

SNAP funds.

Table 1: SNAP Restrictions by State

	Implementation date	Soda		Sports drinks	Energy drinks	Sweetened tea	Fruit drinks	Candy
		Regular	Diet					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
January-June 2026 implementers (treatment states in empirical analysis)								
Indiana	1/1/2026	✓	✓	✓	✓	✓	✓	✓
Iowa[*]	1/1/2026	✓	✓	✓	✓	✓	✓	✓
Nebraska[*]	1/1/2026	✓	✓		✓			
Utah	1/1/2026	✓	✓					
West Virginia[*]	1/1/2026	✓	✓		✓			
Idaho	2/15/2026	✓	✓	✓	✓	✓	✓	✓
Oklahoma	2/15/2026	✓	✓	✓	✓	✓	✓	✓
Louisiana	2/18/2026	✓	✓		✓			✓
Texas[†]	4/1/2026	✓	✓	✓	✓	✓	✓	✓
Florida	4/20/2026	✓	✓		✓			✓
Later expected implementers or waiver vacated (control states in empirical analysis)								
Arkansas	7/1/2026	✓	✓		✓	✓	✓	✓
South Carolina	8/31/2026	✓	✓		✓	✓	✓	✓
North Dakota	9/1/2026	✓	✓		✓			✓
Montana[§]	9/30/2026	✓		✓	✓	✓	✓	✓
Ohio	10/1/2026	✓			✓			
Virginia	10/1/2026	✓	✓		✓			
Wyoming[¶]	2/1/2027	✓	✓		✓			
Missouri	2/15/2027	✓	✓		✓	✓	✓	✓
Kansas	2/15/2027	✓	✓	✓	✓	✓	✓	✓
Hawaii[‡]	4/1/2027	✓			✓			
Nevada	2/1/2028	✓					✓	✓
Colorado[*]	Vacated	✓	✓	✓	✓	✓	✓	
Tennessee[*]	Vacated	✓						✓

Notes: This table presents the implementation date and excluded categories for states that had received federal waivers by July 2026, using information from USDA (2026d) plus state websites. Florida, Missouri, and Montana also exclude prepared desserts in addition to categories presented in this table.

[†]: Texas restricts only if >5g sugar.

[‡]: Hawaii restricts only if >10g sugar per serving.

[§]: Montana restricts only if >10g sugar per 8 ounces.

[*]: A court vacated the waivers of Colorado, Iowa, Nebraska, Tennessee, and West Virginia on June 22, 2026 and ordered that implementation be halted.

[¶]: Wyoming plans to restrict sweetened drinks starting in February 2027 and candy starting in February 2028.

Figure 1: Examples of Shelf Tags and News Coverage Communicating SNAP Restrictions

(a) Arkansas Shelf Tag

(b) Texas Shelf Tag

(c) Indiana TV News Information

Notes: These pictures are from Google image searches for terms like "SNAP sugary drink restrictions." News articles covering the restrictions suggest that shelf tags were posted around the time restrictions were implemented (e.g., Aldridge 2026; Ansell 2026).

3 Data and Pre-Analysis Plan

3.1 Numerator Purchase Data

Our primary dataset is the Numerator Panel Data, accessed through the Kilts Center at the University of Chicago. The Numerator Panel Data is an opt-in panel similar to NielsenIQ Homescan: participants provide demographic information, scan purchases, and can take additional surveys. We chose the Numerator Panel Data because it is a large panel dataset that includes transaction payment methods (including the use of SNAP EBT cards) as well as some restaurant purchases.

Numerator provides household-level data including demographics and purchase receipts. The demographic information includes self-reported household income, household size, the presence of children, and zip code, as well as the age, education, race, and diabetes status of the household member who scans the receipts and answers survey questions. Numerator panelists upload receipts for each purchase. This is different from NielsenIQ Homescan, where panelists scan the UPC for each packaged item they buy. Receipts include an abbreviated name for each item purchased and its price, but not a UPC. The Numerator Panel Data do not include package size or nutrition facts such as sugar content.

The Numerator Panel Data include the method of payment inferred from each receipt, such as cash, credit cards, or Electronic Benefit Transfer (EBT) cards used to spend SNAP benefits. Some grocery transactions have multiple payment methods—for example, if a SNAP recipient buys SNAP-eligible items with an EBT card and other items with cash. However, Numerator records one payment method for each receipt. We refer to a transaction in which EBT was used as an "EBT transaction." Some items in an EBT transaction may have been paid for by another payment method.[7] Appendix C.1 provides details on the Numerator Panel Data.

Measuring purchases. We use the word "consumption" as a synonym for "purchases," with the understanding that not all grocery items purchased are actually ingested. We separate purchases into two distribution channels. The "retail" channel includes purchases at retailers such as grocery, drug, convenience, club, and dollar stores. This is sometimes called "at-home" consumption. The "restaurant" channel includes purchases at limited-service restaurants, such as McDonald's, Subway, or Shake Shack. This is a subset of what is sometimes called "away-from-home" consumption. The Numerator Panel Data do not include vending machines, full-service restaurants, or other away-from-home consumption. SNAP funds can generally be spent at retail locations, but not at restaurants or other away-from-home locations.

Numerator classifies individual items into categories. For the retail channel, our main empirical analyses consider sweetened drink purchases in the soda, sports drink, energy drink, and sweetened tea categories; we do not consider fruit drinks because only those with less than 100 percent juice are excluded, and we are not able to reliably infer juice content for each item. For the restaurant

7. Specifically, the Numerator Panel Data documentation indicates that EBT is always recorded as the payment method if any part of the payment was made via EBT. Numerator flags the entire receipt, not individual items.

channel, we analyze purchases in the soda category; Numerator does not separately categorize restaurant purchases of sports drinks, energy drinks, and sweetened tea because those drinks are not commonly sold at limited-service restaurants. We separate soda into regular and diet categories using the item name—for example, if it includes the words "diet" or "zero."

Most of our analyses require an estimate of the quantity (volume) of drinks consumed, not just expenditures. Since the Numerator Panel Data do not include package sizes, we impute them using auxiliary data on price and package size and a regression model that estimates sweetened drink volume/price ratios as a function of item price, category, month, brand, retail channel, household income, and region. Using the estimated parameters, we then predict each item's volume in the Numerator Panel Data. Appendix C.2 provides details. We also show below that treatment effects on excluded drink expenditures are extremely similar (as a percent of baseline) to treatment effects on quantities.

We aggregate purchases to the household $\times$ category $\times$ month level. We transform to consumption per person-day by dividing by household size and the number of days in the month. We then winsorize all consumption measures at the 99.5th percentile to control outliers. Appendix C.3 shows that household average monthly grocery spending is similar between Numerator and the Consumer Expenditure Survey (CEX), while Numerator households have somewhat more spending on non-alcoholic beverages than in the CEX.

Measuring SNAP participation. We will limit most of our empirical analyses to SNAP recipients. We define $S_{it} \in \{0, 1\}$ as an indicator for whether household i is enrolled in SNAP in month t. We infer S_{it} using income and EBT transactions. We first approximate the SNAP program's income limits by requiring household income to be below a threshold that depends on household size. For those households, we code a household-month as on SNAP ($S_{it} = 1$) if it is part of a sequence of months where (i) there is no more than one consecutive month without an EBT transaction, (ii) at least half of months have EBT transactions, and (iii) at least 20 percent of grocery spending is in EBT transactions. Appendix C.4 provides details. Appendix Table A5 shows that our inferred S_{it} is strongly (although not perfectly) correlated with self-reported SNAP participation from our baseline survey.

S_{it} is measured with error, and our judgment calls affect the rates of false inclusion and exclusion. We coded S_{it} with a goal of keeping false inclusion relatively low, so that the great majority of $S_{it} = 1$ observations are in fact on SNAP. We do this because false inclusion of non-SNAP households dilutes our treatment groups. However, false exclusion of true SNAP recipients makes our estimated effects relevant only to a subset of SNAP households. Section 4 presents robustness checks with alternative definitions of S_{it}. Notwithstanding inevitable measurement error, our ability to measure SNAP receipt based on EBT payments is a valuable feature relative to relying on self-reports, which are lower-frequency and potentially under-reported (Meyer, Mok, and Sullivan 2009).

Sample restrictions. For each month, Numerator defines a "static panel" of about 200,000 households that reliably scan receipts for at least the next 12 months.[8] Our difference-in-differences analyses use data from January 2025 through June 2026. We construct a balanced panel for those analyses by combining the January 2025 and July 2025 static panels. Our primary difference-in-differences analyses use a "primary analysis sample" of $N = 15{,}261$ households with $S_{it} = 1$ in a baseline month that we will define more precisely in Section 4.1 below. In one robustness analysis we also include "non-SNAP households," defined as those with zero EBT transactions for all of 2025 and 2026. Appendix C.5 provides details.

Sample weights. For households ever on SNAP in 2026, we construct sample weights using data from the American Community Survey to match U.S. SNAP recipients on income, share of households with children, shares of households that are White and Black, and shares in each of the four census regions. For non-SNAP households, we construct sample weights to match U.S. non-SNAP recipients on those same characteristics. Appendix C.6 provides details. All analyses using the Numerator household purchase panel are weighted by these sample weights unless explicitly stated.

Summary statistics. Table 2 presents summary statistics on sweetened drink consumption in 2025. Panel (a) presents retail consumption of sweetened drinks by category, among SNAP households in the primary analysis sample. Appendix C.7 presents the underlying distributions for each consumption category. The key takeaway is that regular soda is by far the largest category. Panel (b) presents sweetened drink consumption by channel and payment type, again for SNAP households in the primary analysis sample. About 32 percent of retail sweetened drink purchases were in EBT transactions. In a mechanical model where SNAP restrictions eliminate all excluded drink purchases in EBT transactions and households do not adjust non-EBT excluded drink purchases, the restrictions would thus reduce purchases by 32 percent.

Panel (b) also shows that average consumption of soda at limited-service restaurants is 0.30 ounces per person-day, or about 5 percent of total retail sweetened drink consumption. One should expect the Numerator Panel Data to understate away-from-home sweetened drink consumption because they only capture soda served in limited-service restaurants. Indeed, using nationally representative data from the National Household Food Acquisition and Purchase Survey (FoodAPS), Lin and Zhang (2025) estimate that 78 percent of SNAP recipients' sugary drink consumption volume is bought from grocery stores and other "at home" channels, while 22 percent is bought from "away-from-home" channels.

Panel (c) presents average retail sweetened drink consumption for different groups of households. There are two key results, both of which we anticipated when pre-specifying our empirical strategy. First, among SNAP households, average sweetened drink consumption in 2025 was similar but slightly higher in treatment states than in control states. Our empirical strategy addresses this

8. Specifically, Numerator requires at least two trips per month and five retailers visited over the 12 months. The average panelist has at least 30 trips per month and at least 50 retailers shopped per year.

slight difference. Second, SNAP households consume substantially (39 percent) more sweetened drinks than non-SNAP households. For this reason, our primary empirical analyses do not use non-SNAP households as a control group.

Table 2: Descriptive Statistics on Sweetened Drink Consumption in 2025 (ounces/person-day)

Panel (a): Retail Consumption by Category, among SNAP Households

Total sweetened drinks (1)	Regular soda (2)	Diet soda (3)	Sports drinks (4)	Energy drinks (5)	Sweetened tea (6)
9.61	5.81	1.56	0.85	0.45	0.93

Panel (b): Sweetened Drink Consumption by Channel and Payment Type, among SNAP Households

Retail sweetened drinks			Restaurant soda
EBT transactions (1)	Non-EBT transactions (2)	Total (3)	(4)
3.08	6.53	9.61	0.30

Panel (c): Retail Sweetened Drink Consumption by Household Group

SNAP households			Non-SNAP households
Treatment states (1)	Control states (2)	All (3)	(4)
9.84	9.55	9.61	6.91

Notes: Panels (a) and (b) present average sweetened drink consumption in 2025 among SNAP households (N = 15,261). The level of observation is household-month. Panel (a) looks at retail consumption; total sweetened drinks in column (1) is the sum of columns (2)–(6). Panel (b) presents averages separately by channel (retail vs. restaurant) and within retail by payment type (EBT transaction or not); restaurant consumption includes soda only. Panel (c) presents average retail sweetened drink consumption in 2025 for different groups of households (N = 87,780 for non-SNAP households, N = 3,291 for SNAP households in treatment states, and N = 11,970 for SNAP households in control states).

3.2 Survey Data

To record a richer set of outcomes and moderators, we designed a survey of Numerator panelists. Appendix D provides considerably more detail about the survey.

The survey questions queried the amount of unscanned soda consumption, SNAP enrollment and benefit amounts, opinions about SNAP restrictions, perceived stigma related to SNAP participation, perceived health risk from soft drink consumption, perceived over-consumption of sweetened drinks, and proxies for mental accounting. Numerator fielded our baseline survey in December 2025 and our endline in July 2026.

We significantly oversampled likely SNAP recipients, based on observed EBT transactions in 2025. The baseline survey has 17,819 valid respondents, of whom 8,880 reported receiving SNAP benefits. The endline survey was fielded to people who completed baseline and were still in the Numerator panel. Numerator expected about a 50 percent endline response rate; we asked their team to follow up as intensively as possible. The endline survey has 9,014 valid respondents, of whom 4,425 reported receiving SNAP benefits.

When analyzing the survey data, we define "SNAP households" and "non-SNAP households" based on self-reports during the survey, as the wording of some questions depended on that self-report. We construct a separate set of sample weights for the survey respondents, following the same approach as for the full Numerator household purchase data. All analyses of survey outcomes are weighted by these sample weights unless explicitly stated.

3.3 Pre-Analysis Plan

We submitted a pre-analysis plan (PAP) on April 20, 2026, before we had access to any post-treatment data.[9] The PAP, survey instruments, and data analysis code are available at osf.io/5nf3b. The PAP specified details of data preparation, the primary empirical specification (including the analysis sample, regression equation, and primary outcome variable) as described in Section 4.1 and presented in Table 3, the specific subgroup analyses estimated in Table 5, and the specific alternative specifications estimated in Tables 6 and A14. To be precise about our plans, we also included shells of key tables and figures: Tables 1 to 3, 5 and 6, A9, A13, A14, and A18, and Figures 2 to 4, A15, A18, and A19 were all included in the PAP, populated with hypothetical data. We modified those exhibits only in several superficial ways listed in Appendix E.

While detailed pre-analysis plans have become common for randomized experiments, they are less common for observational data analyses (Ofosu and Posner 2023; Dee 2025), perhaps because researchers have to make more potentially unexpected judgment calls with quasi-experimental analyses.[10] Our ability to pre-specify an empirical strategy was facilitated by the fact that we could carry out placebo analyses in pre-treatment Numerator Panel Data from 2023 to 2025, which allowed us to foresee some analytical issues that would arise.

4 Effects on Purchases

4.1 Empirical Strategy

Setup and notation. The staggered introduction of SNAP restrictions in some states but not others lends itself naturally to a difference-in-differences analysis comparing changes in SNAP recipients' excluded drink purchases after versus before the restrictions in treatment versus control

9. The Kilts Center makes each quarter's Numerator Panel Data available several weeks into the next quarter; the January–March 2026 data became available on April 21.

10. Other observational data studies using pre-analysis plans include Neumark (2001), Hangartner et al. (2019), Baccini, Brodeur, and Weymouth (2021), Bonilla, Dee, and Penner (2021), Clemens and Strain (2021), Brown et al. (2022), and Dench, Pineda-Torres, and Myers (2024).

states. Let i, $s = s(i)$, and t index households, states, and months of sample, respectively, where t refers to a month-year combination (e.g., April 2025 and April 2026 are indexed with a separate value of t). To address staggered implementation, we use a stacked difference-in-differences estimator inspired by Wing, Freedman, and Hollingsworth (2024) that is designed to estimate the average treatment effect on the treated in the presence of heterogeneous effects and staggered adoption (see also Sun and Abraham 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024). Specifically, we aggregate the 10 treatment states into 7 "treatment cohorts" that have the same implementation month and excluded categories, and we define each "cohort" a to be the treatment states with implementation date t_a^* as well as all control states.[11] This gives a set of treatment and control observations for each cohort. To construct our regression sample, we vertically concatenate ("stack") each cohort's observations. In this stacked dataset, treatment households will only appear once, but control households can appear once for every cohort.

Our key outcomes are different types of sugary drink purchases, and our pre-specified primary outcome is retail purchases of cohort a's excluded drink categories. Recall that the set of sweetened drinks excluded by treatment states varies by cohort (Table 1). We therefore normalize all consumption outcomes q_{iat} by the 2025 average for treated households in cohort a, since we would not expect broader and narrower sets of restricted drinks to have the same *level* impact on ounces of drinks consumed. Since control households appear multiple times (once for every cohort a), their drink consumption is defined and normalized differently by cohort.

For each cohort a, we analyze SNAP households for the 12 months before t_a^* and up to six months after t_a^*. SNAP households are defined as households in treatment or control states for which $S_{it} = 1$ in the implementation month t_a^* . Of course, being on SNAP is not a static characteristic of households; as is common with means-tested transfer programs, households may enroll or disenroll. Homonoff and Somerville (2021) document that for SNAP, disenrollment is common; it may sometimes reflect a change in the household's SNAP eligibility (due, e.g., to changes in their income), but the transaction costs required to periodically re-certify eligibility also play an important role. For expositional clarity, we first describe our empirical strategy under the (counterfactual) assumption that SNAP households are continuously enrolled in SNAP and then describe the adjustments we make to account for disenrollment.

Sample. We define a *"SNAP household"* as one who is on SNAP (i.e., $S_{it} = 1$) in the month of implementation of the restrictions (i.e., $t = t_a^*$). We define a *"non-SNAP household"* as one that has no EBT receipts in any of the 18 months in which they are included in the analysis (i.e., the 12 months prior to and up to 6 months after $t = t_a^*$). We exclude the remaining households.

11. For example, the five states that implemented SNAP restrictions in January 2026 aggregate to three treatment cohorts: (i) Nebraska and West Virginia (which exclude regular and diet soda and energy drinks), (ii) Indiana and Iowa (which exclude regular and diet soda, sports drinks, energy drinks, and sweetened tea), and (iii) Utah (which excludes only regular and diet soda).

Estimation assuming static SNAP enrollment. If enrollment in SNAP were constant within households over time, we would estimate the following stacked difference-in-differences analysis:

$$q_{iat} = \tau P_{st} + \lambda_{ia} + \lambda_{at} + \varepsilon_{iat}, \tag{1}$$

where P_{st} is an indicator for whether a SNAP restriction is in place as of month t for treatment states s in cohort a, and λ_{ia} and λ_{at} are (respectively) household $\times$ cohort and cohort $\times$ month-of-sample fixed effects. The key coefficient of interest τ gives the weighted average (over cohorts) treatment effect over all post-treatment month effects in the data. The identifying assumptions behind interpreting τ as the causal effect of the SNAP restriction on the outcome include the standard "parallel trends" assumption that the outcome would have evolved similarly in treatment and control households had the restrictions not gone into effect in the treatment states. They also include the standard "stable unit treatment value assumption" (SUTVA) that the restrictions do not have spillover effects into control states (e.g., through media articles signaling health information).

We estimate all regressions using weighted least squares, interacting three weights: (i) the demographic sample weights, (ii) inverse probability weights that reweight each cohort's control households to have the same 2025 average consumption as the cohort's treatment households, and (iii) "Q-weights" as defined by Wing, Freedman, and Hollingsworth (2024), which ensure that each cohort's treatment and control groups receive equal weight.[12] In all regressions, we use robust standard errors, clustered by state.

Accounting for time-varying SNAP enrollment. As noted above, SNAP households are not necessarily continuously enrolled in SNAP. If changes within households over time in S_{it} were uncorrelated with the outcome q_{iat} and with treatment status T_s, we would simply inflate our estimates in the above equation by the average share of SNAP households who remain enrolled in SNAP in each post-treatment month. More specifically, we would replace P_{st} in equation (1) with $P_{st}S_{it}$, and estimate the instrumental variables (IV) equation:

$$q_{iat} = \tau P_{st} S_{it} + \lambda_{ia} + \lambda_{at} + \varepsilon_{iat}, \tag{2}$$

where we instrument for $P_{st}S_{it}$ with P_{st}. In other words, we instrument for the joint outcome of being on SNAP and having the policy in place with whether the policy is in place.

In practice, our analysis of the pre-treatment data from 2024 and the first half of 2025 suggested that SNAP disenrollment was more likely in (subsequent) treatment states than in control states, although the difference is not statistically distinguishable from zero; see Appendix C.8. We found that SNAP disenrollment is associated with sugary drink consumption changes, so failing to account for differential churn in treatment and control states could bias us toward finding that the restrictions

12. Define N_a^T as the number of households in the treatment group in cohort a states, and define $N^T := \sum_a N_a^T$ as the total number of treated households. Define $N^C := \sum_a N_a^C$ as the number of households in the combined control groups. (This summation intentionally double-counts individual households that appear in control groups for multiple cohorts.) The Q-weight equals 1 for the cohort's treatment households, and $\frac{N_a^T/N^T}{N_a^C/N^C}$ for the cohort's controls.

reduce consumption of the excluded drinks.[13] We therefore supplement the above IV equations with direct controls for enrollment S_{it}.

Our primary difference-in-differences IV specification is therefore

$$q_{iat} = \tau P_{st} S_{it} + \sigma S_{it} + \lambda_{ia} + \lambda_{at} + \varepsilon_{iat}, \tag{3}$$

where we instrument for $P_{st}S_{it}$ with P_{st}. In practice, we show below that the results are not sensitive to the inclusion of this additional S_{it} control. Relatedly, in our analysis period of 2025 through the first half of 2026, we continue to find that disenrollment by SNAP households was more frequent in treatment states than in control states, but this difference remains statistically indistinguishable from zero and from the difference in the year before implementation; see Appendix C.8. The lack of significant differential disenrollment across treatment and control states also helps alleviate concerns about potential policy confounds to our analysis from the July 2025 One Big Beautiful Bill Act (U.S. Congress 2025), which included several changes to SNAP that may affect enrollment and disenrollment.[14]

Finally, while equation (3) represents our primary specification, we start by estimating a more flexible, stacked event study version of this difference-in-differences IV equation in which we estimate separate coefficients for each month m relative to the implementation month t_a^* (defined as $m = 0$). Defining $T_s \in \{0, 1\}$ as an indicator for whether state s is a treatment state, we estimate:

$$q_{iat} = \sum_{m \in \mathcal{M}} \tau_m T_s \cdot \mathbb{1}[t - t_a^* = m] S_{it} + \sigma S_{it} + \lambda_{ia} + \lambda_{at} + \varepsilon_{iat}, \tag{4}$$

where $\mathcal{M} := \{-12, ..., -2, 0, ..., M\}$ is the set of possible months in the event study, excluding $m = -1$ as the omitted category and we instrument for $T_s \cdot \mathbb{1}[t - t_a^* = m] S_{it}$ with $T_s \cdot \mathbb{1}[t - t_a^* = m]$. The key coefficients of interest are the τ_m coefficients, which give the weighted average treatment effect (across cohorts) of the restrictions in month m.

4.2 Results

Consumption trends: treatment vs. control states. Figure 2 presents average consumption of excluded drinks in event time for baseline SNAP households in treatment states and their respective stacked controls. For visual clarity, we normalize each cohort's consumption to its treatment group's 2025 average; in practice, 2025 average excluded drink consumption was about 3 percent

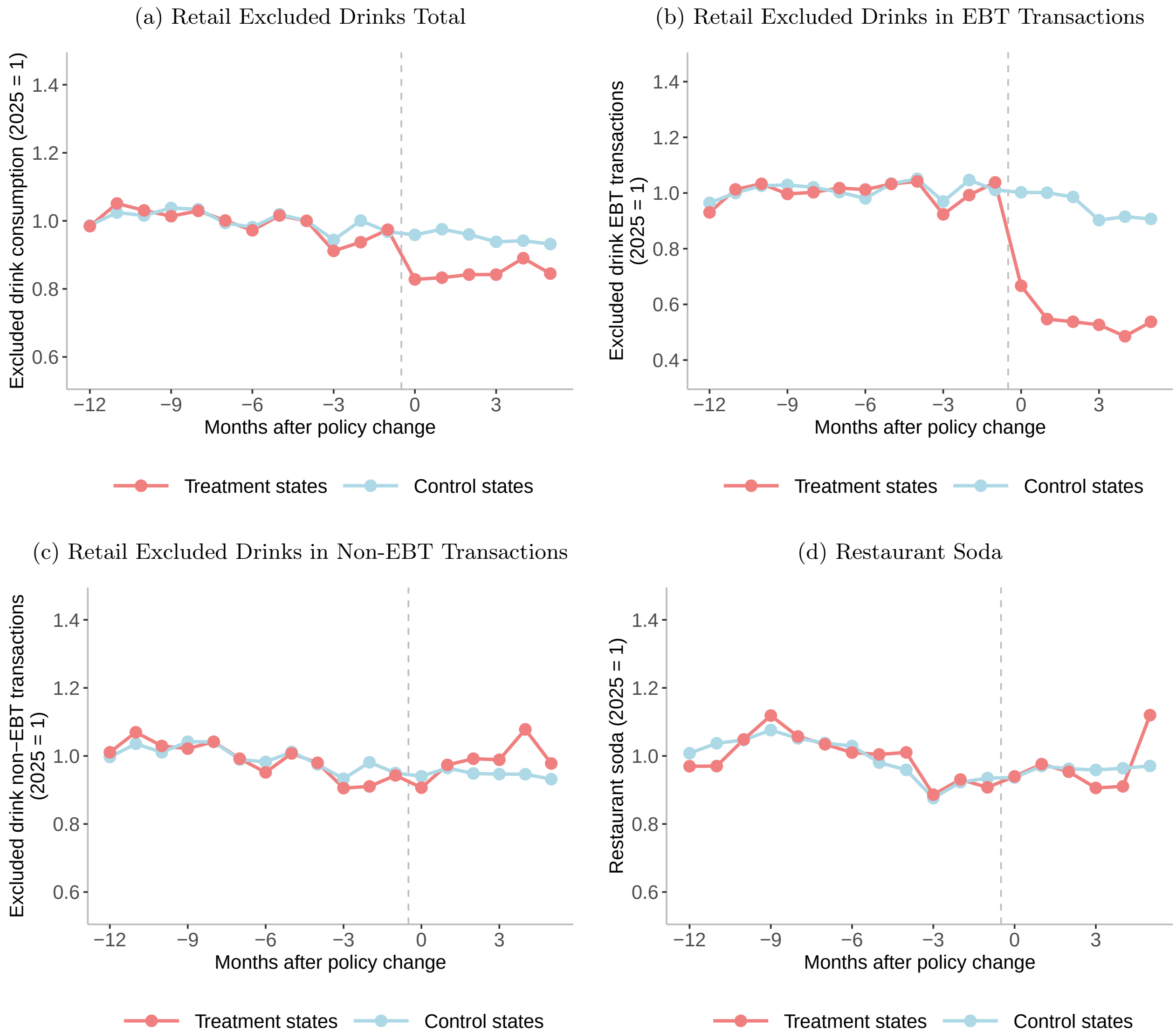
13. This would be the case if disenrollment from SNAP reduces consumption of retail sweetened drinks, either because of a causal effect of SNAP disenrollment or the effect of an omitted factor that affects both disenrollment and drink consumption.

14. Changes as part of OBBBA included expanding work requirements for certain SNAP recipients (most notably, adults ages 55-64 are no longer exempt from work requirements, nor are parents of children ages 14-18), increasing states' share of administrative costs, and establishing penalties for SNAP payment error rates above certain thresholds. Although in principle, as federal changes, they should not affect our staggered roll-out analysis, in practice, implementation timing and efforts have not been uniform across states. States implemented work requirements between late 2025 and mid-2026, with California being the final state to implement in June 2026 (Lai 2025; Ballotpedia News 2026). Cost-sharing and penalties for error rates are slated to begin in October 2027 (U.S. Congress 2025), although these could also be delayed.

higher in treatment states than in control states (Table 2, panel (c)). Panel (a) shows trends in our primary outcome: total retail excluded drink consumption. It shows that consumption is trending similarly in treatment and control states prior to the implementation of the restriction, but then drops sharply in the treatment states relative to the control states. Panels (b) and (c) show these same trends separately for, respectively, retail excluded drink consumption in EBT transactions and in non-EBT transactions. Recall that at least some purchases in EBT transactions are paid for with SNAP (although some may not be), while no purchases on non-EBT transactions are paid for with SNAP. As expected, the decline in retail purchases of excluded drinks is limited to EBT transactions (panel (b)).[15] Finally, panel (d) shows trends in restaurant soda purchases in treatment relative to control states. If individuals reduce their purchases of retail soda in response to the SNAP restrictions, we might expect to see substitution toward restaurant soda (which could never be paid for with SNAP). Panel (d) shows no evidence of such substitution, although, as noted in the discussion of Table 2 above, restaurant soda covers only a fraction of potential excluded drinks consumed away from home (and therefore not covered by SNAP).

15. Panel (c) shows that there is a slight increase in treatment state retail excluded drink purchases in non-EBT transactions in month 4. This also manifests in the panel (a) total and will be visible in Figure 3 below. Our additional investigation shows that this increase appears in multiple states and is not caused by any extreme outlier.

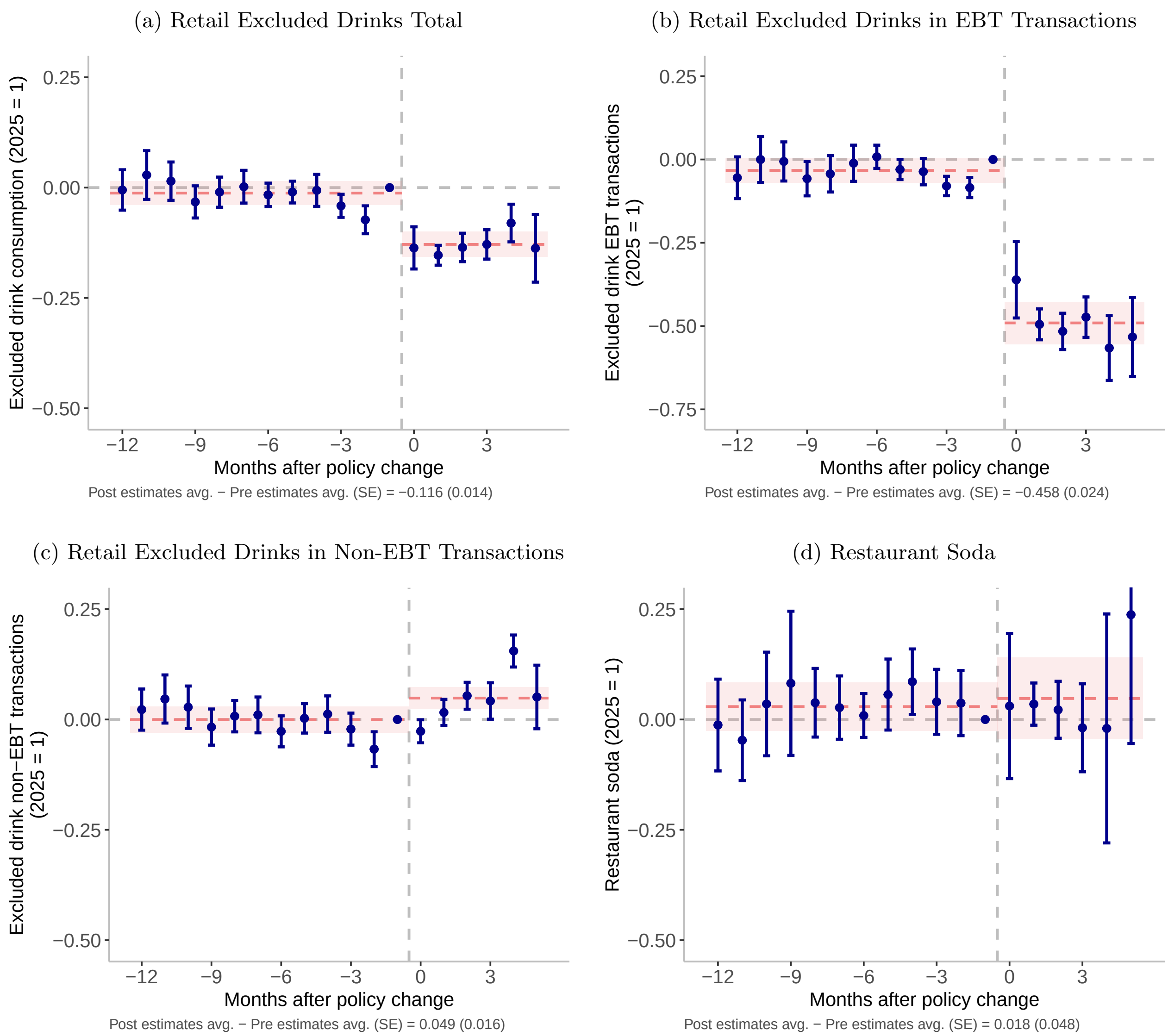
Figure 2: Treatment and Control Trends in Excluded Drink Consumption



Notes: This figure presents average excluded drink consumption in event time among SNAP households in treatment states ($N = 3{,}291$) and their respective stacked controls ($N = 11{,}970$). Each cohort's consumption is normalized by its treatment group's 2025 average. We then stack each cohort's data and estimate the average normalized consumption level by month relative to policy across cohorts, weighting each cohort's controls to match its number of treated households.

Consumption estimates. Figure 3 presents the event study analog of Figure 2 based on estimating the flexible event study equation (4), and Table 3 presents the difference-in-differences results based on estimating equation (3). The results indicate that, on average, the SNAP sweetened drink restrictions caused a 12.4 percent (standard error = 1.0) decline relative to the 2025 average level of retail purchases of excluded drinks in treatment states of 8.9 fluid ounces per person-day. This implies that the restrictions caused a decline in consumption of excluded drinks of 1.11 fluid ounces per person-day.[16] The consumption decline appears immediately following implementation, consistent with enforcement starting immediately, and is similar in magnitude across our six-month period, suggesting little phase-in or adjustment dynamics at least within our time horizon.

