

How Are SNAP Benefits Spent?

Evidence from a Retail Panel

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June 2017

Abstract

We use a novel retail panel with more than six years of detailed transaction records to study the effect of participation in the Supplemental Nutrition Assistance Program (SNAP) on household spending. We frame our approach using novel administrative data from the state of Rhode Island. The marginal propensity to consume SNAP-eligible food (MPCF) out of SNAP benefits is 0.5 to 0.6. The MPCF out of cash is much smaller. These patterns obtain even for households for whom SNAP benefits are economically equivalent to cash in the sense that benefits do not cover all food spending. We reject the hypothesis that households respect the fungibility of money in a semiparametric framework. A model with mental accounting can match the facts.

Keywords: in-kind transfers, mental accounting, fungibility

JEL: D12, H31, I38

*This work has been supported (in part) by awards from the Laura and John Arnold Foundation, the National Science Foundation, the Robert Wood Johnson Foundation's Policies for Action program, and the Russell Sage Foundation. Any opinions expressed are those of the author(s) alone and should not be construed as representing the opinions of these Foundations. We also appreciate support from the Population Studies and Training Center at Brown University. This project benefited from the suggestions of Ken Chay, Raj Chetty, David Cutler, Amy Finkelstein, Xavier Gabaix, Peter Ganong, Ed Glaeser, Nathan Hendren, Hilary Hoynes, Larry Katz, David Laibson, Kevin Murphy, Mandy Pallais, Devin Pope, Diane Whitmore Schanzenbach, and Andrei Shleifer, from audience comments at Brown University, Clark University, Harvard University, the Massachusetts Institute of Technology, UC Berkeley, Stanford University, Princeton University, and the Quantitative Marketing and Economics Conference, and from comments by discussant J.P. Dubé. We thank our dedicated research assistants for their contributions. E-mail: justine_hastings@brown.edu, jesse_shapiro_1@brown.edu.

1 Introduction

This paper studies how receipt of benefits from the Supplemental Nutrition Assistance Program (SNAP) affects household spending. SNAP is of special interest to economists for at least two reasons. First, the program is economically important: it is the second-largest means-tested program in the United States after Medicaid (Congressional Budget Office 2013), enrolling 19.6 percent of households in fiscal 2014.¹

Second, the program’s stated objectives sit awkwardly with economic theory. On signing the bill to implement the predecessor Food Stamp Program, President Lyndon Johnson declared that the program would “enable low-income families to increase their food expenditures” (Johnson 1964). The Food and Nutrition Service of the USDA says that SNAP is important for “helping low-income families put food on the table” (FNS 2012). Yet although SNAP benefits can only be spent on food, textbook demand theory (Mankiw 2000; Browning and Zupan 2004) predicts that, for the large majority of SNAP recipients who spend more on food than they receive in benefits,² SNAP benefits are economically equivalent to cash.³ As typical estimates of the marginal propensity to consume food (MPCF) out of cash income are close to 0.1,⁴ the textbook treatment says that SNAP benefits should mostly subsidize nonfood spending.

Estimating the effect of SNAP benefits on spending is challenging because it requires good measurement of household spending and suitably exogenous variation in program participation or benefits. Survey-based measures of household spending are error-prone and sensitive to the mode of elicitation (Ahmed et al. 2006; Browning et al. 2014; Battistin and Padula 2016). Important components of SNAP eligibility and benefit rules are set nationally, and major program changes

¹There were 22,744,054 participating households in fiscal 2014 (FNS 2016a) and 116,211,092 households in the US on average from 2010-2014 (US Census Bureau 2016).

²Hoynes et al. (2015) find that spending on food at home is at or above the SNAP benefit level for 84 percent of SNAP recipient households. Trippe and Ewell (2007) report that 73 to 78 percent of SNAP recipients spend at least 10 percent more on food than they receive in SNAP benefits.

³Consider a household with monthly income y and SNAP benefits b . If the household spends f on SNAP-eligible food then she has $y - \max(0, f - b)$ available to buy other goods. Let $U(f, n)$ denote the household’s strictly monotone, differentiable, and strictly quasiconcave utility function defined over the dollar amount of SNAP-eligible food consumption f and other consumption n . Suppose that there is a solution $f^* = \arg \max_f U(f, y - \max(0, f - b))$ such that $f^* > b$. The first-order necessary condition for this program is a necessary and sufficient condition for a solution to the program $\max_f U(f, y + b - f)$ in which the benefits are given in cash. Therefore $f^* = \arg \max_f U(f, y + b - f)$.

⁴Castner and Mabli (2010) estimate an MPCF out of cash income of 0.07 for SNAP participants. Hoynes and Schanzenbach (2009) estimate an MPCF out of cash income of 0.09-0.10 for populations with a high likelihood of participating in the Food Stamp Program.

have often coincided with other policy changes or economic shocks (Congressional Budget Office 2012), making it difficult to separate the effect of SNAP from the effect of these contextual factors.

In this paper we analyze a novel panel consisting of detailed transaction records from February 2006 to December 2012 for nearly half a million regular customers of a large US grocery retailer. The data contain information on method of payment, including whether payment was made using a government benefit card. We use the panel to study the effect of transitions on and off of SNAP, and of legislated changes in SNAP benefits, on household spending.

We adopt three approaches to isolate the causal effect of SNAP on spending: a panel event-study design using trends prior to SNAP adoption to diagnose confounds, an instrumental variables design exploiting plausibly exogenous variation in the timing of program exit, and a differences-in-differences design exploiting legislated changes to benefit schedules.

We motivate each of these approaches with findings from novel Rhode Island administrative data. The data show that household income and size change in the months preceding a household's transition on to SNAP, motivating our panel event-study design. The data also show that SNAP spell lengths are typically divisible by six months because of the recertification process, motivating our instrumental-variables design. National administrative records show discrete jumps in SNAP benefits associated with legislated program changes in 2008 and 2009, motivating our differences-in-differences design.

By construction our retail panel includes purchases at a single grocery chain. Rhode Island administrative data show that it is possible to reliably infer transitions onto SNAP using data from a single grocery chain, by focusing on consecutive periods of non-SNAP use followed by consecutive periods of SNAP use. Additional data, including a survey conducted by the retailer, show that SNAP participation is only weakly related to a household's choice of retailer.

Graphical analysis of our panel event-study design shows that after adoption of SNAP, households in the retailer panel increase SNAP-eligible spending by about \$110 a month, equivalent to a bit more than half of their monthly SNAP benefit. There is no economically meaningful trend in average SNAP-eligible spending prior to adoption of SNAP. Graphical analysis of our instrumental-variables and differences-in-differences designs implies an MPCF out of SNAP in the range of 0.5 to 0.6, consistent with the panel event-study design.

We exploit large swings in gasoline prices during our sample period to estimate the MPCF

out of cash for the retail panelists. We observe gasoline spending at the retailer and confirm that increases in gasoline prices lead to significant additional out-of-pocket expenses for panelist households. We estimate that every \$100 per month of additional gasoline spending reduces food spending by less than \$10, in line with past estimates of the MPCF out of cash for the SNAP-recipient population (e.g., Castner and Mabli 2010) but far below the estimated MPCF out of SNAP.

Turning to SNAP-ineligible spending at the retailer, we estimate a marginal propensity to consume (MPC) out of SNAP benefits of 0.02, which is statistically indistinguishable from the estimated MPC out of cash of 0.04.

We develop an economic model of food spending by households for whom SNAP benefits do not cover all food spending and are therefore fungible with cash. We show how to test the hypothesis of fungibility, allowing for the endogeneity of cash income and SNAP benefits, and for the possibility that different households' consumption functions do not share a common parameterization or parametric structure. Our tests consistently reject the null hypothesis that households treat SNAP benefits as fungible with other income.

We turn next to evidence on the psychological reasons for the observed departure from fungibility. We discuss responses to qualitative interviews conducted at a food pantry as part of a Rhode Island pilot proposal to modify SNAP benefit timing. Respondents were not scientifically sampled, and it is not appropriate to derive general conclusions from these interviews. Nevertheless, it is interesting that interview responses often indicate a difference in how households plan to spend SNAP and cash. Using our retail panel, we show that SNAP receipt reduces the store-brand share of expenditures and the share of items on which coupons are redeemed, but only for SNAP-eligible foods.

These findings suggest a possible role for mental accounting in explaining the high MPCF out of SNAP. We specify a parametric model of behavior that exhibits short-run time preference (Laibson 1997), price misperception (Liebman and Zekchauser 2004; Ito 2014), and mental accounting (Thaler 1999; Farhi and Gabaix 2015). Both price misperception and mental accounting are able to explain departures from fungibility, but only mental accounting is able to match the observed MPCF out of SNAP and the observed pattern of changes in shopping effort.

This paper contributes to a large literature on the effects of SNAP, and its predecessor the Food Stamp Program, on food spending, recently reviewed by Bitler (2015) and Hoynes and Schanzen-

bach (2016). There are four strands to this literature. The first strand studies the effect of converting food stamp benefits to cash. Moffitt (1989) finds that a cashout in Puerto Rico did not affect food spending. Wilde and Ranney (1996) find that behavior in two randomized cashout interventions is not consistent with fungibility; Schanzenbach (2002) finds that behavior in these same interventions is consistent with fungibility.⁵ The second strand, reviewed in Fox et al. (2004), either compares participants to nonparticipants or relates a household's food spending to its benefit amount in the cross-section or over time. Wilde (2001) and Hoynes and Schanzenbach (2009), among others, criticize this strand of the literature for using a source of variation in program benefits that is likely related to non-program determinants of spending.⁶ The third strand studies randomized evaluations of program extensions or additions. Collins et al. (2016) study a randomized evaluation of the Summer Electronic Benefit Transfer for Children program and use survey data to estimate an MPCF out of program benefits of 0.58.

The fourth strand exploits policy variation in program availability and generosity. Studying the initial rollout of the Food Stamp Program using survey data, Hoynes and Schanzenbach (2009) estimate an MPCF out of food stamps of 0.16 to 0.32, with confidence interval radius ranging from 0.17 to 0.27. Hoynes and Schanzenbach (2009) estimate an MPCF out of cash income of 0.09 to 0.10 and cannot reject the hypothesis that the MPCF out of food stamps is equal to the MPCF out of cash income. Studying the effect of a 2009 SNAP benefit expansion using survey data, Beatty and Tuttle (2015) estimate an MPCF out of SNAP benefits of 0.53 to 0.64 (they do not report a confidence interval on these values) and an MPCF out of cash income of 0.15.⁷ Closest to our study, Bruich (2014) uses retail scanner data with method-of-payment information to study the effect of a 2013 SNAP benefit reduction, estimating an MPCF out of SNAP benefits of 0.3 with confidence interval radius of 0.15.⁸ Bruich (2014) does not report an MPCF out of cash income. We estimate an MPCF out of SNAP benefits of 0.5 to 0.6 with confidence interval radius as low as

⁵Fox et al. (2004) question the validity of the findings from Puerto Rico and one of the randomized interventions, arguing that the best evidence indicates that cashout reduces food spending.

⁶Wilde et al. (2009) address the endogeneity of program benefits by exploiting variation in whether household food spending is constrained by program rules. Li et al. (2014) use panel data to study the evolution of child food insecurity in the months before and after family entry into the food stamp program.

⁷Nord and Prell (2011) estimate the effect of the 2009 benefit expansion on food security and food expenditures. Ratcliffe et al. (2011) and Yen et al. (2008) estimate the effect of SNAP and food stamps, respectively, on food insecurity, using state-level policy variables as excluded instruments.

⁸Andreyeva et al. (2012) and Garasky et al. (2016) use retail scanner data to describe the food purchases of SNAP recipients, but not to estimate the causal effect of SNAP on spending.

0.015, and an MPCF out of cash income of no more than 0.1.

This paper contributes new evidence of violations of fungibility in a large-stakes real-world decision with significant policy relevance. That households mentally or even physically separate different income sources according to spending intentions is well-documented in hypothetical-choice scenarios (e.g., Heath and Soll 1996; Thaler 1999) and ethnographic studies (e.g., Rainwater et al. 1959). Much of the recent evidence from real-world markets is confined to settings with little direct policy relevance (e.g., Milkman and Bashears 2009; Hastings and Shapiro 2013; Abeler and Marklein 2017). Important exceptions include Kooreman's (2000) study of a child tax credit in the Netherlands, Feldman's (2010) study of a change in US federal income tax withholding, and Benhassine et al.'s (2015) study of a labeled cash transfer in Morocco.

Methodologically, this paper shows how to test for the fungibility of money without assuming that the consumption function takes a particular parametric form or that the consumption function is identical for all households.⁹ Our approach nests Kooreman's (2000), but avoids the concern that a rejection of fungibility is due to misspecification of functional forms (Ketcham et al. 2016).¹⁰

Finally, the paper presents new evidence from novel administrative data on SNAP recipients in Rhode Island, including the first evidence we are aware of from state administrative data on how household wage income evolves before and after entry into SNAP.¹¹ Although we present these findings primarily as background, they are of interest in their own right as evidence on the contextual factors associated with SNAP adoption.

⁹Whereas classical tests of consumer rationality (Varian 1983; Blundell et al. 2003) require observing price changes, we provide a set of intuitive sufficient conditions on the model and the measurement process that permit testing based on income variation alone.

¹⁰Our use of a parametric model to quantify simultaneously the predictions of multiple psychological departures from the neoclassical benchmark is similar in spirit to DellaVigna et al. (forthcoming). Hastings and Shapiro (2013) and Ganong and Noel (2017) also compare the predictions of alternative psychological models.

¹¹Other recent studies analyzing linked unemployment insurance and SNAP data include Anderson et al. (2012) and Leung and O'Leary (2015).

2 Background and evidence from administrative and survey data

2.1 Rhode Island administrative data

We use Rhode Island state administrative records housed in a secure facility at the Rhode Island Innovative Policy Laboratory at Brown University. Personally identifiable information has been removed from the data and replaced with anonymous identifiers that make it possible for researchers with approved access to join and analyze records associated with the same individual while preserving anonymity. These records are not linked to our retail panel.

We obtain the state's SNAP records from October 2004 through June 2016. These data define the months of benefit receipt and the collection of individuals associated with every household on SNAP in every month. We assume that a household's composition is unchanged prior to its first benefit receipt and that it does not change from its most recent composition between the end of any given period of benefit receipt and the start of the next period. We exclude from our analysis any household whose membership we cannot uniquely identify in every month,¹² or whose adult composition changes during the sample period. The final sample consists of 184,308 unique households.

From SNAP records we compute, for each household and month, the total number of children in the household under five years old. From the records of the state unemployment insurance system we compute, for each household and quarter,¹³ the sum of total unemployment insurance benefits received from and total earnings reported to the state unemployment insurance system by all individuals who are in the household as of the quarter's end.¹⁴ We refer to this total as household income, but we note that it excludes income not reported to the Rhode Island unemployment insurance system, such as social security benefits and out-of-state earnings.

We also obtain records of all debits and credits to the SNAP Electronic Benefit Transfer (EBT)

¹²This can occur either because we lack a unique identifier for a member individual or because a given individual is associated with multiple households in the same month.

¹³Data on earnings are missing from our database for the fourth quarter of 2004 and the second quarter of 2011.

¹⁴We exclude from our analysis any household-quarter in which the household's total quarterly earnings exceed the 99.9999th percentile or in which unemployment insurance benefits in any month of the quarter exceed three times the four-week equivalent of the 2016 maximum weekly benefit of \$707 (Rhode Island Department of Labor and Training 2016).

cards of Rhode Island residents for the period September 2012 through October 2015. From these we identify all household-months in which the household received a SNAP benefit and all household-months in which the household spent SNAP benefits at a large, anonymous retailer in Rhode Island (“Rhode Island Retailer”) chosen to be similar to the retailer that provided our retail panel. Although these data can be linked to the SNAP records using a household identifier, we do not exploit that link in the analysis that follows.

2.2 Changes in household circumstances around SNAP adoption

Household income and household size are major determinants of SNAP eligibility (FNS 2016b). We therefore hypothesize that entry into SNAP is associated with a decline in household income and a rise in household size. Figure 1 confirms this hypothesis in our administrative data. The figure shows panel event-study plots of household income and number of children as a function of time relative to SNAP adoption, which we define to occur on the first quarter or month, respectively, of a household’s first SNAP spell. In the period of SNAP adoption, household income declines and the number of children rises, on average.

Past research shows that greater household size and lower household income are associated, respectively, with greater and lower at-home food expenditures among the SNAP-recipient population (Castner and Mabli 2010).¹⁵ It is therefore unclear whether these contextual factors should contribute a net rise or fall in food expenditures in the period of SNAP adoption. Because figure 1 shows that these factors trend substantially in the periods preceding SNAP adoption, we can assess their net effect by studying trends in spending prior to adoption.

Figure 1 therefore motivates our panel event-study research design, in which we use trends in spending prior to SNAP adoption to diagnose the direction and plausible magnitude of confounds.

¹⁵Past research also finds that unemployment—a likely cause of the decline in income associated with SNAP adoption—is associated with a small decline in spending on food for home consumption. Using cross-sectional variation in the Continuing Survey of Food Intake by Individuals, Aguiar and Hurst (2005) estimate that unemployment is associated with 9 percent lower at-home food expenditure. Using pseudo-panel variation in the Family Expenditure Survey, Banks et al. (1998) estimate that unemployment is associated with a 7.6 percent decline in the sum of food consumed in the home and domestic energy. Using panel variation in the Panel Study of Income Dynamics, Gough (2013) estimates that unemployment is associated with a statistically insignificant 1 to 4 percent decline in at-home food expenditure. Using panel variation in checking account records, Ganong and Noel (2016) estimate that the onset of unemployment is associated with a 3.1 percent decline in at-home food expenditure. Aggregate data seem to confirm these findings: real average annual at-home food expenditure fell by 1.6 percent from 2006 to 2009, during which time the unemployment rate more than doubled (Kumcu and Kaufman 2011).

2.3 Length of SNAP spells and the certification process

When a state agency determines that a household is eligible for SNAP, the agency sets a certification period at the end of which benefits will terminate if the household has not documented continued eligibility.¹⁶ The certification period may not exceed 24 months for households whose adult members are elderly or disabled, and may not exceed 12 months otherwise (FNS 2014). In practice, households are frequently certified for exactly these lengths of time, or for other lengths divisible by 6 months (Mills et al. 2014).

Figure 2 shows the distribution of SNAP spell lengths in Rhode Island administrative data. The figure shows clear spikes in the density at spell lengths divisible by 6 months.

Figure 2 motivates our instrumental variables research design, which exploits the six-month divisibility of certification periods as a source of plausibly exogenous timing of program exit.

2.4 Legislated changes in SNAP benefit schedules

Appendix figure 1 shows the average monthly SNAP benefit per US household from February 2006 to December 2012, which coincides with the time frame of our retail panel. The series exhibits two discrete jumps, which correspond to two legislated changes in the benefit schedule: an increase before October 2008 due to the 2008 Farm Bill and an increase in April 2009 due to the American Recovery and Reinvestment Act.

Appendix figure 1 motivates our differences-in-differences research design, which exploits these legislated benefit increases to estimate the MPCF out of SNAP.

2.5 Inferring SNAP adoption from single-retailer data

Households can spend SNAP at any authorized retailer, but we will conduct our analysis of food spending using data from a single retail chain. Changes in a household's choice of retailer could be mistaken for program entry and exit in single-retailer data. We use our EBT panel to evaluate the importance of these mistakes and to determine how best to infer program transitions in single-retailer data.

¹⁶Federal rules state that “the household’s certification period must not exceed the period of time during which the household’s circumstances (e.g., income, household composition, and residency) are expected to remain stable” (FNS 2014).

For each $K \in \{1, \dots, 12\}$ and for each household in our EBT panel, we identify all cases of K consecutive months without SNAP spending at the Rhode Island Retailer followed by K consecutive months with SNAP spending at the Rhode Island Retailer. We then compute the share of these transition periods in which the household newly enrolled in SNAP within two months of the start of SNAP spending at the retailer, where we define new enrollment as receipt of at least \$10 in SNAP benefits following a period of at least three consecutive months with no benefit.

Figure 3 plots the share of households newly enrolling in SNAP as a function of the radius K of the transition period. For low values of K , many transitions reflect retailer-switching rather than new enrollments in SNAP. The fraction of transitions that represent new enrollments increases with K . For $K = 6$ and above, the fraction constituting new enrollments is over 86 percent. When we focus on households who do the majority of their SNAP spending at the retailer in question—a sample arguably more comparable to the households in our retail panel—this fraction rises to 96 percent.

Figure 3 motivates our definition of SNAP adoption in the retailer data.

2.6 SNAP participation and choice of retailer

Even if we isolate suitably exogenous changes in SNAP participation and benefits, our analysis of single-retailer data could be misleading if SNAP participation directly affects retailer choice.

Ver Ploeg et al. (2015) study the types of stores at which SNAP recipients shop using nationally representative survey data collected from April 2012 through January 2013. For 46 percent of SNAP recipients, the primary grocery retailer is a supercenter, for 43 percent it is a supermarket, for 3 percent it is another kind of store, and for 8 percent it is unknown. The corresponding values for all US households are 45 percent, 44 percent, 4 percent, and 7 percent. As with primary stores, the distribution of alternate store types is nearly identical between SNAP recipients and the population as a whole. SNAP recipients' choice of store type is also nearly identical to that of low-income non-recipients.

The online appendix presents analogous evidence on choice of retail chain using the same data as Ver Ploeg et al. (2015). We find that SNAP participation is not strongly related to households' choice of retail chain. While this evidence does not speak directly to the causal effect of SNAP on

retailer choice, it seems to cast doubt on the hypothesis that SNAP receipt per se is a major factor determining where households shop for food.

As further evidence, a companion note to this paper analyzes Nielsen Homescan data and finds little relationship at the state-year level between changes in the market shares of major retailers and changes in the number of SNAP recipients in the state.

In the next section we present further evidence on retailer substitution using survey data collected by the retailer that supplied our panel.