Figure 3: Event Study: Treatment Relative to Control Trends in Excluded Drink Consumption



Notes: This figure presents event study estimates using equation (4). The blue lines from each point estimate indicate 95 percent confidence intervals (based on clustering on state). The red dashed lines are averages of the pre- and post-implementation coefficients. The pink shaded areas are 95 percent confidence intervals for the pre and the post-period estimates, calculated using the delta method. The number of unique households is $N = 15{,}261$.

16. The 2025 mean for retail excluded drinks is lower than the 2025 mean for retail sweetened drinks in Table 2 because it only measures excluded drinks (see Table 1).

We examine the composition of the decline in retail purchases of excluded drinks. It reflects a 43.6 percent (standard error = 2.0) decline in retail purchases of excluded drinks in EBT transactions (column 2), with no evidence of declines in non-EBT transactions; for non-EBT transactions, the estimate (column 3) is 2.7 percent (standard error = 1.2). All of the excluded drink categories experienced statistically significant declines in retail purchases (Appendix Table A14), with magnitudes ranging from a 21.3 percent (standard error = 2.0) decline in sports drinks to an 8.9 percent (standard error = 1.6) decline in diet soda. Purchases of regular soda declined by 12.5 percent (standard error = 1.4).

Table 3: Effects of SNAP Restrictions

	Retail excluded drinks total (1)	Retail excluded drinks in EBT transactions (2)	Retail excluded drinks in non-EBT transactions (3)	Restaurant soda (4)
Treatment effect ($\hat{\tau}$)	-0.124	-0.436	0.027	0.003
	(0.010)	(0.020)	(0.012)	(0.036)
2025 treatment average	8.91	2.90	6.01	0.33
(fl oz/person-day)	(0.78)	(0.23)	(0.56)	(0.05)
N	15,261	15,261	15,261	15,261

Notes: This table presents estimates of the effects of SNAP restrictions on excluded drink consumption, estimated using equation (3). The consumption outcome variables are normalized by the 2025 average outcome level for treated households in each cohort. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Substitution. From a health perspective, a key question is whether excluding some sugary drinks from SNAP increases consumption of sugar in other forms. One natural substitution could be to sugary drinks purchased at away-from-home channels (e.g., restaurants and vending machines), which were never eligible for SNAP. Column (4) of Table 3 shows no evidence of substitution to soda purchased at limited-service restaurants: the point estimate is a statistically insignificant 0.3 percent (standard error = 3.6) increase, and the 95 percent confidence bound rejects an increase of more than 7.3 percent. As described in Section 3.1, the Numerator Panel Data probably capture only a fraction of total away-from-home sugary drink consumption. However, if we assume that limited-service restaurant purchases are representative of all away-from-home consumption, since away-from-home is 22 percent of total sugary drink consumption (Lin and Zhang 2025), our confidence bound would reject an away-from-home increase that offsets more than $22\%/78\% \times 7.3/12.4 \approx 17$ percent of the estimated at-home consumption decrease.

Another natural form of substitution could be to retail purchases of sugary drinks or sugary foods that are still included in SNAP. We test for such substitution by re-estimating equation (3) with calorie consumption per person-day in potential substitute categories as dependent variables, dropping from the analysis any treatment states that exclude the substitute category from SNAP.

The first row of Table 4 reports the estimates, with different columns examining substitution to different categories when they are not excluded. We then translate the estimated percentage effects on these substitute categories into units of calories per person-day by multiplying the estimates by the 2025 treatment group average calories per person-day within each column's sample. Finally, we construct calorie diversion ratios by dividing each column's substitution effect by the excluded drink calorie reduction in the states in each column's sample.

All treatment states exclude regular and diet soda, but they differ on whether and which additional forms of sugary drinks and sugary foods are excluded, allowing us to examine substitution to these categories when they are not excluded. For example, in column (2), we estimate that when fruit drinks (which include fruit juices) are not excluded from SNAP, SNAP restrictions increased fruit drink consumption by a statistically significant 5.9 percent (standard error = 1.18). Multiplying by the 2025 average gives an increase in fruit drinks of 1.69 calories per person-day. Within that column's sample, SNAP restrictions reduced excluded drink consumption by 4.64 calories per person-day. Thus, the diversion ratio is $1.69/4.64 \approx 36.3$ percent.

To examine the total diversion to other sugary drinks when they are not excluded, we consider not only diversion to fruit drinks (column 2) but also potential diversion to sports drinks, energy drinks, and sweetened tea (column 1). At baseline, however, these categories in column (1) represent only about 15 percent as many calories per day as fruit drinks, so that evidence of some diversion to them when they are not excluded (point estimate 3.2 percent, standard error = 1.9) does not have much quantitative impact. Adding the diversion ratios in columns (1) and (2) suggests that when SNAP restrictions exclude only soda, consumers substitute about 39 percent of the excluded drink calorie reduction to other sugary drinks and fruit juices that are still eligible for SNAP. This suggests a potentially important message for policy designers: when states define their SNAP restrictions to exclude only some sugary drink categories, the effects are partially undone by substitution to non-excluded categories.

Columns (3) and (4) test for substitution to sugary foods. Column (3) shows no statistically significant substitution to candy when not excluded. Column (4) shows a marginally significant *decrease* in sugary snacks when not excluded, which suggests that these are complements; we suspect that this result is spurious. The diversion ratio point estimates for candy and sugary snacks are opposite-signed and about equal in magnitude. Together, the two columns provide no statistical evidence for substitution to sugary foods. This is consistent with the Allcott, Lockwood, and Taubinsky (2019a) finding of limited substitution from sugary drinks to sugary foods in response to sugary drink price increases. In column (5), the large standard error on the diversion ratio shows that we do not have the power to detect whether SNAP restrictions affect overall grocery calorie consumption.

Table 4: Substitution Effects of SNAP Restrictions on Non-Excluded Retail Categories

	Sports/energy drinks & sweetened tea when not excluded (1)	Fruit drinks when not excluded (2)	Candy when not excluded (3)	Sugary snacks when not excluded (4)	All groceries (5)
Treatment effect ($\hat{\tau}$)	0.032	0.059	0.019	-0.028	-0.005
	(0.019)	(0.012)	(0.016)	(0.015)	(0.006)
2025 treatment average (calories/person-day)	4.36	28.38	71.32	108.85	934.98
	(0.34)	(1.22)	(2.59)	(7.24)	(59.04)
Treatment effect (calories/person-day)	0.14	1.69	1.33	-3.02	-4.58
	(0.08)	(0.34)	(1.15)	(1.68)	(5.99)
Sample excluded drink reduction (calories/person-day)	4.64	4.64	3.30	6.33	5.63
	(0.64)	(0.64)	(1.05)	(0.83)	(0.66)
Diversion ratio	0.030	0.363	0.402	-0.477	-0.813
	(0.018)	(0.089)	(0.371)	(0.272)	(1.067)
N	13,610	13,610	11,432	14,147	15,261

Notes: This table presents estimates of the effects of SNAP restrictions on retail consumption of non-excluded food and drink categories, estimated using equation (3). In each column, treatment states that exclude the respective category from SNAP are dropped from the regression, so that estimates reflect substitution to the respective category when it is not excluded. "Sports/energy & sweetened tea when not excluded" (column 1) refers to any of those outcomes that are not excluded from SNAP in the five states that do not exclude at least one of those items from SNAP (see Table 1). "Sugary snacks" (column 4) include packaged portable sweet snacks, yogurt covered snacks, dessert snacks, bakery desserts, ice cream & frozen desserts, toaster pastries, packaged cookies, glazed/coated nuts and fruit. The first row presents the estimated (percentage) treatment effects, while the second row presents the 2025 average calories per person-day in treatment states. Multiplying the second row by the first row gives the estimated treatment effect in calories per person-day (row 3). To calculate (in the final row) the diversion ratio (i.e., the share of calories per person-day that consumers substitute to), we calculate the ratio of the treatment effect on substitute good calories per person-day (row 3) to the treatment effect on sample excluded drink calories per person-day (row 4). The treatment effect on sample excluded drink reduction (row 4) is calculated by estimating equation (3) in the same sample, with the outcome in calories per person-day, and then multiplying by (-1) times the 2025 treatment average excluded drink calories per person-day. Diversion ratio standard errors are computed using the delta method. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Subgroup analysis. Table 5 reports potential heterogeneity in the effects of SNAP restrictions on our primary outcome (total retail excluded drink consumption) across seven complementary pairs of household subgroups, as specified in our pre-analysis plan.[17] Across many of these pairs—including baseline consumption levels of excluded drinks, whether individuals are non-diabetic, and whether the respondent believes they drink soft drinks more often than they should—we find limited evidence of treatment effect heterogeneity. The fact that we estimate similar proportional effects for individuals with different levels of baseline consumption of the excluded drinks will be relevant for the welfare analysis in Section 6, where implications depend on the reduction in the *level* of

17. Appendix Figure A17 shows the overlap across the subgroups. Several of these groupings are based on answers to our baseline survey and are therefore only available for households that responded to the survey; variable definitions for these survey-based subgroup analyses are given in Appendix D.2.

consumption of sugary drinks implied by constant proportional reductions across households with different baseline levels. We find suggestive evidence of stronger effects among households that perceive little or no health risk from consuming soft drinks (Panel (a) columns (5) and (6)).

We find much stronger effects among households with a higher SNAP share of their total spending on groceries or on excluded drinks (panel (b) columns (1) through (4)). This heterogeneity by SNAP share of spending is consistent with a mental accounting model, where the effect of SNAP restrictions on consumption scales with the SNAP benefit level. Intuitively, in a model of mental accounting, SNAP restrictions should not be expected to have much effect on households with low SNAP benefit levels; we present and discuss such a model in more detail in Section 6.1. While we do not directly observe SNAP benefit levels in the Numerator Panel Data, we do observe average monthly spending on EBT transactions, which is much higher for those with an above- versus below-median share of grocery spending on EBT ($305 vs. $158 over our 18-month analysis period); this implies correspondingly higher SNAP benefits, since households' SNAP benefits are largely spent in their entirety each month, often within the first two weeks after benefits receipt (Castner et al. 2025).

Table 5: Effects of SNAP Restrictions on Total Retail Excluded Drinks, by Subgroups

Panel (a): Market Failure Proxies

	Baseline quantity		Drink more than should		Perceived health risk		Diabetes	
	Above median	Below median	Yes	No	Very little or none	Somewhat to great deal	Diabetic or pre-diabetic	Non-diabetic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect ($\hat{\tau}$)	-0.112	-0.158	-0.130	-0.106	-0.164	-0.098	-0.117	-0.116
	(0.011)	(0.034)	(0.014)	(0.016)	(0.026)	(0.023)	(0.022)	(0.017)
p-value (difference)	0.195		0.278		0.057		0.986	
2025 treatment average	15.41	2.99	11.95	7.16	11.47	9.40	10.20	9.10
(fl oz/person-day)	(0.88)	(0.37)	(0.85)	(0.59)	(0.78)	(0.85)	(0.92)	(0.80)
N	8,582	8,483	3,225	2,326	1,721	3,830	3,107	7,888

Panel (b): Mental Accounting Proxies

	Baseline share groceries on EBT		Baseline share excluded drinks on EBT		MPC out of SNAP and cash	
	Above median	Below median	Above median	Below median	Higher MPC out of SNAP	Same/higher out of cash
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect ($\hat{\tau}$)	-0.173	-0.070	-0.181	-0.074	-0.100	-0.133
	(0.016)	(0.014)	(0.018)	(0.012)	(0.027)	(0.012)
p-value (difference)	$<$0.001		$<$0.001		0.262	
2025 treatment average	8.37	9.49	7.71	10.28	10.17	10.04
(fl oz/person-day)	(0.78)	(0.81)	(0.54)	(1.05)	(0.97)	(0.75)
N	7,631	7,630	7,509	8,363	1,841	3,710

Notes: This table reports the effects of SNAP restrictions on total retail excluded drink consumption for complementary pairs of household subgroups, estimated using equation (3). Panel (a) presents subgroups capturing heterogeneity by proxies for the magnitude of market failures. Columns (1) and (2) split the sample by above- versus below-median excluded drink consumption in 2025, where the median is computed within the unweighted sample. Columns (3) and (4) split the sample based on responses to our baseline survey question about whether the respondent drinks soft drinks "more often than I should." Columns (5) and (6) split the sample based on responses to our baseline survey question about how much drinking soft drinks "every day would increase your risk of health problems like type 2 diabetes or obesity." Columns (7) and (8) split the sample based on householder's self-reported diabetes status in Numerator's opt-in attributes survey data (see Appendix C.1). Panel (b) presents subgroups capturing heterogeneity suggestive of a mental accounting mechanism. Columns (1) and (2) split the sample by above- versus below-median share of groceries purchased in EBT transactions. Columns (3) and (4) split the sample by above- versus below-median share of excluded drinks purchased in EBT transactions. Columns (5) and (6) split the sample based on responses to baseline survey questions about how much more money they would spend on food if they received an additional \$100 in SNAP benefits or an additional \$100 in cash. Appendix D.2 provides more detail on each of the survey questions and how we coded the responses into categories. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample; note that in the baseline quantity and the baseline share excluded drinks on EBT sample splits, some control households can contribute to both above- and below-median samples.

Sensitivity analysis. We examine the sensitivity of our estimated impact on total retail purchases of excluded drinks to a number of alternative specifications. Table 6 summarizes the results, with column (1) reproducing our baseline specification from Table 3, column (1). Overall, across these alternative specifications the estimates fall into a fairly tight range around our baseline estimate of a 12.4 percent decline (standard error = 1.0); specifically, they range from a 10.4 percent (standard error = 1.1) decline to a 13.6 percent (standard error = 1.8) decline.

To account for the fact that our SNAP households can disenroll from SNAP, our primary specification is an IV strategy in which we instrument for whether the household is on SNAP in a treatment state post-treatment with whether the household is in a treatment state post-treatment, and also control for whether the household-month is on SNAP (S_{it}) (see equation (3)). The first stage is 0.92 (standard error = 0.006) (Appendix Table A13), indicating that, on average, 92 percent of households in treatment states remain enrolled in SNAP over the post-period. An important potential concern with this approach is that the control variable S_{it} may be endogenous to the SNAP restrictions. For example, SNAP restrictions might reduce the probability that treatment households are enrolled in SNAP if the restrictions increase the stigma from enrollment, or otherwise make enrollment in SNAP less appealing. In practice, Appendix C.8 explores this and finds no evidence of endogenous disenrollment. Disenrollment rates are slightly higher for SNAP households in treatment states relative to control states in the 2025-2026 analysis period, but the difference is not statistically distinguishable from zero, or from the higher disenrollment rates in treatment states relative to control states in the 2024-2025 pre-treatment period. In addition, column (2) shows that our estimate is very similar if we drop the control for S_{it}. Likewise, column (3) shows that, as expected given the approximate 8 percent decline in SNAP enrollment in the post period, an intent-to-treat estimate of equation (3) is slightly smaller (-11.5 percent) than our baseline estimate. Column (4) shows that the results are slightly larger (-12.7 percent) if we estimate a Poisson model of the intent-to-treat, compared to our baseline approach of scaling the dependent variable by the 2025 mean in the treatment states in that cohort.

The remainder of Table 6 shows that the baseline estimate is robust to alternative IV specifications. Column (5) shows similar results if we use retail spending on excluded drinks rather than imputed quantities as our outcome. Columns (6) and (7) show that excluding either the inverse probability weights or the sample weights also produces very similar (or slightly larger) results, as does estimating a standard two-way fixed effects estimator without stacking cohorts (column 8). In column (9) we report a triple-difference specification that uses non-SNAP households as an additional control. This produces a slightly smaller effect than our primary specification (10.4 percent decline with standard error = 1.1). Finally, since, as described in Section 3.1, our approach to measuring SNAP enrollment (S_{it}) inevitably contains measurement error, in Appendix C.4 we show that our estimates are robust to alternative ways of defining S_{it} (see Appendix Table A15).

Table 6: Alternative Specifications: Effects of SNAP Restrictions

	Primary specification	Drop S_{it}	Primary ITT	Poisson ITT	Primary (\$/person-day)	Drop IPWs	Drop sample weights	TWFE with IV	Triple differences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment effect ($\hat{\tau}$)	-0.124	-0.126	-0.115	-0.127	-0.129	-0.127	-0.134	-0.136	-0.104
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.011)	(0.018)	(0.011)
2025 treatment average	8.91	8.91	8.91	8.91	0.50	8.91	9.57	8.83	8.91
(fl oz/person-day)	(0.78)	(0.78)	(0.78)	(0.78)	(0.05)	(0.78)	(0.85)	(0.75)	(0.78)
N	15,261	15,261	15,261	15,261	15,261	15,261	15,261	14,918	103,041

Notes: This table presents robustness checks for the effects of SNAP restrictions on total retail excluded drink purchases. Column (1) restates our primary specification, estimated using equation (3). Column (2) reports a version of equation (3) where we drop the S_{it} control. Column (3) reports the intent-to-treat version of equation (3), which substitutes P_{at} for $P_{at}S_{it}$ and does not instrument. Column (4) reports a Poisson estimate of the intent-to-treat effect. Column (5) reports a version of equation (3) with q_{iat} defined as excluded drink spending (in \$/person-day) instead of quantity purchased; the 2025 average reported in that column is in units of \$/person-day. Column (6) reports a version of equation (3) estimated without inverse probability weights. Column (7) reports a version of equation (3) estimated without sample weights. Column (8) reports a standard two-way fixed effects estimator that parallels equation (3) without stacking the cohorts. Column (9) reports a triple-difference version of equation (3) that uses non-SNAP households as a separate control group. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

5 Survey Evidence

We supplement our analysis of the impact of the SNAP restrictions on consumption with information from an original survey of a subsample of Numerator households. This provides additional evidence on both the public's views on these policies and the impact of the policies on the perceptions and beliefs of SNAP recipients.

The survey sample is about 30 percent of the full sample used in the consumption analysis. Appendix Table A16 assesses the representativeness of this subsample. It shows that the survey sample is similar to the full sample in terms of average baseline sugary drink consumption, EBT spending, and demographics. In addition, the estimated treatment effect of the SNAP restrictions on retail excluded drink consumption within this subsample is very similar to that in the full sample. We thus conclude that the survey results likely generalize to the full sample used in the consumption analysis, which is designed to be representative of the full SNAP population.

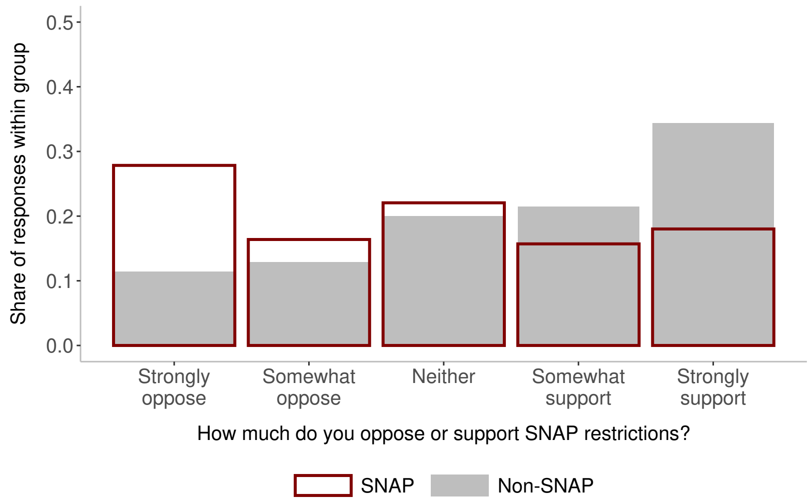
5.1 Descriptive Evidence on Public Opinion and Perceptions

As described above, the SNAP restrictions are controversial. We therefore investigated public opinion about whether and why the restrictions should be adopted. Specifically, the survey asked "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?"

Policy support is strikingly different between SNAP recipients and non-recipients: SNAP recipients were much less likely to support restrictions on their purchases. Specifically, Figure 4 shows that while 34 percent of non-SNAP recipients reported that they "strongly support" the restrictions, only 18 percent of SNAP recipients did. And while 28 percent of SNAP recipients reported that they "strongly oppose" the restrictions, only 11 percent of non-SNAP recipients did.[18] Our results echo other survey evidence from Liscow and Pershing (2022) indicating that while Americans overall strongly prefer in-kind transfers rather than cash, lower-income people (who are more likely to receive transfers) tend to prefer cash.

18. We also surveyed economic and health experts about their policy preferences and reasoning, using the exact same set of questions (see Appendix A). Experts' views were closely aligned with the general public. Like non-SNAP recipients, some experts opposed SNAP restrictions, but the majority supported them. In particular, only 5 percent reported that they strongly oppose the restrictions, while 57 percent reported that they somewhat or strongly support them.

Figure 4: July 2026 Public Opinion about SNAP Restrictions



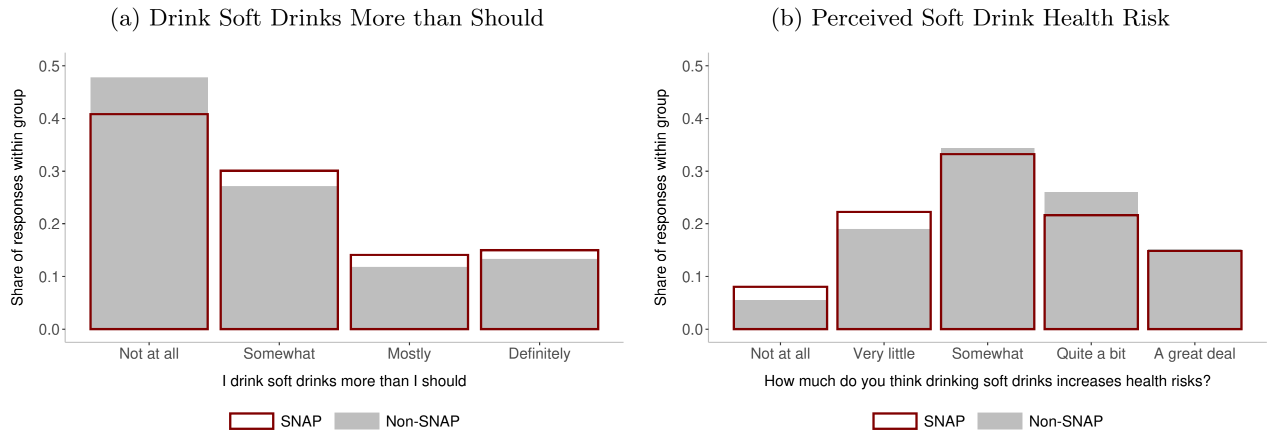
Notes: Our endline survey asked, "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?" This figure presents the distribution of responses from the endline survey. Households are classified as "SNAP" (N = 4,425) versus "Non-SNAP" (N = 4,340) based on whether they reported receiving SNAP benefits in the past three months.

Conditional on support versus opposition, SNAP and non-SNAP recipients have similar reasoning for their views (Figure A19). Among people who opposed restrictions, the most popular listed reason was libertarian ("the government should let people make their own decisions") followed by concerns related to stigma (restrictions "could make recipients feel judged"). Among people who supported restrictions, the four listed reasons (related to paternalism, health care costs, stewardship of government funds, and information signaling) were roughly equally popular. Liscow and Pershing (2022) also find that among those who support in-kind transfers, paternalism is often cited as a rationale for this policy preference.

In line with a paternalistic rationale for SNAP restrictions, the survey results are consistent with the presence of two types of internalities for SNAP recipients: self-control problems and limited information about the health risks of soda. Specifically, Figure 5 shows that about 60 percent of SNAP recipients in our baseline survey reported that they drink soft drinks somewhat, mostly, or definitely more often than they should, while about 30 percent report that drinking soft drinks poses no or very little health risk.[19]

19. We present the response distributions for perceived health risk and self-control questions from the baseline survey because the self-control question was presented only at baseline, as documented in Appendix D.4.2.

Figure 5: Survey Results Related to Soft Drink Over-Consumption Internalities



Notes: This figure presents the distribution of responses for the self-control question and the perceived soft drink health risk question from our baseline survey. For the self-control question, our baseline survey asked, "Please indicate how much the next statement reflects how you typically are: I drink soft drinks, soda, or pop more often than I should." For the perceived health risk question, our baseline survey asked, "How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?" Households are classified as "SNAP" (N = 8,880) versus "Non-SNAP" (N = 8,349) based on whether they reported receiving SNAP benefits in the past three months.

5.2 Effects on SNAP Recipients' Perceptions

To examine the impact of the SNAP restrictions on the perceptions of SNAP recipients, we combine our baseline and endline surveys in a simple difference-in-differences design. This allows us to examine changes in survey outcomes among SNAP recipients in states with restrictions, after the restrictions take effect. Specifically, we estimate:

$$y_{it} = \tau P_{st} + \lambda_i + \lambda_t + \varepsilon_{it}. \tag{5}$$

where λ_i and λ_t are household and survey period fixed effects, respectively, and P_{st} is an indicator that takes the value of 1 for the endline period in the 10 states that had implemented SNAP restrictions in January–June 2026.[20] We limit the sample to households that reported on the baseline survey that they had received SNAP benefits in the past three months. We weight observations by their sample weights and cluster standard errors by state. The key coefficient τ represents the causal effect of the restrictions on the perceptions of SNAP recipients.

Table 7 presents the results. All outcomes are standardized by the baseline period mean and standard deviation.[21] We find no statistically significant impacts of the SNAP restrictions on SNAP recipients' support for the policy (column 1) or on their perception of the health risks of consuming soft drinks (column 2).

20. We exclude Arkansas from the analysis because our endline survey was fielded in mid-July 2026 and restrictions in that state only took effect on July 1.

21. Appendix Table A18 presents the same estimates with the outcomes in the original ordinal 1-5 scales.

Stigma. The remainder of Table 7 shows estimated impacts on SNAP recipients' perception of stigma. One of the major concerns of policy opponents of restrictions was that they would stigmatize recipients (Schanzenbach 2013; Bosso 2024; Janssen 2025; Plata-Nino 2025; Washington 2026). This concern is echoed by the academic literature exploring the ethics of SNAP restrictions (Barnhill 2011; Gundersen 2020; Krobath et al. 2025).

Concerns about increased stigma for SNAP recipients centered on two related but distinct forms of stigma: stigma while shopping with SNAP benefits and stigma from being on SNAP. The first, a form of "treatment" stigma (Stuber and Schlesinger 2006), concerns SNAP recipients feeling judged when they shop using SNAP benefits. The second, a form of "identity stigma" (Stuber and Schlesinger 2006), concerns SNAP recipients feeling disrespected and/or restricted by SNAP policies, affecting their sense of identity as a person on SNAP. Indeed, when we asked about reasons for opposing the SNAP restrictions (among those who did), the most common two reasons (Figure A19, panel (b)) were that we should "let people make their own decisions" (related to identity stigma) and that SNAP restrictions "could make recipients feel judged" (related to treatment stigma).

We asked survey questions designed to measure both types of stigma at baseline before any policies were introduced (December 2025) and at endline (July 2026). Appendix Figure A18 presents the underlying distribution of responses at baseline. We measured treatment stigma by SNAP recipient responses to the question "in the past 3 months, how often did you feel judged when paying for groceries with SNAP benefits;" respondents were given five options from "never" to "most or all of the time," which were coded 1 to 5, respectively. At baseline, the average response among SNAP recipients in the treatment states was 2.19 (where 2 corresponded to "rarely"). We measured identity stigma by SNAP recipient responses to the statement "the SNAP policies currently in place in my state are disrespectful toward people on SNAP" and to the statement "the SNAP policies currently in place in my state take away personal freedom;" for each question they were asked to rate their agreement using five responses from "strongly disagree" to "strongly agree," which were coded 1 through 5 respectively. At baseline, the average response of SNAP recipients in the treatment states was 3 to each of these identity stigma questions (where 3 corresponded to "neither disagree nor agree").

The results in columns (3) through (5) indicate that the SNAP restrictions had a statistically significant impact on increasing both treatment stigma and identity stigma. The SNAP restrictions increased SNAP recipients' reports of "feeling judged" when paying for groceries with SNAP benefits by a statistically significant 0.10 standard deviations (standard error = 0.03), or about 0.13 points on the 1-5 scale (Appendix Table A18). The restrictions also increased SNAP recipients' feeling that SNAP policies in their state were disrespectful to people on SNAP (column 4) by 0.12 standard deviations (standard error = 0.05), and their feeling that SNAP policies took away their freedom (column 5) by 0.17 standard deviations (standard error = 0.05); these correspond to increases of about 0.16 to 0.22 points on the 1-5 scale (Appendix Table A18).

To measure whether a reminder of the restrictions affected reported perceptions, we randomly assigned survey respondents equally between versions that described the SNAP restrictions and asked about support at the beginning of the survey versus at the end, as described in Appendix D.4. Appendix Table A19 shows that, as would be expected, this did not substantially change the treatment effects on feeling judged when paying for groceries (which is a retrospective evaluation that should not be affected by a reminder of the policy), or on perceived health risk or support for restrictions (which both had insignificant overall effects). However, this reminder about the SNAP restrictions roughly doubled the estimated treatment effect on the share of SNAP recipients saying that the policies in their state were disrespectful or took away freedom. This provides suggestive evidence that the changes in identity stigma were caused by the restrictions.

Table 7: Effects on Perceived Health Risk, Stigma, Agency, and Policy Support

	Policy support (1)	Perceived health risk (2)	Felt judged (3)	Policies disrespectful (4)	Policies take away freedom (5)
Treatment effect ($\hat{\tau}$)	0.033	-0.030	0.101	0.123	0.169
	(0.033)	(0.039)	(0.028)	(0.053)	(0.052)
N	4,456	4,456	4,297	4,456	4,456

Notes: This table presents standardized effects of SNAP restrictions on perceived health risk, stigma, agency, and policy support, estimated using equation (5). The sample is limited to households that reported on the baseline survey that they had received SNAP benefits in the past three months. For column (1), the survey question was "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?" Response options were "Strongly oppose," "Somewhat oppose," "Neither oppose nor support," "Somewhat support," "Strongly support," which were coded as 1-5, respectively. For column (2), the survey question was "How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?" Response options were "Not at all," "Very little," "Somewhat," "Quite a bit," and "A great deal," which were coded as 1-5, respectively. For column (3), the survey question for SNAP households was "In the past 3 months, how often did you feel judged when paying for groceries with your SNAP benefits?" Response options were "Never," "Rarely," "Sometimes," "Often," "Most or all of the time," which were coded as 1-5, respectively. For column (4), the survey question was "The SNAP policies currently in place in my state are disrespectful toward people on SNAP." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. For column (5), the survey question was "The SNAP policies currently in place in my state take away personal freedom." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. Each of those survey outcomes is standardized by the baseline period mean and standard deviation. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

6 Modeled Effects on Consumer Welfare and Fiscal Externalities

The stated goal of SNAP sugary drink restrictions is not to reduce sugary drink consumption *per se* but rather to improve population health. In the presence of internalities and/or externalities, improving health by restricting choice can increase social welfare. With that motivation, this section uses our estimate of the consumption effect to calibrate models that predict social welfare gains. Section 6.1 models consumer welfare in the presence of mental accounting and a sugary drink consumption internality. Section 6.2 models effects on health and health care costs, which represent negative fiscal externalities because governments pay most of SNAP recipients' health care costs.[22] These analyses require a number of additional assumptions and external parameter estimates, some of which were estimated without quasi-experimental variation. These results are thus more speculative than those in our earlier sections.

We analyze two counterfactual scenarios, indexed $r = \{0, 1\}$. In the baseline scenario ($r = 0$), SNAP benefits can be spent on sugary drinks. In the restriction scenario ($r = 1$), all sugary drinks are excluded from SNAP nationwide, where "sugary drinks" is defined to include regular soda, sports drinks, energy drinks, sweetened teas, and fruit drinks that are not 100 percent juice, but not diet soda or 100 percent fruit juice.[23] Our empirical findings in Section 4 suggest that when policies exclude only a subset of sugary drinks, SNAP households partially substitute to non-excluded drinks, so that effects would likely be smaller. Our calibrations use the estimated effect on retail excluded drink purchases from column (1) of Table 3.

6.1 Consumer Welfare

6.1.1 Overview

Our consumer welfare model is inspired by Section 3.6 of Farhi and Gabaix (2015). Two distortions—mental accounting and sugary drink over-consumption internalities—drive a wedge between "normative utility," which we use for welfare analysis, and "decision utility," which determines consumer choice. The treatment effect of SNAP restrictions identifies the mental accounting magnitude, and we import the over-consumption internality magnitude from Allcott, Lockwood, and Taubinsky (2019a).

Mental accounting is consistent with some of the subgroup analyses in Table 5, as well as with Hastings and Shapiro (2018). However, mental accounting is not the only potential explanation for

22. About 85 percent of SNAP recipients are covered by Medicare and Medicaid, which are publicly financed. Most health care costs for the remaining 15 percent are also likely publicly financed: about half are uninsured (and therefore receive considerable publicly funded care), and the other half are privately insured through tax-subsidized programs (Kaiser Family Foundation 2025). We therefore assume that all health care spending of SNAP recipients represents a fiscal externality.

23. As summarized in Allcott, Lockwood, and Taubinsky (2019b), public health research suggests that the relevant health harms arise from consuming sugar via drinks. We evaluate a policy excluding all sugary drinks because we are not aware of any health reason to exclude only a subset. We do not model the exclusion of diet soda because our health risk and internality parameters do not apply to low- or no-calorie sweeteners. Although 100 percent fruit juice may harm health through similar channels, taxes and other policies targeting sugary drinks generally do not cover naturally occurring sugars, so our policy evaluation follows that convention.

the consumption effects. One potential alternative is that the restrictions signal that the government believes that sugary drinks are unhealthy. However, Table 7 shows no evidence that people in treatment states updated their health risk perceptions after the restrictions were implemented. A second alternative explanation is that SNAP benefits are not inframarginal for some households. We explore this in an alternative model described below. We also find similar welfare effects in a "sufficient statistic" model described below that does not specify any particular mechanism for the consumption effects of SNAP restrictions.

A sugary drink over-consumption internality due to self-control problems and imperfect information is consistent with SNAP participants' self-reports in Figure 5, as well as with the policy reasoning of some researchers (in Figure A4) and other Americans (in Figure A19). Moreover, Allcott, Lockwood, and Taubinsky (2019a) find that sugary drink consumption is conditionally associated with lower nutrition knowledge, and that many Americans say they drink more sugary drinks than they should. Temptation problems are also consistent with the results of Sadoff, Samek, and Sprenger (2020), who find that people buy healthier foods when ordering in advance. To the extent that our assumed internality is driven by present focus, our welfare analyses adopt a "long-run criterion," placing all normative weight on choices made in advance.