3 Retailer data and definitions

3.1 Purchases and demographics

We obtained anonymized transaction-level data from a large U.S. grocery retailer with gasoline stations on site. The data comprise all purchases in five states made using loyalty cards by customers who shop at one of the retailer's stores at least every other month.¹⁷ We refer to these customers as households. We observe 6.02 billion purchases made on 608 million purchase occasions by 486,570 households from February 2006 through December 2012. We exclude from our analysis the 1,214 households who spend more than \$5,000 in a single month.

For each household, we observe demographic characteristics including age, household composition, and ZIP code. We use these data in robustness checks and to study heterogeneity in our estimates.

For each item purchased, we observe the quantity, the pre-tax amount paid, a flag for the use of WIC, and the dollar amount of coupons or other discounts applied to the purchase.

For each purchase occasion, we observe the date, a store identifier, and a classification of the store into a retailer division, which is a grouping based on the store's brand and distribution geography. We also observe the main payment method used for the purchase, defined as the payment method (e.g., cash, check, government benefit) accounting for the greatest share of expenditure. For purchase occasions in March 2009 and later, we additionally observe the exact breakdown of

¹⁷The retailer also provided us with data on the universe of transactions at a single one of the retailer's stores. In the online appendix we show that our estimates of the MPCF are similar between our baseline panel and this alternative panel.

spending by payment method.

We classify a purchase occasion as a *SNAP purchase occasion* if the main payment method is a government benefit and WIC is not used. Using the detailed payment data for purchase occasions in March 2009 and later, we calculate that SNAP is used in only 0.23 percent of the purchase occasions that we do not classify as SNAP purchase occasions. The appendix table shows that our key results are not sensitive to excluding WIC users from the sample.

We define a *SNAP month* as any household-month with positive total spending across SNAP purchase occasions.¹⁸ Of the household-months in our panel, 7.8 percent are SNAP months. Of the households in our panel, 43 percent experience at least one SNAP month.

3.2 Product characteristics

The retailer provided us with data on the characteristics of each product purchased, including an indicator for whether the product is store-brand, a text description of the product, and the product's location within a taxonomy.

We classify products as SNAP-eligible or SNAP-ineligible based on the retailer's taxonomy and the guidelines for eligibility published on the USDA website.¹⁹ Among all non-fuel purchases in our data, 71 percent of spending goes to SNAP-eligible products, 25 percent goes to SNAP-ineligible products, and the remainder goes to products that we cannot classify.

We use our detailed payment data for purchases made in SNAP months in March 2009 or later to validate our product eligibility classification. Among all purchases made at least partly with SNAP in which we classify all products as eligible or ineligible, in 98.6 percent of cases the expenditure share of SNAP-eligible products is at least as large as the expenditure share paid with SNAP. Among purchases made entirely with SNAP, in 98.7 percent of cases we classify no items as SNAP-ineligible. Among purchases in which all items are classified as SNAP-ineligible, in more than 99.9 percent of cases SNAP is not used as a payment method.

¹⁸Using our detailed payment data for March 2009 and later, we can alternatively define a SNAP month as any month in which a household uses SNAP. This definition agrees with our principal definition in all but 0.27 percent of household-months.

¹⁹Grocery and prepared food items intended for home consumption are generally SNAP-eligible (FNS 2017). Alcohol, tobacco, pet food, and prepared food intended for on-premise consumption are SNAP-ineligible (FNS 2017).

3.3 Shopping effort

For each household and month we compute the store-brand share of expenditures and the share of items for which coupons are redeemed for both SNAP-eligible and SNAP-ineligible purchases. Prior evidence suggests that both of these can serve as a proxy for households' efforts to save money.²⁰ We adjust these measures for the composition of purchases as follows. For each item purchased, we compute the store-brand share of expenditure among other households buying an item in the same product category in the same retailer division and the same calendar month and week. The expenditure-weighted average of this measure across purchases by a given household in a given month is the predicted store-brand share, i.e. the share of expenditures that would be store-brand if the household acted like others in the panel who buy the same types of goods. Likewise, we compute the share of other households buying the same item in the same retailer division, month, and week who redeem coupons, and compute the average of this measure across purchases by a given household in a given month to form a predicted coupon use. We subtract the predicted from the actual value of each shopping effort measure to form measures of *adjusted store-brand share* and *adjusted coupon redemption share*.

3.4 Monthly spending and benefits

For each household in our panel we calculate total monthly spending on SNAP-eligible items, fuel, and SNAP-ineligible items excluding fuel. We calculate each household's total monthly SNAP benefits as the household's total spending across all SNAP purchase occasions within the month.²¹

Our data corroborate prior evidence (e.g., Hoynes et al. 2015) that, for most households, SNAP benefits do not cover all SNAP-eligible spending. For 93 percent of households who ever use SNAP, average SNAP-eligible spending in non-SNAP months exceeds average SNAP benefits in SNAP months. SNAP-eligible spending exceeds SNAP benefits by at least \$10 in 93 percent of SNAP months and by at least 5 percent in 92 percent of SNAP months. The appendix table reports estimates of key parameters for the subset of households for whom, according to various

²⁰Store-brand items tend to be less expensive than national-brand alternatives, and correspondingly are more popular among lower-income households (Bronnenberg et al. 2015). Coupon use rose during the Great Recession, reflecting households' greater willingness to trade time for money (Nevo and Wong 2015).

²¹Our concept of total SNAP benefits has a correlation of 0.98 with the exact amount of SNAP spending calculated using detailed payment information in SNAP months March 2009 and later.

definitions, SNAP benefits are inframarginal to total food spending.

3.5 SNAP adoption

Motivated by the analysis in section 2.5, we define a *SNAP adoption* as a period of six or more consecutive non-SNAP months followed by a period of six or more consecutive SNAP months. We refer to the first SNAP month in an adoption as an *adoption month*. We define a *SNAP adopter* as a household with at least one SNAP adoption. Our panel contains a total of 24,456 SNAP adopters.²²

Panel A of figure 4 shows the share of SNAP adopters with positive SNAP spending in each of the 12 months before and after a household's first SNAP adoption. Panel B of figure 4 shows average SNAP benefits before and after adoption. Following adoption, the average household receives just over \$200 in monthly SNAP benefits. For comparison, the average US SNAP benefit per household in fiscal 2009, roughly at the midpoint of our sample period, was \$276 (FNS 2016a). The average benefit in fiscal 2008 was \$227 (FNS 2016a). The online appendix presents a comparison of average benefits between SNAP adopters in the retail panel and demographically similar households in publicly available administrative records.

We conduct the bulk of our analysis using the sample of SNAP adopters. The appendix table presents our key results for a broader sample and for a more stringent definition of SNAP adoption.

3.6 Retailer share of wallet

Spending patterns suggest that panelists buy a large fraction of their groceries at the retailer. Mabili and Malsberger (2013) estimate average 2010 spending on food at home by SNAP recipients of \$380 per month using data from the Consumer Expenditure Survey. Hoynes et al. (2015) find that average per-household food expenditures are 20 to 25 percent lower in the Consumer Expenditure Survey than in the corresponding aggregates from the National Income and Product Accounts. In the six months following a SNAP adoption, average monthly SNAP-eligible spending in our data is \$469.

²²To assess the potential for false positives in our definition of SNAP adoption, we identified the set of all cases in which a household exhibits six or more consecutive SNAP months with SNAP spending at or below five dollars, followed by six or more consecutive SNAP months with spending above five dollars. Such cases are likely not true adoptions but could arise if households' propensity to spend SNAP at the retailer fluctuates sufficiently from month to month. We find no such cases in our data. When we increase the cutoff to ten dollars, we find one such case.

Panelists also seem to buy a large fraction of their gasoline at the retailer: average monthly fuel spending at the retailer is \$97 in the six months following SNAP adoption, as compared to Mabl and Malsberger’s (2013) estimate of \$115.

Survey data from the retailer suggest that SNAP use is associated with a reduction in the retailer’s share of overall category spending. During the period June 2009 to December 2011, the retailer conducted an online survey on a convenience sample of customers. The survey asked:

About what percentage of your total overall expenses for groceries, household supplies, or personal care items do you, yourself, spend in the following stores?

Respondents were presented with a list of retail chains including the one from which we obtained our data. Excluding responses in which the reported percentages do not sum to 100, we observe at least one response from 961 of the households in our panel. Among survey respondents that ever use SNAP, the average reported share of wallet for the retailer is 0.61 for those surveyed during non-SNAP months ($N = 311$ survey responses) and 0.53 for those surveyed during SNAP months ($N = 80$ survey responses).²³ The same qualitative pattern obtains among SNAP adopters, and in responses to a retrospective question about shopping frequency.²⁴

Taken at face value, these findings suggest that retailer substitution will tend, if anything, to bias downward the estimated effect of SNAP participation on food spending. In the appendix table we verify that our results are robust to restricting attention to households with relatively few supermarkets in their county, for whom opportunities to substitute across retailers are presumably more limited.

4 Descriptive evidence

4.1 Marginal propensity to consume out of SNAP benefits

Figure 5 shows the evolution of monthly spending before and after SNAP adoption for our sample of SNAP adopters. Each plot shows coefficients from a regression of spending on a vector

²³The difference in means is statistically significant ($t = 2.15$, $p = 0.032$).

²⁴The question asks, “In your opinion, do you think you, yourself have been shopping more, less, or about the same amount at the retailer over the past 3 months?” Among households surveyed in a SNAP month, 60 percent report that their frequency of shopping at the retailer has stayed “about the same.” Among those saying that it has not stayed the same, a majority (59 percent) say that it has decreased.

of indicators for months relative to the household's first SNAP adoption. Panel A shows that SNAP-eligible spending increases by approximately \$110 in the first few months following SNAP adoption. Recall from figure 4 that the average household receives monthly SNAP benefits of approximately \$200 following SNAP adoption. Taking the ratio of the increase in spending to the benefit amount, we estimate an MPCF out of SNAP benefits between 0.5 and 0.6. The online appendix shows that the increase in SNAP-eligible spending at adoption is greatest for those households who experience the greatest increase in SNAP benefits. The online appendix also shows that the increase in spending at adoption is similar for both perishable and non-perishable items.

Panel B shows that SNAP-ineligible spending increases by approximately \$5 following SNAP adoption, implying an MPC of a few percentage points. The increase in SNAP-ineligible spending is smaller in both absolute and proportional terms than the increase in SNAP-eligible spending. The online appendix shows directly that the share of spending devoted to SNAP-eligible items increases significantly following SNAP adoption. This finding is not consistent with the hypothesis that SNAP leads to a proportional increase in spending across all categories due to substitution away from competing retailers.

Following the analysis in section 2.2, trends in spending prior to adoption should provide a sense of the influence of changes in contextual factors on spending. Panel A shows very little trend in SNAP-eligible spending prior to SNAP adoption. Panel B shows, if anything, a slight decline in SNAP-ineligible spending prior to adoption, perhaps due to economic hardship. Neither of these patterns seems consistent with the hypothesis that the large increase in SNAP-eligible spending that occurs at SNAP adoption is driven by changes in contextual factors. Consistent with an important role for SNAP, the online appendix shows that the increase in spending at adoption is concentrated in the early weeks of the month, when SNAP benefits are typically spent.