6.1.2 Model: Utility, Demand, and Welfare

We model three goods: sugary drinks s, other (SNAP-eligible) groceries g, and a numeraire n. Let $j \in \{s, g, n\}$ index goods. Consumers choose quantities q_j given prices p_j; we normalize $p_g = p_n = 1$. Income is w, and the SNAP benefit amount is b. Our primary analyses assume that the SNAP benefit is always inframarginal.

Consumers' true (or "normative") utility is Cobb-Douglas: $U = q_s^{\alpha_s} q_g^{\alpha_g} q_n^{\alpha_n}$, with $\sum_j \alpha_j = 1$, subject to the budget constraint $\sum_j q_j p_j = w + b$.[24] Consumers may not maximize normative utility, for two reasons. First, they may over-consume sugary drinks due to "internalities" such as self-control problems and/or imperfect information. We model this by assuming that consumers maximize "decision utility" with Cobb-Douglas parameters $\tilde{\alpha}_j$. Internalities cause sugary drink over-consumption if $\tilde{\alpha}_s > \alpha_s$. Second, they may over-consume SNAP-eligible goods due to mental accounting. Define $\mathcal{E}^r$ as the set of SNAP-eligible goods in scenario r; $\mathcal{E}^0 = \{s, g\}$ when sugary drinks are included, and $\mathcal{E}^1 = \{g\}$ when sugary drinks are excluded. Define $\mathcal{I}^r$ as the set of SNAP-ineligible goods. Following Farhi and Gabaix (2015), we model mental accounting by assuming that consumers follow a psychological "default expenditure" on SNAP-eligible items equal to the standard Cobb-Douglas expenditure share plus an additional weight $0 \le \beta \le 1$ on the SNAP benefit.

With these two biases, consumers maximize decision utility $\tilde{U} = q_s^{\tilde{\alpha}_s} q_g^{\tilde{\alpha}_g} q_n^{\tilde{\alpha}_n}$, with $\sum_j \tilde{\alpha}_j = 1$, subject to budget constraint $\sum_j q_j p_j = w + b$ and the mental accounting default expenditure constraint $\sum_{j \in \mathcal{E}^r} p_j q_j = (w + b) \sum_{j \in \mathcal{E}^r} \tilde{\alpha}_j + \beta b$. At an interior optimum, demand for goods eligible

24. Standard Cobb-Douglas utility generates constant expenditure shares and unit-elastic demand for each good. As discussed in Allcott, Lockwood, and Taubinsky (2019b), existing estimates of the elasticity of demand for sugary drinks are broadly in the range of -1, roughly consistent with the Cobb-Douglas assumption.

for SNAP is

$$q_j = \frac{\tilde{\alpha}_j}{p_j}\left(w + b + \frac{\beta b}{\sum_{k\in\mathcal{E}^r}\tilde{\alpha}_k}\right), \tag{6}$$

while demand for goods ineligible for SNAP is

$$q_j = \frac{\tilde{\alpha}_j}{p_j}\left(w + b - \frac{\beta b}{\sum_{k\in\mathcal{I}^r}\tilde{\alpha}_k}\right). \tag{7}$$

The difference between demand in equation (6) and equation (7) gives the effect of excluding sugary drinks from SNAP-eligible groceries:

$$\Delta q_s = -\frac{\tilde{\alpha}_s}{p_s}\beta b\left(\frac{1}{\tilde{\alpha}_s + \tilde{\alpha}_n} + \frac{1}{\tilde{\alpha}_s + \tilde{\alpha}_g}\right). \tag{8}$$

The consumption reduction is linear in $\tilde{\alpha}_s\beta b$. If there is zero consumption at baseline (implying $\tilde{\alpha}_s = 0$), no mental accounting ($\beta = 0$), or no SNAP benefit ($b = 0$), then excluding good s from SNAP does not affect consumption. For a given level of mental accounting β, the consumption reduction increases linearly in the benefit level b, which motivated the heterogeneity analysis in Table 5.

Inserting the demand functions into normative utility gives normative indirect utility $V^r(w)$, which varies by scenario r. We define equivalent variation (EV) as the payment that equalizes normative utility between the baseline scenario (with the payment) and restriction scenario:

$$V^1(w) = V^0(w + EV). \tag{9}$$

EV is our measure of consumer welfare.

6.1.3 Estimation

Our model simulates consumption and welfare for each SNAP household in our Numerator Panel Data in 2025. For each Numerator household, we construct household income per person w, sugary drink and other grocery expenditure shares, and sugary drink price. We predict each Numerator household's SNAP benefit per person b (conditional on income, household size, and state) from a regression in the SNAP Quality Control microdata.

We estimate a homogeneous mental accounting parameter β and recover each household's α_j and $\tilde{\alpha}_j$ parameters in two steps. First, we use indirect inference to find the β that generates a simulated average treatment effect equal to the primary estimate from Table 3, while finding the household-specific $\tilde{\alpha}_j$ that match each household's baseline expenditure shares implied by equations (6) and (7). Second, we infer each household's normative utility parameters α_j by approximating the Allcott, Lockwood, and Taubinsky (2019a) estimate that American households would consume about one-third fewer sugary drinks if they were unaffected by imperfect information and self-control problems. Appendix H.1 provides details on the data and estimation.

The estimated mental accounting parameter is $\hat{\beta} \approx 0.18$. This means that mental accounting diverts an additional 18 percent of the SNAP benefit to spending on eligible goods instead of ineligible goods. In the baseline scenario with sugary drinks eligible for SNAP ($r = 0$), the average household's marginal propensity to consume SNAP-eligible groceries out of SNAP benefits is 0.42. For comparison, Hastings and Shapiro (2018) estimate that the marginal propensity to consume groceries out of SNAP benefits is 0.5 to 0.6.

6.1.4 Simulation Results

Table 8 presents counterfactual simulation results. Column (1) presents the baseline scenario with no restrictions ($r = 0$), while column (2) presents the scenario with all sugary drinks excluded from SNAP ($r = 1$). The restrictions reduce sugary drink spending by $26 per person-year, of which consumers divert 65 percent ($17/$26) to other grocery spending and 35 percent to the numeraire. The resulting other grocery spending is $2,743 per year. The bottom row shows that excluding sugary drinks from SNAP increases modeled consumer welfare by $8.09 per SNAP recipient per year, which sums to $336 million per year across all SNAP recipients nationwide. This welfare gain is driven by the fact that sugary drinks are assumed to be over-consumed relative to the normative utility optimum.

Columns (3) and (4) present additional counterfactuals that illustrate the economics of SNAP restrictions. Column (3) presents the scenario in which each household's consumption maximizes normative utility. In that scenario, sugary drink spending is lower because it is not inflated by the internality, and other grocery spending is also lower because it is not inflated by mental accounting. SNAP restrictions generate about 16 percent of the consumer welfare gains achieved under the consumer welfare optimum.

Column (4) models a hypothetical quantity-equivalent sugary drink tax imposed only on SNAP recipients with lump-sum redistribution to those recipients—that is, a uniform ad valorem tax that reduces average sugary drink consumption by the same amount as SNAP restrictions. The quantity-equivalent tax rate is 14.4 percent. The average tax paid, and thus the lump-sum redistribution amount, is $26 per person-year, of which consumers spend 19 percent on other groceries and 81 percent on the numeraire. The resulting other grocery spending is $2,731 per year, which is $12 less than under SNAP restrictions in column (2). The difference arises because under SNAP restrictions, consumers must divert more of the reduced sugary drink spending to other SNAP-eligible groceries to maintain the mental accounting default expenditure. By contrast, the redistributed tax revenues are not subject to mental accounting, and consumers spend most of the redistributed revenues on the numeraire. Since other groceries were already over-consumed relative to the normative optimum, the reduced diversion to other groceries under the tax means that the tax generates larger welfare gains than SNAP restrictions: $9.35 versus $8.09 per person-year.

Table 8: Consumer Welfare Results

	No restrictions (1)	SNAP restrictions (2)	Consumer welfare optimum (3)	Quantity-equivalent tax (4)
Sugary drink consumption (fl oz/person-year)	3,459	3,029	2,114	3,029
Sugary drink spending ($/person-year)	$210	$184	$129	$211
Other grocery spending ($/person-year)	$2,726	$2,743	$2,490	$2,731
Equivalent variation ($/person-year)	$0.00	$8.09	$49.91	$9.35

Notes: This table presents the average consumption of sugary drinks q_s, average spending on sugary drinks $p_s q_s$ and other SNAP-eligible groceries $p_g q_g$, and the equivalent variation under four policy scenarios. Column (1) presents the baseline scenario, with sugary drinks eligible for SNAP. Column (2) presents the scenario with all sugary drinks excluded from SNAP. Column (3) presents the consumer welfare optimum. Column (4) presents a quantity-equivalent tax with lump-sum redistribution. In that column, sugary drink spending is tax-inclusive.

Alternative models. Our baseline consumer welfare model makes several assumptions that are potentially consequential. We therefore explore how our key consumer welfare results are affected by alternative plausible parameterizations and models. We briefly summarize here; Appendix H.2 describes these models and results in more detail.

Our primary model assumes that all households are at interior solutions, meaning that they spend at least some of their money on SNAP-eligible goods. In an alternative model with 10 percent of (sample-weighted) households at corner solutions, the modeled welfare gains decrease materially, to $4.84 per person-year for the average household. The reason is that SNAP restrictions generally reduce welfare for households at corners, by forcing subsidized spending into a narrower basket of goods.

In our primary model, SNAP generates over-consumption of eligible goods, and we saw above that substitution to other groceries attenuates the modeled welfare gains from SNAP restrictions relative to the quantity-equivalent tax. However, one of the primary motivations for SNAP was to increase grocery consumption, and Chorniy, Finkelstein, and Notowidigdo (2025) show that SNAP benefits do not increase drug and alcohol use in the way that cash payments do. In an alternative model with a positive internality from grocery consumption relative to the numeraire, substitution to other groceries is more normatively valuable, and SNAP restrictions generate larger welfare gains.

We also consider a quasilinear model with only two goods (sugary drinks and the numeraire), which allows us to model SNAP restrictions as a generic demand shift, while being agnostic about the mechanism through which the restrictions act (e.g., mental accounting, information signaling, or something else) and what consumers substitute to. The welfare gain is very similar to that in the Cobb-Douglas model. This suggests that as long as there is not too much substitution to third goods whose consumption is also distorted, the welfare effects depend less on the specific behavioral mechanism and more on several key sufficient statistics, including the sugary drink internality and the treatment effect of the restrictions.

6.2 Health Costs

In theory, consumers' choices reflect their private trade-offs between enjoying the taste of sugary drinks and additional disease risk, so our model's (internality-adjusted) consumer welfare gain from SNAP restrictions includes the private benefit from the reductions in body weight and diabetes risk quantified above. However, since SNAP recipients' health care costs are mostly publicly funded (see footnote 22), any health improvements may also generate an external benefit (on top of the consumer welfare gain) in the form of reduced public spending on health care.

The fiscal externality depends on how the SNAP restrictions affect health and how health in turn affects health care spending. Both are much more difficult to measure directly than the effects of the SNAP restrictions on consumption; the relevant consumption data are more readily available in large samples and health effects can take more time to manifest than our six-month horizon. We therefore perform a surrogacy analysis (Prentice 1989; Athey et al. 2026) to calibrate the magnitude of this potential fiscal externality. This section provides a brief overview of our approach; Appendix I provides additional details.

We treat our estimated proportional consumption effect (τ) as an intermediate outcome and use additional data from the 2021-2023 National Health and Nutrition Examination Survey (NHANES) on sugary drink consumption and health status, as well as external estimates, to derive implied changes in health and health care spending. The NHANES data on sugary drink consumption allow us to translate our estimated proportional consumption reduction into reductions in total daily caloric intake. We assume (consistent with our estimates in Section 4 and Allcott, Lockwood, and Taubinsky 2019b) that individuals do not meaningfully compensate for reductions in consumption of excluded sugary drinks by consuming more calories from other sources (e.g., compensating for drinking less soda by eating more desserts), so that our estimated reduction in retail sugary drinks consumption represents the overall net impact of the SNAP restrictions on sugar consumption and total calories consumed.

For potential health benefits, we focus on weight loss (specifically, changes in Body Mass Index, or BMI) and type 2 diabetes because these are the best-documented health harms associated with sugary drink consumption, including in some randomized trials (Malik et al. 2010; Ebbeling et al. 2012; Tate et al. 2012; Malik et al. 2013). We translate reductions in caloric intake into long-term weight loss (Hall, Schoeller, and Brown 2018) and rates of type 2 diabetes (Qin et al. 2020; Smith et al. 2026), and finally translate these predicted health improvements into projected reductions in health care spending (Cawley et al. 2015; Parker et al. 2024). The use of individual-level NHANES observations allows us to account for potentially important relationships between variables. For example, the constant proportional consumption reduction we estimate (τ) implies different levels of caloric reduction across individuals with heterogeneous baseline consumption, which is relevant because the relationship between consumption and diabetes is non-linear in consumption (Smith et al. 2026). In addition, effects on health care spending are non-linear in baseline BMI, changes in BMI, and diabetes status (Cawley et al. 2015).

We limit our analysis to potential fiscal externalities on adult SNAP recipients, ignoring potential effects on children for whom there is less guidance from the epidemiological literature. We focus on a 10-year simulation horizon, which we refer to hereafter as the "steady-state" effects of the SNAP restrictions. This time horizon captures the vast majority of weight change expected from sustained reductions in caloric intake (Hall et al. 2011), while reducing uncertainty inherent in longer-term projections and aligning with prior nutrition policy simulation studies (Basu et al. 2014; Basu and Maciejewski 2019; Grummon et al. 2019; Smith et al. 2026). Table A28 presents results.

We estimate that excluding sugary drinks from SNAP would reduce health care costs by about \$800 million a year after 10 years. This reflects our calculations that the SNAP restriction would cause the average adult SNAP recipient to lose 0.27 pounds and reduce the probability that SNAP recipients develop type 2 diabetes over the next 10 years by 2.5 percent, which corresponds to preventing about 33,000 new cases of diabetes after 10 years.

The estimated \$800 million annual reduction in the fiscal externality is substantially larger than the estimated \$336 million gain in consumer welfare from excluding sugary drinks. Put differently, about 70 percent of the overall social welfare gain from the SNAP restrictions comes through the fiscal externality of reduced public health care spending. Thus, while we estimate that the consumer welfare optimal level of sugary drink consumption for SNAP recipients is 2,114 fluid ounces per person-year, or about 30 percent lower than the consumption achieved by the SNAP restrictions (Table 8), once the fiscal externality is accounted for, the socially optimal level of sugary drink consumption falls substantially further, to 1,306 fluid ounces per person-year.

7 Conclusion

Using the staggered implementation of SNAP restrictions across states in the first six months of 2026, we find clear and statistically precise evidence that the restrictions reduced excluded drink consumption by about 12 percent. Since SNAP benefits are inframarginal in-kind transfers for most SNAP recipients, neoclassical economic models predict that the restrictions would not affect consumption (Hoynes and Schanzenbach 2009; Schanzenbach 2017). Our findings are instead consistent with recent evidence that SNAP recipients do not treat SNAP dollars as fungible with cash (Hastings and Shapiro 2018; Song 2022; Chorniy, Finkelstein, and Notowidigdo 2025). Using our reduced-form results and external parameters to calibrate consumer choice and health models, we calculate that excluding all sugary drinks from SNAP nationwide would generate net benefits of about \$1.1 billion per year, of which about 70 percent is from reduced fiscal externalities from public spending on health care, and the remainder comes from increased consumer welfare in the presence of internalities that cause over-consumption of sugary drinks.

Many states have implemented restrictions that exclude only some sugary drinks. Our estimates suggest that when only some sugary drinks are excluded from SNAP, SNAP recipients substituted to the sugary drink and fruit juice categories that are still included. This substitution partially offsets the consumption reductions and would correspondingly reduce the consumer welfare and health care spending benefits.

We conclude by briefly mentioning several areas of future work. First, our survey data suggest that the SNAP restrictions increased SNAP recipients' feelings of stigma, a central concern raised by opponents of the restrictions (e.g., Schanzenbach 2013). In particular, after states implemented restrictions, SNAP recipients in those states were more likely to report feeling judged when using SNAP, and to say that SNAP policies are disrespectful and take away their freedom. Our welfare analysis, however, did not account for such potential stigma costs. This is a potentially important limitation. Determining whether stigma effects are persistent and incorporating their cost in welfare analysis would be valuable. It is also challenging. One potential approach would be to elicit stated willingness to pay for SNAP with and without the restrictions. A complementary approach would be to examine whether the restrictions affect take-up of SNAP and to use any changes in take-up to back out a revealed preference change in the value of SNAP, in the spirit of Rafkin (2023) and Finkelstein, Notowidigdo, and Rafkin (2026).

Second, our consumer welfare and health modeling takes our consumption effect estimates as steady-state impacts. However, it is possible that the long-run consumption effects could differ from what we estimated. For example, there is existing evidence of a role for early-life nutritional habits in later-life consumption (e.g., Atkin 2013), raising the possibility that the SNAP restrictions could lead to long-term changes in consumption and health through habit formation.

Finally, we use a model of mental accounting to explain why the restrictions affected consumption. For our welfare analysis, it is not important to understand exactly why people seem to treat SNAP dollars differently from other sources of income. However, if policymakers want to leverage the psychology of mental accounting to shift consumption, it may be useful to have a deeper understanding of how and why consumers treat different sources of income differently.

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Appendix

Table of Contents

A Expert Survey

In July 2026, prior to sharing the results of the paper, we fielded an "expert" survey eliciting researchers' priors about the effects of SNAP restrictions as well as policy opinions and reasoning. The sampling frame included: (i) all Affiliated Scholars in the National Bureau of Economic Research (NBER) programs in public, labor, and health economics (614 people), (ii) all participants in the 2026 Behavioral Economics Annual Meeting (BEAM) (108 people), (iii) researchers who were corresponding authors on published papers between 2016-2026 funded by the Robert Wood Johnson Healthy Eating Research program (140 people), and (iv) principal investigators of grants on SNAP funded by the National Institutes of Health between 2016-2026 (50 people). Of the 897 people in the sampling frame (707 from NBER/BEAM and 190 from public health/medicine after removing duplicates), we received valid responses from 336 (259 from NBER/BEAM and 77 from public health/medicine), for a response rate of 37 percent (37 percent for NBER/BEAM and 41 percent for public health/medicine).

The survey asked people to report their field (health economics, labor studies, public economics, behavioral economics, public health, or other). The survey then asked people to predict the policy's effect on consumption.

> *Based on your best guess and without consulting any other references, do you predict that excluding sweetened drinks from SNAP increases, decreases, or does not impact the quantity of excluded drinks purchased by SNAP recipients?*
>
> - *Increases excluded drink purchases*
> - *Decreases excluded drink purchases*
> - *Does not impact excluded drink purchases*
>
> For those who responded "increases" or "decreases": *By what percent do you think [including/excluding] sweetened drinks from SNAP will decrease excluded drink purchases by SNAP recipients? (e.g., for a XX.X% decrease, please enter XX.X).*
>
> ______

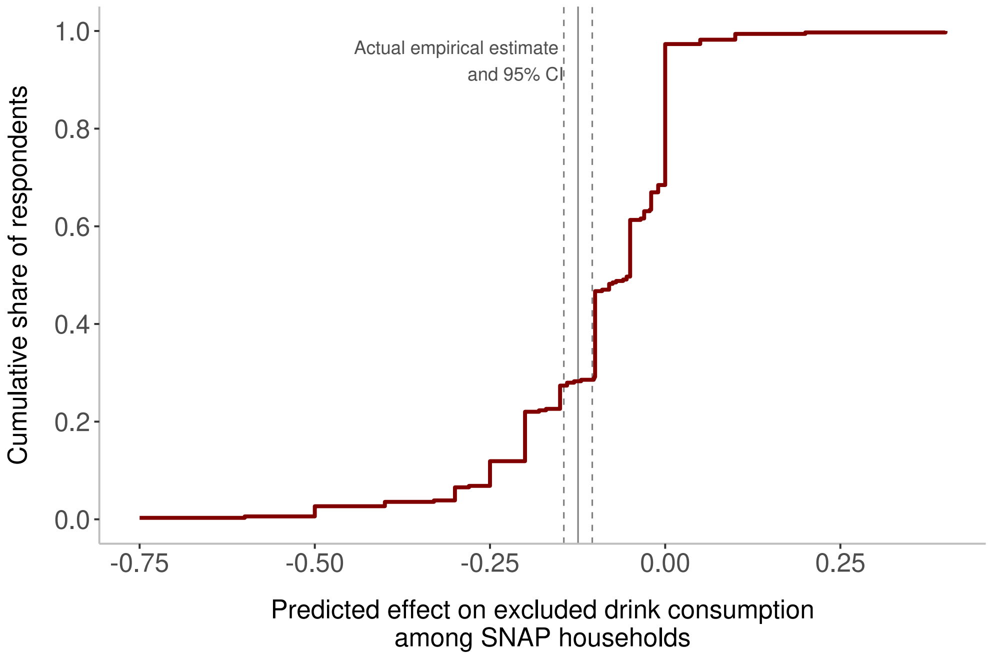
Next, the survey asked people whether they support or oppose the policy and asked for reasons why, using the exact same wording as in the survey of Numerator panelists. Finally, the survey asked the following open-ended policy reasoning question:

> *We are very interested in understanding why you made these predictions. If you are willing, describe below the factors or underlying mechanisms that influenced your predictions.*

Appendix A.1 presents the survey results. Appendix A.2 presents the survey instrument in more detail.

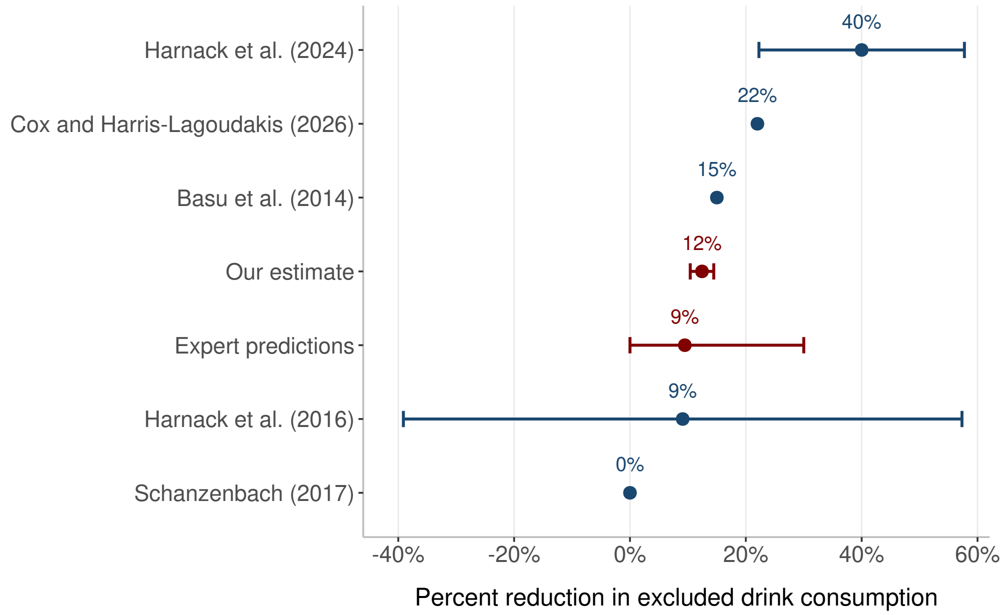
A.1 Survey Results

Figure A1: Cumulative Distribution of Predicted Changes in Excluded Drink Consumption



Notes: This figure presents the cumulative distribution of predicted changes ($N = 336$) in excluded drink consumption among SNAP households. The vertical lines show our primary estimate of the actual effect from Table 3 column (1), and its 95 percent confidence interval.

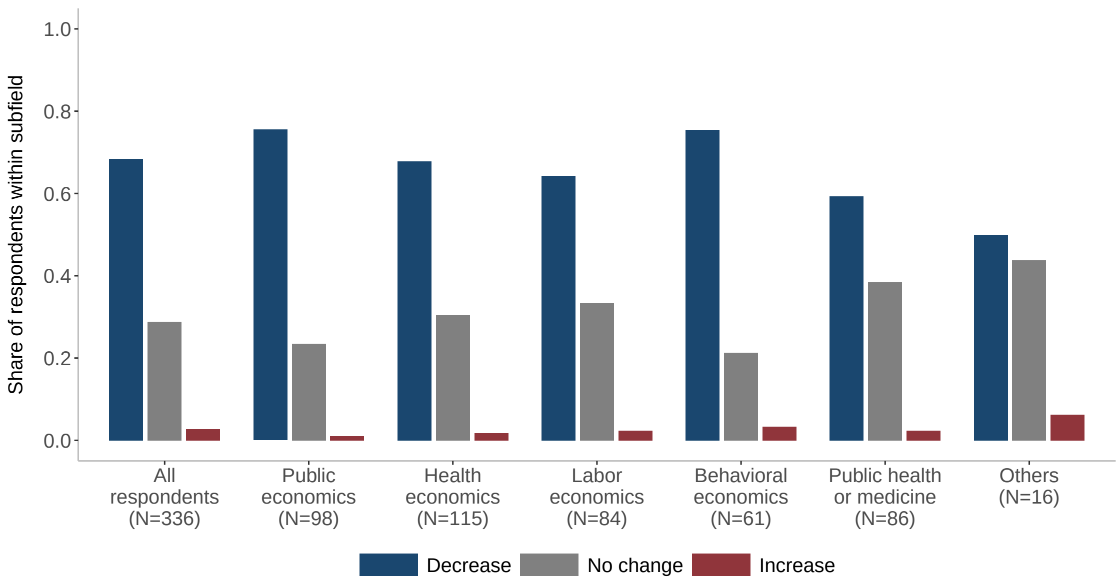
Figure A2: Other Estimates of the Effects of SNAP Restrictions on Sugary Drink Consumption



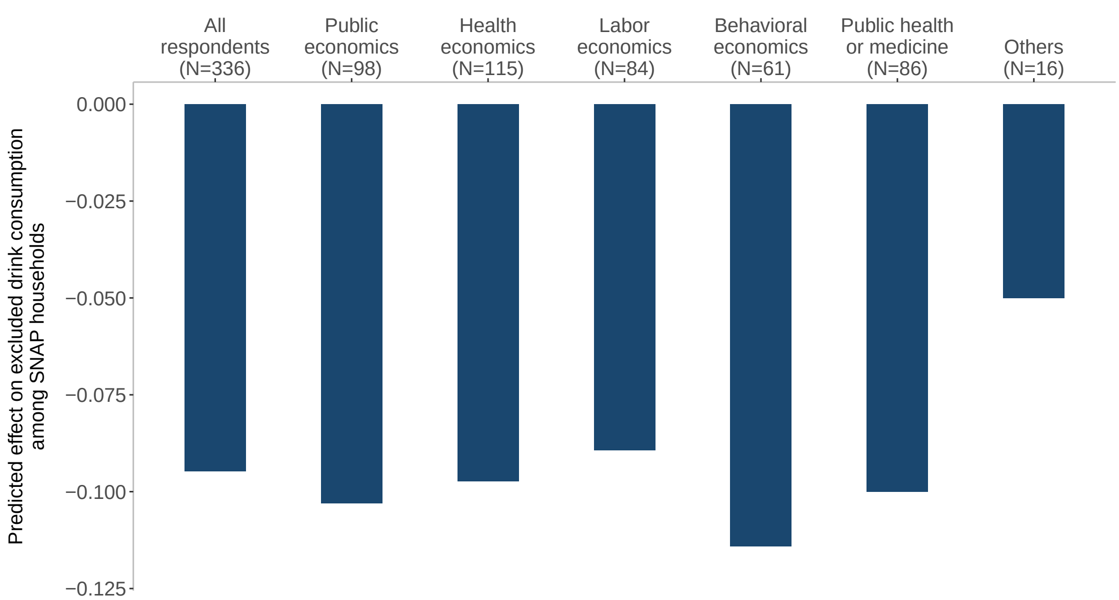
Notes: The reported effects for Harnack et al. (2016) and Harnack et al. (2024) are (Restriction group follow-up average - Control group follow-up average) / (Control group follow-up average), each using results reported in Table 3 of the respective paper, with standard errors computed via the delta method. Harnack et al. (2024) do not report baseline spending on sugary drinks, so we cannot construct a difference-in-differences estimate. The Harnack et al. and "Our estimate" rows present point estimates and 95 percent confidence intervals. The "Expert predictions" row presents the mean and 5th-95th percentile range of expert predictions, using the data reported in Appendix Figure A1. The Schanzenbach (2017) row is from the quote, "consumers would change the payment method used for buying soda, but likely will not change the amount of soda they buy."

Figure A3: Experts' Predicted Direction and Magnitude of Effect on Excluded Drink Purchases, by Field

(a) Direction of Effect

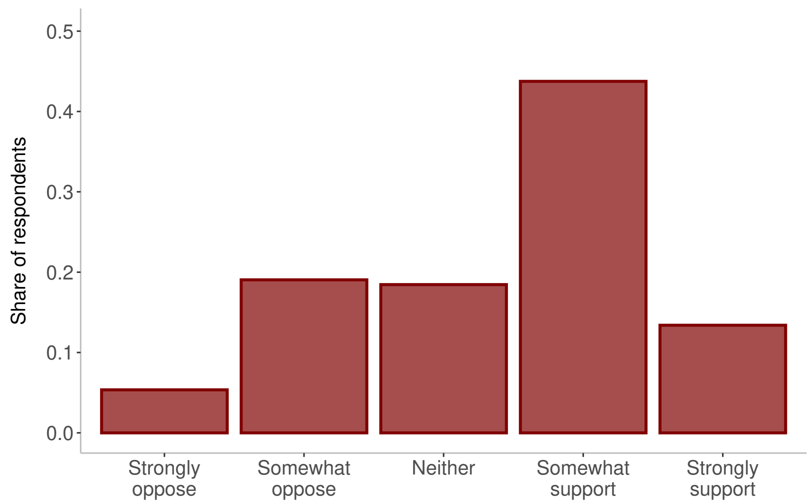


(b) Magnitude of Effect



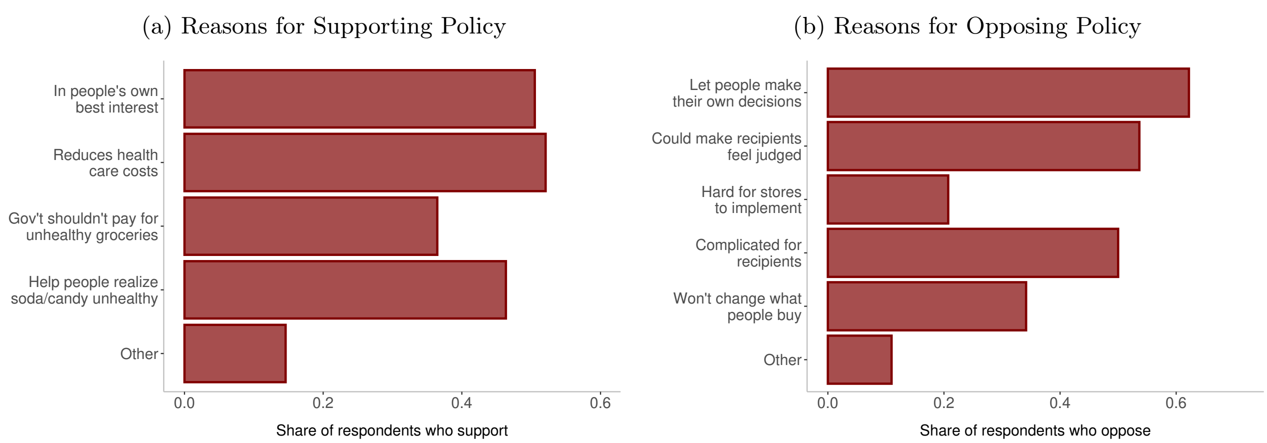
Notes: This figure presents experts' predictions ($N = 336$) of the sign and magnitude of the effect of excluding sugary drinks from SNAP on purchases of excluded drinks, by self-reported research field.

Figure A4: Experts' Opinions about SNAP Restrictions



Notes: Our expert survey asked, "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?" This figure presents the distribution of responses ($N = 336$).

Figure A5: Experts' Policy Reasoning



Notes: Our expert survey asked respondents their most important reasons for supporting or opposing SNAP restrictions. Panel (a) presents the distribution of responses for experts who supported these policies ($N = 192$). Panel (b) presents the distribution of responses for experts who opposed these policies ($N = 82$).

Figure A6: Experts' Predicted Effect Reasoning: Word Cloud

Notes: Our expert survey asked, "We are very interested in understanding why you made these predictions. If you are willing, describe below the factors or underlying mechanisms that influenced your predictions." This figure presents the word cloud for the most frequently mentioned single words or two-word phrases. Single words mentioned more than five times and two-word phrases mentioned more than two times are selected. A set of pre-selected universal verbs, adverbs, conjunctions, and relative pronouns are removed.