Figure 6 shows the evolution of monthly spending during a monthly clock that begins at SNAP adoption and resets every six months. Panels A and B show that SNAP participation and benefits fall especially quickly in the first month of the clock, consistent with the finding in section 2.3 that SNAP spell lengths tend to be divisible by six months. Participation and benefits also fall more quickly in the sixth month, perhaps reflecting error in our classification of adoption dates.

Panel C of figure 6 shows that the pattern of SNAP-eligible spending closely follows that of SNAP benefits. Benefits decline by about \$12 more in the first month of the cycle than in the

second. Correspondingly, SNAP-eligible spending declines by \$6 to \$7 more in the first month than in the second. Taking the ratio of these two values implies an MPCF out of SNAP benefits between 0.5 and 0.6, consistent with the evidence in figure 5. The online appendix shows that patterns similar to those in figure 6 obtain for a sample of households that exhibit a period of six consecutive non-SNAP months after adoption, for whom short-run “churn” off of and back on to SNAP (Mills et al. 2014) is less likely to be a factor.

Appendix figure 2 plots the evolution of SNAP-eligible spending around the legislated benefit changes described in section 2.4. The plot shows that likely SNAP recipients’ SNAP-eligible spending increases relative to that of likely non-recipients around the periods of benefit increases. The online appendix reports the results of a differences-in-differences analysis of these changes in the spirit of Bruich (2014) and Beatty and Tuttle (2015). We estimate an MPCF out of SNAP benefits of 0.53, and if anything a negative effect of benefit expansions on SNAP-ineligible spending.

4.2 Marginal propensity to consume food out of cash

Two pieces of indirect evidence suggest that the MPCF out of cash is much below the values of 0.5 to 0.6 that we estimate for the MPCF out of SNAP.

The first is that, for the average SNAP recipient, food at home represents only 18 percent of total expenditure (Mabli and Malsberger 2013). Engel’s Law (Engel 1857; Houthakker 1957) holds that the budget share of food declines with total resources, and hence that the budget share exceeds the MPCF. Engel’s Law is not consistent with a budget share of 0.18 and an MPCF of 0.5 to 0.6.

The second is that prior estimates of the MPCF out of cash for low-income populations are far below 0.5. Castner and Mabli (2010) estimate an MPCF of 0.07 for SNAP recipients. Hoynes and Schanzenbach (2009) estimate an MPCF of 0.09-0.10 for populations with a high likelihood of entering the Food Stamp Program. Assessing the literature, Hoynes and Schanzenbach (2009) note that across “a wide range of data (cross sectional, time series) and econometric methods” past estimates of the MPCF out of cash income are in a “quite tight” range from 0.03 to 0.17 for low-income populations.

For more direct evidence, we study the effect on spending of the large changes in gasoline

prices during our sample period. These changes affect the disposable income available to households and therefore give us a window into the MPCF out of cash income.

Panel A of figure 7 shows the time-series relationship between gasoline prices and fuel expenditure for SNAP adopters at different quartiles of the distribution of average fuel expenditure. Those households in the upper quartiles exhibit substantial changes in fuel expenditure when the price of gasoline changes. For example, during the run-up in fuel prices in 2007, part of an upward trend often attributed to increasing demand for oil from Asian countries (e.g., Kilian 2010), households in the top quartile of fuel spending increased their spending on fuel by almost \$100 per month. Households in lower quartiles increased their fuel spending by much less.

Panel B of figure 7 shows the time-series relationship between gasoline prices and SNAP-eligible expenditure for the same groups of households. The relationship between the two series does not appear consistent with an MPCF out of cash income of 0.5 to 0.6. For example, if the MPCF out of cash income were 0.5 we would expect households in the top quartile of fuel spending to decrease SNAP-eligible spending significantly during the run-up in fuel prices in 2007. In fact, we see no evidence of such a pattern, either looking at the top quartile in isolation, or comparing it to the lower quartiles.

The absence of a strong response of SNAP-eligible spending to fuel prices is consistent with prior evidence of a low MPCF out of cash. It is not consistent with the hypothesis that changes in income drive large changes in the retailer's share of wallet, as such income effects would lead to a relationship between gasoline prices and measured SNAP-eligible spending.

4.3 Quantitative summary

Table 1 presents two-stage least squares (2SLS) estimates of a series of linear regression models. In each model the dependent variable is the change in spending from the preceding month to the current month. The endogenous regressors are the change in the SNAP benefit and the change in the additive inverse of fuel spending. The coefficients on these endogenous regressors can be interpreted as MPCs. Each model includes calendar month fixed effects. Household fixed effects are implicit in the first-differencing of the variables in the model.

All models use the interaction of the change in the price of regular gasoline and the house-

hold's average monthly number of gallons of gasoline purchased as an excluded instrument. This instrument permits estimating the MPC out of cash following the logic of figure 7.

Models (1), (2), and (3) of table 1 use the change in SNAP-eligible spending as the dependent variable. The models differ in the choice of excluded instruments for SNAP benefits. In model (1), the instrument is an indicator for whether the month is an adoption month. In model (2), it is an indicator for whether the month is the first month of the six-month SNAP clock. These instruments permit estimating the MPCF out of SNAP following the logic of figures 5 and 6, respectively. In model (3), both of these instruments are used.

Estimates of models (1), (2), and (3) indicate an MPCF out of SNAP between 0.55 and 0.59 and an MPCF out of cash close to 0. In model (3), confidence intervals exclude an MPCF out of SNAP below 0.57 and an MPCF out of cash above 0.1. In all cases, we reject the null hypothesis that the MPCF out of SNAP is equal to the MPCF out of cash.

Model (4) parallels model (3) but uses SNAP-ineligible spending as the dependent variable. We estimate an MPC out of SNAP of 0.02 and an MPC out of cash of 0.04. We cannot reject the hypothesis that these two MPCs are equal.

The appendix table shows that the conclusion that the MPCF out of SNAP exceeds the MPCF out of cash is robust to a number of changes in sample and specification, including excluding households for whom SNAP benefits may not be economically equivalent to cash, restricting to single-adult households to limit the role of intra-household bargaining, focusing on SNAP exit instead of SNAP adoption, and excluding households who adopted during the Great Recession.

The online appendix reports that the implied MPCF out of SNAP is slightly higher in the household's first SNAP adoption than in subsequent SNAP adoptions. We cannot reject the hypothesis that the MPCF is equal between first and subsequent adoptions, and the MPCF out of SNAP does not differ meaningfully according to the SNAP penetration in the household's local area. The online appendix also reports estimates of the MPCF out of SNAP and cash for various demographic groups.

5 Model and tests of fungibility

5.1 Model

In each month $t \in \{1, \dots, T\}$, household i receives SNAP benefits $b_{it} \geq 0$ and disposable cash income $y_{it} > 0$. The household chooses food expenditure f_{it} and nonfood expenditure n_{it} to solve

$$\begin{aligned} & \max_{f, n} U_i(f, n; \xi_{it}) \\ \text{s.t. } & n \leq y_{it} - \max(0, f - b_{it}) \end{aligned} \quad (1)$$

where ξ_{it} is a preference shock and $U_i(\cdot)$ is a utility function strictly increasing in f and n . The variables $(b_{it}, y_{it}, \xi_{it})$ are random with support Ω_i .

Assumption 1. *For each household i , optimal food spending can be written as*

$$f_{it} = f_i(y_{it} + b_{it}, \xi_{it}) \quad (2)$$

where $f_i(\cdot)$ is a function with range $[0, y_{it} + b_{it}]$.

A sufficient condition for assumption 1 is that, for each household i , at any point $(b, y, \xi) \in \Omega_i$ the function $U_i(f, y + b - f; \xi)$ is smooth and strictly concave in f and has a stationary point $f^* > b$. Then optimal food spending exceeds the level of SNAP benefits even if benefits are disbursed as cash, so the “kinked” budget constraint in (1) does not affect the choice of f_{it} .

For each household and month, an econometrician observes data $(f_{it}, b_{it}, y_{it}, z_{it})$ where z_{it} is a vector of instruments. A concern is that ξ_{it} is determined partly by contextual factors such as job loss that directly affect y_{it} and b_{it} .

Assumption 2. *Let $v_{it} = (y_{it} + b_{it}) - E(y_{it} + b_{it} | z_{it})$. For each household i , the instruments z_{it} satisfy*

$$(\xi_{it}, v_{it}) \perp z_{it}. \quad (3)$$

Proposition 1. *Under assumptions 1 and 2, for each household i*

$$E(f_{it} | z_{it}) = \varphi_i(E(y_{it} + b_{it} | z_{it})) \quad (4)$$

for some function $\varphi_i(\cdot)$.

Proof. Let P_i denote the CDF of (ξ_{it}, ν_{it}) . Then

$$\begin{aligned} \mathbb{E}(f_{it}|z_{it}) &= \int f_i(\mathbb{E}(y_{it} + b_{it}|z_{it}) + \nu_{it}, \xi_{it}) dP_i(\xi_{it}, \nu_{it}|z_{it}) \\ &= \int f_i(\mathbb{E}(y_{it} + b_{it}|z_{it}) + \nu_{it}, \xi_{it}) dP_i(\xi_{it}, \nu_{it}) \\ &= \varphi_i(\mathbb{E}(y_{it} + b_{it}|z_{it})) \end{aligned}$$

where the first equality follows from assumption 1 and the second from assumption 2. See Blundell and Powell (2003, p. 330). \square

Example. (Cobb-Douglas) Suppose that for each household i there is $\beta_i \in (0, 1)$ such that:

$$U_i(f, n, \xi) = \begin{cases} (f - \xi)^{\beta_i} (n + \xi)^{1-\beta_i}, & \text{if } f \geq \xi \geq -n \\ -\infty, & \text{otherwise} \end{cases} \quad (5)$$

with $\beta_i(y + b) + \xi > b$ and $(1 - \beta_i)(y + b) > \xi$ at all points in Ω_i . Then assumption 1 holds with

$$f_i(y_{it} + b_{it}, \xi_{it}) = \beta_i(y_{it} + b_{it}) + \xi_{it}. \quad (6)$$

and, under assumption 2, proposition 1 applies with

$$\varphi_i(\mathbb{E}(y_{it} + b_{it}|z_{it})) = \alpha_i + \beta_i \mathbb{E}(y_{it} + b_{it}|z_{it}) \quad (7)$$

for $\alpha_i \equiv \mathbb{E}(\xi_{it})$.

Remark 1. In his study of a child tax credit in the Netherlands, Kooreman (2000) assumes a version of (6), which he estimates via ordinary least squares using cross-sectional data under various restrictions on α_i , β_i , and ξ_{it} .

5.2 Testing for fungibility

Index a family of perturbations to the model by γ . Let f_{it}^γ be food spending under perturbation γ , with

$$f_{it}^\gamma = f_i(y_{it} + b_{it}, \xi_{it}) + \gamma b_{it} \quad (8)$$

for $f_i(\cdot)$ the function defined in assumption 1. We may think of γ as the excess sensitivity of food spending to SNAP benefits. The null hypothesis that the model holds is equivalent under (8) to $\gamma = 0$.

Let $Y_{it} = E(y_{it} + b_{it} | z_{it})$ and $B_{it} = E(b_{it} | z_{it})$ and observe that

$$f_{it}^\gamma - E(f_{it}^\gamma | Y_{it}) = \gamma(B_{it} - E(B_{it} | Y_{it})) + e_{it} \quad (9)$$

where $E(e_{it} | Y_{it}, B_{it}) = 0$. The nuisance terms $\varphi_i(\cdot)$ have been “partialled out” of (9) as in Robinson (1988). The target γ can be estimated via OLS regression of $(f_{it}^\gamma - E(f_{it}^\gamma | Y_{it}))$ on $(B_{it} - E(B_{it} | Y_{it}))$.

Remark 2. It is possible to allow for measurement error in f_{it} that depends on $(y_{it} + b_{it})$. Say that for known function $m(\cdot)$, unknown function $\lambda_{it}(\cdot)$, and unobserved measurement error η_{it} independent of z_{it} we have that measured food spending \hat{f}_{it} follows

$$m(\hat{f}_{it}) = m(f_{it}) + \lambda_{it}(y_{it} + b_{it}, \eta_{it}). \quad (10)$$

Then under perturbations $m(f_{it}^\gamma) = m(f_{it}) + \gamma b_{it}$ an analogue of (9) holds, replacing f_{it}^γ with $m(f_{it}^\gamma)$. Examples include additive measurement error, where $m(\cdot)$ is the identity function, and multiplicative measurement error, where $m(\cdot)$ is the natural logarithm. The latter case has a simple interpretation as one in which the econometrician observes spending at a single retailer whose share of total household food spending is given by $\exp(\lambda_{it}(y_{it} + b_{it}, \eta_{it}))$. The appendix table presents estimates corresponding to this case.

Remark 3. The reasoning above is unchanged if b_{it} and y_{it} are each subject to an additive measurement error that is mean-independent of z_{it} . In this case, we can simply let Y_{it} and B_{it} represent the conditional expectations of the corresponding mismeasured variables.

5.3 Implementation and results

With (9) in mind, estimation proceeds in three steps:

Step 1. Estimate (Y_{it}, B_{it}) from (y_{it}, b_{it}, z_{it}) , yielding estimates $(\hat{Y}_{it}, \hat{B}_{it})$.

Step 2. Estimate $(E(f_{it}^\gamma | Y_{it}), E(B_{it} | Y_{it}))$ from $(f_{it}^\gamma, \hat{Y}_{it}, \hat{B}_{it})$, yielding estimates $(\widehat{E(f_{it}^\gamma | Y_{it})}, \widehat{E(B_{it} | Y_{it})})$.

Step 3. Estimate γ from $(f_{it}^\gamma - \widehat{E(f_{it}^\gamma | Y_{it})}, \hat{B}_{it} - \widehat{E(B_{it} | Y_{it})})$, yielding estimate $\hat{\gamma}$.

We let f_{it}^γ be SNAP-eligible spending, b_{it} be SNAP benefits, and y_{it} be the additive inverse of fuel spending. We let the instruments z_{it} be given by the number of SNAP adoptions experienced by household i as of calendar month t , and the product of the average price of regular gasoline with the household's average monthly number of gallons of gasoline purchased.

In step 1, we estimate (Y_{it}, B_{it}) via first-differenced regression of $(y_{it} + b_{it})$ and b_{it} on z_{it} .

In step 2, we consider four specifications for estimating $(E(f_{it}^\gamma | Y_{it}), E(B_{it} | Y_{it}))$. In the first, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , pooling across households. In the second, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , separately by household. In the third, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on a linear spline in \hat{Y}_{it} with knots at the quintiles, separately by household. In the fourth, we estimate these via locally weighted polynomial regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , separately by household. Thus, the first specification implicitly treats φ_i as linear and homogeneous across households, the second treats φ_i as linear and heterogeneous across households, and the third and fourth allow φ_i to be nonlinear and heterogeneous across households.

In step 3, we estimate γ via first-differenced regression of $(f_{it}^\gamma - \widehat{E(f_{it}^\gamma | Y_{it})})$ on $(\hat{B}_{it} - \widehat{E(B_{it} | Y_{it})})$.

Table 2 presents the results. Across all four specifications, our estimates of γ are greater than 0.5, and in all cases we can reject the null hypothesis that $\gamma = 0$ with a high level of confidence. The online appendix presents simulation evidence on the size of these tests and presents estimates using an alternative method of computing standard errors. The appendix table presents a range of robustness checks for these tests, including one in which we deseasonalize the dependent variable.

6 Interpretation

We speculate that households treat SNAP benefits as part of a separate mental account, psychologically earmarked for spending on food. In this section we discuss results of qualitative interviews conducted at a food pantry in Rhode Island. We then present quantitative evidence on changes in shopping effort at SNAP adoption. Finally, we present a parametric model that quantifies the potential role of several psychologically motivated departures from fungibility, including mental accounting.

6.1 Qualitative interviews with SNAP-recipient households

As part of preparation related to a state proposal to pilot a change to SNAP benefit distribution, Rhode Island Innovative Policy Laboratory staff conducted a series of qualitative interviews at a large food pantry in Rhode Island in May, July, and August 2016. Interviewees were approached in the waiting room of the pantry and were offered a \$5 gift card to a grocery retailer in exchange for participating. Interviews were conducted in English and Spanish.

Interviewees were selected from those waiting to be served at the food pantry and were not sampled scientifically. Interviews were conducted primarily to inform the implementation of the pilot program and the responses should not be taken to imply any generalizable conclusions. We report them here as context for our quantitative evidence.

Of the 25 interviews conducted, 19 were with current SNAP recipients. Of these, all but three reported spending non-SNAP funds on groceries each month, with an average out-of-pocket spending of \$100 for those reporting positive out-of-pocket spending.

Each interviewee was asked the following two questions, which we refer to as SNAP and CASH:

(SNAP) Imagine that **in addition to your current benefit, you received an extra \$100 in SNAP benefits** at the beginning of the month. How would this change the way that you spend your money during the month? [emphasis added]

(CASH) Imagine that **you received an additional \$100 in cash** at the beginning of the month. How would this change the way that you spend your money during the month? [emphasis added]

Of the 16 SNAP-recipient interviewees who report nonzero out-of-pocket spending on groceries, 14 chose to answer questions SNAP and CASH.

Interviewers recorded verbal responses to each question as faithfully as possible. The most frequently occurring word in response to the SNAP question is “food,” which occurs in 8 of the 14 responses. Incorporating mentions of specific foods or food-related terms like “groceries,” the fraction mentioning food rises to 10 out of 14 responses. The word “food” occurs in 3 of the 14 responses to CASH; more general food related terms occur in 5 of the 14 responses to CASH.

Several responses seem to suggest a difference in how the household would spend \$100 depending on the form in which it arrives. For example, in response to question SNAP one interviewee said “[I would] buy more food.” In response to CASH the same interviewee said “[I would buy] more household necessities.” Another interviewee said in response to SNAP that “[I would buy] more food, but the same type of expenses. If I bought \$10 of sugar, now [I would buy] 20.” In response to CASH, the same interviewee said that “[I would spend it on] toilet paper, soap, and other necessary home stuff, or medicine.” A third interviewee said in response to SNAP that “I would buy more food and other types of food...” and in response to CASH that “I could buy basic things that I can’t buy with [SNAP].”²⁵

Some responses suggest behavior consistent with inframarginality. For example one interviewee’s answer to SNAP included the observation that “I would probably spend \$100 less out of pocket,” although this interviewee also mentions increasing household expenditures on seafood and produce. Another interviewee answered SNAP with “[I] would spend all in food, and also buy soap [and] things for [my] two kids.”

6.2 Quantitative evidence on shopping effort

If SNAP recipients consider SNAP benefits to be earmarked for food, they may view a dollar saved on food as less valuable than a dollar saved on nonfood purchases. To test this hypothesis, we study the effect of SNAP on bargain-seeking behavior.

Figure 8 shows the evolution of the adjusted store-brand share before and after SNAP receipt for our sample of SNAP adopters. Each plot shows coefficients from a regression of the adjusted

²⁵The bracketed term is a translation for the Spanish word *cupones*. This word is literally translated as “coupons” but is often used to refer to SNAP. See, for example, Project Bread (2016).

store-brand share on a vector of indicators for months relative to SNAP adoption. Among SNAP-eligible items, panel A shows a trend towards a greater store-brand share prior to SNAP adoption, perhaps reflecting the deterioration in households' economic well-being that normally triggers entry into a means-tested program. Once households adopt SNAP, there is a marked and highly statistically significant drop in the store-brand share. Because we have adjusted store-brand share for the composition of purchases, this decline is driven not by changes in the categories of goods purchased, but by a change in households' choice of brand within a category.

Panel B of figure 8 shows an analogous plot for SNAP-ineligible items. The adjusted store-brand share of SNAP-ineligible expenditure rises before SNAP adoption and does not decline significantly following adoption. Regression analysis presented in the online appendix shows that we can confidently reject the hypothesis that the change in adjusted store-brand share at SNAP adoption is equal between SNAP-eligible and SNAP-ineligible products. The online appendix shows that this pattern holds even in the later weeks of the month, a fact that we return to in the next subsection.

Figure 9 shows analogous evidence for coupon use. Following SNAP adoption, the average adjusted coupon redemption share declines for both SNAP-eligible and SNAP-ineligible products, but the decline is more economically and statistically significant for SNAP-eligible products than for SNAP-ineligible products. Because we have adjusted the coupon redemption share for the basket of goods purchased, these patterns are not driven by changes in the goods purchased, but rather by households' propensity to redeem coupons for a given basket of goods. Regression analysis presented in the online appendix shows that we can reject the hypothesis that the change in the adjusted coupon redemption share at SNAP adoption is equal between SNAP-eligible and SNAP-ineligible products. The online appendix also reports regression analysis using an alternative measure of coupon redemptions that exploits data on the set of coupons mailed to individual households.

6.3 Quantitative model of psychological forces

We now specify a model that organizes our findings and quantifies the potential role of various psychologically motivated departures from fungibility.

The model considers a single household in a single month which is in turn divided into two or more periods indexed by $w \in \{1, \dots, W\}$. In each period the household chooses food consumption f_w and nonfood consumption n_w and the effort s_w^f and s_w^n devoted to shopping for food and nonfood purchases. Greater effort translates into lower prices. Specifically, the price of food consumption in period w is given by $d\left(\frac{s_w^f}{f_w}\right)$ and the price of nonfood consumption is given by $d\left(\frac{s_w^n}{n_w}\right)$, where $d(x) = x^{-\rho}$ and $\rho \geq 0$ is a parameter.

Letting b_w and y_w denote the amount of SNAP benefits and cash, respectively, available at the end of period w , we suppose that

$$\begin{aligned} b_w &= b_{w-1} - \min \left\{ b_{w-1}, d\left(\frac{s_w^f}{f_w}\right) f_w \right\} \\ y_w &= m + y_{w-1} - d\left(\frac{s_w^n}{n_w}\right) n_w - \max \left\{ d\left(\frac{s_w^f}{f_w}\right) f_w - b_{w-1}, 0 \right\} \end{aligned} \quad (11)$$

where $b_0 \geq 0$ is the monthly SNAP benefit, $y_0 = 0$ is cash holdings, and $m > 0$ is a per-period cash income. We assume that the household cannot borrow between periods and ends the month penniless (respectively, $b_w, y_w \geq 0$ for all w and $b_W = y_W = 0$).