Table A1: Expert Prediction Reasoning: Selected Highlights

Field	Verbatim response
Health economics	"I think SNAP is fairly fungible to income. I might expect very small changes in consumption, but if it's fungible, there shouldn't be much."
	"There are clearly frictions that make people more likely to consume product eligible for SNAP even when theoretically fungible."
	"Sugary drinks are addictive, so raising the price (which in essence the lack of SNAP support does) will reduce consumption by those who are not yet addicted, but not others - who will substitute from other goods to the higher cost of these goods."
Labor economics	"since benefits aren't covering all grocery costs, in principle there's no reason to expect a drop, but there's probably some friction that leads to a modest drop"
	"Recent evidence suggests that people engage in a form of mental accounting with restricted transfers; they do not treat them as being as fungible as they are. (My views may have been influenced by the research of the team fielding this survey.)"
Public economics	"Literature on SNAP shows it is inframarginal, ie if ppl spend more on grocery shopping than SNAP anyways, then they can buy non-sweet drinks with SNAP and sweet drinks with rest."
	"basic theory would tell you no effect, just reshuffling of how it is paid for, but it is likely that SNAP recipients occasionally are unwilling or unable to spend on a given trip more than benefits either because of liquidity or because of mental accounting"
	"part of me wanted to think through the budget line and the possibility people would consume some anyway even at higher prices, but another part of me thought of the flypaper effect results and went with that. I didn't have a good off the top of my head sense of the magnitude of the subsidy so kind of made up a modest decrease."
Behavioral economics	"I have a vague memory of seeing research showing that there is mental accounting ie nonfungibility with SNAP purchases. Hence I chose decrease. When it came to the percentage I confess I had no idea - no relevant 'anchors' to go on"
	"Standard price response to decrease demand and also potential salience effects / learning effects that decrease demand"
	"Evidence that people substitute around these bans (e.g., from soft drinks to milkshakes); other expenditure substitution patterns."
Public health or medicine	"I think the stigma around implementing this policy and the negative consequences it may have for SNAP participants emotionally must be considered. I support policies that promote healthy eating but not at the expense of making marginalized groups feel more marginalized and stigmatized. I also think the way stores implement this has not been given adequate time and resources to figure out."
	"This ban has unintended consequences. What if a family has a child in sports - banning sports drinks may be harmful as electrolytes can be necessary. Additionally, this policy assumes that everyone on SNAP is unhealthy - there are diabetics who may need sugar, people with food allergies or conditions that have implications what they can and should consume."

Notes: Our expert survey asked, "We are very interested in understanding why you made these predictions. If you are willing, describe below the factors or underlying mechanisms that influenced your predictions." This table presents selected responses that we thought were interesting to highlight.

A.2 Survey Instruments

The expert survey asked researchers to predict the effect of excluding sweetened drinks from SNAP on purchases of the excluded drinks and to report their opinions about these restrictions. Respondents were instructed to give their best guesses without consulting external references. The policy-support response scale was randomly reversed. The response options for the policy-reasoning questions were randomized, except that the open-ended "Other" option remained last. Conditional follow-up questions were displayed based on respondents' answers.

Respondent background.

1. **Institutional role.**

What is your role at your institution?

Response options: Faculty or post-doc; Researcher outside of academia; Graduate/undergraduate student; Other, please specify.

2. **Field of research.** *Select all that apply.*

How would you characterize your field of research or specialization?

Response options: Health economics; Labor studies; Public economics; Behavioral economics; Public health; Medicine; Other, please specify.

Study description and predictions. Respondents were provided with the following background:

> The next questions are about the Supplemental Nutrition Assistance Program (SNAP), formerly known as Food Stamps. Many states have recently implemented or approved plans to disallow spending of SNAP benefits on sweetened drinks, including regular soda, diet soda, energy drinks, sports drinks, sweetened tea, and similar products. We will ask you for your best guess about the impact of excluding sweetened drinks from SNAP and your opinion on this policy.

3. **Predicted effect on excluded-drink purchases.**

Based on your best guess and without consulting any other references, do you predict that excluding sweetened drinks from SNAP increases, decreases, or does not affect the quantity of excluded drinks purchased by SNAP recipients?

Response options: Increases excluded-drink purchases; Decreases excluded-drink purchases; Does not affect excluded-drink purchases.

3a. **Predicted percentage increase.** *Displayed only if Question 3 = Increases.*

By what percent do you think excluding sweetened drinks from SNAP will increase excluded-drink purchases by SNAP recipients? For example, for an $XX.X\%$ increase, enter $XX.X$.

Response options: Open numeric response in percentage points.

3b. Predicted percentage decrease. *Displayed only if Question 3 = Decreases.*

By what percent do you think excluding sweetened drinks from SNAP will decrease excluded-drink purchases by SNAP recipients? For example, for an $XX.X\%$ decrease, enter $XX.X$.

Response options: Open numeric response in percentage points.

After the respondent entered a percentage increase or decrease, the survey displayed the entered value and asked the respondent to proceed if it was correct or return to the previous page to revise it.

Policy opinions and reasoning.

4. Support for restrictions.

As mentioned, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?

Response options: Strongly support; Somewhat support; Neither oppose nor support; Somewhat oppose; Strongly oppose.

4a. Reasons for supporting the restrictions. *Displayed only if Question 4 = Strongly support or Somewhat support. Select all that apply.*

Again, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. You indicated that you support these policies. Select the most important reasons for your position.

- Eating healthier is in people's own best interest.
- Eating healthier reduces health care costs, which saves taxpayers money.
- Even if these policies do not change what people buy, the government should not pay for unhealthy groceries.
- These policies could help people realize that soft drinks and candy are unhealthy.
- Other, please specify: ____________________________.

4b. **Reasons for opposing the restrictions.** *Displayed only if Question 4 = Somewhat oppose or Strongly oppose. Select all that apply.*

Again, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. You indicated that you oppose these policies. Select the most important reasons for your position.

- It's important to let people make their own decisions about what to eat and drink.
- These policies could make SNAP recipients feel judged or disrespected.
- It could be hard for stores to implement these policies.
- These policies make SNAP complicated for recipients.
- Other, please specify: ___________________________.
- I don't think these policies will change what people buy.

4c. **Reasons for neither supporting nor opposing.** *Displayed only if Question 4 = Neither oppose nor support. Select all that apply.*

Again, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. You indicated that you neither oppose nor support these policies. Select the most important reasons for your position.

- It's important to let people make their own decisions about what to eat and drink.
- These policies could make SNAP recipients feel judged or disrespected.
- It could be hard for stores to implement these policies.
- These policies make SNAP complicated for recipients.
- Eating healthier is in people's own best interest.
- I don't think these policies will change what people buy.
- Eating healthier reduces health care costs, which saves taxpayers money.
- Even if these policies do not change what people buy, the government should not pay for unhealthy groceries.
- These policies could help people realize that soft drinks and candy are unhealthy.
- Other, please specify: ___________________________.

5. **Reasoning behind predictions.**

We are very interested in understanding why you made these predictions. If you are willing, describe the factors or underlying mechanisms that influenced your predictions.

Response options: Open-text response.

6. Additional comments.

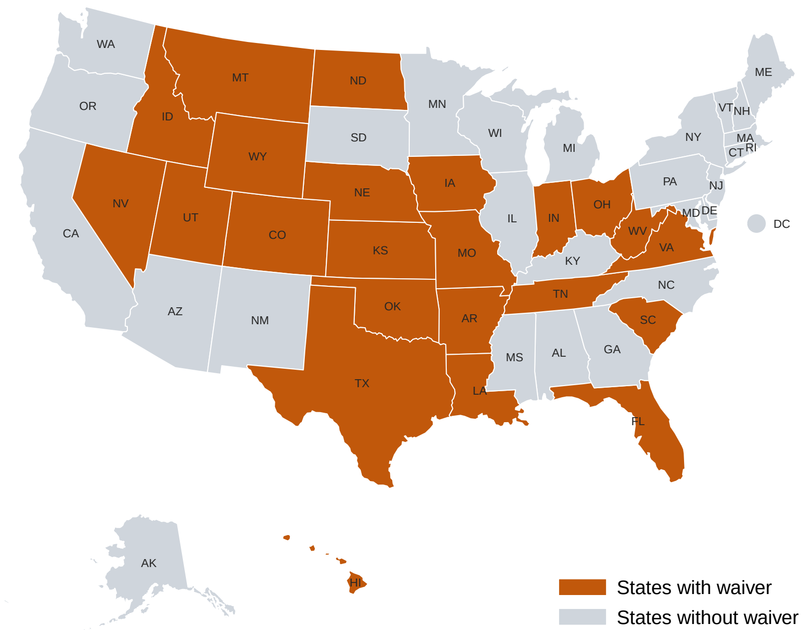
Anything else you want to tell us?

Response options: Open-text response.

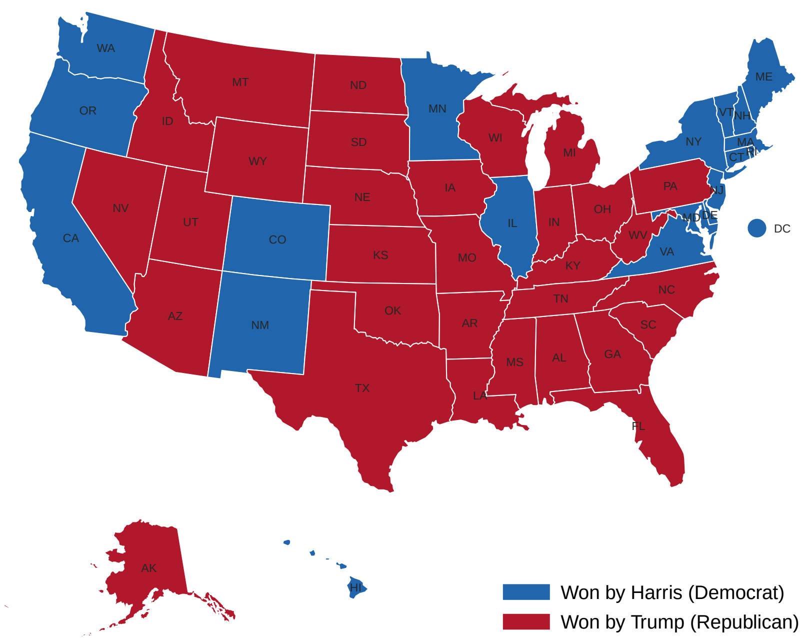
B Policy Overview Appendix

Figure A7: Maps of SNAP Restrictions and 2024 Presidential Election Results

(a) SNAP Restrictions



(b) 2024 Presidential Election Results



Notes: Panel (a) presents states with federal waivers to remove sweetened drinks from SNAP, as presented in Table 1. Panel (b) presents 2024 election results, using data from Federal Election Commission (2025).

C Numerator Purchase Data Appendix

C.1 Numerator Panel Overview

Any household can download the Receipt Hog app and scan their receipts; these data are contained in the Numerator OmniPanel which includes item-level records of household purchases. App users receive financial incentives to upload their receipts. The Receipt Hog app collects records from more than 1.3 million households each year.

Item-level data are collected by households uploading their receipts and Numerator reading off information from the receipts using OCR tools. The item-level information contains the item description, brand name, number of item purchases, item unit price, and the item total price. Numerator processes item-level data and generates category labels. Receipt-level information includes the total price, transaction date, store location, payment method, WIC indicator, and SNAP indicator. Household-level data include demographics (e.g., household size, postal code, employment status, annual income, marital status) that are collected from a required user demographics survey within the Receipt Hog app. All survey responses are provided by the Receipt Hog app account owner, and person-level information such as gender and age is attributed to the person who scans receipts and answers survey questions on behalf of the household.

Households are instructed to scan all retail product categories—not just groceries, but also electronics, apparel, cosmetics, cleaning supplies, and alcohol. Critically for our purposes, food delivery orders, fast food purchases, coffee shops, and sit-down dining restaurants are also included. Service-based industries or non-retail transactions where no consumer good is purchased (e.g., utility bills, medical treatments, rent, and car repairs) are excluded.

Static panel. Numerator invites a subset of Receipt Hog app user households to join Numerator's "static panel" for a 12-month window. Households are invited based on their demographic profile and consistent longitudinal purchase history (i.e., regular scanning). Specifically, Numerator aims to select nationally representative households who have consistent longitudinal purchase histories (defined as a minimum requirement of 2 trips per month for 12 consecutive months and 5 retailers shopped per year). The average panelist has at least 30 trips per month and at least 50 retailers shopped per year. Numerator requires that each household remain in the static panel for at least 12 months in order to be included in the data, and it creates a new static panel each month. In 2025, the target sample size for the static panel was 200,000.

The analyses in this paper are based on the static panels. We select the intersection of static panelists in the January 2025 and July 2025 static panels to form an 18-month panel.[25] As shown in the first two rows of Appendix Table A6, this brings our sample of about 1.5 million households in the 2025 to 2026 OmniPanel down to about 172,000 households. We describe the additional sample restrictions in Section C.5; these utilize our coding of whether a household is on SNAP, which we

25. For some of our analyses, particularly when we were developing the pre-analysis plan, we used the intersection of static panelists in the January 2024 and January 2025 static panels to form a 24-month balanced panel.

describe in the next subsection.

Household attributes. In addition to basic demographics, other person-level householder attributes, such as psychographic variables and health conditions, are collected by Numerator's opt-in attribute surveys distributed to its Receipt Hog app users. Specifically for the diabetes attribute we utilize for the sample split in Table 5, respondents are asked about their current diabetes status, and we classified them as "diabetic or pre-diabetic" if they responded that they had pre-diabetes, type 1 diabetes, or type 2 diabetes. We coded them as "non-diabetic" if they reported they had no diabetes.

C.2 Quantity Imputation

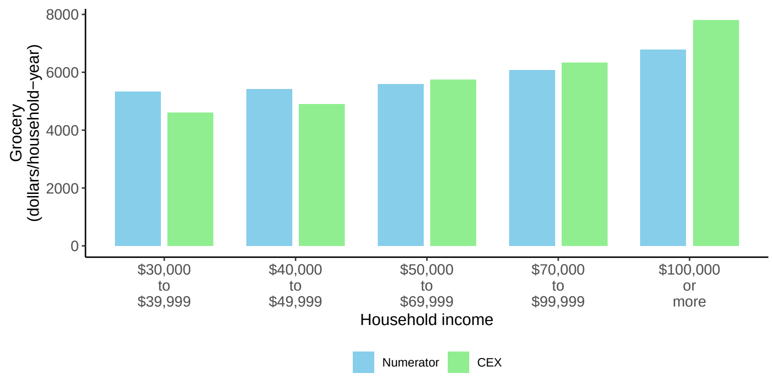
The Numerator Panel Data record dollar spending and item counts for each transaction but do not report beverage volume. To recover volume, we train a predictive model using auxiliary data on price and package size and apply the fitted model to Numerator Panel Data to back out beverage consumption in fluid ounces from the observed dollar spending.

Specifically, we model the log real price per fluid ounce as a function of a large number of characteristics (store type, brand, drink category, month, region, income, and CPI-adjusted item unit price). We then compute the predicted price for each beverage item in the Numerator Panel Data using the fitted values from the model, and we compute the item quantity in fluid ounces by dividing the observed item unit price in the Numerator Panel Data by the predicted price per fluid ounce. For restaurant purchases, we multiply the predicted price per fluid ounce by 2 based on the ratio of grocery and restaurant price per fluid ounce in USDA's National Household Food Acquisition and Purchase Survey (FoodAPS) data.

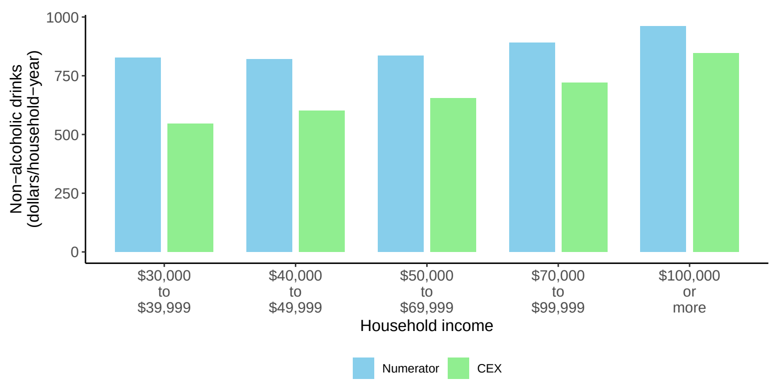
C.3 Validation of Numerator Panel Data Against the CEX

Figure A8: Average Household Annual Expenditure

(a) Grocery Expenditure



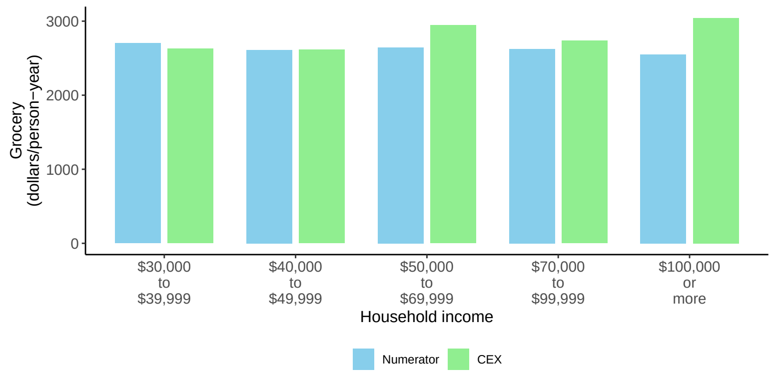
(b) Retail Non-Alcoholic Drinks Expenditure



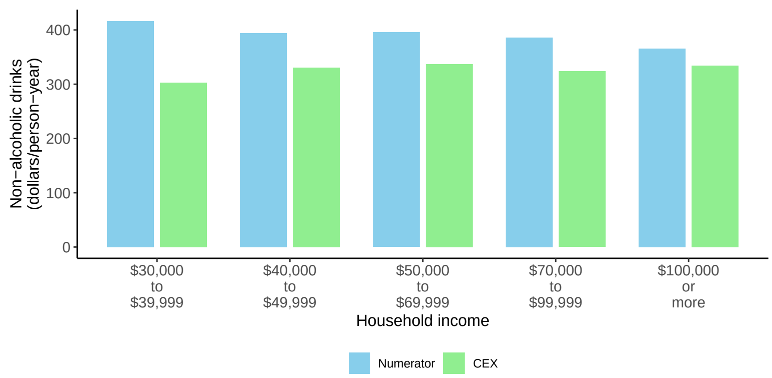
Notes: This figure compares household annual expenditures between 2025-2026 Numerator analysis sample and 2023 CEX data. All dollar values are adjusted to 2025 value, except income, using city-average food and beverage CPI for grocery expenditure and city-average nonalcoholic beverage CPI for nonalcoholic drinks. Panel (a) presents households' annual grocery (SNAP-eligible food at home and nonalcoholic beverages) expenditure. Panel (b) presents households' annual retail non-alcoholic drinks expenditure.

Figure A9: Average Household Annual Expenditure per Household Member

(a) Grocery Expenditure



(b) Retail Non-Alcoholic Drinks Expenditure



Notes: This figure compares annual expenditures per household member between 2025-2026 Numerator analysis sample and 2023 CEX data. All dollar values are adjusted to 2025 value, except income, using city-average food and beverage CPI for grocery expenditure and city-average nonalcoholic beverage CPI for nonalcoholic drinks and sweetened drinks expenditure. Panel (a) presents households' annual grocery (SNAP-eligible food at home and nonalcoholic drinks) expenditure. Panel (b) presents households' annual retail non-alcoholic drinks expenditure.

C.4 SNAP Participation Variable

Definition. The construction of an indicator for whether a household-month is on SNAP ($S_{it} = 1$) includes risks of both false negatives (based on "under-scanning" which can lead to missing EBT receipts) and false positives (based on "over-scanning" such as scanning others' receipts). Since our baseline specification is limited to those on SNAP in the month of the policy implementation, we chose to err on the side of false negatives rather than false positives, since false positives would (mechanically) attenuate any estimated treatment effect of the policy. As a result, our sample may be skewed toward SNAP households with more grocery spending on EBT, and thus potentially larger SNAP benefits.

We code a household-month as on SNAP ($S_{it} = 1$) based on the following procedure. We first define a household as "potentially ever on SNAP" if it ever has a month in the data with an EBT receipt and its (self-reported and time-invariant) income is below a household-size-specific income threshold (where household size is also self-reported and time-invariant).[26]

For any "potentially ever on SNAP" households, we then define non-SNAP spells as any period with at least two consecutive months without an EBT receipt; any remaining periods constitute a potential spell. Potential spells thus consist of household-months with at most one month without EBT receipts. For each potential spell, we first remove spells that have fewer than 50 percent of months within the spell with an EBT receipt; specifically, if less than 50 percent of the months within the spell have an EBT receipt, we code all household-months in the spell as $S_{it} = 0$. Having thus removed potential spells that are too "sparse" in EBT receipt, for the remaining potential spells, if the gap between two adjacent months with EBT receipt is one, we recode the months in the gap as $S_{it} = 1$. Then, within each spell, we recode all household-months within the spell to $S_{it} = 0$ if the share of spending in that spell that is on EBT receipts is less than 20 percent. Finally, if the recoded potential SNAP spell is less than three months, we code the household-months as not on SNAP; otherwise we code the household months in that potential SNAP spell as on SNAP.

Categories of SNAP participation. For our regression analyses, we define the set of households in each cohort in the stacked dataset as those on SNAP in the implementation month of SNAP restriction; i.e., a household is a SNAP household in either treatment or control states if $S_{it} = 1$ in $t = t^*_a$ for cohort a. This is our main definition of SNAP household used in the analysis. In a few places, we use additional definitions. Specifically,

- We analyze non-SNAP households as an additional contrast in the triple-difference regression in Table 6, column (9). For this analysis, we define a non-SNAP household as never having any EBT receipts in the 18 months that they are included in the sample. We also use this definition for non-SNAP households in Table 2.

26. To ensure that a potential SNAP household does not have implausibly high income, we exclude one-person households with annual income above \$40k, two-person households with annual income above \$70k, three-person households with annual income above \$100k, and households with 4 or more people with annual income above \$130k. We choose these thresholds to be more generous than actual SNAP eligibility income thresholds due to likely measurement error in the self-reported income, as well as potential variation in income within a household over time.

- In constructing sample weights (see Appendix C.6), we define the households "ever on SNAP" during 2026 as households with $S_{it} = 1$ for any month in 2026; this provides a parallel definition to the ACS data target, which specifies households on SNAP as those who were ever on SNAP during the past 12 months. This definition is used to construct the sampling weights for SNAP households as shown in Appendix Table A7 and in the summary statistics reported in Appendix Table A9. For the sampling weights for non-SNAP households, we use the above definition.

Detailed SNAP participation coding procedure. For completeness, we outline the "pseudo code" of our coding procedure for our main SNAP participation variable:

STEP 1. Construct $\tilde{S}_{it} = 1$ to denote a household-month that is potentially on SNAP. Define $\tilde{S}_{it} = 1$ if the household satisfies the household-size-specific income threshold and the household has any EBT receipts in that month.

STEP 2. Construct final S_{it} by:

- (i) First define non-participation spells as any months with $\tilde{S}_{it} = 0$ for at least 2 consecutive months as non-spells. Any months remaining with $\tilde{S}_{it} = 1$ are a "potential spell."

- (ii) If a remaining potential spell has an average $\tilde{S}_{it}$ within the spell less than $p^{freq} = 0.5$ (e.g., if the sequence of $\tilde{S}_{it}$ were (0,1,0,1,0)), then we set $\tilde{S}_{it} = 0$ for all months in that potential spell.

- (iii) Lastly, within any potential spell, we replace any remaining $\tilde{S}_{it} = 0$ with $\tilde{S}_{it} = 1$ (which is at most one month in a row given the steps above).

In other words, in steps (ii) and (iii), we assume that within a potential SNAP spell, a month with zero EBT receipts surrounded by at least two months with EBT receipts is assumed to be a false negative (e.g., missing receipts), so we "correct" this by setting the gap of 1-month with no EBT receipt between two months with EBT receipts as $\tilde{S}_{it} = 1$. However, filling in the gaps can also introduce some false positives, so we want to prevent a potential spell that is "too sparse" but fulfills the condition to be filled in. Thus, before filling in the gaps, we impose the average of $\tilde{S}_{it}$ over the potential spell of $p^{freq} \geq 0.5$, where p^{freq} is the share of months within the spell where $\tilde{S}_it = 1$. Any potential spell that has less than half of months with EBT receipts will not be filled in and will be coded as $\tilde{S}_{it} = 0$ for every month in the potential spell.

To further reduce the likelihood of false positives after filling in the gaps, we next:

- (iv) Calculate the share of grocery spending on EBT receipts for each spell (which we define as p^{ebt}), and we set $\tilde{S}_{it} = 0$ for all months in a potential spell with less than $p^{ebt} = 0.2$ share on EBT in that spell.

Finally, we assume that potential spells with short enough consecutive $\tilde{S}_{it} = 1$ that are isolated from other potential spells are more likely to be false positives, which we correct as follows:

- (v) Require at least a 3-month spell; i.e., set $\tilde{S}_{it}$=0 if the potential spell is less than 3 months long.

The remaining $\tilde{S}_{it} = 1$ are defined as $S_{it} = 1$.

3. For the regression sample, we define "baseline" SNAP recipients as those with $S_{it} = 1$ in the

month of policy implementation.

Example spell construction. Table A3 presents two example households and their grocery and EBT spending over 24 months. Assume both households satisfy our income requirements so any month with positive EBT spending is coded $\tilde{S}_{it} = 1$. Household example 1 indicates 0 EBT spending in months 2 and 5 in the data. Based on our procedure, months 1 to 9 are considered a "potential spell" with more than half of months with EBT spending. Months 2 and 5, which are surrounded by at least 2 months of $\tilde{S}_{it} = 1$, are recoded as $\tilde{S}_{it} = 1$. To eventually code the spell of months 1 to 9 as a SNAP spell for downstream analyses, we calculate the share of grocery spending on EBT over months 1 to 9, which is 26.9 percent and is larger than 20 percent, so this spell satisfied the material EBT share requirement and every month in this spell will be coded $S_{it} = 1$. Short spells including month 13 and months 19-20 will be coded as $S_{it} = 0$.

For household example 2, months 6 to 14 are a "potential spell" with more than half of months with EBT spending, so we code $\tilde{S}_{it} = 1$ for all months in this spell. We calculate that 19.3 percent of total grocery spending is on EBT, which is smaller than 20 percent, so this spell will be coded as a non-SNAP spell where all months are coded $S_{it} = 0$. Because month 19 is a single month with EBT spending, this month will also be coded $S_{it} = 0$. As a result, household example 2 eventually has all months coded as $S_{it} = 0$.

Table A3: Grocery and EBT Spending for Example Households

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
Household example 1																								
EBT spending	249	0	94	496	0	111	67	317	295	0	0	0	73	0	0	0	0	0	83	179	0	0	0	0
Grocery spending	549	491	822	628	723	657	682	612	881	883	690	797	850	805	639	662	867	937	743	664	665	1071	334	1158
Household example 2																								
EBT spending	0	0	0	0	0	1	70	41	89	61	102	134	93	118	0	0	0	0	20	0	0	0	0	0
Grocery spending	10	415	267	80	220	370	385	247	516	405	471	590	448	238	288	169	94	177	370	282	195	250	182	321

Notes: This table shows two hypothetical examples of raw spells of EBT spending and grocery spending of a household over a 24-month period.

Comparison to alternative SNAP definitions. Table A4 compares spell-level statistics across several alternative specifications for defining S_{it} (columns 1 through 5) as well as to target moments from various data sources (column 6), including Consumer Expenditure Surveys (CE) Public Use Microdata (PUMD) for monthly grocery spending, American Community Survey (ACS) 2024 one-year estimates for household annual income, and USDA SNAP Quality Control data for monthly SNAP benefits received.

Table A4 shows that in general the difference between monthly EBT spending when on SNAP and target USDA Quality Control data's monthly SNAP benefits received is large, so this encourages a specification with higher monthly EBT spending; e.g., minimum material EBT share being 0.2 or 0.3 over the potential spell so that we exclude more false positives and small-dollar SNAP recipients. While the minimum share of months with EBT in general does not have a significant impact, minimum material EBT share of 0.3 reduces the sample size substantially more than minimum share of 0.2. Therefore, a primary specification for S_{it} uses a minimum material EBT share of 0.2 and a minimum share of months with EBT of 0.5.

Figure A10 compares moments and distribution of spell-level statistics between our sample and target datasets for our primary specification for coding consecutive $S_{it} = 1$. We tend to have lower average household annual income but higher median income than ACS data, slightly lower monthly grocery spending than CEX data, and lower monthly SNAP benefits than USDA QC data.

Table A5 quantitatively illustrates the consequence of the goal of S_{it} construction. Panel (a) shows that we have a higher false positive rate than a false negative rate, and after our S_{it} construction, panel (b) shows lower false positive rate than false negative rate. Therefore, in any downstream analysis where we use baseline SNAP households with S_{it} in the policy implementation month, our sample is likely a mix of true positives with a very small share of false positives to reduce the mechanical impact of false positives while skewing toward households with a somewhat higher-than-average share of grocery spending on EBT.

Robustness of estimates to alternative definitions. Our primary estimates define $S_{it} = 1$ only if at least 20 percent of grocery spending is in EBT transactions and at least 50 percent of months in the spell have EBT transactions. Appendix Table A15 compares the effects of SNAP restrictions on retail excluded drink purchases under different S_{it} constructions, e.g., different combinations of minimum share of EBT spending over potential spell and minimum share of months with EBT receipts over potential spell as in A4. Column (1) shows our baseline estimate of a 12.4 percent decline in purchases of retail excluded drinks. The results change slightly, in expected ways, if we change the minimum share of grocery spending done with EBT transactions (columns 2 and 3). If we reduce the minimum EBT/grocery ratio to 10 percent (column 2), this includes more SNAP recipients with lower SNAP benefit amounts and possibly more false positives. Correspondingly, the estimated effect shrinks from -0.124 to -0.113. Symmetrically, if we increase the minimum EBT/grocery ratio to 30 percent (column 3), this excludes more low-benefit households and possible false positives, and the estimated effect magnifies to -0.144. Columns (4) and (5) show that if we keep the 20 percent minimum EBT share but change the required minimum percent of months in a spell that have EBT transactions to 30 or 70 percent, the results are essentially unchanged.

Table A4: Spell-Level Summary Statistics: 2023-2025 Panel

	S_{it}: [min EBT/grocery ratio, min share months with EBT]					Target moments
	[0.2, 0.5]	[0.1, 0.5]	[0.3, 0.5]	[0.2, 0.3]	[0.2, 0.7]	
	(1)	(2)	(3)	(4)	(5)	(6)
Average annual household income ($)	38,540	40,189	36,893	38,551	38,240	52,321
Median annual household income ($)	35,000	35,000	35,000	35,000	35,000	31,044
Average grocery spending ($/month)	529	560	507	529	537	505
Median grocery spending ($/month)	476	501	456	476	483	292
Average EBT spending when on SNAP ($/month)	221	185	253	221	227	353
Median EBT spending when on SNAP ($/month)	186	148	216	186	191	300
Share of spells $\geq$ 36 months	0.222	0.174	0.262	0.221	0.232	0.133
Number of households ever on SNAP in 2025	18,750	24,419	14,249	18,771	18,315	

Notes: This table presents the spell-level summary statistics under various constructions of S_{it} and compares to target statistics from publicly available data. Target sources are ACS 2024 one-year estimate for household annual income, 2024 QC Micro data for EBT spending, and 2023 CEX data for monthly grocery spending (SNAP-eligible food at home and nonalcoholic beverages). All dollar values are adjusted 2025 dollars, except income, using city-average food and beverage CPI. Numerator sample is 2023-2025 static panel for all spell-level statistics. The Numerator sample is weighted using weights that are constructed for ever-SNAP households who have $S_{it} = 1$ in any month t after spell construction with the specified parameters in each column.

Table A5: SNAP Measure Comparisons

(a) Self-reported SNAP versus purchase record

	Reported on SNAP	Reported not on SNAP
Any EBT receipt	48%	16%
No EBT receipt	2%	33%

(b) Self-reported SNAP versus S_{it}

	Reported on SNAP	Reported not on SNAP
$S_{it} = 1$	34%	6%
$S_{it} = 0$	17%	44%

(c) Share with positive EBT spending

	Reported on SNAP	Reported not on SNAP
$S_{it} = 1$	100%	100%
$S_{it} = 0$	88%	24%

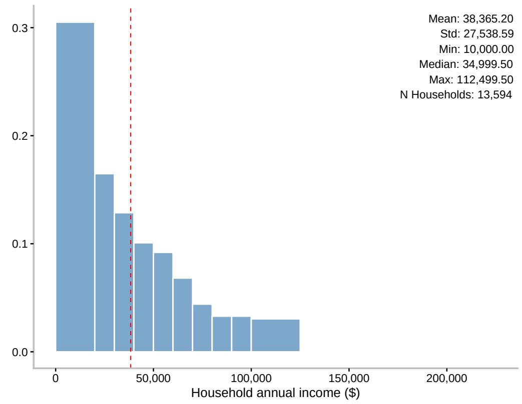
(d) Mean EBT spending conditional on any EBT receipts

	Reported on SNAP	Reported not on SNAP
$S_{it} = 1$	$264	$188
$S_{it} = 0$	$129	$85

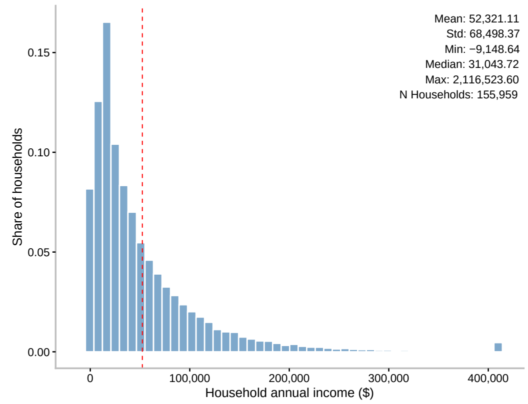
Notes: This table presents the comparison between reported on SNAP/not on SNAP during past 3 months as of the time of baseline survey in December 2025, and "ever on SNAP" during same months based on our primary S_{it} variable and EBT receipts record. Sample is our baseline analysis sample as defined in Section C.5 below. Panel (a) presents the share of all households self-reported on or not on SNAP in the baseline survey who appeared to have or not have EBT receipts in the purchase panel during the 3 months before baseline survey. Panel (b) presents share of all households self-reported on or not on SNAP in the baseline survey who are coded with any month $S_{it} = 1$ or all months $S_{it} = 0$ during the 3 months before baseline survey. Panel (c) presents the share of households in each cell with any EBT receipts, and panel (d) presents the average monthly EBT spending conditional on households with any EBT receipts.