The household's per-period felicity function is given by

$$v\left(f_w, n_w, s_w^f, s_w^n\right) = \beta \ln(f_w) + (1 - \beta) \ln(n_w) - c\left(s_w^f + s_w^n\right) \quad (12)$$

where $c > 0$ is a scalar cost of effort.

Maximization of the undiscounted sum of felicities in (12) subject to the constraints in (11) can be thought of as a neoclassical benchmark.²⁶ We allow for three departures from this benchmark. First, we allow for short-run time preference following Laibson (1997) by supposing that future felicity is discounted at rate $\gamma \geq 0$. Second, we allow that the household has a target level of monthly food spending $\beta W m + b_0$ that corresponds to spending the Cobb-Douglas share β of cash income, plus all of SNAP benefits, on food. Following Farhi and Gabaix (2015), departures between actual and target spending lead to a utility loss of $\kappa \geq 0$ per dollar. Combining these two

²⁶If $y_0 > 0$ and $m = 0$, this benchmark is a special case of the model in section 5, where we set the preference shock $\xi_{it} = 0$ and think of the monthly utility function $U_i(\cdot)$ as the maximum undiscounted sum of felicities attainable for given total monthly food and nonfood expenditure.

forces means that in any period w' the household acts to maximize the objective

$$\sum_{w \geq w'} \exp(-\gamma \mathbf{1}_{w > w'}) v \left(f_w, n_w, s_w^f, s_w^n \right) \\ - \kappa \exp(-\gamma \mathbf{1}_{w > w'}) \left| \beta W m + b_0 - \sum_w d \left(\frac{s_w^f}{f_w} \right) f_w \right|.$$

Note that while our specification of the objective function follows Farhi and Gabaix (2015), our specification of the default spending level is *post hoc*.

Third, we allow, following Liebman and Zeckhauser (2004), that receipt of an in-kind benefit may lead the household to misperceive the price of food. We operationalize this idea by supposing that in any period w' the household believes that the price of food in any period $w \geq w'$ is $\left(1 - \sigma \frac{b_{w'-1}}{y_{w'-1} + m}\right) d \left(\frac{s_w^f}{f_w}\right)$ where $\sigma \in [0, 1]$ is a parameter with $\sigma = 0$ corresponding to correct perceptions.²⁷

We set $W = 2$ so that cash payments arrive biweekly, which is the modal frequency reported in Burgess (2014). We set the discount rate $\gamma = -\ln(0.96)$ to match the time preference needed to match the within-month decline in food consumption under log utility in Shapiro (2005).

We set b_0 for households on SNAP equal to the average SNAP benefit in our sample of SNAP adopters in the six months following adoption. We then compute the average SNAP-eligible spending for SNAP adopters in the six months following adoption \bar{f}_1 and choose m so that $\bar{f}_1 / (2m + b_0) = 0.18$, where 0.18 is the expenditure share of food at home for SNAP recipients reported in Mabli and Malsberger (2013, figure 2). Given m , we choose β to equal the ratio of monthly SNAP-eligible spending prior to adoption \bar{f}_0 to total monthly cash income, i.e. $\beta = \bar{f}_0 / 2m$. We choose \bar{f}_0 so that the difference $(\bar{f}_1 - \bar{f}_0)$ is equal to b_0 times the MPCF out of SNAP estimated in column 3 of table 1.

We set the elasticity of prices paid with respect to shopping effort $\rho = 0.085$, at the midpoint of the range reported in Aguiar and Hurst (2007, p. 1548) for their primary measure of shopping effort. We set the cost c of shopping effort to $(1/80)$. If we interpret shopping effort in units of hours per period, this can be interpreted as saying that one hour is equivalent hedonically to

²⁷If in some period w' the household's desired choices lead to a violation of the budget constraints, we suppose that nonfood consumption $n_{w'}$ adjusts to the highest feasible value given the household's other choices. If this adjustment is insufficient we suppose that $f_{w'}$ adjusts in a similar manner. In practice these contingencies do not arise in the numerical cases we consider.

an increase in both food and nonfood consumption of $(1/80)$ log points, as would be the case if c reflected the value of time for a household earning all consumption through a forty-hour work week.

We set $\sigma = 1$, a value that can be thought of as corresponding loosely to the finding in Ito (2014) that households respond only to average prices and not to marginal prices.

Given the values of the other parameters, we set the value of κ so that the model's predictions for monthly food expenditure while on SNAP match the observed value \bar{f}_1 . Implicitly, this means that the model's predictions will also match the observed MPCF out of SNAP.

We solve the model as follows. If either there are no SNAP benefits or food expenditure is below the psychological default $\beta Wm + b_0$, we can solve for shopping effort in each period in closed form by exploiting necessary conditions for a local optimum. We solve for food and nonfood consumption in the first period numerically, optimizing with respect to the consumer's misperceived budget constraint. We then take as given the levels (b_1, y_1) of assets in the second period and solve numerically for second-period consumption levels.

Table 3 presents the results. Column (1) presents empirical counterparts to model outputs. Column (2) presents the model's implications under the neoclassical benchmark. The remaining columns add, respectively and cumulatively, time preference, price misperception, and mental accounting.

The first row of table 3 shows the MPCF out of SNAP. The estimated value is 0.59. As expected, the neoclassical benchmark in column (2) fails to replicate the high MPCF, implying instead a much smaller value of 0.15. Adding short-run time preference in column (3) does not meaningfully change the prediction. In principle, short-run time preference could lead to a high MPCF as the household tries to exhaust SNAP benefits early in order to consume more in the first period. In practice, SNAP benefits account for a small enough share of total food spending that this force is unimportant quantitatively. Adding price misperception in column (4) leads to a significant increase in the MPCF, to 0.31, because the household now perceives food to be cheaper at the margin when SNAP benefits have not been exhausted. Finally, adding mental accounting in column (5) mechanically delivers the observed MPCF of 0.59, as the household strives to reach its default level of spending. (Recall that the parameter κ is chosen to match the observed MPCF.)

The second two rows of table 3 show the percent change in effective shopping effort for food

and nonfood. We operationalize this concept by focusing in the data on the change in the adjusted store-brand share from figure 8, and in the model on the change in the effective price $d()$. Both the neoclassical benchmark in column (2) and the model with short-run time preference in column (3) fail to predict that shopping effort declines more for food than for nonfood purchases, as we observe in figure 8. Instead, these models predict equal declines in shopping effort for the two groups of products. By contrast, both the price misperception model in column (4) and the mental accounting model in column (5) predict a greater decline in shopping effort for food than nonfood products. The model with mental accounting in column (5) produces the best quantitative match to the data. This finding is not mechanical, as we did not use the data on store-brand shares to fit model parameters.

The final row of table 3 describes the behavior of shopping effort in the second period of the month. Empirically, as we document further in the online appendix, the decline in shopping effort is greater for food relative to nonfood products even in the second half of the month. Neither the neoclassical benchmark in column (2) nor the model with short-run time preference in column (3) can match this fact, because under both models households should equate the return to shopping effort across domains in all periods. In principle, price misperceptions could explain this finding, but because SNAP benefits are largely exhausted by the start of the second period, column (4) shows that this model also counterfactually predicts equal changes in shopping effort for food and nonfood. In contrast, the mental accounting model in (5) correctly predicts the sign and order of magnitude of the observed change.

To summarize, we find that neither the neoclassical benchmark nor a model with short-run time preference can rationalize the observed MPCF out of SNAP. A model with price misperceptions comes closer but still under-predicts the MPCF. A model with mental accounting can fit the observed MPCF and also matches evidence about changes in shopping effort that was not used to fit the model. As an additional piece of evidence on the mental accounting channel, the online appendix shows that the MPCF out of SNAP is slightly larger for households who exhibit a greater correlation between octane choice and the price of regular gasoline, which Hastings and Shapiro (2013) argue can be explained by mental accounting.

7 Conclusions

We use data from a novel retail panel to study the effect of the receipt of SNAP benefits on household spending behavior. Novel administrative data motivate three approaches to causal inference. We find that the MPCF out of SNAP benefits is 0.5 to 0.6 and larger than the MPCF out of cash. We argue that these findings are not consistent with households treating SNAP funds as fungible with non-SNAP funds, and we support this claim with formal tests of fungibility that allow different households to have different consumption functions.

We speculate that households treat SNAP benefits as part of a separate mental account. Responses to hypothetical choice scenarios in qualitative interviews suggest that some households plan to spend SNAP benefits differently from cash. Quantitative evidence shows that, after SNAP receipt, households reduce shopping effort for SNAP-eligible products more so than for SNAP-ineligible products. A *post-hoc* model of mental accounting based on Farhi and Gabaix (2015) matches these facts, whereas other psychologically motivated departures from the neoclassical benchmark do not.

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Table 1: Estimated marginal propensities to consume

	(1)	(2)	(3)	(4)
	SNAP-eligible spending	SNAP-eligible spending	SNAP-eligible spending	SNAP-ineligible spending
MPC out of				
SNAP benefits	0.5891 (0.0074)	0.5495 (0.0360)	0.5884 (0.0073)	0.0230 (0.0043)
cash	-0.0019 (0.0494)	-0.0013 (0.0494)	-0.0020 (0.0494)	0.0421 (0.0688)
p-value for equality of MPCs	0.0000	0.0000	0.0000	0.7764
Instruments:				
Change in price of regular gasoline ×(Household average gallons per month)	Yes	Yes	Yes	Yes
SNAP adoption	Yes	No	Yes	Yes
First month of SNAP clock	No	Yes	Yes	Yes
Number of household-months	2005392	2005392	2005392	2005392
Number of households	24456	24456	24456	24456

Notes: The sample is the set of SNAP adopters. The unit of observation is the household-month. Each column reports coefficient estimates from a 2SLS regression, with standard errors in parentheses clustered by household and calendar month using the method in Thompson (2011). All models are estimated in first differences and include calendar month fixed effects. Endogenous regressors are SNAP benefits and the additive inverse of fuel spending; coefficients on these regressors are reported as marginal propensities to consume. The “price of regular gasoline” is the quantity-weighted average spending per gallon on regular grade gasoline among all households before any discounts or coupons. “Household average gallons per month” is the average monthly number of gallons of gasoline purchased by a given household during the panel. “SNAP adoption” is an indicator for whether the month is an adoption month as defined in section 3.5. “First month of SNAP clock” is an indicator equal to one in the first month of a six-month clock that begins in the most recent adoption month. The indicator is set to zero in the first six months (inclusive of the adoption month) following the most recent adoption, in any month after the first 24 months (inclusive of the adoption month) following the most recent adoption, and in any month for which there is no preceding adoption.

Table 2: Tests of fungibility

	Consumption function:		
	Linear, homogeneous	Linear, heterogeneous	Nonlinear, heterogeneous (Local regression)
Excess sensitivity to SNAP benefits ($\hat{\gamma}$)	0.5809 (0.1631)	0.6166 (0.1809)	0.7296 (0.1826)
p -value for $\gamma=0$	0.0006	0.0010	0.0001
[bootstrap p -value]	[0.0000]	[0.0000]	[0.0000]
Number of household-months	1944056	1944056	1944056
Number of households	23708	23708	23708

Notes: The sample is the set of SNAP adopters that purchase fuel at least once. The unit of observation is the household-month. The table presents estimates of the excess sensitivity γ to SNAP benefits using the three-step estimator described in section 5.3. Let f_{it}^γ be SNAP-eligible spending, b_{it} be SNAP benefits, and y_{it} be the additive inverse of fuel spending. Let z_{it} be a vector consisting of the number of SNAP adoptions experienced by household i as of calendar month t , and the product of the average price of regular gasoline with the household's average monthly number of gallons of gasoline purchased. First, we estimate $Y_{it} = E(y_{it} + b_{it} | z_{it})$ and $B_{it} = E(b_{it} | z_{it})$ via first-differenced regression of $(y_{it} + b_{it})$ and b_{it} on z_{it} , respectively, producing estimates $(\hat{Y}_{it}, \hat{B}_{it})$. Next, we estimate $(E(f_{it}^\gamma | Y_{it}), E(B_{it} | Y_{it}))$ via four different methods, producing estimates $(\widehat{E(f_{it}^\gamma | Y_{it})}, \widehat{E(B_{it} | Y_{it})})$. In the first column, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , pooling across households. In the second column, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , separately by household. In the third column, we estimate these via first-differenced regression of f_{it}^γ and \hat{B}_{it} on a linear spline in \hat{Y}_{it} with knots at the quintiles, separately by household. In the fourth column, we estimate these via locally weighted linear regression of f_{it}^γ and \hat{B}_{it} on \hat{Y}_{it} , separately by household, with Gaussian kernel and the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996). Finally, we estimate γ via first-differenced regression of $(f_{it}^\gamma - E(\widehat{f_{it}^\gamma} | Y_{it}))$ on $(\hat{B}_{it} - E(\widehat{B_{it}} | Y_{it}))$, reporting in parentheses the asymptotic standard errors clustered by household and calendar month using the method in Thompson (2011). We report two p -values for the hypothesis that $\gamma = 0$. The first p -value is based on the asymptotic standard errors; the second p -value (in brackets) is based on a nonparametric bootstrap over households with 30 replicates. Missing values in the fourth column are due to a small number of cases in which the rule-of-thumb bandwidth is ill-defined.

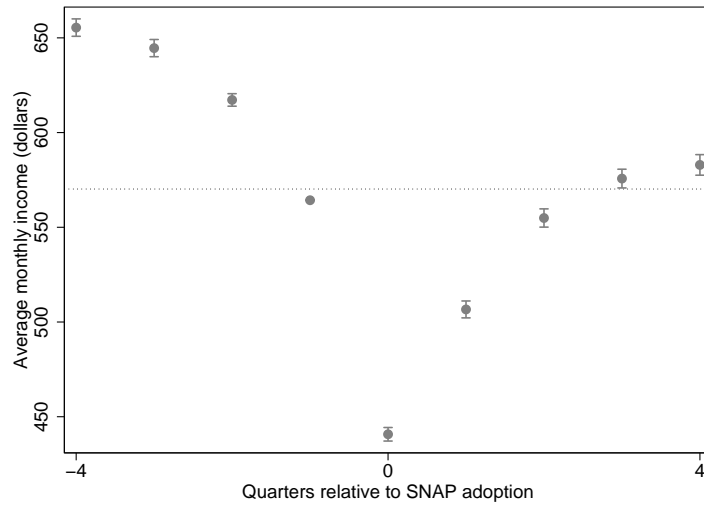
Table 3: Quantitative implications of psychological departures from fungibility

	Observed		Simulated		
	(1)	(2)	(3)	(4)	(5)
MPCF out of SNAP	0.5884 (0.0061)	0.1454 (0.0006)	0.1452 (0.0006)	0.3109 (0.0011)	0.5890 (0.0063)
Percent change in effective shopping effort for					
Food	-0.0263 (0.0013)	-0.0064 (0.0000)	-0.0064 (0.0000)	-0.0137 (0.0001)	-0.0238 (0.0003)
Nonfood	-0.0008 (0.0039)	-0.0064 (0.0000)	-0.0064 (0.0000)	-0.0052 (0.0000)	-0.0032 (0.0000)
Food relative to nonfood, second period	-0.0202 (0.0062)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0106 (0.0003)
Time preference	-	No	Yes	Yes	Yes
Price misperception	-	No	No	Yes	Yes
Mental accounting	-	No	No	No	Yes

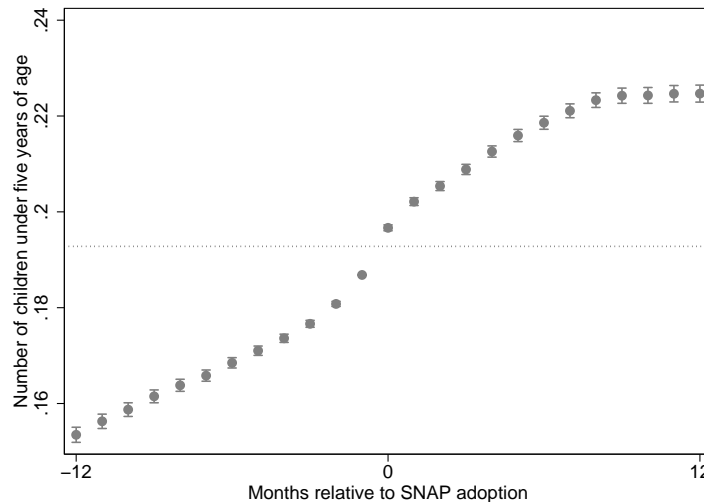
Notes: Column (1) shows empirical estimates for the sample of SNAP adopters. Columns (2) through (5) show analogues computed from the model described in section 6.3. In column (2) we set $\gamma = \sigma = \kappa = 0$. In column (3) we set $\sigma = \kappa = 0$ and $\gamma = -\ln(0.96)$. In column (4) we set $\kappa = 0$, $\sigma = 1$, and $\gamma = -\ln(0.96)$. In column (5) we set κ so that the MPCF out of SNAP equals that from column (1). The observed MPCF out of SNAP is the estimate from column (3) of table 1. The simulated MPCF out of SNAP is the difference in total monthly food expenditure $\sum_w f_w d \left(\frac{d^f}{f_w} \right)$ with and without SNAP, divided by the amount of SNAP benefits b_0 for SNAP recipients. The observed percent change in effective shopping effort for food (nonfood) items is the estimated effect of SNAP on the adjusted store-brand share for SNAP-eligible (SNAP-ineligible) purchases, as shown in the online appendix, divided by the expenditure-weighted average store-brand share of SNAP-eligible (SNAP-ineligible) purchases in the six months prior to adoption. The simulated percent change in effective shopping effort for food is the ratio of the effective shopping effort $\left(\sum_w f_w d \left(\frac{d^f}{f_w} \right) / \sum_w f_w \right)^{-1}$ with SNAP to the effective shopping effort without SNAP, less one. The simulated percent change in effective shopping effort for nonfood is defined analogously. The observed percent change in effective shopping effort for food relative to nonfood in the second period is computed as follows. We compute the expenditure-weighted average store-brand share of purchases in the second two weeks of the month in the six months prior to adoption, $\left(\frac{s_f^{pre-SNAP}}{s_f^{pre-SNAP} + s_h^{pre-SNAP}} \right)$, where f denotes SNAP-eligible items and h denotes SNAP-ineligible items. We define $\left(\frac{s_f^{post-SNAP}}{s_f^{post-SNAP} + s_h^{post-SNAP}} \right)$ as the sum of $\left(\frac{s_f^{pre-SNAP}}{s_f^{pre-SNAP} + s_h^{pre-SNAP}} \right)$ and the corresponding estimated effect of SNAP on the adjusted store-brand share in the second two weeks of the month, as shown in the online appendix. The percent difference shown for each period w is given by $\frac{\left(\frac{s_f^{post-SNAP}}{s_f^{post-SNAP} + s_h^{post-SNAP}} \right) - \left(\frac{s_f^{pre-SNAP}}{s_f^{pre-SNAP} + s_h^{pre-SNAP}} \right)}{\left(\frac{s_f^{pre-SNAP}}{s_f^{pre-SNAP} + s_h^{pre-SNAP}} \right)} - 1$. The simulated percent difference in effective shopping effort for food relative to nonfood is given by computing the ratio of the effective shopping effort $\left(d \left(\frac{s_f^f}{f_2} \right) / d \left(\frac{s_h^f}{n_2} \right) \right)^{-1}$ with SNAP to the effective shopping effort without SNAP, less one. Standard errors in parentheses are obtained via a nonparametric bootstrap over households with 30 replicates.

Figure 1: Household income and size before and after SNAP adoption

Panel A: Household income from in-state earnings and unemployment insurance benefits