Figure A10: Sample and Target Variable Distributions

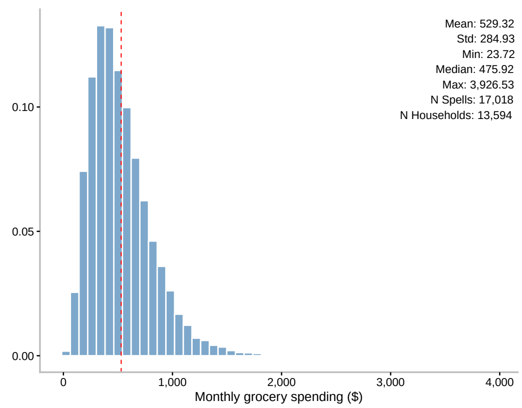
(a) Numerator: Annual Income



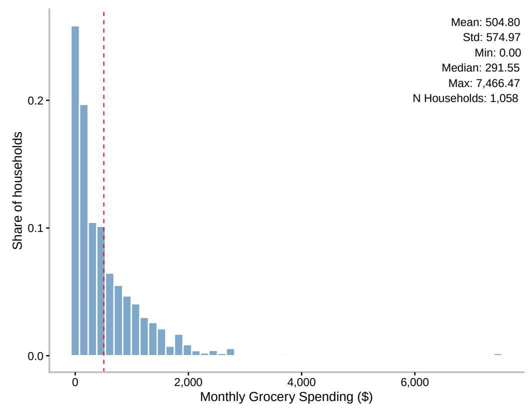
(b) ACS: Annual Income



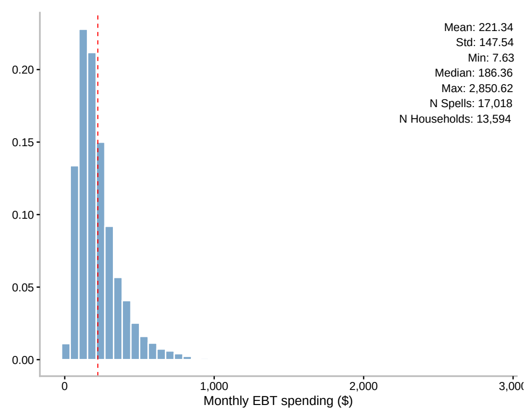
(c) Numerator: Grocery Spending



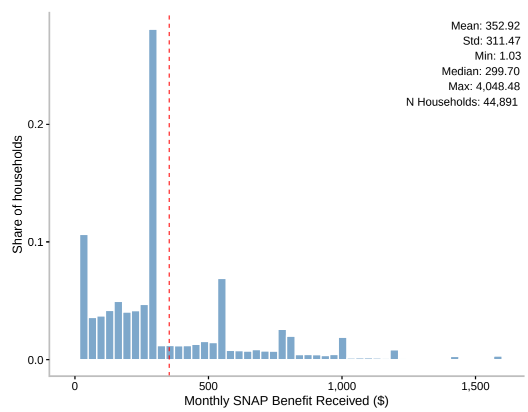
(d) CEX Micro: Grocery Spending



(e) Numerator: EBT Spending



(f) QC: SNAP Benefits



Notes: The left column reports Numerator sample distributions for households who have $S_{it} = 1$ in any month t after spell construction using $p^{ebt} = 0.2$, $p^{freq} = 0.5$, minimum spell length of 3 months, and fill-in gap parameter $y = 1$. The right column reports the corresponding target distributions. Target sources are ACS 2024 one-year estimate for household annual income, 2024 SNAP Quality Control Micro data for monthly SNAP benefits, 2023 CEX data for monthly grocery spending (SNAP-eligible food at home and nonalcoholic beverages). All dollar values are adjusted to 2025 value, except income, using city-average food and beverage CPI. Numerator sample is 2023-2025 static panel. The Numerator sample is weighted, where weights are constructed for ever-SNAP households who have $S_{it} = 1$ in any month t after spell construction.

C.5 Sample Restrictions

We start with the static panel of approximately 172,000 households that scan receipts in all 18 months of our analysis period from January 2025 through June 2026 (see row 2 of Appendix Table A6). To arrive at our baseline analysis sample of SNAP households, we further restrict to SNAP households, defined as those with $S_{it} = 1$ in the month of implementation; this creates our baseline analysis sample of 15,261 households; of these, 3,291 are in treatment states and 11,970 are in control states.

In some analyses, we also analyze non-SNAP households, defined as those who never had any EBT receipts during the 18-month study period. As shown in the penultimate row of the table, there are 87,780 such households.

Table A6: Numerator Sample Restrictions

	Household observations (1)	Household-month observations (2)
OmniPanel in 2025-2026	1,551,627	16,745,117
Static panel in analysis period	172,221	3,099,978
Baseline SNAP households in analysis sample	15,261	274,698
Non-SNAP households in analysis sample	87,780	1,580,040

Notes: This table presents the sample restriction process from Numerator OmniPanel to our analysis samples. The first row presents the sample size for Numerator OmniPanel, which is the original data recording every receipt uploaded to the Receipt Hog app without any sample restriction. The second row presents the static panel sample size. This is a balanced panel with a subset of high-quality receipts selected by Numerator (see Section C). We select the intersection of households who are selected in the January 2025 and July 2025 static panel cohorts, which creates an 18-month balanced panel. From the static panel, we select SNAP households, who are those with $S_{it} = 1$ in the month of implementation, and non-SNAP households, who never have any EBT receipts in the 18 months of analysis.

C.6 Sample Weighting

We use entropy balancing (Hainmueller 2012) to construct individual weights to match sample moments of a selected set of demographic variables. Specifically, we construct weights to match the ACS one-year mean estimates of the share of households below census median income (rounded median to nearest income bin edge in the Numerator Panel Data), the share of households with children, share of Black and White households, and the share of households living in the West, South, and Northeast regions of the United States. We construct the weights using the static panel (see row 2 of Table A6), separately for the approximately 15,000 households who are ever on SNAP (defined for this purpose as those who had any $S_{it} = 1$ during 2026) and for the approximately 100,000 non-SNAP households (defined, as in the rest of the analysis, as those who had no EBT receipt during the 18-month analysis period from January 2025 through June 2026). We use all 50 states and D.C. to create the weights (i.e., the treatment, control, and excluded states in Table 1).

Appendix Tables A7 and A8 show (for ever-on-SNAP and non-SNAP households, respectively) that we match the respective ACS 2024 one-year means exactly (by construction). Figure A11 presents the distribution of the demographic weights.

Table A7: Average Household Characteristics among Ever-on-SNAP Households

	Households ever on SNAP		ACS Mean
	Unweighted mean (1)	Weighted mean (2)	(3)
Panel (a): Matched Characteristics			
Share below census median income	0.53	0.49	0.49
Household includes children	0.33	0.42	0.42
Race			
White	0.49	0.42	0.42
Black	0.23	0.24	0.24
Other	0.28	0.34	0.34
Census region			
Midwest	0.20	0.19	0.19
Northeast	0.18	0.21	0.21
South	0.37	0.36	0.36
West	0.26	0.23	0.23
Panel (b): Unmatched Characteristics			
Income	34,150	36,334	50,899
College graduate	0.62	0.62	0.14
Age	50.04	48.86	50.78
Household size	2.93	3.13	2.81

Notes: This table presents 2025 summary statistics for households 'ever on SNAP' in 2026 ($N = 16{,}126$). The highest income bin is '\$250,000+'; we top-code at \$300,000. The highest age bin is '65+'; we top-code at 70. The highest household size bin is '7+'; we top-code at 7. ACS means are for 2024 one-year estimates among households who report being on SNAP in the last 12 months. The table shows results based on all 50 states and D.C. since they are all used in creating the sample weights.

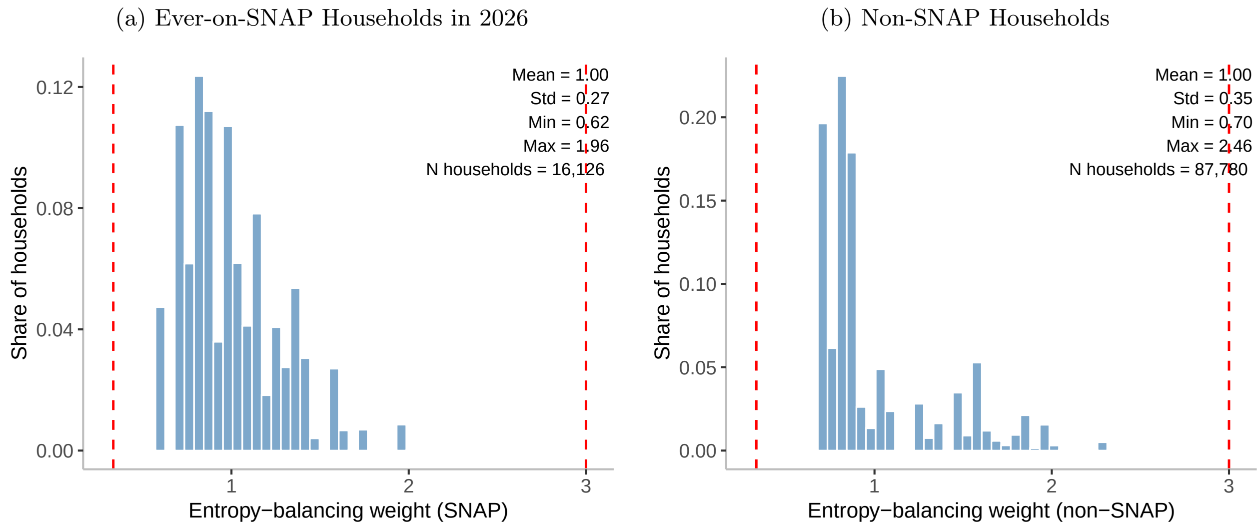
Table A8: Average Household Characteristics among Non-SNAP Households

	Non-SNAP households		ACS Mean
	Unweighted mean	Weighted mean	
	(1)	(2)	(3)
Panel (a): Matched Characteristics			
Share below census median income	0.47	0.51	0.51
Household includes children	0.22	0.25	0.25
Race			
White	0.78	0.65	0.65
Black	0.07	0.11	0.11
Other	0.15	0.24	0.24
Census region			
Midwest	0.25	0.22	0.22
Northeast	0.18	0.17	0.17
South	0.40	0.39	0.39
West	0.17	0.22	0.22
Panel (b): Unmatched Characteristics			
Income	108,904	105,300	120,888
College graduate	0.84	0.84	0.41
Age	52.84	51.59	52.01
Household size	2.60	2.68	2.31

Notes: This table presents 2025 summary statistics for Non-SNAP households (N = 87,780). The highest income bin is '$250,000+', which we top-code at $300,000. The highest age bin is '65+', which we top-code at 70. The highest household size bin is '7+', which we top-code at 7. ACS means are for 2024 one-year estimates among households who do not report being on SNAP in the last 12 months. Table shows results based on all 50 states and D.C. since they are all used in creating the sample weights.

Figure A11 presents the distribution of entropy balancing weights for SNAP and non-SNAP households. For downstream analyses, we had planned to winsorize the weights at $[1/3, 3]$ but, as shown by the vertical lines in the figure, this was non-binding.

Figure A11: Entropy Balancing Demographic Weights for Purchase Panel



Notes: This figure presents the entropy balancing distributions of the weights. Red dashed lines represent the winsorization threshold for the weights, which is non-binding given current distributions (i.e., no observation is winsorized at 1/3 or 3). Panel (a) presents the distribution for the households with $S_{it} = 1$ in any of the months during 2026 in the 18-month analysis sample. Panel (b) presents the distribution for households that have no EBT receipts during the all months of the analysis sample.

Table A9: Average Household Characteristics

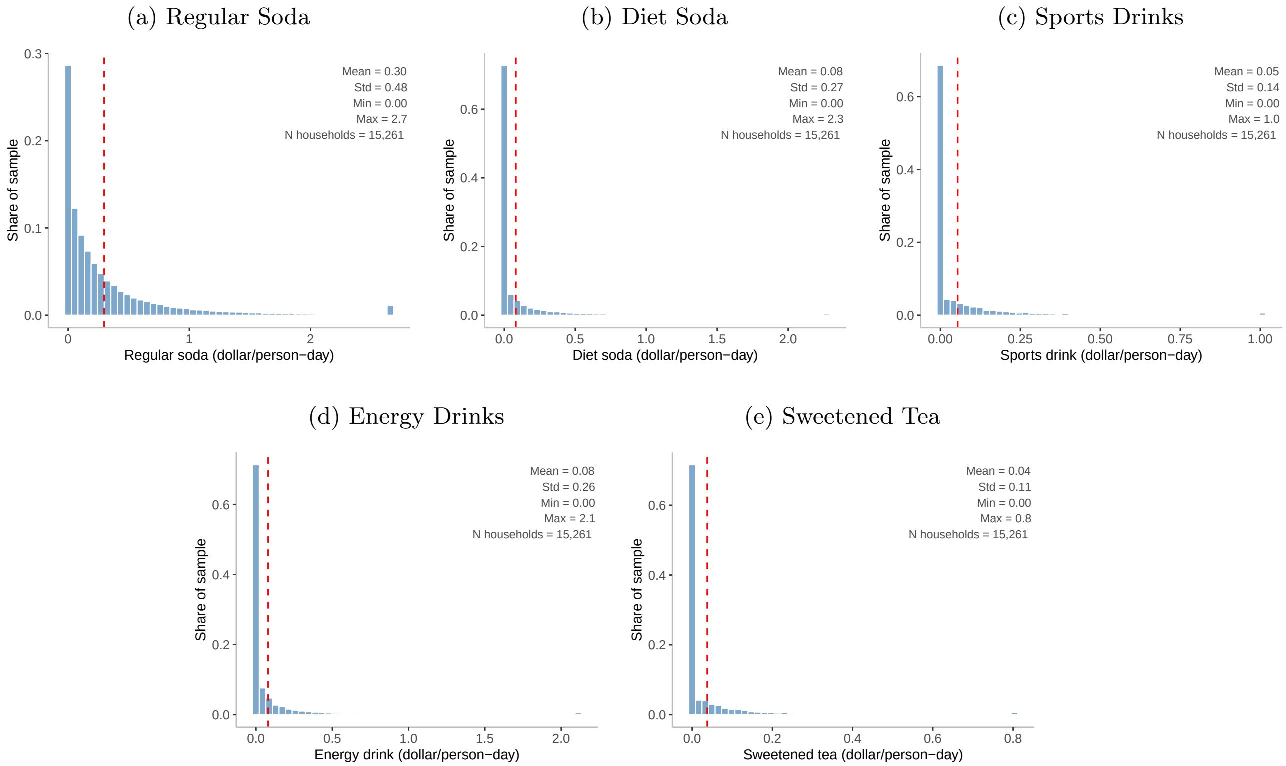
	Households ever on SNAP		Non-SNAP households		All households	
	Unweighted mean (1)	Weighted mean (2)	Unweighted mean (3)	Weighted mean (4)	Unweighted mean (5)	Weighted mean (6)
Income	34,150	36,334	108,904	105,300	97,302	95,405
Share below census median income	0.53	0.49	0.47	0.51	0.49	0.51
College graduate	0.62	0.62	0.84	0.84	0.80	0.80
Age	50.04	48.86	52.84	51.59	52.40	51.20
Household size	2.93	3.13	2.60	2.68	2.65	2.74
Household includes children	0.33	0.42	0.22	0.25	0.23	0.27
Race						
White	0.49	0.42	0.78	0.65	0.73	0.61
Black	0.23	0.24	0.07	0.11	0.09	0.13
Other	0.28	0.34	0.15	0.24	0.17	0.26
Census region						
Midwest	0.20	0.19	0.25	0.22	0.24	0.21
Northeast	0.18	0.21	0.18	0.17	0.18	0.18
South	0.37	0.36	0.40	0.39	0.40	0.39
West	0.26	0.23	0.17	0.22	0.18	0.22

Notes: This table presents summary statistics for households "ever on SNAP" (N = 16,126) and Non-SNAP households (N = 87,780) in all 50 states and D.C. We match on West, South, Northeast, White, Black, share below census median income (rounded median), and share of households with children. The highest income bin is '\$250,000+', which we top-code at \$300,000. The highest age bin is '65+', which we top-code at 70. The highest household size bin is '7+', which we top-code at 7.

C.7 Consumption and Spending Distributions

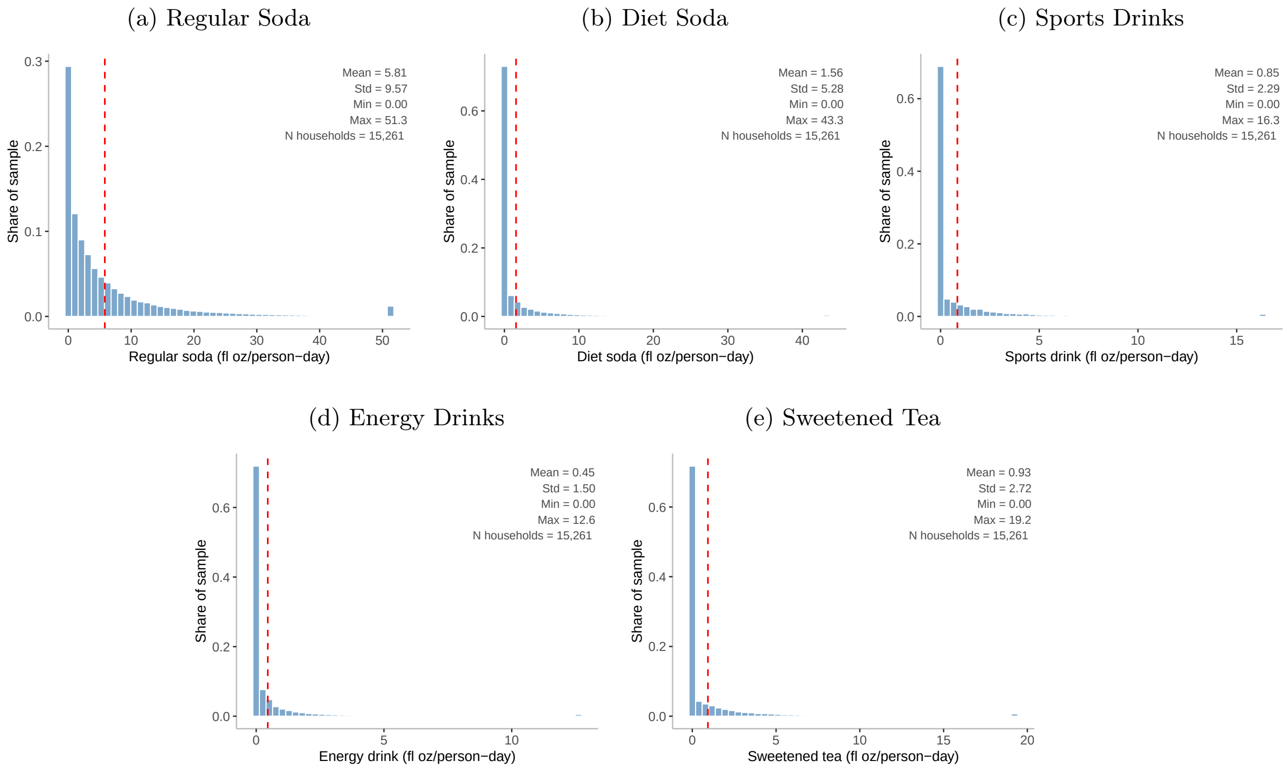
Figure A12 and A13 present the distributions of spending and quantity purchased for sweetened drinks in 2025, across household-month observations for SNAP households in the final analysis sample.

Figure A12: Retail Sweetened Drink Spending Distributions



Notes: This figure presents the distributions of retail sweetened drink spending by category in 2025, in dollars per person-day, for SNAP households in the final analysis sample (N = 15,261). The level of observation is household-month. The red dashed line represents the average of the distribution. All variables are winsorized at the 99.5th percentile.

Figure A13: Retail Sweetened Drink Consumption Distribution

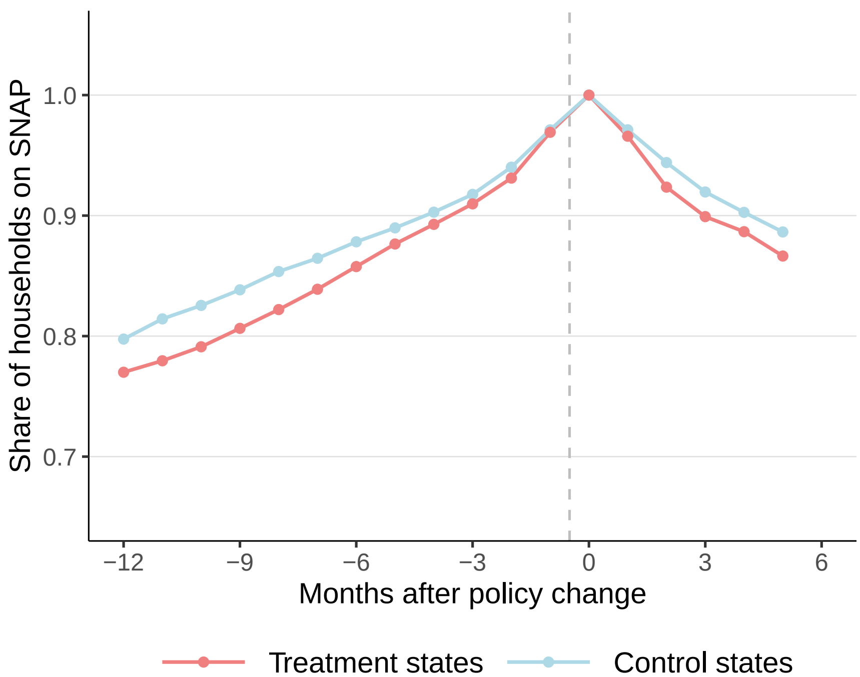


Notes: This figure presents the distributions of retail sweetened drink consumption by category in 2025, in fluid ounces per person-day, for SNAP households in the final analysis sample (N = 15,261). The level of observation is household-month. The red dashed line represents average of the distribution. All variables are winsorized at 99.5th percentile.

C.8 Analysis of SNAP Churn

Using data from 2024 and the first half of 2025 (i.e., prior to any SNAP restrictions which were first implemented starting January 1, 2026), we find that SNAP disenrollment was somewhat more frequent for SNAP households in (subsequent) treatment states than in control states (see Appendix Figure A14). Specifically, Appendix Figure A14 shows that for SNAP households (as of the 2025 placebo implementation date), 11.4 percent of those in control states and 13.4 percent of those in treatment states have disenrolled at event time 5 ($t = t_a^* + 5$), i.e. six months after the 'implementation' date, although this difference is not statistically significant (p-value = 0.2).

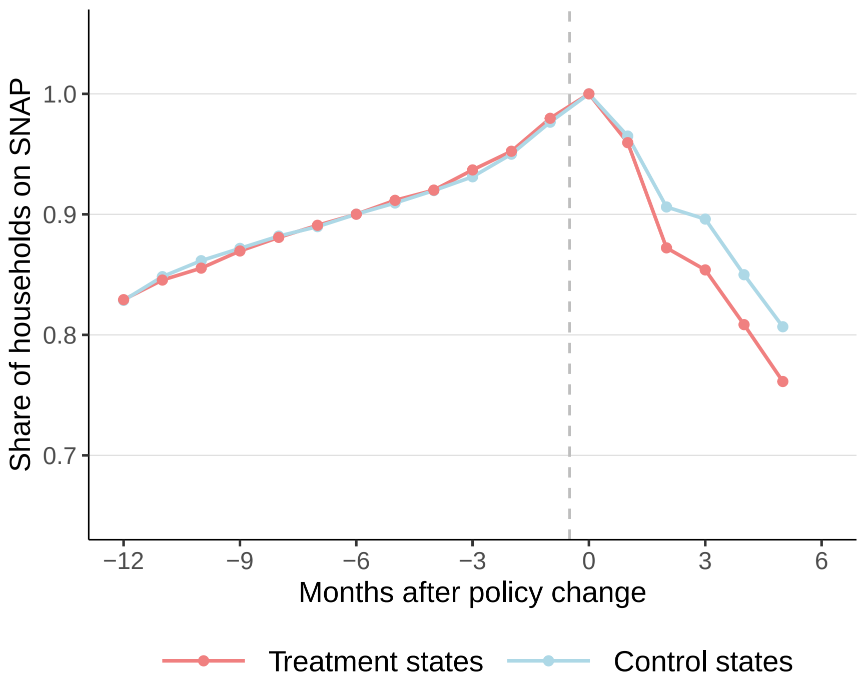
Figure A14: Placebo Analysis of Monthly SNAP Participation among SNAP Households (2024-2025)



Notes: This figure presents placebo analysis of the monthly SNAP enrollment rate in "event time" among SNAP households in treatment states and their respective stacked controls, weighting each cohort's controls to match its number of treated households. Specifically, it analyzes data from January 2024 through June 2025, assigning each cohort the implementation date in 2025 that it in fact received in 2026.

Appendix Figure A15 shows the same disenrollment analysis for SNAP households as the placebo analysis in Appendix Figure A14 but now using the actual (2026) implementation dates. Once again there is evidence of differential disenrollment: six months after implementation at event time 5 ($t = t_a^* + 5$), about 23.9 percent of SNAP households have disenrolled in treatment states, compared to 19.3 percent in control states, although this difference is also not statistically significant (p-value = 0.12).

Figure A15: Monthly SNAP Participation among SNAP Households (2025-2026)



Notes: This figure presents analysis of the monthly SNAP enrollment rate in event time among SNAP households in treatment states and their respective stacked controls, weighting each cohort's controls to match its number of treated households. Specifically, it analyzes data from 2025 through June 2026, assigning each cohort its 2026 implementation date.

To examine whether the implementation of the SNAP restrictions affected rates of disenrollment among SNAP households in the treatment states, we examine whether differential disenrollment for households in treatment versus control states differed in the 2024-2025 placebo analysis compared to the 2025-2026 actual analysis. Specifically, for both analyses we limit our regression sample to event time 5 and estimate

$$S_{ia,t=t_a^*+5} = \alpha + \tau P_{s,t=t_a^*+5} + \varepsilon_{ia,t=t_a^*+5}, \tag{10}$$

where S_{iat} is an indicator variable for whether household i in cohort a and calendar month t is enrolled in SNAP; recall that by definition $S_{iat} = 1$ in the cohort-specific implementation month $t = t_a^*$. As in the estimating equation (1), P_{st} is an indicator for whether a SNAP restriction is in place as of month t for treatment states s in cohort a.

We then combine these two analyses into a difference-in-differences analysis:

$$S_{ia,t=t_a^*+5} = \alpha + \tau P_{s,t=t_a^*+5} \cdot \mathbb{1}[\text{Panel 25-26}] + \gamma_1 P_{s,t=t_a^*+5} + \gamma_2 \mathbb{1}[\text{Panel 25-26}] + \varepsilon_{ia,t=t_a^*+5}, \tag{11}$$

where j denotes whether the observation is in the 2025-2026 panel or the 2024-2025 'placebo' panel. Appendix Table A10 shows the results. The first two columns show the results from estimating the single difference equation (10) for the 2024-2025 placebo panel (column (1)) and the 2025-2026 panel (column (2)). The final column shows the results from the difference-in-differences specification in equation (11). In both periods SNAP households in treatment states are more likely to disenroll six months after the start of the analysis, although differences between treatment and control are not statistically distinguishable. Column (3) shows that the difference in disenrollment rates grows (in absolute value) in the actual analysis period relative to the placebo analysis period; while this is consistent with the restrictions decreasing demand to remain enrolled in SNAP, the difference-in-differences analysis is not statistically significant (p-value = 0.25); SNAP households in treatment states are 2.5 percentage points (standard error = 2.2) less likely than SNAP households in control states to remain enrolled six months after implementation relative to before implementation.

Table A10: Effect of SNAP Restrictions on Churning of SNAP Recipients

	Placebo difference (1)	Actual difference (2)	difference-in-differences (3)
Treatment effect ($\hat{\tau}$)	-0.020	-0.045	-0.025
	(0.015)	(0.028)	(0.022)
Baseline year treatment average	0.913	0.938	
	(0.007)	(0.006)	
N	9,676	11,789	15,422

Notes: This table presents the estimate $\hat{\tau}$ of SNAP enrollment churning between treatment and control states. Columns (1) and (2) present estimates from equation (10) using event time 5 of 2024-2025 placebo data and 2025-2026 actual data for SNAP restriction analysis. Column (3) presents the estimate from equation (11). N represents the number of unique households in the sample. "Baseline year treatment average" gives the average enrollment rate over event time -5 through -1. Standard errors (clustered on state) are in parentheses.

D Survey Data Appendix

D.1 Survey Design and Analysis

Survey design. We contracted with Numerator to get survey responses from 18,000 households in its static panels. We deliberately over-sampled likely SNAP households. Specifically, we contracted for 13,000 respondents who were likely on SNAP (split 5,000 in treatment states defined based on implementation schedules at the time of fielding as Arkansas, Colorado, Florida, Idaho, Indiana, Iowa, Louisiana, Nebraska, Oklahoma, Texas, Utah, and West Virginia), 8,000 in control states, and 5,000 not on SNAP (split 50-50 between treatment and control states).

For purposes of the sampling scheme, we used the July 2024 and June 2025 static panels to create and send Numerator a list of "likely on SNAP" households defined as those who had any EBT receipts in 2 out of 3 months in each of the first and second quarters of 2025. We asked Numerator to select "not on SNAP" households as those who had no EBT receipts during the 12 months prior to November 2025, one month before the baseline survey was fielded.

Defining SNAP participation. For the survey analysis, we define "SNAP households" and "non-SNAP households" based on self-reports during the baseline survey, as the wording of some questions depended on that self-report. Specifically, the question asks "In the past 3 months, did you or any member of your household receive SNAP benefits? This includes any SNAP benefits, even if the amount is small and even if benefits are received on behalf of children in the household."

Appendix Table A5 shows the confusion matrix of self-reported SNAP participation and the definition of SNAP participation we use in the analysis of the Numerator purchase data (S_{it} as defined in Section C.4).

Sample weights. We construct a separate set of sample weights for the survey participants, following the same approach as for the full Numerator household purchase data (see Appendix C.6). We use entropy balancing (Hainmueller 2012) to construct individual weights that match sample moments of a selected set of demographic variables to the ACS 2024 one-year estimates. We use the following demographic variables: the share of households below census median income (rounded median to nearest income bin edge in Numerator Panel Data), the share of households with children, the share of Black and White households, and the share of households living in the West, South, and Northeast regions of the United States. We match ACS 2024 one-year estimates for likely on SNAP and non-SNAP households as defined for purposes of surveying (see above). For downstream analyses, we winsorize weights at $[1/3, 3]$ following our PAP (but this turned out to be non-binding).

Appendix Tables A11 and A12 present the weighted and unweighted demographics that match the respective ACS 2024 one-year estimates. Figure A16 presents the distribution of demographic weights.

Table A11: Average Household Characteristics among Likely SNAP Households in Baseline Survey

	Likely SNAP households		ACS
	Unweighted mean	Weighted mean	Mean
	(1)	(2)	(3)
Panel (a): Matched Characteristics			
Share below census median income	0.30	0.49	0.49
Household includes children	0.36	0.42	0.42
Race			
White	0.55	0.42	0.42
Black	0.21	0.24	0.24
Other	0.25	0.34	0.34
Census region			
Midwest	0.19	0.19	0.19
Northeast	0.17	0.21	0.21
South	0.43	0.36	0.36
West	0.21	0.23	0.23
Panel (b): Unmatched Characteristics			
Income	46,504	36,551	50,899
College graduate	0.66	0.61	0.14
Age	51.96	50.54	50.78
Household size	2.74	2.80	2.81

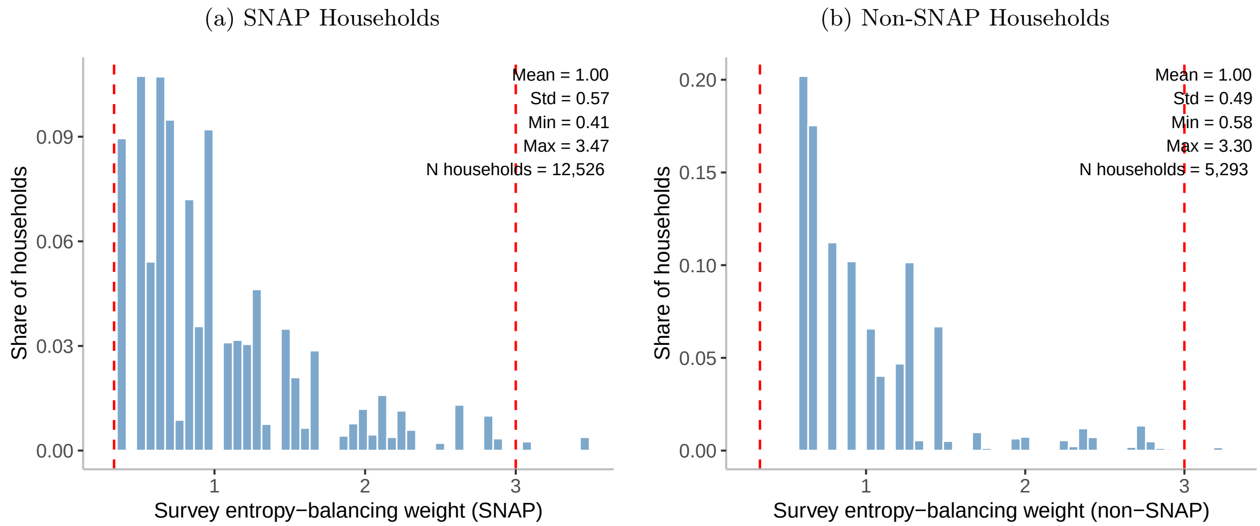
Notes: This table presents 2025 summary statistics for households likely on SNAP, who had any EBT receipts in 2 out of 3 months in each of first and second quarter of 2025 and took the baseline survey (N = 12,526). The highest income bin is '$125,000+', which we top-code at $150,000. ACS means are for 2024 one-year estimates among households who report being on SNAP in the last 12 months. Table shows results based on all 50 states and D.C. since they are all used in creating the sample weights.

Table A12: Average Characteristics among Non-SNAP Households in Baseline Survey

	Non-SNAP households		ACS Mean
	Unweighted mean	Weighted mean	
	(1)	(2)	(3)
Panel (a): Matched Characteristics			
Share below census median income	0.47	0.51	0.51
Household includes children	0.24	0.25	0.25
Race			
White	0.79	0.65	0.65
Black	0.06	0.11	0.11
Other	0.15	0.24	0.24
Census region			
Midwest	0.24	0.22	0.22
Northeast	0.13	0.17	0.17
South	0.50	0.39	0.39
West	0.13	0.22	0.22
Panel (b): Unmatched Characteristics			
Income	89,195	86,242	120,888
College graduate	0.83	0.84	0.41
Age	54.67	53.45	52.01
Household size	2.44	2.46	2.31

Notes: This table presents 2025 summary statistics for non-SNAP households, who had no EBT receipts during the 12 months prior to November 2025 and took the baseline survey ($N = 5{,}293$). The highest income bin is '\$125,000+'; we top-code at \$150,000. ACS means are for 2024 one-year estimates among households who report being on SNAP in the last 12 months. Table shows results based on all 50 states and D.C. since they are all used in creating the sample weights.

Figure A16: Entropy Balancing Demographic Weights for Baseline Survey



Notes: This figure presents the entropy balancing weight distributions. Red dashed lines represent the winsorization threshold for the weights. Panel (a) presents the distribution for the sampled households likely on SNAP, who had any EBT receipts in 2 out of 3 months in both the first and second quarters of 2025 and took the baseline survey. 0.6 percent of observations are winsorized at 1/3 or 3. Panel (b) presents the distribution for households who had no EBT receipts during past 12 months prior to November 2025 and took the baseline survey. 0.2 percent of observations are winsorized at 1/3 or 3.

Unscanned soda purchases. While our sample is limited to active panelists, it is understood that even these panelists may not successfully scan all of their purchases. For example, Einav, Leibtag, and Nevo (2008) document "under-scanning" in the NielsenIQ Homescan data. To measure under-scanning, the survey asked, "In the past week, how often did you drink regular or diet soft drinks (also called soda or pop) that you didn't scan in the Receipt Hog app? This includes soft drinks from restaurants, vending machines, stadiums, or any other soft drinks you didn't scan in the Receipt Hog app." Options included Never, 1 time, 2–3 times, 4–6 times, 1 time per day, 2 times per day, 3 times per day or more.

D.2 Variable Definitions

Drink more than should. Respondents were asked whether they drink soft drinks "more often than they should." We coded their answer as "yes" if they responded "Definitely," "Mostly," or "Somewhat" and "no" if they responded "Not at all."

Perceived health risk. Respondents were asked about how much drinking soft drinks "every day would increase your risk of health problems like type 2 diabetes or obesity." We coded their answer as "very little or none" if they responded "Not at all" or "Very little." We coded their answer as "somewhat to great deal" if they responded "Somewhat," "Quite a bit," or "A great deal."

MPC out of SNAP and cash. Respondents were asked how much more money they would spend on food if they received an additional \$100 in SNAP benefits or an additional \$100 in cash. We coded them as "higher MPC out of SNAP" if they reported more additional food spending after receiving \$100 in SNAP benefits than after receiving \$100 in cash; we coded them as "same or higher MPC out of cash" if they reported the same or more additional food spending after receiving \$100 in cash than after receiving \$100 in SNAP benefits.

D.3 Baseline Survey Responses

Appendix Figure A18 presents the distribution of baseline responses among (self-identified) SNAP and non-SNAP recipients. Panel (a) shows their answers regarding the frequency of unscanned soda purchases. The median (and mean) panelist reports drinking unscanned soft drinks 2.5 times (3.3 times) per week. Assuming that each drink is 12 ounces, the median and mean are 1.4 and 2.9 ounces/person-day, respectively. Comparing this to the statistics in Table 2, this suggests that unscanned consumption is 22.9 percent of total consumption.

D.4 Survey Instruments

D.4.1 Baseline Survey

Throughout the baseline survey, *[state-specific SNAP name]* was replaced with the name used for SNAP in the respondent's state. Before the first SNAP-related question, respondents were informed that SNAP is the Supplemental Nutrition Assistance Program, formerly known as the Food Stamp Program, and that benefits are provided on an electronic debit (EBT) card. All questions were single-response items.

Respondents were randomly assigned with equal probability to two survey versions. Group A received the policy-support question as Question 2, before the perceived-risk question, whereas Group B received the identical question as Question 13, after the time-preference question. For both versions, the policy-support response scale was randomly reversed. The displayed order of the Question 7 scale was flipped, the Question 8 scale was randomly reversed, and the two response options for Question 12 were randomized. For Questions 9–11, respondents saw either Question 10 or Question 11, depending on their reported SNAP participation. Questions 9—11 were displayed in random order.

Questions and response options.

1. **Frequency of unscanned soda consumption.**

   In the past week, how often did you drink regular or diet soft drinks (also called soda or pop) that you didn't scan in the Receipt Hog app? This includes soft drinks from restaurants, vending machines, stadiums, or any other soft drinks you didn't scan in the Receipt Hog app.

   Response options: Never; 1 time; 2–3 times; 4–6 times; 1 time per day; 2 times per day; 3 times per day or more.

2. **Support for restrictions—Group A version.** *Displayed only to Group A; response order randomly reversed.*

   Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. How much do you oppose or support this policy?

Response options: Strongly oppose; Somewhat oppose; Neither oppose nor support; Somewhat support; Strongly support.

3. Perceived risk of soda consumption.

How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?

Response options: Not at all; Very little; Somewhat; Quite a bit; A great deal.

4. SNAP participation.

In the past 3 months, did you or any member of your household receive *[state-specific SNAP name]* benefits? This includes any *[state-specific SNAP name]* benefits, even if the amount is small and even if benefits are received on behalf of children in the household.

Response options: Yes; No; Don't know.

5. Embarrassment—SNAP recipients. *Displayed only if Question 4 = Yes.*

In the past 3 months, how often did you feel judged when paying for groceries with your *[state-specific SNAP name]* benefits?

Response options: Never; Rarely; Sometimes; Often; Most or all of the time; I did not pay for groceries with my *[state-specific SNAP name]* benefits in the past 3 months.

6. Embarrassment—non-SNAP recipients. *Displayed only if Question 4 = No.*

In the past 3 months, how often do you think *[state-specific SNAP name]* participants felt judged when paying for groceries with their *[state-specific SNAP name]* benefits?

Response options: Never; Rarely; Sometimes; Often; Most or all of the time.

7. Perceived social stigma. *Displayed on the same page as Question 8; scale order flipped.*

The *[state-specific SNAP name]* policies currently in place in my state are disrespectful toward people on *[state-specific SNAP name]*.

Response options: Strongly disagree; Somewhat disagree; Neither disagree nor agree; Somewhat agree; Strongly agree.

8. Perceived agency. *Displayed on the same page as Question 7; scale order randomly reversed.*

The *[state-specific SNAP name]* policies currently in place in my state take away personal freedom.

Response options: Strongly disagree; Somewhat disagree; Neither disagree nor agree; Somewhat agree; Strongly agree.

9. **Mental accounting—cash.**

Imagine that you received an additional $100 in cash each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

10. **Mental accounting—SNAP benefits, SNAP recipients.** *Displayed only if Question 4 = Yes*

Imagine that in addition to your current benefit, you received an extra $100 in *[state-specific SNAP name]* benefits each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

11. **Mental accounting—SNAP benefits, non-SNAP recipients.** *Displayed only if Question 4 = No*

Imagine that you received $100 in *[state-specific SNAP name]* benefits each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

12. **Time preferences.** *Response order randomized.*

Imagine that you could choose to receive $100 today or $110 one month from now. Which would you prefer?

Response options: $100 today; $110 one month from now.

13. **Support for restrictions—Group B version.** *Displayed only to Group B; identical to Question 2; response order randomly reversed.*

Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. How much do you oppose or support this policy?

Response options: Strongly oppose; Somewhat oppose; Neither oppose nor support; Somewhat support; Strongly support.

14. **Self-control.**

Please indicate how much the next statement reflects how you typically are: "I drink soft drinks, soda, or pop more often than I should."

Response options: Definitely; Mostly; Somewhat; Not at all.

D.4.2 Endline Survey

The endline survey was distributed to respondents who completed the baseline survey. It repeated a subset of the baseline questions; specifically, we removed survey questions on time preferences and self-control. We added questions about respondents' reasons for supporting or opposing the restrictions and, among current SNAP recipients, monthly SNAP benefits and out-of-pocket grocery spending (grocery spending excluding SNAP benefits used).

Respondents retained their baseline survey assignment. Group A received the policy-support question as Question 2, whereas Group B received the identical question as Question 13. The policy-support response scale was randomly reversed. The response options for the policy-reasoning questions were randomized, except that the open-ended "Other" option remained last. The Question 7 scale was flipped, and the Question 8 scale was randomly reversed. Baseline Questions 12 and 14 were omitted from the endline survey. For Questions 9–11, respondents saw either Question 10 or Question 11, depending on their reported SNAP participation. These questions were displayed in random order at baseline, but displayed in the order shown below at endline.

Before each respondent's first SNAP-related question, respondents were informed that SNAP is the Supplemental Nutrition Assistance Program, formerly known as the Food Stamp Program, and that *[state-specific SNAP name]* benefits are provided on an electronic debit (EBT) card.

Questions and response options.

1. **Frequency of unscanned soda consumption.**

In the past week, how often did you drink regular or diet soft drinks (also called soda or pop) that you didn't scan in the Receipt Hog app? This includes soft drinks from restaurants, vending machines, stadiums, or any other soft drinks you didn't scan in the Receipt Hog app.

Response options: Never; 1 time; 2–3 times; 4–6 times; 1 time per day; 2 times per day; 3 times per day or more.

2. **Support for restrictions—Group A version.** *Displayed only to Group A; response order randomly reversed.*

Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. How much do you oppose or support this policy?

Response options: Strongly oppose; Somewhat oppose; Neither oppose nor support; Somewhat support; Strongly support.

2_A. **Policy reasoning—Group A version.** *Displayed after Question 2. Select all that apply; response order randomized except for "Other."*

Again, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. You indicated that you *[Question 2 response]* these policies. Select the most important reasons for your position.

- Eating healthier is in people's own best interest. *[Shown if neutral or support.]*
- Eating healthier reduces health care costs, which saves taxpayers money. *[Shown if neutral or support.]*
- Even if these policies do not change what people buy, the government should not pay for unhealthy groceries. *[Shown if neutral or support.]*
- These policies could help people realize that soft drinks and candy are unhealthy. *[Shown if neutral or support.]*
- It's important to let people make their own decisions about what to eat and drink. *[Shown if oppose or neutral.]*
- These policies could make *[state-specific SNAP name]* recipients feel judged or disrespected. *[Shown if oppose or neutral.]*
- It could be hard for stores to implement these policies. *[Shown if oppose or neutral.]*
- These policies make *[state-specific SNAP name]* more complicated for recipients. *[Shown if oppose or neutral.]*
- I don't think these policies will change what people buy. *[Shown if oppose or neutral.]*
- Other, please specify: ______________________________.

3. **Perceived risk of soda consumption.**

How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?

Response options: Not at all; Very little; Somewhat; Quite a bit; A great deal.

4. **SNAP participation.**

In the past 3 months, did you or any member of your household receive *[state-specific SNAP name]* benefits? This includes any *[state-specific SNAP name]* benefits, even if the amount is small and even if benefits are received on behalf of children in the household.

Response options: Yes; No; Don't know.

4_A. **Monthly SNAP benefit amount.** *Displayed only if Question 4 = Yes.*

In the most recent month that your household received *[state-specific SNAP name]* benefits, how many dollars of benefits did your household receive? If you don't know exactly, please give your best guess.

Response options: Open numeric response in dollars.

4_A1. **Confirmation of SNAP benefit amount.** *Displayed only if Question 4 = Yes.*

You responded that, in the most recent month that your household received *[state-specific SNAP name]* benefits, your household received $*[response to Question 4_A]* in benefits. If that is correct, please click "Yes." If it is incorrect, please click "Back" and enter the correct number.

Response options: Yes.

4_B. **Out-of-pocket grocery spending.** *Displayed only if Question 4 = Yes.*

For many households, *[state-specific SNAP name]* benefits cover only part of their grocery spending, so they also need to spend some of their own money on groceries. In the most recent month that your household received *[state-specific SNAP name]* benefits, how much of your household's own money did you spend on groceries? If you don't know exactly, please give your best guess. If your household did not use any of its own money for groceries, enter 0.

Response options: Open numeric response in dollars.

4_B1. **Confirmation of out-of-pocket spending.** *Displayed only if Question 4 = Yes.*

You responded that, in the most recent month that your household received *[state-specific SNAP name]* benefits, your household spent $*[response to Question 4_B]* of its own money on groceries. If that is correct, please click "Yes." If it is incorrect, please click "Back" and enter the correct number.

Response options: Yes.

5. **Embarrassment—SNAP recipients.** *Displayed only if Question 4 = Yes.*

In the past 3 months, how often did you feel judged when paying for groceries with your *[state-specific SNAP name]* benefits?

Response options: Never; Rarely; Sometimes; Often; Most or all of the time; I did not pay for groceries with my *[state-specific SNAP name]* benefits in the past 3 months.

6. **Embarrassment—non-SNAP recipients.** *Displayed only if Question 4 = No.*

In the past 3 months, how often do you think *[state-specific SNAP name]* participants felt judged when paying for groceries with their *[state-specific SNAP name]* benefits?

Response options: Never; Rarely; Sometimes; Often; Most or all of the time.

7. **Perceived social stigma.** *Displayed on the same page as Question 8; scale order flipped.*

The *[state-specific SNAP name]* policies currently in place in my state are disrespectful toward people on *[state-specific SNAP name]*.

Response options: Strongly disagree; Somewhat disagree; Neither disagree nor agree; Somewhat agree; Strongly agree.

8. **Perceived agency.** *Displayed on the same page as Question 7; scale order randomly reversed.*

The *[state-specific SNAP name]* policies currently in place in my state take away personal freedom.

Response options: Strongly disagree; Somewhat disagree; Neither disagree nor agree; Somewhat agree; Strongly agree.

11. **Mental accounting—cash.**

Imagine that you received an additional $100 in cash each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

9. **Mental accounting—SNAP benefits, SNAP recipients.** *Displayed only if Question 4 = Yes.*

Imagine that, in addition to your current benefit, you received an extra $100 in *[state-specific SNAP name]* benefits each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

10. **Mental accounting—SNAP benefits, non-SNAP recipients.** *Displayed only if Question 4 = No.*

Imagine that you received $100 in *[state-specific SNAP name]* benefits each month. How much more money would you spend on food each month as a result of this change?

Response options: $0; $10; $20; $30; $40; $50; $60; $70; $80; $90; $100.

13. **Support for restrictions—Group B version.** *Displayed only to Group B; identical to Question 2; response order randomly reversed.*

Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. How much do you oppose or support this policy?

Response options: Strongly oppose; Somewhat oppose; Neither oppose nor support; Somewhat support; Strongly support.

13_A. **Policy reasoning—Group B version.** *Displayed after Question 13. Select all that apply; response order randomized except for "Other."*

Again, some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using *[state-specific SNAP name]* benefits. You indicated that you *[Question 13 response]* these policies. Select the most important reasons for your position.

- Eating healthier is in people's own best interest. *[Shown if neutral or support.]*
- Eating healthier reduces health care costs, which saves taxpayers money. *[Shown if neutral or support.]*
- Even if these policies do not change what people buy, the government should not pay for unhealthy groceries. *[Shown if neutral or support.]*
- These policies could help people realize that soft drinks and candy are unhealthy. *[Shown if neutral or support.]*
- It's important to let people make their own decisions about what to eat and drink. *[Shown if oppose or neutral.]*
- These policies could make *[state-specific SNAP name]* recipients feel judged or disrespected. *[Shown if oppose or neutral.]*
- It could be hard for stores to implement these policies. *[Shown if oppose or neutral.]*
- These policies make *[state-specific SNAP name]* more complicated for recipients. *[Shown if oppose or neutral.]*
- I don't think these policies will change what people buy. *[Shown if oppose or neutral.]*
- Other, please specify: ____________________________.

E Deviations from Pre-Analysis Plan

The pre-analysis plan is available from osf.io/5nf3b. The PAP provided shells of key tables and figures, populated with pre-treatment data. In preparing this manuscript, we modified those tables and figures in the following ways:

- Table 1: Added labels grouping states by implementation date.
- Table 3: Re-ordered the columns. Figures 3 and A15: re-ordered the panels.
- Figures 2 and A15: removed the confidence intervals.
- Figure 3: Changed the statistics below each panel to be "Post estimates avg. – Pre estimates avg."
- Table 6: Re-ordered the columns.
- Table 5: Added p-values of differences between each subgroup pair.
- Table A9: Added columns (3) and (4) for "Non-SNAP households."
- Figure A19, panel (b): Added bar for "Won't change what people buy," as this response option was added to the survey plan after the PAP was submitted.
- Table A18: Re-ordered the columns.

F Purchase Effects Appendix

Table A13: First Stage

	(1)	(2)
P_{at}	0.922 (0.006)	0.923 (0.006)
S_{it}		0.107 (0.023)
N	15,261	15,261

Notes: This table presents estimates of the first stage of the instrumental variables regression in equation (3). P_{at} is an indicator for whether SNAP restrictions have been implemented in cohort a as of time t, and S_{it} is an indicator for household i's SNAP participation at time t. The first stage regression is $P_{at}S_{it} = \tau P_{at}\big(+\sigma S_{it}\big) + \lambda_{ia} + \lambda_{at} + \varepsilon_{iat}$. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample. Column (1) is the alternative estimate without the S_{it} control, corresponding to column (2) of Table 6. Column (2) is the primary estimate, corresponding to column (1) of Table 3. In both columns, the excluded instrument is P_{at}. F-statistics are > 100 for P_{at} in both columns.

Table A14: Effects of SNAP Restrictions by Retail Category

	Excluded drinks (1)	Regular soda (2)	Diet soda (3)	Sports drinks (4)	Energy drinks (5)	Sweetened tea (6)
Treatment effect ($\hat{\tau}$)	-0.124	-0.125	-0.089	-0.213	-0.169	-0.148
	(0.010)	(0.014)	(0.016)	(0.020)	(0.037)	(0.028)
2025 treatment average	8.91	6.05	1.55	0.87	0.42	0.89
(fl oz/person-day)	(0.78)	(0.33)	(0.17)	(0.02)	(0.07)	(0.03)
N	15,261	15,261	15,261	13,621	15,189	13,621

Notes: This table presents estimates of the effects of SNAP restrictions on consumption of retail drinks by drink category, estimated using equation (3). For these regressions, the post-treatment indicator P_{at} is constructed to match restrictions for the category in each specific column; because different states ban different combinations of subcategories, the 2025 treatment average of each subcategory in columns (2) through (6) does not sum to treatment average for excluded drinks in column (1). Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Table A15: Effects of SNAP Restrictions on Retail Excluded Drink Consumption with Alternative Constructions of S_{it}

	S_{it}: [min material EBT share, min share months with EBT]				
	[0.2, 0.5] (1)	[0.1, 0.5] (2)	[0.3, 0.5] (3)	[0.2, 0.3] (4)	[0.2, 0.7] (5)
Treatment effect ($\hat{\tau}$)	-0.124	-0.113	-0.144	-0.125	-0.123
	(0.010)	(0.009)	(0.009)	(0.010)	(0.011)
2025 treatment average	8.91	9.01	8.67	8.90	8.94
(fl oz/person-day)	(0.78)	(0.80)	(0.75)	(0.78)	(0.79)
N	15,261	18,884	11,995	15,284	15,020

Notes: This table presents estimates of the effect of SNAP restrictions on retail excluded drink consumption, estimated using equation (3). Column (1) presents the primary estimate, from column (1) of Table 3. Columns (2) through (5) present the effect under alternative constructions of S_{it}. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Table A16: Effects of SNAP Restrictions on Retail Excluded Drink Consumption among Baseline Survey Respondents

	Analysis sample for purchase effects (1)	Baseline survey respondents (2)
Treatment effect ($\hat{\tau}$)	-0.124	-0.122
	(0.010)	(0.012)
2025 treatment average (fl oz/person-day)	8.91	10.08
	(0.78)	(0.79)
EBT grocery spending ($/household-month)	242.70	269.00
Household income	35,990	33,525
Age	48.89	50.85
College graduate	0.62	0.62
Household size	3.12	2.93
Household includes children	0.42	0.39
N	15,261	5,551

Notes: This table presents estimates of the effect of SNAP restrictions on retail excluded drink consumption, estimated using equation (3). Column (1) presents the primary estimate, from column (1) of Table 3. Column (2) limits the sample to baseline survey respondents. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Figure A17: Confusion Matrix: Subgroup Indicators

	Baseline quantity: above−median	Baseline share groceries on EBT: above−median	Baseline share excluded drinks on EBT: above−median	Diabetic or pre−diabetic	Drink more than should	Perceived health risk: very little or none	Higher MPC out of SNAP
Baseline quantity: above−median	n=7,729	46.7% (n=3,611)	41.9% (n=3,236)	22.6% (n=1,747)	27.3% (n=2,112)	13.6% (n=1,054)	13.5% (n=1,043)
Baseline share groceries on EBT: above−median	47.3% (n=3,611)	n=7,631	71.7% (n=5,468)	18.4% (n=1,406)	22.0% (n=1,679)	12.4% (n= 943)	13.4% (n=1,025)
Baseline share excluded drinks on EBT: above−median	47.6% (n=3,236)	80.4% (n=5,468)	n=6,805	18.8% (n=1,276)	22.4% (n=1,527)	12.2% (n= 833)	13.1% (n= 894)
Diabetic or pre−diabetic	56.2% (n=1,747)	45.3% (n=1,406)	41.1% (n=1,276)	n=3,107	29.5% (n= 916)	13.6% (n= 422)	15.3% (n= 474)
Drink more than should	65.5% (n=2,112)	52.1% (n=1,679)	47.3% (n=1,527)	28.4% (n= 916)	n=3,225	30.5% (n= 983)	32.4% (n=1,045)
Perceived health risk: very little or none	61.2% (n=1,054)	54.8% (n= 943)	48.4% (n= 833)	24.5% (n= 422)	57.1% (n= 983)	n=1,721	31.8% (n= 548)
Higher MPC out of SNAP	56.7% (n=1,043)	55.7% (n=1,025)	48.6% (n= 894)	25.7% (n= 474)	56.8% (n=1,045)	29.8% (n= 548)	n=1,841

Share of row subgroup: 100%, 75%, 50%, 25%

Diagonal: N of users in the row subgroup. Off−diagonal: Share of the row subgroup (N / row diagonal N). N = 15,261.

Notes: This figure presents the confusion matrix between indicators for median baseline excluded drink consumption, grocery share on EBT, excluded drink share on EBT, and diabetes status, in SNAP households in Numerator sample, as well as survey indicators for survey questions: drinking more than should, perceived very little or no health risk from soda, and higher MPC out of SNAP compared to cash.

G Survey Results Appendix

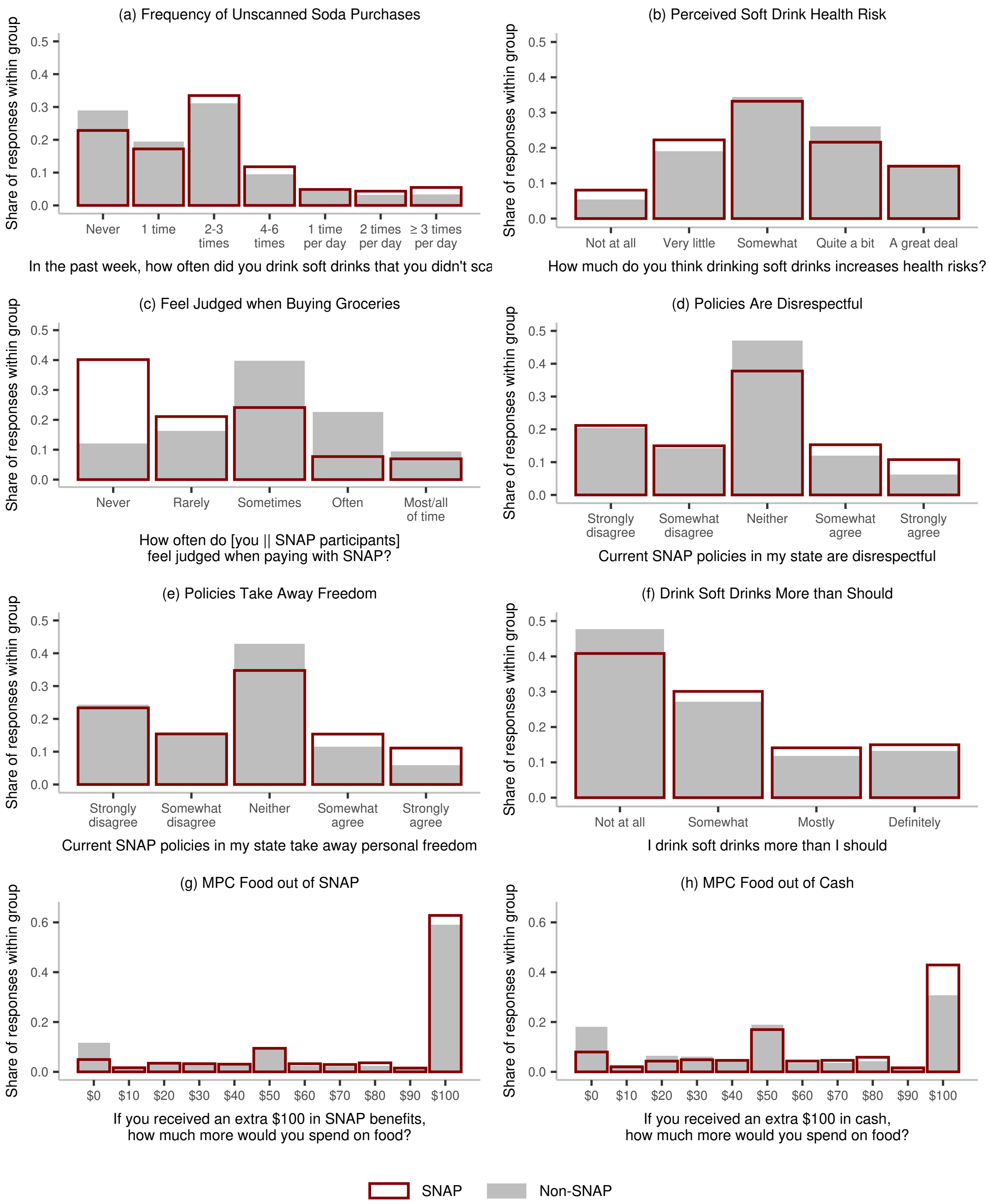
Table A17 presents estimates of the effects of SNAP restrictions on unscanned purchases.

Table A17: Effects on Unscanned Soft Drink Purchases

	Unscanned soft drinks (fl oz/person-day)
Treatment effect ($\hat{\tau}$)	0.009
	(0.249)
2025 treatment average	3.32
	(0.25)
$\hat{\tau}$/Baseline	0.003
	(0.075)
N	4,456

Notes: This table presents the effects of SNAP restrictions on survey reports of unscanned soft drink purchases, estimated using equation (5). The sample is limited to households that reported on the baseline survey that they had received SNAP benefits in the past three months. The survey question was, "In the past week, how often did you drink regular or diet soft drinks (also called soda or pop) that you didn't scan in the Receipt Hog app? This includes soft drinks from restaurants, vending machines, stadiums, or any other soft drinks you didn't scan in the Receipt Hog app." Response options were "Never," "1 time," "2–3 times," "4–6 times," "1 time per day," "2 times per day," and "3 times per day or more." These were translated into fluid ounces assuming 12 fluid ounces per drink. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

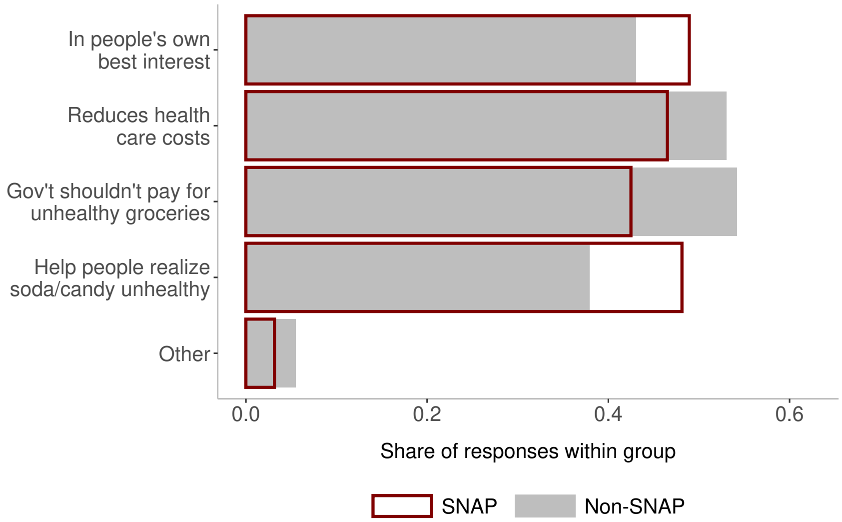
Figure A18: Distributions of Baseline Survey Responses



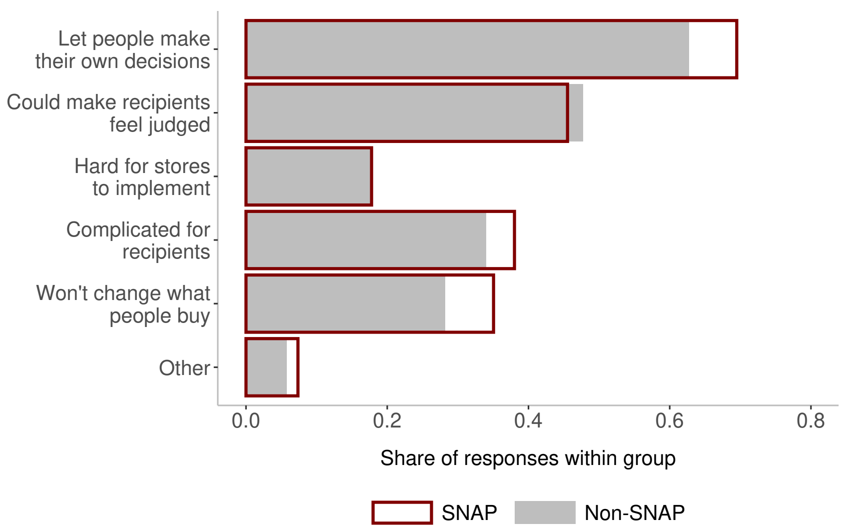
Notes: This figure presents the distribution of responses for eight key questions from our baseline survey. Households are classified as "SNAP" (N = 12,526) versus "Non-SNAP" (N = 5,293) based on whether they reported receiving SNAP benefits in the past three months.

Figure A19: Policy Reasoning

(a) Reasons for Supporting Policy



(b) Reasons for Opposing Policy



Notes: Our endline survey asked respondents their most important reasons for supporting or opposing SNAP restrictions. Panel (a) presents the distribution of responses for people who supported these policies. Panel (b) presents the distribution of responses for people who opposed these policies. Households are classified as "SNAP" (N = 4,425) versus "Non-SNAP" (N = 4,340) based on whether they reported receiving SNAP benefits in the past three months. Among self-reported SNAP recipients, 1,492 supported the policy, and 1,957 respondents opposed the policy. Among self-reported non-SNAP recipients, 2,422 supported the policy, and 1,052 respondents opposed the policy.

Table A18: Effects on Perceived Health Risk, Stigma, Agency, and Policy Support in Ordinal Scale

	Policy support (1)	Perceived health risk (2)	Felt judged (3)	Policies disrespectful (4)	Policies take away freedom (5)
Treatment effect ($\hat{\tau}$)	0.047	-0.034	0.126	0.156	0.217
	(0.048)	(0.044)	(0.035)	(0.067)	(0.066)
2025 treatment average	2.90	3.04	2.14	2.95	3.00
	(0.06)	(0.02)	(0.05)	(0.06)	(0.08)
N	4,456	4,456	4,297	4,456	4,456

Notes: This table presents the effects of SNAP restrictions on perceived health risk, stigma, agency, and policy support, estimated using equation (5). The sample is limited to households that reported on the baseline survey that they had received SNAP benefits in the past three months. For column (1), the survey question was "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?" Response options were "Strongly oppose," "Somewhat oppose," "Neither oppose nor support," "Somewhat support," "Strongly support," which were coded as 1-5, respectively. For column (2), the survey question was "How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?" Response options were "Not at all," "Very little," "Somewhat," "Quite a bit," and "A great deal," which were coded as 1-5, respectively. For column (3), the survey question for SNAP households was "In the past 3 months, how often did you feel judged when paying for groceries with your SNAP benefits?" Response options were "Never," "Rarely," "Sometimes," "Often," "Most or all of the time," which were coded as 1-5, respectively. For column (4), the survey question was "The SNAP policies currently in place in my state are disrespectful toward people on SNAP." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. For column (5), the survey question was "The SNAP policies currently in place in my state take away personal freedom." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

Appendix Table A19 shows impacts on perceptions separately for respondents who were asked about their support for the SNAP restrictions (and thus reminded of the policies) at the beginning or the end of the survey. See Appendix D.4 for the survey instrument.

Table A19: Effects on Perceived Health Risk, Stigma, Agency, and Policy Support by Randomization Group

	Policy support (1)	Perceived health risk (2)	Felt judged (3)	Policies disrespectful (4)	Policies take away freedom (5)
Panel (a): Reminded about SNAP Restriction Policies at *Beginning* of Survey					
Treatment effect ($\hat{\tau}$)	0.024	-0.014	0.100	0.166	0.239
	(0.038)	(0.046)	(0.052)	(0.076)	(0.064)
N	2,197	2,197	2,115	2,197	2,197
Panel (b): Reminded about SNAP Restriction Policies at *End* of Survey					
Treatment effect ($\hat{\tau}$)	0.043	-0.047	0.100	0.082	0.100
	(0.044)	(0.057)	(0.045)	(0.051)	(0.069)
N	2,259	2,259	2,182	2,259	2,259

Notes: This table presents the effects of SNAP restrictions on perceived health risk, stigma, agency, and policy support, estimated using equation (5). The sample is limited to households that reported on the baseline survey that they had received SNAP benefits in the past three months. For column (1), the survey question was "Some states have proposed or put in place policies that remove items like soft drinks and candy from the list of things that can be bought using SNAP benefits. How much do you oppose or support this policy?" Response options were "Strongly oppose," "Somewhat oppose," "Neither oppose nor support," "Somewhat support," "Strongly support," which were coded as 1-5, respectively. For column (2), the survey question was "How much do you think drinking 1 serving of soft drinks, soda, or pop every day would increase your risk of health problems like type 2 diabetes or obesity?" Response options were "Not at all," "Very little," "Somewhat," "Quite a bit," and "A great deal," which were coded as 1-5, respectively. For column (3), the survey question for SNAP households was "In the past 3 months, how often did you feel judged when paying for groceries with your SNAP benefits?" Response options were "Never," "Rarely," "Sometimes," "Often," "Most or all of the time," which were coded as 1-5, respectively. For column (4), the survey question was "The SNAP policies currently in place in my state are disrespectful toward people on SNAP." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. For column (5), the survey question was "The SNAP policies currently in place in my state take away personal freedom." Response options were "Strongly disagree," "Somewhat disagree," "Neither disagree nor agree," "Somewhat agree," "Strongly agree," which were coded as 1-5, respectively. Each of those survey outcomes is standardized by the baseline period mean and standard deviation before splitting into two randomization groups. Standard errors (clustered on state) are in parentheses. N represents unique households in the sample.

H Consumer Welfare Appendix

H.1 Data and Estimation

H.1.1 Data

As the sample for the consumer welfare simulations, we use the 18,424 Numerator households in the January 2025 to June 2026 nationwide balanced panel that have $S_{it} = 1$ for at least one month in 2025.

Estimated sugary drink consumption. We define good s as all sugary drinks: regular soda, sports drinks, energy drinks, sweetened tea, and fruit drinks excluding 100% fruit juice. While we are able to directly observe expenditures that fall in the first four categories in the Numerator sample, we cannot cleanly separate sweetened fruit drinks from 100% juice with the Numerator Panel Data alone. In order to get an estimate of household expenditures on sugary fruit drinks, we calculate the estimated share of all fruit drink consumption that is sweetened fruit drinks. To find this share, we use reported dietary recalls from participants in the 2021-2023 National Health and Nutrition Examination Survey (NHANES). As participants who complete a dietary recall report the specific kinds of foods and beverages they consume, as well as the quantity consumed and where they obtained each item from, we are able to estimate what percent of total fruit drink consumption consists of sugary fruit drink consumption. For this exercise, we filter the 2021-2023 NHANES sample to participants who completed a day 1 dietary recall and reported living in a household that receives SNAP, giving us a final sample of 1,337 respondents.

For each participant, we filter their reported consumption to fruit drinks that were purchased in a grocery store, convenience store, or non-specified store. We identify fruit drinks by matching each reported item's code to the 2021-2023 Food and Nutrient Database for Dietary Studies (FNDDS) (USDA 2024a). To determine which codes constituted a sweetened fruit drink or 100% juice, we use the description of the drink in the FNDDS. Next, for each participant, we find their total consumption of store-bought sugary fruit drinks $q_i^{\text{fruit drink}}$ and store-bought 100% juice $q_i^{\text{100\% juice}}$ in fluid ounces. Then, we find the average percentage of all consumed fruit drinks and juice that were sweetened fruit drinks with

$$\lambda = \frac{\sum_i \omega_i q_i^{\text{fruit drink}}}{\sum_i \omega_i (q_i^{\text{fruit drink}} + q_i^{\text{100\% juice}})}, \tag{12}$$

where ω_i denotes day 1 dietary recall weights. We find $\lambda = 0.5021$, or 50.21 percent of total fruit drink and juice consumption consisted of sugary fruit drinks. We then apply this percentage to the total imputed fruit drink quantities and expenditures for each SNAP household in the Numerator Panel Data. This assumes that the price of sugary fruit drinks is the same as the price of 100% juice. We then have that $q^0_{i,\text{fruit drinks}} = \lambda q^0_{i,\text{all fruit drinks}}$ in the Numerator sample.

Predicted SNAP benefit amounts. Calibrating the model also requires an estimate of per-person annual SNAP benefits b_i for each SNAP household in the Numerator sample. However, we do not observe SNAP benefit amounts in the Numerator Panel Data, so we must impute them based on each household's characteristics, such as their income, household size, and state.

We do this using SNAP Quality Control (QC) data for FY2024, which provides a sample that is representative of the population of SNAP recipients nationwide. The QC data are assembled every fiscal year by the QC System, and consist of data from case reviews that are performed by each state and territory's SNAP agencies. These reviews are done as part of efforts to evaluate the distribution of benefits and avoid payment errors. Households selected for review provide information such as household size, gross income, state of residence, and monthly benefits received (USDA 2026e). We use the FY2024 data, which is the most recently published data. As the Numerator sample is limited to households in the 50 U.S. states and Washington, D.C., we drop any households in the QC sample that live in Guam or the U.S. Virgin Islands. This leaves us with a sample of 44,416 SNAP households.

In a given month m in fiscal year t, each SNAP household's monthly benefits $B_{i,\text{month}}$ are determined with the following formula:

$$B_{i,\text{month}} = \text{MaxAllotment}_{st}(n_i) - 0.3 \times \text{NetIncome}_{i,\text{month}}, \tag{13}$$

where $\text{MaxAllotment}_{st}(n)$ is the maximum possible monthly SNAP benefit a household located in state s with n individuals can receive in year t, and $\text{NetIncome}_{i,\text{month}}$ is the household's monthly net income after deductions (USDA 2026c). Table A20 provides the maximum allotments for fiscal years 2024-2026.

Table A20: Maximum Allotment Schedules for the U.S., FY2024–2026

Fiscal year	Household size 1	2	3	4	5	6	7	8	Each additional household member
Panel (a): Contiguous U.S.									
FY2024	$291	$535	$766	$973	$1,155	$1,386	$1,532	$1,751	$219
FY2025	$292	$536	$768	$975	$1,158	$1,390	$1,536	$1,756	$220
FY2026	$298	$546	$785	$994	$1,183	$1,421	$1,571	$1,789	$218
Panel (b): Alaska Urban									
FY2024	$374	$686	$983	$1,248	$1,482	$1,778	$1,966	$2,246	$281
FY2025	$377	$692	$991	$1,258	$1,494	$1,793	$1,982	$2,265	$283
FY2026	$385	$707	$1,015	$1,285	$1,529	$1,838	$2,031	$2,314	$282
Panel (c): Alaska Rural I									
FY2024	$477	$875	$1,253	$1,591	$1,890	$2,268	$2,506	$2,865	$358
FY2025	$481	$882	$1,263	$1,604	$1,905	$2,287	$2,527	$2,888	$361
FY2026	$491	$901	$1,295	$1,639	$1,950	$2,344	$2,590	$2,950	$360
Panel (d): Alaska Rural II									
FY2024	$581	$1,065	$1,525	$1,937	$2,300	$2,760	$3,051	$3,487	$436
FY2025	$586	$1,074	$1,538	$1,953	$2,319	$2,783	$3,076	$3,516	$440
FY2026	$598	$1,097	$1,576	$1,995	$2,374	$2,853	$3,152	$3,591	$438
Panel (e): Hawaii									
FY2024	$527	$967	$1,385	$1,759	$2,088	$2,506	$2,770	$3,166	$396
FY2025	$517	$948	$1,357	$1,723	$2,046	$2,456	$2,714	$3,102	$388
FY2026	$506	$929	$1,334	$1,689	$2,010	$2,415	$2,668	$3,040	$371

Notes: This table presents the maximum allotments for fiscal years 2024 through 2026 for the United States excluding Guam and the U.S. Virgin Islands (USDA 2026b). The values for FY2024 in each panel are used to find r_i^{QC} for each observation in the FY2024 QC data. The values for FY2025 and FY2026 are used to find the imputed benefit b_i, as the fiscal year runs from October to September and the 2025 Numerator sample runs from January to December. We assume that any Numerator households located in Alaska receive benefits according to the schedule in panel (b).

To account for changes in the maximum allotments over time, rather than directly imputing SNAP benefits for each Numerator household, we instead impute a benefit ratio r_i, where

$$r_i = \frac{n_i b_i}{12\text{MaxAllotment}_{st}(n_i)}. \tag{14}$$

This allows us to impute per-person benefits b_i for each household while accounting for changes in the generosity of SNAP benefits over time.

In the QC data, we compute r_i^{QC} for each household by dividing their final calculated monthly benefit (not the reported benefit) by the calculated maximum allotment for the household:

$$r_i^{QC} = \frac{B_{i,\text{month}}}{\text{MaxAllotment}_{s,FY2024}(n_i)}. \tag{15}$$

Since we are only able to observe income buckets in the Numerator Panel Data, we group the QC households into comparable income buckets. We also group all households with incomes of \$80,000 and higher into one bucket within both the Numerator and QC samples. This is due to sparsity in the QC data for households with incomes of \$80,000 or higher. We also top-code all QC households with 7 or more people, to be consistent with the structure of the Numerator Panel Data.

We estimate the benefit ratio using a QC household-weighted fractional-logit model

$$\operatorname{logit}\Big(\mathbb{E}[r_i^{QC} \mid X_i]\Big) = \alpha + \sum_k \theta_k \mathbf{1}\{Y_i \in k\} + \sum_n \delta_n \mathbf{1}\{N_i = n\} + \sum_s \gamma_s \mathbf{1}\{S_i = s\}, \tag{16}$$

where Y_i is the household's income bucket, N_i is household size, and S_i is state of residence. We use the FYWGT variable as the weight for each observation in the QC data. Using a fractional logit model ensures that the predicted benefit ratio is always on the unit interval.

With the fitted model, we then estimate the benefit ratio $\widehat{r}_i$ for each Numerator SNAP household. Since the maximum allotments have changed since 2024, the fiscal year runs from October to September, and our sample consists of households on SNAP between January and December 2025, we impute the annual benefit per-person $\widehat{b}_i$ for each household with

$$\widehat{b}_i^{2025} = 12\frac{\widehat{r}_i}{n_i}\left(0.75\text{MaxAllotment}_{si}^{FY2025}(n_i) + 0.25\text{MaxAllotment}_{si}^{FY2026}(n_i)\right), \tag{17}$$

where the values of $\text{MaxAllotment}_{si}^{FY2025}(n)$ and $\text{MaxAllotment}_{si}^{FY2026}(n)$ can be found in Table A20. We assume that all Alaska households are located in urban areas.

Finally, for households of size 1 or 2, there are minimum allotments that vary by state (USDA 2026b). We clamp our estimates for these households to ensure that predicted benefits for households of these sizes do not fall below these minimums. In FY2025, this minimum was \$23 per month per household in the contiguous U.S. and D.C., \$30 in Alaska, and \$41 in Hawaii. In FY2026, this minimum was \$24 per month per household in the contiguous U.S. and D.C., \$31 in Alaska, and \$41 in Hawaii.

Summary statistics. Each household in the 2025 Numerator sample gives us pre-restriction household income, total expenditures for sugary drinks and other SNAP-eligible groceries, and household size. Using this, the sugary fruit drink share, and imputed benefits b, we construct pre-restriction household income per person w, sugary drink expenditure share $q_s^0 p_s/(w+b)$, other grocery expenditure share $q_g^0 p_g/(w+b)$, and excluded drink price p_s. We winsorize sugary drink and other grocery expenditure shares from the top at 0.1 and 0.5, respectively. We also impose a floor on grocery spending at $q_g \geq b$, which guarantees that the SNAP benefit is inframarginal. Panel (a) of Table A21 presents the averages of those five variables across Numerator SNAP households. Appendix Figure A20 presents the full distributions.

Table A21: Model Data and Parameters

Parameter	Description	Value
	Panel (a): Average Empirical Inputs	
w	Income ($/person-year)	12,763
b	SNAP benefit ($/person-year)	1,466
$\frac{q_s^0 p_s}{w+b}$	Sugary drink expenditure share	0.018
$\frac{q_g^0 p_g}{w+b}$	Other grocery expenditure share	0.249
p_s	Sugary drink price ($/fl oz)	0.06
τ	Average treatment effect (share of consumption)	-0.124
		(0.010)
	Panel (b): Parameter Assumed Common Across Households	
γ	Share of sugary drink consumption from internalities	1/3
	Panel (c): Averages of Household-Specific Utility Parameters	
$\tilde{\alpha}_s$	Decision utility sugary drink parameter	0.016
$\tilde{\alpha}_g$	Decision utility other grocery parameter	0.219
$\tilde{\alpha}_n$	Decision utility numeraire parameter	0.765
α_s	Normative utility sugary drink parameter	0.011
α_g	Normative utility other grocery parameter	0.220
α_n	Normative utility numeraire parameter	0.769
	Panel (d): Estimated Mental Accounting Parameter	
β	Mental accounting parameter	0.184
		(0.017)

Notes: Panel (a) presents the averages across households of observed or estimated parameters. The average treatment effect is from column (1) of Table 3. Panel (b) presents the assumed average share of sugary drink consumption caused by internalities, which is imported from Allcott, Lockwood, and Taubinsky (2019a). Panel (c) presents the averages across households of inferred utility parameters. Panel (d) presents the mental accounting parameter estimated via indirect inference. Standard errors are in parentheses in panels (a) and (d).

H.1.2 Estimation Appendix

As described in Section 6.1.3, we estimate the model in two steps. First, we use indirect inference to find the β that generates a simulated treatment effect equal to the primary estimate of $\widehat{\tau}$, while finding the household-specific $\tilde{\alpha}_i$ that match each Numerator household's expenditure shares implied by equations (6) and (7). Second, we infer each household's normative utility parameters α_i assuming the Allcott, Lockwood, and Taubinsky (2019a) estimate of the share of sugary drink consumption explained by internalities.

Step 1: indirect inference. We search over values of β to find the value such that the simulated average treatment effect (as a proportion of baseline consumption) matches the primary estimate from column (1) of Table 3, which is $\widehat{\tau} = -0.124$. We now index household-specific parameters by i

and use the "hat" to indicate an empirical estimate. Define ω_i as household i's sample weight. We require that the simulated average treatment effect $\tau^{sim}(\beta)$ equals the empirical estimate $\widehat{\tau}$:

$$\underbrace{\frac{\sum_i \omega_i \left(q^1_{is}(\tilde{\alpha}_i,\beta) - q^0_{is}(\tilde{\alpha}_i,\beta)\right)}{\sum_i \omega_i q^0_{is}(\tilde{\alpha}_i,\beta)}}_{\tau^{sim}(\beta)} = \widehat{\tau}, \tag{18}$$

subject to the constraint that simulated baseline consumption equal observed 2025 sugary drink and grocery consumption q_{ij}, for all households:

$$q^0_{ij}(\tilde{\alpha}_i,\beta) = q_{ij}, \ \forall i, j \in \{s, g\}. \tag{19}$$

$\tau^{sim}(\beta)$ is strictly decreasing in β, so there is a unique value of β that satisfies equation (18).

To satisfy the constraint, for every trial value of β, we find the values of $\tilde{\alpha}_i$ implied by the observed expenditure shares. Define observed 2025 expenditure shares as $x_{ij} = \frac{q_{ij}p_{ij}}{w_i+b_i}$. Rearranging the demand functions from equations (6) and (7) and requiring $\sum_j \tilde{\alpha}_{ij} = 1, \forall i$ gives the following equations for $\tilde{\alpha}_j$:

$$\begin{aligned}
\tilde{\alpha}_{is} &= \frac{x_{is}}{x_{is}+x_{ig}} \cdot \left(x_{is} + x_{ig} - \frac{\beta b_i}{w_i+b_i}\right) \\
\tilde{\alpha}_{ig} &= \frac{x_{ig}}{x_{is}+x_{ig}} \cdot \left(x_{is} + x_{ig} - \frac{\beta b_i}{w_i+b_i}\right) \\
\tilde{\alpha}_{in} &= 1 - \tilde{\alpha}_{is} - \tilde{\alpha}_{ig}.
\end{aligned} \tag{20}$$

Using the delta method, the standard error on $\widehat{\beta}$ is

$$SE(\widehat{\beta}) = \frac{SE(\widehat{\tau})}{\left|\frac{d\tau^{sim}(\beta)}{d\beta}\right|}. \tag{21}$$

We evaluate the derivative numerically in the neighborhood of the estimated $\widehat{\beta}$. Given that the sample of simulated households is so large (18,424 households), simulation noise is negligible.

Step 2: inferring normative α. To infer the normative utility parameters α_{is}, α_{ig}, and α_{in} we use Allcott, Lockwood, and Taubinsky (2019a)'s estimate that American households would consume about $\gamma = 1/3$ less sugary drinks if they were unaffected by internalities from imperfect information and self-control problems. We assume that this internality parameter is constant across households (see Table A21, panel (b)).

We approximate this by assuming

$$\alpha_{is} = (1-\gamma)\tilde{\alpha}_{is}. \tag{22}$$

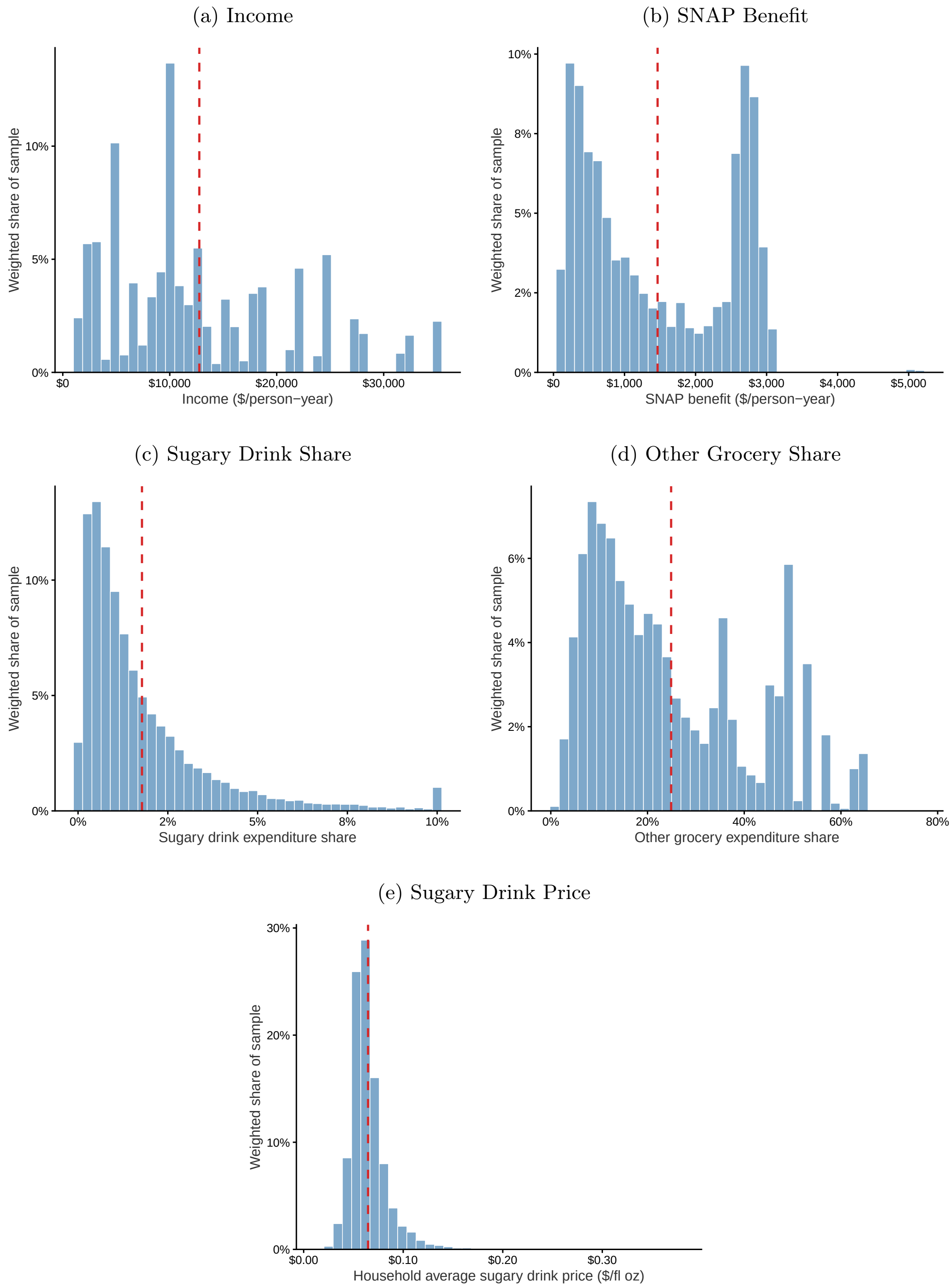
We then assume that the other two normative utility parameters adjust proportionally:

$$\begin{array}{l} \alpha_{ig} = \tilde{\alpha}_{ig}\left(1 + \frac{\gamma\tilde{\alpha}_{is}}{\tilde{\alpha}_{ig}+\tilde{\alpha}_{in}}\right) \\ \alpha_{in} = \tilde{\alpha}_{in}\left(1 + \frac{\gamma\tilde{\alpha}_{is}}{\tilde{\alpha}_{ig}+\tilde{\alpha}_{in}}\right) \end{array}. \tag{23}$$

Estimation results. Panel (c) of Table A21 presents the averages of the household-specific inferred structural parameters. Appendix Figure A21 presents the full distributions. For sugary drinks and other groceries, the average $\tilde{\alpha_j}$ parameters in Panel (c) are slightly lower than the corresponding average expenditure shares in Panel (a). They would be exactly the same in a standard Cobb-Douglas model, but mental accounting implies expenditure shares on goods s and g that are higher than $\tilde{\alpha}_s$ and $\tilde{\alpha}_g$. The average sugary drink normative utility parameter α_s is about 2/3 of the average decision utility parameter $\tilde{\alpha_s}$, which follows from our implementation of the Allcott, Lockwood, and Taubinsky (2019a) estimate. To maintain $\sum_j \alpha_{ij} = 1$, the other grocery and the numeraire normative utility parameters are slightly higher than the corresponding decision utility parameters.

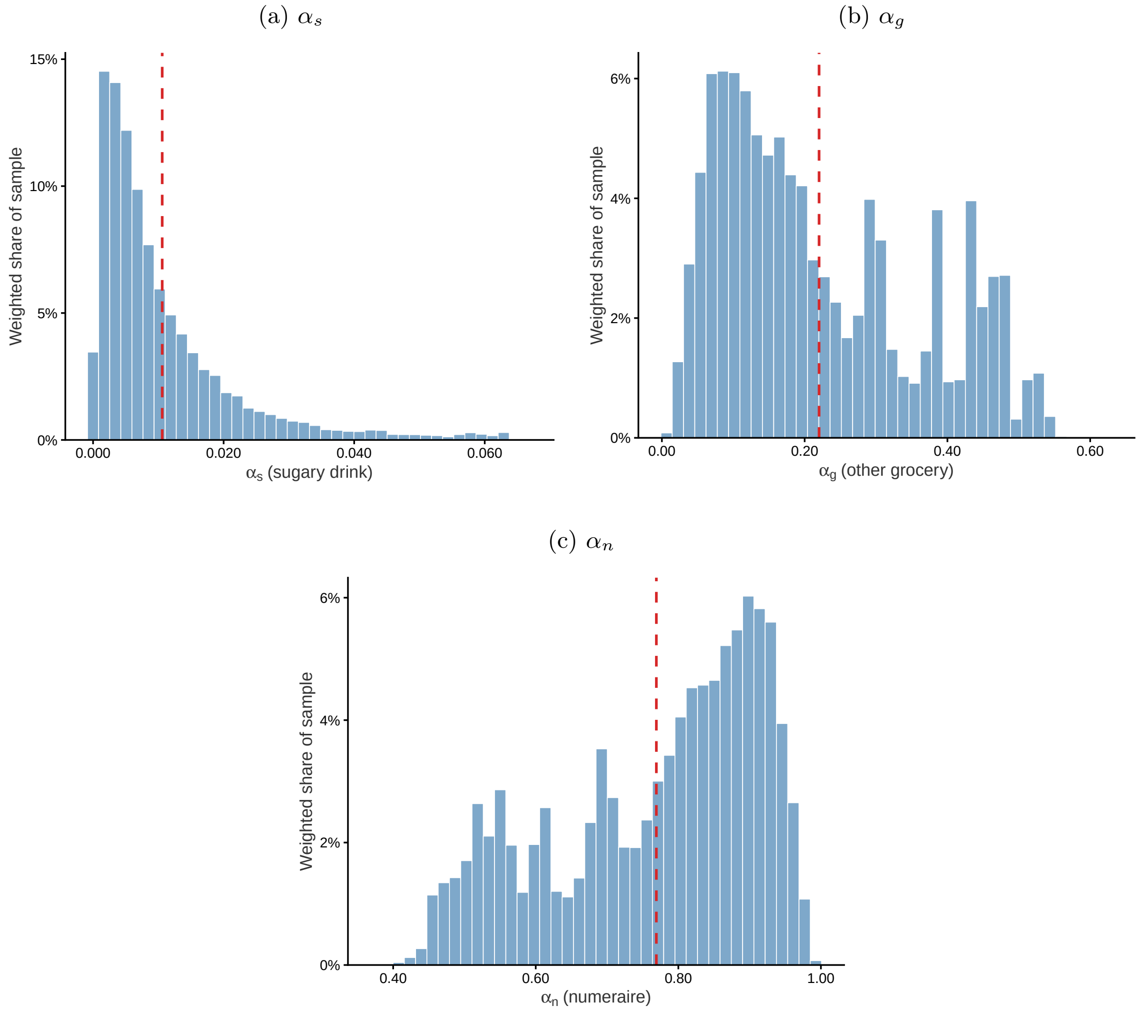
Panel (d) of Table A21 shows that we estimate a mental accounting parameter of $\hat{\beta} = 0.18$.

Figure A20: Distributions of Observed Data Across Households



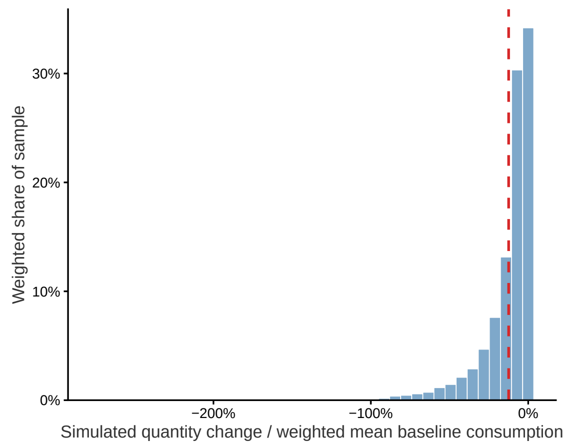
Notes: This figure presents the distributions of winsorized observed data across households in our Numerator Panel Data primary analysis sample. Vertical red lines are sample means. We winsorize expenditure shares for sugary drinks and other groceries from the top at 0.1 and 0.5, respectively. We also winsorize the other grocery expenditure share from the bottom at $b/(w+b)$.

Figure A21: Distributions of Inferred Structural Parameters Across Households



Notes: This figure presents the distributions of inferred normative utility parameters across households. Vertical red lines are sample means.

Figure A22: Distribution of Simulated Treatment Effects Across Households



Notes: This figure presents the distribution across households of simulated treatment effects normalized by sample average consumption: $\frac{q^1_{is}(\tilde{\alpha}_i,\beta)-q^0_{is}(\tilde{\alpha}_i,\beta)}{\bar{q}^0_s}$, where $\bar{q}^0_s = \frac{\sum_i \omega_i q^0_{is}(\tilde{\alpha}_i,\beta)}{\sum_i \omega_i}$. The vertical red line is the sample mean, which is being matched to the empirical estimate of $\widehat{\tau} = -0.124$.

H.2 Alternative Consumer Welfare Models

This section presents the details of the alternative consumer welfare models introduced in Section 6.1. We consider four alternative models: a Cobb-Douglas model with heterogeneous households and corner solutions, a Cobb-Douglas model with a representative household, a Cobb-Douglas model with a representative household and positive grocery internality, and a two-good quasilinear model. Section H.2.1 formalizes the models, and Section H.2.2 presents results.

H.2.1 Alternative Model Details

Cobb-Douglas model with heterogeneous households and corner solutions. In our primary model, we winsorize grocery spending so that SNAP benefits are inframarginal for all households, and the demand functions in equations (6) and (7) require interior solutions. In Section 2, however, we report estimates from other papers that the SNAP benefit may not be inframarginal for a small share of households. For example, Hastings and Shapiro (2018) report that average SNAP-eligible spending in non-SNAP months exceeds average SNAP benefits in SNAP months for 94 percent of recipients. We thus consider an alternative model where some households are at corner solutions—i.e., the SNAP benefit b exceeds the amount they would spend on SNAP-eligible goods under equation (6). For these households, excluding sugary drinks will reduce modeled sugary drink spending even without mental accounting—i.e., even if $\beta = 0$.

Allowing for corner solutions, the demand function for goods eligible for SNAP is

$$q_j = \frac{\tilde{\alpha}_j}{p_j} \max\left\{ w + b + \frac{\beta b}{\sum_{k\in\mathcal{E}^r}\tilde{\alpha}_k}, \frac{b}{\sum_{k\in\mathcal{E}^r}\tilde{\alpha}_k} \right\}, \tag{24}$$

while demand for goods ineligible for SNAP is

$$q_j = \frac{\tilde{\alpha}_j}{p_j} \min \left\{ w + b - \frac{\beta b}{\sum_{k \in \mathcal{I}^r} \tilde{\alpha}_k}, \frac{w}{\sum_{k \in \mathcal{I}^r} \tilde{\alpha}_k} \right\}. \tag{25}$$

There are two key challenges in identifying the model with corner solutions. First, we do not know how many Numerator households are at corner solutions, because we do not observe actual benefits and grocery spending and EBT transactions are both measured with error due to under- and over-scanning. In a conservative assumption relative to the above statistic from Hastings and Shapiro (2018), we assume that 10 percent of households are at corner solutions at baseline. We assume that these "baseline corner" households are randomly drawn from the larger set of households with imputed SNAP benefit b_i larger than observed 2025 grocery spending q^0_{ig}. For the baseline corner households, we then replace q^0_{ig} with the amount implied by the SNAP benefit and their observed sugary drink spending: $q^0_{ig} = b_i - p_{is} q^0_{is}$.

Second, if a household is at a corner solution, their $\tilde{\alpha}_g$ and $\tilde{\alpha}_s$ parameters are bounded but not uniquely identified by their expenditures. We define M as the proportion by which SNAP inflates consumption of SNAP-eligible goods relative to the expenditure share implied by $\tilde{\alpha}_g$ and $\tilde{\alpha}_s$. For a given M, the decision utility parameters $\tilde{\alpha}_j$ are

$$\begin{aligned} \tilde{\alpha}_{ij} &= \frac{p_{ij} q^0_{ij}}{M(b_i + w_i)}, \forall j \in \{s, g\} \\ \tilde{\alpha}_{in} &= 1 - \tilde{\alpha}_{is} - \tilde{\alpha}_{ig}. \end{aligned} \tag{26}$$

For lack of a better assumption, for households at a corner, we assume that they are exactly at the corner—i.e., both options in equations (24) and (25) give the same consumption q_j. This implies that $M = \frac{1}{1-\beta}$. Our indirect inference estimation routine then searches over β, updating M in each iteration.

To determine equivalent variation, we insert the demand functions from equations (24) and (25) into normative utility. We recompute demand and potential corner solutions at compensating income $w_i + EV_i$.

Representative household Cobb-Douglas model. For the representative household model, we replace the empirical distribution of households in Appendix H.1.2 with a single representative household whose income, SNAP benefits, and baseline spending equal the sample averages. We then apply all equations from Appendix H.1.2 to this representative household.

Representative household Cobb-Douglas with positive grocery internality. One implication of the mental accounting model is that mental accounting generates over-consumption of groceries (good g), resulting in welfare loss. However, one of the primary policy motivations for SNAP was to increase grocery consumption, and Chorniy, Finkelstein, and Notowidigdo (2025) show that SNAP benefits do not increase drug and alcohol use in the way that cash payments do.

We thus consider an alternative model where there is a positive internality from grocery consumption. More precisely, we allow for an additional benefit in normative utility from consuming g, such that $\alpha_g > \tilde{\alpha}_g$. In this model, the decision utility parameters $\tilde{\alpha}_j$ are unchanged, but the normative utility parameters α_j are different. We model a representative household so as to avoid additional effects of heterogeneity.

We pin down α_g by assuming that the SNAP benefit is "optimally calibrated" in the sense that when s is excluded from SNAP, expenditures on g are normatively optimal. To implement this, we re-arrange equation (6) and divide by $w + b$ to get the grocery expenditure share when s is excluded: $\frac{p_g q_g^1}{w+b} = \tilde{\alpha}_g + \frac{\beta b}{w+b}$. The grocery expenditure share under normative demand is α_g. Thus, the assumption is

$$\alpha_g = \tilde{\alpha}_g + \frac{\beta b}{w + b}. \tag{27}$$

The normative sugary drink parameter α_s is again determined by equation (22), and then $\alpha_n = 1 - \alpha_s - \alpha_g$.

Quasilinear sufficient statistic model. We specify a quasilinear model with only two goods: sugary drinks and a numeraire. This allows us to model the consumer surplus effect of a generic demand shift that reduces sugary drink consumption, while being agnostic about the mechanism through which restrictions act (e.g., mental accounting, information signaling, persuasive advertising, SNAP benefits being marginal, etc.) as well as what goods consumers substitute to. This is a kind of reduced-form behavioral welfare analysis framework, as in Allcott and Taubinsky (2015) and Allcott et al. (2025).

We approximate normative utility locally as quadratic in sugary drink consumption:

$$U(q_s, q_n) = aq_s - \frac{c}{2}q_s^2 + q_n, \qquad c > 0. \tag{28}$$

The parameter c is the slope of normative inverse demand.

The consumer faces the budget constraint $p_s q_s + q_n = w + b$. Because utility is quasilinear, income does not affect sugary drink demand within the relevant range.

As in the primary model, consumers may over-consume sugary drinks because of internalities such as imperfect information or self-control problems. We represent these internalities as a money-metric difference $\varphi > 0$ between decision utility and normative utility. In the absence of SNAP restrictions, decision utility is

$$\widetilde{U}^0(q_s, q_n) = aq_s - \frac{c}{2}q_s^2 + \varphi q_s + q_n. \tag{29}$$

Thus, φ is the amount by which the consumer's decision marginal willingness to pay for sugary drinks exceeds normative marginal willingness to pay.

We model the SNAP restrictions as shifting decision demand inward by an amount δ, without directly affecting normative utility. Decision utility under the restriction is therefore

$$\widetilde{U}^1(q_s, q_n) = aq_s - \frac{c}{2}q_s^2 + (\varphi - \delta)q_s + q_n, \tag{30}$$

where $\delta \geq 0$. This formulation does not require the restriction literally to reduce the underlying internality by δ. Instead, δ is the money-metric equivalent of the reduced-form inward demand shift caused by the policy.

Normative inverse demand is

$$P^*(q_s) = a - cq_s. \tag{31}$$

Baseline decision inverse demand and post-restriction decision inverse demand are, respectively,

$$P^0(q_s) = a + \varphi - cq_s \tag{32}$$

and

$$P^1(q_s) = a + \varphi - \delta - cq_s. \tag{33}$$

Thus, baseline decision inverse demand lies φ above normative inverse demand, and the SNAP restriction shifts decision inverse demand downward by δ.

Maximizing decision utility subject to the budget constraint gives baseline sugary drink demand

$$q_s^0 = \frac{a + \varphi - p_s}{c}, \tag{34}$$

and post-restriction demand

$$q_s^1 = \frac{a + \varphi - \delta - p_s}{c}. \tag{35}$$

Normatively optimal demand is

$$q_s^* = \frac{a - p_s}{c}. \tag{36}$$

Subtracting equation (36) from equation (34) shows that baseline over-consumption is

$$q_s^0 - q_s^* = \frac{\varphi}{c}. \tag{37}$$

As in the primary model, we use the Allcott, Lockwood, and Taubinsky (2019a) estimate to assume that a share γ of baseline sugary drink consumption is attributable to internalities. Thus,

$$q_s^* = (1 - \gamma)q_s^0, \tag{38}$$

or equivalently,

$$q_s^0 - q_s^* = \gamma q_s^0. \tag{39}$$

Combining equations (37) and (39) gives the baseline money-metric bias:

$$\varphi = c\gamma q_s^0. \tag{40}$$

To express this bias using observable quantities and an elasticity parameter, define $\eta > 0$ as the absolute value of the local price elasticity of baseline sugary drink demand:

$$\eta \equiv -\frac{\partial q_s^0}{\partial p_s}\frac{p_s}{q_s^0}. \tag{41}$$

Because $\partial q_s^0/\partial p_s = -1/c$, equation (41) implies

$$cq_s^0 = \frac{p_s}{\eta}. \tag{42}$$

The baseline money-metric bias can therefore be written as

$$\varphi = \frac{\gamma p_s}{\eta}. \tag{43}$$

Intuitively, under the locally linear approximation, the price increase required to eliminate a fraction γ of baseline consumption is $\gamma p_s/\eta$.

We next use the estimated treatment effect to infer the size of the policy-induced demand shift. Define the proportional treatment effect as in the empirical analysis:

$$\tau \equiv \frac{q_s^1 - q_s^0}{q_s^0}, \qquad \tau < 0. \tag{44}$$

Thus,

$$q_s^1 = (1+\tau)q_s^0 \tag{45}$$

and the quantity reduction is

$$q_s^0 - q_s^1 = -\tau q_s^0. \tag{46}$$

Subtracting equation (35) from equation (34) also gives

$$q_s^0 - q_s^1 = \frac{\delta}{c}. \tag{47}$$

Combining equations (42), (46), and (47), the money-metric equivalent of the inward demand shift is

$$\delta = -\frac{\tau p_s}{\eta}. \tag{48}$$

Because $\tau < 0$, the implied δ is positive. The ratio of the policy-induced shift to the baseline bias is

$$\frac{\delta}{\varphi} = \frac{-\tau}{\gamma}. \tag{49}$$

Thus, the SNAP restriction closes a fraction $-\tau/\gamma$ of the gap between baseline and normatively optimal consumption. If $\tau = -\gamma$, the restriction implements the normative optimum. If $-\tau < \gamma$, the restriction only partially corrects over-consumption; if $-\tau > \gamma$, it reduces consumption below the normative optimum.

Because utility is quasilinear, the change in consumer welfare is the change in normative consumer surplus. The welfare gain from reducing consumption from q_s^0 to q_s^1 is

$$\Delta W = \int_{q_s^1}^{q_s^0} [p_s - P^*(q)] \, dq. \tag{50}$$

At baseline consumption, equations (31) and (34) imply

$$p_s - P^*(q_s^0) = \varphi. \tag{51}$$

Thus, the marginal unit eliminated beginning at q_s^0 generates a welfare gain of φ: its market price exceeds its normative marginal value by the full baseline bias.

At post-restriction consumption, normative marginal willingness to pay is higher because consumption is lower. Using equations (31), (46), and (48),

$$p_s - P^*(q_s^1) = \varphi - \delta. \tag{52}$$

Because inverse demand is locally linear, the average marginal welfare gain over the units eliminated is the average of the two endpoint wedges:

$$\frac{\varphi + (\varphi - \delta)}{2} = \varphi - \frac{\delta}{2}. \tag{53}$$

The total welfare gain is therefore

$$\Delta W = \left(\varphi - \frac{\delta}{2}\right)(q_s^0 - q_s^1). \tag{54}$$

Equation (54) provides a simple rectangle representation: the welfare gain equals the quantity reduction multiplied by the average marginal welfare gain from the units no longer consumed.

Equivalently, welfare can be decomposed into an internality-reduction rectangle and a conventional (perceived) consumer-surplus triangle:

$$\Delta W = \underbrace{\varphi(q_s^0 - q_s^1)}_{\text{internality reduction benefit}} - \underbrace{\frac{\delta}{2}(q_s^0 - q_s^1)}_{\text{decision consumer surplus loss}}. \tag{55}$$

The first term values each unit of reduced consumption at the baseline money-metric bias φ. The second term recognizes that, as the policy reduces consumption, the forgone units have progressively higher normative value.

Substituting equations (43), (46), and (48) into equation (54) and rearranging gives

$$\Delta W = \frac{p_s q_s^0}{\eta}\left(-\gamma\tau - \frac{\tau^2}{2}\right). \tag{56}$$

Equation (56) requires four "sufficient statistics": baseline sugary drink expenditure $p_s q_s^0$, the proportional treatment effect τ, the assumed internality share γ, and the absolute value of the local demand elasticity η. It does not require specifying the particular mechanism through which SNAP restrictions reduce demand.

To quantify equation (56), we use parameters corresponding to the primary model: $\tau = -0.124$, $\gamma = 1/3$, demand elasticity $\eta = 1$, and the representative household's sugary drink expenditure.

H.2.2 Alternative Model Results

Table A22 presents parameter estimates for the primary and alternative Cobb-Douglas models. The primary estimates in column (1) are the same as in Table A21.

Table A23 presents the consumer welfare effects of SNAP restrictions under the primary and alternative models. The primary model result is the same as in Table 8.

Table A22: Parameter Estimates Under Alternative Cobb-Douglas Models

Parameter	Primary: Cobb-Douglas, heterogeneous households (1)	Heterogeneous households, corner solutions (2)	Representative household (3)	Representative household, positive grocery internality (4)
Panel (a): Decision Utility Parameters				
$\tilde{\alpha}_s$	0.016	0.016	0.013	0.013
$\tilde{\alpha}_g$	0.219	0.217	0.172	0.172
$\tilde{\alpha}_n$	0.765	0.767	0.815	0.815
Panel (b): Normative Utility Parameters				
α_s	0.011	0.011	0.009	0.009
α_g	0.220	0.219	0.173	0.193
α_n	0.769	0.770	0.818	0.798
Panel (c): Corner Model Inflation Factor				
M		1.221		
Panel (d): Estimated Mental Accounting Parameter				
β	0.184	0.181	0.203	0.203
	(0.017)	(0.017)	(0.017)	(0.017)

Notes: This table presents estimated parameters for alternative models. Column (1) presents estimates for the primary model described in Section 6.1. Column (2) presents estimates for the Cobb-Douglas model with corner solutions. Column (3) presents estimates for the representative household Cobb-Douglas model. Column (4) presents estimates for the representative household Cobb-Douglas model with positive grocery internality. In columns (1) and (2), the $\tilde{\alpha}$ and α parameters are averages across heterogeneous households. Standard errors on the estimated β parameters are in parentheses.

Table A23: Consumer Welfare Effects Under Alternative Models

	Equivalent variation per SNAP participant ($/person-year) (1)	Nationwide equivalent variation ($ millions/year) (2)
Primary: Cobb-Douglas, heterogeneous households	$8.09	$336
Heterogeneous households, corner solutions	$4.84	$201
Representative household	$7.39	$307
Representative household, positive grocery internality	$9.53	$396
Quasilinear sufficient-statistic model	$7.09	$295

Notes: This table presents the equivalent variation results obtained from the primary model described in Section 6.1 and alternative models described in Appendix H.2. Column (1) reports the average equivalent variation per SNAP recipient. Column (2) reports the nationwide equivalent variation, which is obtained by multiplying the values in column (1) by the national SNAP population in 2025.

I Predicting Effects of SNAP Restrictions on Weight Loss, Diabetes Incidence, and Health Care Costs

I.1 Overview of Model Predicting Health Outcomes

We project the estimated effects of the SNAP sweetened drink restrictions on steady-state weight loss and type 2 diabetes rates, and we then project the impacts of steady-state weight loss and type 2 diabetes rates on health care costs. We do not model mortality risk.

We use the following logic model to translate changes in sweetened drink purchases into potential changes in health outcomes:

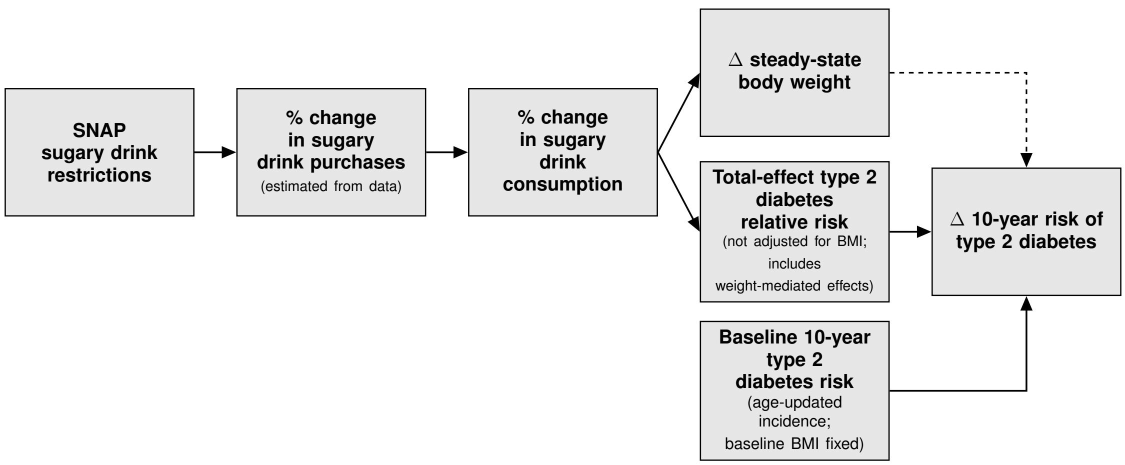


Figure A23: Logic Model Depicting How SNAP Sugary Drink Restrictions Could Affect Health Outcomes

Our reduced-form estimates indicate that SNAP restrictions led to lower purchases of sugary drinks at grocery stores. Purchases may not translate one-for-one into consumption, however; some divergence may occur because of food waste or stockpiling. We assume that the rate of divergence is not affected by the policy, which implies that changes in purchases translate proportionally into changes in consumption. We also assume (consistent with our estimates in Section 4 and Allcott, Lockwood, and Taubinsky 2019b) that individuals do not meaningfully compensate for reductions in consumption of excluded sugary drinks by consuming more calories from other sources (e.g., compensating for drinking less soda by eating more desserts).

Using nationally representative data on sugary drink consumption of SNAP recipients from the 2021-2023 National Health and Nutrition Examination Survey (NHANES), we translate our estimate of the percent change in sugary drink consumption into changes in calories consumed from sugary drinks. We then use epidemiological estimates of how changes in calorie and sugary drink intake affect body weight and the rate of onset of type 2 diabetes—together with NHANES data on SNAP recipients' body mass index (BMI) and type 2 diabetes rates—to project changes in BMI and type 2 diabetes. Finally, we use external estimates to translate changes in weight and diabetes rates into implications for annual health care spending.

We focus our analysis on a 10-year simulation horizon to align with typical policy planning horizons, reduce uncertainty associated with longer-term extrapolation, and align with other nutrition policy evaluations (Basu et al. 2014; Basu and Maciejewski 2019; Grummon et al. 2019; Smith et al. 2026). Moreover, nearly all weight loss occurs in the first three years after a reduction in calorie intake (Hall et al. 2011), making a 10-year time horizon sufficient to capture weight loss benefits from restrictions. We refer to our 10-year estimates as "steady-state estimates."

For all of these analyses, we limit ourselves to impacts on adults ages 18 and older. Type 2 diabetes is very rare among children, with a national prevalence of 1.4 per 1,000 children (Zang et al. 2026).

I.2 Implementation Details

We consider two nationwide counterfactual scenarios, indexed $r = \{0, 1\}$. In the "baseline" scenario $r = 0$, SNAP benefits can be spent on sugary drinks. In the "policy" scenario $r = 1$, all sugary drinks are excluded from SNAP, where "all sugary drinks" includes regular soda, sports drinks, energy drinks, sweetened teas, and fruit drinks that are not 100 percent juice, but excludes diet soda and 100 percent fruit juice. Define q_s^r as consumption of sugary drinks in scenario r from retail stores (i.e., grocery stores and other places where SNAP funds can be spent). The percentage treatment effect of SNAP exclusion is $\tau = (q_s^1 - q_s^0)/q_s^0$. We assume that the effects of SNAP restrictions on all sugary drinks are the same as our empirical estimate of the SNAP restrictions on sweetened drinks (which include diet drinks in addition to sugary drinks) in percentage terms. In some states, SNAP restrictions include diet drinks, so our assumption implies that the effects of SNAP restrictions are equi-proportional across the different types of excluded drinks.

I.2.1 Data

The main data we use are the 2021-2023 National Health and Nutrition Examination Survey (NHANES). We use this to measure consumption of sugary drinks, body mass index (BMI), and diabetes rates among SNAP recipients ages 18 and over. NHANES is a continuous cross-sectional survey that collects detailed dietary and anthropometric data (CDC 2024b). It uses multi-stage probability sampling such that the weighted sample is nationally representative of the civilian, non-institutionalized U.S. population. Participants complete repeated 24-hour dietary recalls to assess dietary intake and visit mobile examination centers for body and health measurements, including weight and height. Participants also complete surveys about their demographic characteristics, including their SNAP participation.

We drop individual observations if they do not report being a recipient of SNAP benefits or did not complete a day one dietary recall. Additionally, we drop observations with missing height, weight, or baseline diabetes status. Baseline diabetes status is from participants' self-report of whether they have been diagnosed with diabetes by a doctor or medical professional.[27] Those who

27. The survey does not distinguish between type 1 diabetes (the body does not make enough insulin) and type 2 diabetes (the body cannot use insulin properly). We assume that those who self-report having diabetes have type 2

report 'Yes' are coded with $D_i^0 = 1$, while those who report 'No' or 'Borderline' are coded with $D_i^0 = 0$. Those who responded 'Don't know', refused to respond, or had a missing response were treated as missing a baseline diabetes status and thus were dropped from the sample. We calculate BMI using each participant's measured height and weight from NHANES examination data and code those with a BMI greater than or equal to 30 as being obese. Our analyses apply dietary recall sample weights, following NHANES recommendations (CDC 2026c).

Table A24 reports the sample statistics for the final analytic sample of 787 adult SNAP recipients. About half of the sample has obesity (i.e., BMI of at least 30kg/m^2 based on measured height and weight), and 17 percent have diabetes.

Table A24: NHANES Adult SNAP Recipient Analytic Sample

Statistic	Value
Adult SNAP recipients, unweighted	787
Mean age (years)	45.5
Mean BMI	32.0
Obesity prevalence	50.7%
Doctor-diagnosed diabetes prevalence (all types)	17.1%
Mean sugary drink intake (fl oz/day)	10.1

Notes: This table presents summary statistics for the NHANES sample of SNAP recipients ages 18 and over that we use to simulate the effects of the SNAP sweetened drink restrictions on body weight and diabetes incidence. The first row reports the number of observations, and the remaining rows report weighted averages using the dietary recall sample weights, following NHANES recommendations (CDC 2026c).

As part of the dietary recalls conducted by NHANES, participants report consumption of all foods and beverages they consumed on the recall day, as well as where the food was acquired. For projecting health outcomes, we focus on the consumption of sugary drinks that participants reported they acquired from stores (including "grocery/supermarket" stores, "convenience type" stores and stores with "no additional info" reported), given that SNAP restrictions only affect foods purchased from stores.

To identify sugary drinks that would be subject to restrictions as they have been implemented to date, we match each reported food item to the USDA Food and Nutrient Database for Dietary Studies (FNDDS) using its code in NHANES. Via the FNDDS, we are able to observe each item's broad category (e.g., soda, energy & sports drinks), as well as its nutrient profile. Using this information, we identified 94 beverages that likely fall into the categories of drinks that have been subject to SNAP restrictions. Additionally, we are able to use the FNDDS entry for each item to convert participants' reported consumption in grams to fluid ounces.

The average recipient consumes about 10.1 fluid ounces of store-bought sugary drinks per day

given that the vast majority (90-95 percent) of diagnosed diabetes cases are type 2 (CDC 2026b).

(Table A24); this is quite similar to our estimate of 9.6 fluid ounces per day in the Numerator Panel Data (Table 2).

A limitation of the NHANES data is that we observe whether the individual has diabetes, but not rates of incidence of new cases, which are needed for the calibration. We therefore also use data from the 2021-2024 National Health Interview Survey (NHIS) to calculate the average incidence rate (i.e., rate of newly diagnosed type 2 diabetes cases) for each age $\times$ BMI category (CDC 2024a). For each participant in the 2021-2024 NHIS samples, we are able to observe their age, reported BMI, reported doctor-diagnosed diabetes status, diabetes type, and how long since they were diagnosed with diabetes. We divide the sample into three age groups (18-44, 45-64, and 65+), as well as four BMI groups (underweight [BMI<18.5], healthy weight [18.5 to <25], overweight [25 to <30], and obese [≥ 30]). We filter the sample to respondents aged 18 or older, and drop any respondents who do not report all of the aforementioned variables. We also remove respondents who report being diagnosed with type 1 or another type of diabetes. Finally, we drop respondents who report being diagnosed with type 2 diabetes more than a year ago. This gives us a sample of 104,613 respondents. We do not filter the sample based on whether participants reported being on SNAP or not, as this would reduce the size of the sample by roughly 90 percent and cause our incidence rates to be imprecise.

We adapt the procedures used by the U.S. Diabetes Surveillance System to calculate the average annual incidence rates for each group (CDC 2026a). Specifically, we treat any participant who reported that 0 years had passed since being diagnosed with type 2 diabetes as being diagnosed in the past year, and code each participant as contributing 1 new case ($d_i = 1$). Additionally, we treat half of participants who reported that 1 year had passed since their diagnosis as also being diagnosed in the past year, and code each participant in this category as contributing 0.5 of a new case ($d_i = 0.5$). We choose to pool four years of data to calculate incidence rates to improve the quality of estimates for categories with low incidence rates. We also calculate one incidence rate for the 'underweight' BMI category due to the low prevalence of cases.

Then, for all NHIS participants, we code whether or not they are at risk of developing type 2 diabetes. Those who developed type 2 diabetes 1 year ago are assigned $s_i = 0.5$. Additionally, those who developed type 2 diabetes less than a year ago or report that they have not developed type 2 diabetes are assigned $s_i = 1$.

To find the average annual incidence rate per 1,000 adults in each age $\times$ BMI group, we calculate

$$r_{a,b} = 1000 \times \frac{\sum_i \omega_i \mathbf{1}\{A_i = a\}\mathbf{1}\{B_i = b\} d_i}{\sum_i \omega_i \mathbf{1}\{A_i = a\}\mathbf{1}\{B_i = b\} s_i},$$

where a is the age group, b is the BMI group, and ω are the NHIS sample weights. Because very few type 2 diabetes cases are reported by participants in the underweight BMI group (BMI < 18.5), we compute one incidence rate for the underweight group with

$$r_b = 1000 \times \frac{\sum_i \omega_i \mathbf{1}\{B_i = b\} d_i}{\sum_i \omega_i \mathbf{1}\{B_i = b\} s_i}$$

We calculate age- and BMI-specific incidence rates because diabetes risk varies with both (Narayan et al. 2007). Table A25 reports the average annual type 2 diabetes incidence rates in each age x BMI category for the 104,613 adults. We group these adults into three age groups (18-44, 45-64, ≥ 65) and four BMI categories (underweight, healthy weight, overweight, and obese). Due to the low frequency of new type 2 diabetes cases for individuals in the underweight BMI category (BMI < 18.5), we calculate one rate for all individuals in this category, regardless of age group.

Table A25: Type 2 Diabetes Incidence Rates by Age and BMI, 2021–2024

Age group	Underweight BMI < 18.5	Healthy weight $18.5 \leq$ BMI < 25	Overweight $25 \leq$ BMI < 30	Obese BMI ≥ 30
18–44	0.70	0.46	1.41	6.13
	(pooled $N =$ 1,838)	($N =$ 14,773)	($N =$ 13,055)	($N =$ 12,949)
45–64	0.70	3.59	5.75	15.74
	(pooled $N =$ 1,838)	($N =$ 8,954)	($N =$ 11,747)	($N =$ 11,254)
65+	0.70	1.93	5.88	12.70
	(pooled $N =$ 1,838)	($N =$ 10,830)	($N =$ 11,332)	($N =$ 7,881)

Notes: This table presents the calculated incidence rates of type 2 diabetes per 1,000 adults for the 2021-2024 NHIS sample by age and BMI group. These rates correspond to the values of r_{a_i,b_i} that are used in the model for calculating the reduction in the prevalence of type 2 diabetes. The estimate for adults with BMI in the underweight category is pooled across age groups.

I.2.2 Predicting Changes in Body Weight

We use the human metabolism model in Hall, Schoeller, and Brown (2018) to predict steady-state weight change from changing sugary drink consumption according to the following formula:

$$\Delta W_i = \tau q_i^0 \psi / \phi, \tag{57}$$

where τ denotes the percent reduction in sugary drink consumption due to the restrictions that we estimate using the Numerator Panel Data, q_i^0 denotes person i's baseline sugary drink consumption from stores (in fluid ounces per day), ψ denotes calories per fluid ounce, and ϕ denotes the translation from calorie reduction into steady-state weight loss.

We calibrate the parameters using both NHANES data and external estimates. We observe q_i^0 (in grams per day) directly in the NHANES and convert to fluid ounces per day using the FNDDS; as shown in Table A24, average consumption is 10.1 fluid ounces per day. We assume the average sugary drink has $\psi = 11.7$ calories per fluid ounce (Long et al. 2015). Finally, we assume that a reduction of $\phi = 55$ calories per day causes one pound of steady-state weight loss based on Hall, Schoeller, and Brown (2018).

I.2.3 Predicting Changes in Type 2 Diabetes Incidence

To project effects on type 2 diabetes, we use epidemiological estimates that relate changes in sugary drink consumption to changes in type 2 diabetes incidence. These estimates capture the overall association between sugary drink consumption and type 2 diabetes risk, including both direct pathways (such as through changes in blood glucose or insulin resistance) and indirect pathways operating through body weight (Qin et al. 2020).

Specifically, to predict the impact of the restrictions on the number of new type 2 diabetes (T2D) cases that develop over a 10-year period (i.e., the "incidence rate"), we use an approach similar to Smith et al. (2026). Let D_i^0 be an indicator denoting a participant's baseline T2D status. We split the NHANES sample into two groups: those who report having T2D ($D_i^0 = 1$), and those who report either having borderline T2D or not having T2D ($D_i^0 = 0$). Having T2D is an absorbing state.

Participants who do not have T2D at baseline ($D_i^0 = 0$) can transition to having T2D, with transition probabilities that are age- and BMI-specific. For those with $D_i^0 = 0$, we define π_i^0 as the baseline probability of developing diabetes over a 10-year period. We assume that π_i^0 is given by

$$\pi_i^0 = 1 - \prod_{t=0}^{9} \exp\left(-\frac{r_{a_i+t,b_i}}{1000}\right), \tag{58}$$

where r_{a_i+t,b_i} is the diabetes incidence rate for the individual's age and BMI group in year t from the NHIS (see Appendix Table A25), with the individual's age updated each year.

Among the people who do not already have type 2 diabetes, we predict the change in the probability of type 2 diabetes using the following formula

$$\Delta\pi_i = \pi_i^0 \cdot (RR^{\tau q_i^0/8.45} - 1), \tag{59}$$

where τ is the percent reduction in sugary drink consumption from the SNAP restrictions, q_i^0 is individual i's baseline sugary drink consumption per day, and RR corresponds to the relative risk associated with consuming an additional 8.45 fluid ounce serving of sugary drinks per day. We assume $RR = 1.19$ based on evidence from a recent meta-analysis of epidemiological studies (Qin et al. 2020). Following Smith et al. (2026), we use this meta-analysis because it examines the association between sugary drink consumption and type 2 diabetes without adjusting for body mass index, which allows the association to account for both the potential direct effects of sugary drinks on type 2 diabetes risk, as well as the indirect effects of sugary drinks coming through steady-state changes in body weight. The Qin et al. (2020) relative risk estimate corresponds to consuming an additional serving (i.e., 250 mL) of sugary drinks per day, or 8.45 fluid ounces per day. To apply this relative risk in the model, we first convert the reduction in sugary drink consumption from ounces to 8.45-fluid-ounce servings by dividing the quantity reduction by 8.45. We then estimate the corresponding reduction in type 2 diabetes risk associated with the τq_i^0 reduction in sugary drink consumption.

I.2.4 Predicting the Reduction in Health Care Costs

We translate the predicted changes in body weight and diabetes incidence into implications for changes in annual health care spending based on estimates in Cawley et al. (2015) and Parker et al. (2024), respectively.

To do this, we define D_i^0 as person i's baseline diabetes status and B_i^0 as their baseline BMI when sugary drinks are eligible for SNAP. We define the function $\Gamma(B_i^0, D_i^0)$ as the annual health care spending change per unit of BMI change, which depends on the individual's initial BMI and diabetes status following Cawley et al. (2015). We calibrate the $\Gamma(.)$ function using the cost-savings estimates in Cawley et al. (2015). The estimates are reported in Appendix Table A26 and show an increasing and nonlinear relationship between baseline BMI and the estimated cost savings per unit of BMI change, with larger cost savings for individuals with higher initial BMIs. Because Cawley et al. (2015) report savings estimates for discrete BMI reductions (5 percent, 10 percent, etc.), we scale their percent reduction estimates linearly to match each participant's simulated percentage change in BMI. Similarly, because Cawley et al. (2015) report savings estimates for discrete starting BMI values (e.g., 30, 31, 32, etc.), we use linear interpolation between the two relevant BMI values. We then apply the appropriate cost savings based on the participant's baseline BMI and type 2 diabetes status. By conditioning savings on participants' starting type 2 diabetes status, this approach avoids "double-counting" cost-savings benefits of weight loss that are mediated through averted cases of type 2 diabetes.

We define the parameter Λ to be the average annual health care spending due to type 2 diabetes. We calibrate Λ using the per-case annual attributable health care costs from type 2 diabetes reported in Parker et al. (2024), which is the American Diabetes Association's national economic cost estimate.

Given these definitions, we predict the annual health care spending change from the predicted change in BMI and the predicted change in diabetes incidence using the following equation:

$$\Delta C_i = \underbrace{\Delta B_i \cdot \Gamma(B_i^0, D_i^0)}_{\text{BMI effect}} + \underbrace{(1 - D_i^0)\Delta\pi_i \cdot \Lambda}_{\text{Diabetes effect}}, \tag{60}$$

where ΔB_i is individual i's predicted change in BMI and $\Delta\pi_i$ is the individual's predicted change in diabetes incidence. Because the diabetes incidence effects take 10 years to manifest, equation (60) predicts annual health care cost reductions starting 10 years in the future. To predict changes in BMI, we use the predicted change in body weight from equation (57) and convert to a predicted change in BMI. We then multiply this by $\Gamma(.)$. To predict changes in diabetes incidence, we use the predicted change from equation (59). We then multiply this by Λ.

Table A26: Annual Health Care Savings per One-Unit Reduction in BMI

Baseline BMI B	No baseline T2D $\Gamma(B,0)$	Baseline T2D $\Gamma(B,1)$
30	\$266.12	\$593.95
31	\$283.24	\$657.08
32	\$301.44	\$726.93
33	\$320.76	\$804.21
34	\$341.30	\$889.69
35	\$363.12	\$984.28
36	\$386.30	\$1,088.91
37	\$410.92	\$1,204.68
38	\$437.09	\$1,332.75
39	\$464.88	\$1,474.45
40	\$494.40	\$1,631.21
41	\$525.76	\$1,804.64
42	\$559.07	\$1,996.52
43	\$594.45	\$2,208.81
44	\$632.03	\$2,443.66
45	\$671.94	\$2,703.50

Notes: Cells report annual health care savings in 2026 dollars per one-unit reduction in BMI. The displayed values are calculated with $\Gamma(B,D) = \frac{C_{5\%}(B,D)}{0.05B} \times \frac{335.123}{218.056}$, where $C_{5\%}(B,D)$ are the costs reported in Tables 2 and 3 from Cawley et al. (2015). For baseline BMI values below 30, $\Gamma(B,D) = 0$. For baseline BMI values above 45, $\Gamma(B,D) = \Gamma(45,D)$. For non-integer values, linear interpolation is used to find $\Gamma(B,D)$.

I.3 Results

Table A27 summarizes the parameters used in our calibration exercise and their sources. Table A28 displays the predicted effects of SNAP sugary drink restrictions on health and health care costs.

We emphasize that there is considerable uncertainty around many of the parameters used to generate the health care spending projections, which makes it hard to draw strong conclusions. In some cases, however, it is relatively straightforward to assess sensitivity to different assumptions. For example, we assume no "offset" effects in terms of households substituting from sugary drinks to other sugary foods, or substituting from sugary drinks purchased in grocery stores to sugary drinks at restaurants. While our substitution effect estimates as well as prior work (e.g., Allcott, Lockwood, and Taubinsky 2019a) both suggest that this is a reasonable assumption, there is other evidence of small-to-modest offset effects. For example, Tordoff and Alleva (1990) randomized 30 adults to receive an additional 530 calories per day from sugary drinks for three weeks and found that calorie intake from the rest of the diet fell by only 195 calories per day relative to baseline (i.e., without additional sugary drinks), implying that 37 percent of the added calories were offset by reduced caloric intake elsewhere. This is on the high end of offset factors reported in the literature; other studies also estimate lower (but non-zero) offset factors (see, e.g., DiMeglio and Mattes 2000;

Wymelbeke et al. 2003; Reid et al. 2007; Finkelstein et al. 2013). To the extent that our estimates overstate the actual decrease in calories and sugar consumption because of non-zero offset effects, one can simply scale our health spending estimates by $(1 - \chi)$, where χ is the true offset effect.[28]

Table A27: Health Model Parameters

Parameter	Description	Value	Source
ψ	Average calorie content of sugary drinks	11.7 kcal/fl oz	Long et al. (2015)
ϕ	Sustained calorie reduction associated with one pound of steady-state weight loss	55 kcal/day per lb	Hall et al. (2018)
$r_{a,b}$	Annual T2D incidence rate for age group a and BMI group b	See Table A25	CDC (2024)
RR	T2D relative risk per additional 250 mL/day (8.45 fl oz/day) of sugary drink consumption	1.19	Qin et al. (2020)
$\Gamma(B, D)$	Annual health care savings per one-unit BMI reduction, indexed by baseline BMI and T2D status and converted from 2010 dollars to 2026 dollars	See Table A26	Cawley et al. (2015); BLS CPI
Λ	Annual attributable health care cost of T2D, converted from 2022 dollars to 2026 dollars	\$13,766.55/case	Parker et al. (2024); BLS CPI

Notes: This table presents a summary of the parameters used in the health effects model. The annual attributable health care cost of T2D is calculated as $\Lambda = \$12{,}022 \times (335.123/292.655) = \$13{,}766.55$.

28. For predicting changes in body weight, this scaling is exact because the predicted decrease in body weight is linear in the reduction in calories. For predicting changes in diabetes incidence, the model is nonlinear, but we have explored sensitivity to different offset effects, and we have found that we can very closely approximate the predicted change in diabetes incidence when there is a non-zero offset effect by simply multiplying our zero-offset prediction by $(1 - \chi)$.

Table A28: Predicted Annual Consumption and Adult Health Effects of SNAP Restrictions

	Per SNAP recipient (1)	Total (million) (2)
Panel (a): Sugary Drink Consumption Reduction		
Numerator SNAP recipients (fl oz/year)	430	17,885
NHANES adult SNAP recipients (fl oz/year)	458	11,670
Panel (b): Predicted Adult Health Effects		
Steady-state weight loss (pounds)	0.266	6.8
Type 2 diabetes cases averted over 10 years	0.13 pp	0.033
Steady-state annual health care cost reduction ($/year)	$31.45	$801.1

Notes: This table reports the reduction in sugary drink consumption for all SNAP recipients and the estimated adult health effects from the restrictions, assuming all sugary drinks are excluded. Column (1) reports the average per SNAP recipient or adult, and column (2) aggregates the average over the population of SNAP recipients and adult SNAP recipients. The estimated number of adult SNAP recipients was found by multiplying the average number of total SNAP recipients by the percent of SNAP recipients who were adults. The results in panel (a) are found by calculating $\hat{\tau} \times \bar{q}_s^0$ in the Numerator and NHANES SNAP samples, where $\bar{q}_s^0$ is the annual consumption of sugary drinks per person in the representative household. The results in the three rows in Panel (b) are found by solving equations (57), (59), and (60), respectively, for each participant in the NHANES sample and taking the average.