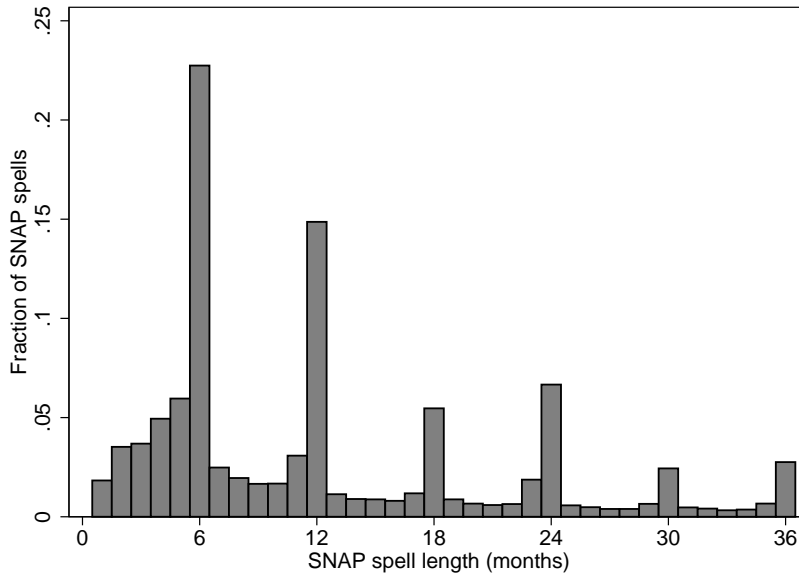


Panel B: Number of children under five years of age



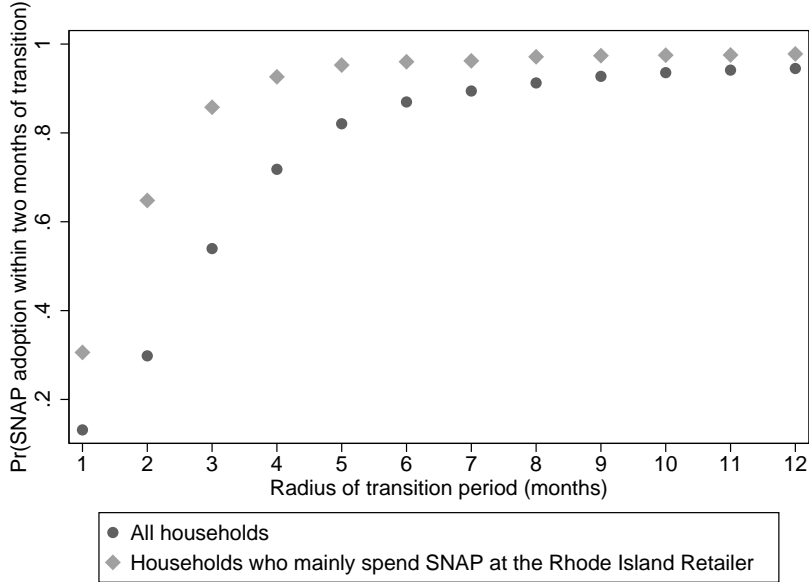
Notes: Data are from Rhode Island administrative records from October 2004 through June 2016. See section 2.1 for details on sample definition and variable construction. Each panel plots coefficients from a regression of the dependent variable on a vector of lead and lagged indicators for periods relative to SNAP adoption, defined as the first period in which the household receives SNAP. The period immediately prior to adoption (“-1”) is the omitted category. Each regression includes time period fixed effects, household fixed effects, and indicators for observations more than one year before or after adoption. In panel A, a time period is a calendar quarter and the unit of analysis is a household-quarter. In panel B, a time period is a month and the unit of analysis is the household-month. In both panels, the error bars are ± 2 coefficient standard errors and standard errors are clustered by household. Dotted lines show the sample mean of the dependent variable across observations within one year (4 quarters or 12 months) of SNAP adoption. Each coefficient series is shifted by a constant so that the observation-count-weighted mean of the regression coefficients is equal to the sample mean of the corresponding dependent variable.

Figure 2: Distribution of lengths of SNAP spells



Notes: Data are from Rhode Island administrative records from October 2004 through June 2016. See section 2.1 for details on sample definition and variable construction. The plot shows a histogram of the distribution of SNAP spell lengths, where a spell is defined as a set of consecutive months in which the household is entitled to a SNAP benefit in each month according to state program records. Spells longer than 36 months are excluded from the sample.

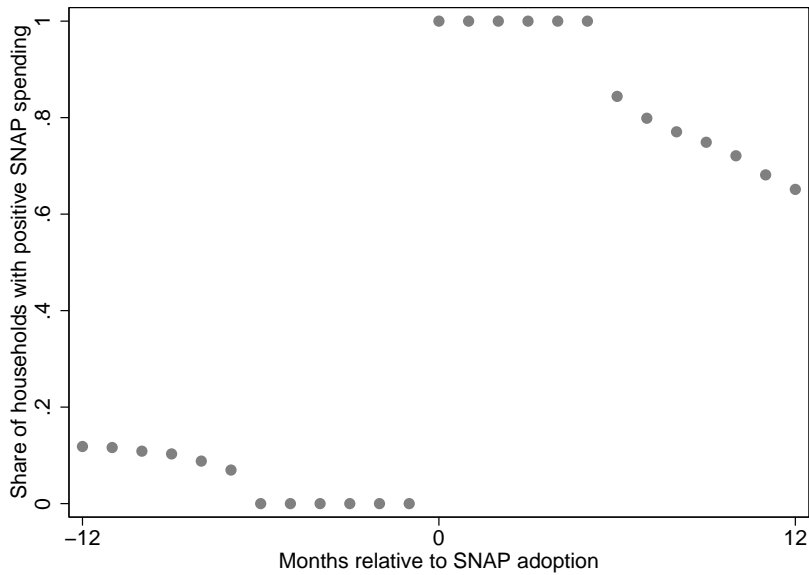
Figure 3: Inferring SNAP adoption from single-retailer data



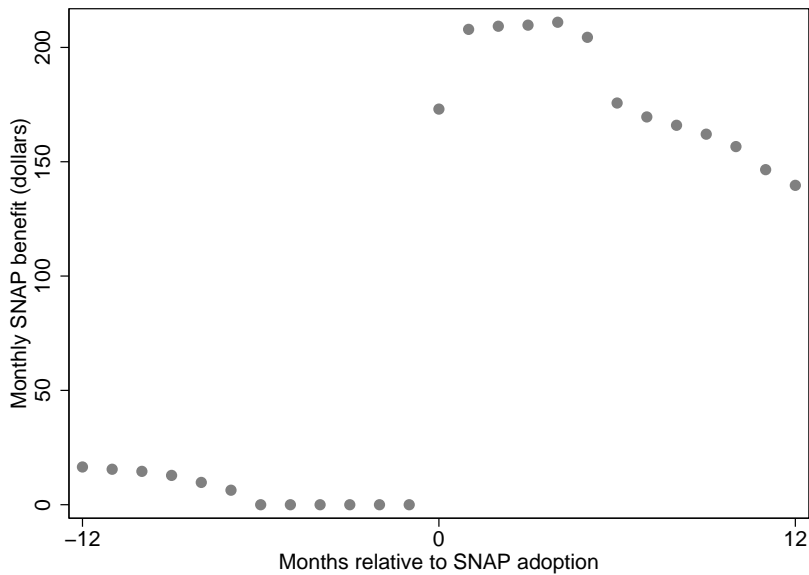
Notes: Data are from Rhode Island EBT transaction records from September 2012 through October 2015. See section 2.1 for details on sample definition and variable construction. The figure plots the fraction of transition periods of a given radius in which the household newly enrolled in SNAP within two months of the start of SNAP spending at the Rhode Island Retailer. We define new enrollment as the receipt of at least \$10 in SNAP benefits following a period of at least three consecutive months with no benefit. A transition period of radius K is a period in which a household exhibits K consecutive months without SNAP spending at the Rhode Island Retailer followed by K consecutive months with SNAP spending at the Rhode Island Retailer. Households who mainly spend SNAP at the Rhode Island Retailer are those who spend at least half of their total EBT expenditures between September 2012 and October 2015 at the Rhode Island Retailer.

Figure 4: SNAP use and benefits before and after SNAP adoption

Panel A: SNAP use

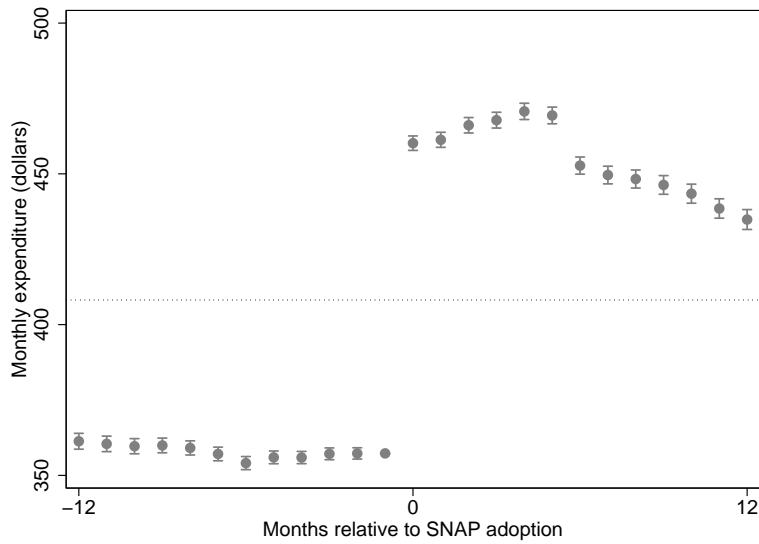


Panel B: SNAP benefits

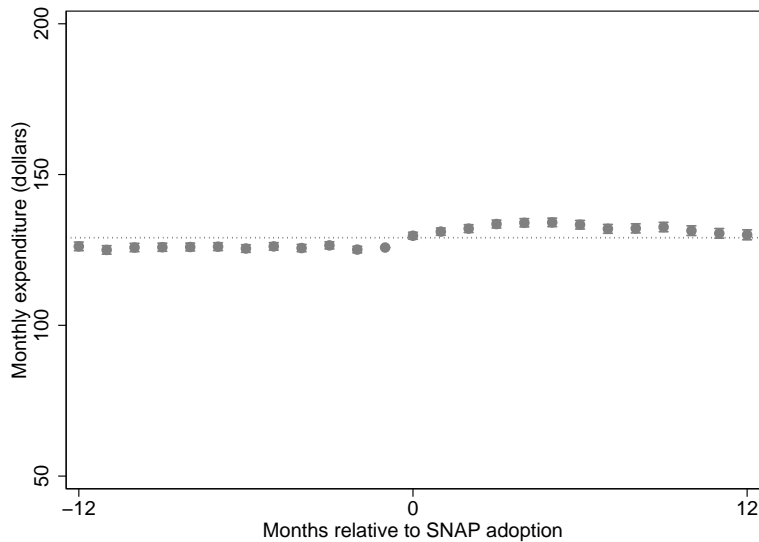


Notes: The sample is the set of SNAP adopters. Panel A plots the share of households with positive SNAP spending in each of the 12 months before and after the household's first SNAP adoption. Panel B plots the average SNAP benefit in each of the 12 months before and after the first SNAP adoption.

Figure 5: Monthly expenditure before and after SNAP adoption, by SNAP eligibility of product
Panel A: SNAP-eligible spending



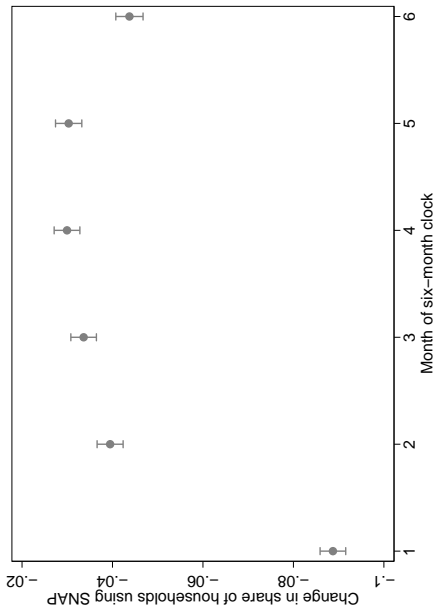
Panel B: SNAP-ineligible spending



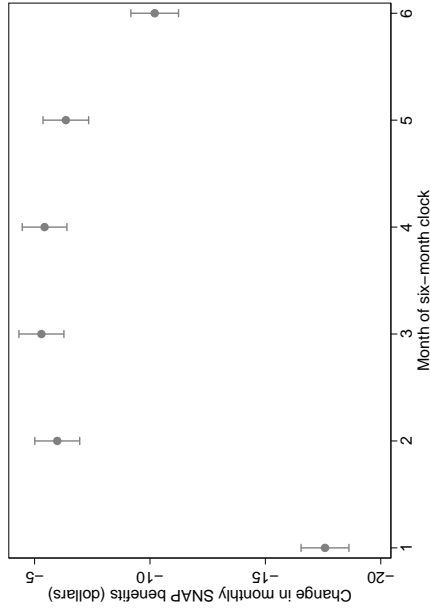
Notes: Each figure plots coefficients from a regression of SNAP-eligible or SNAP-ineligible spending on a vector of lead and lagged indicators for month relative to the household’s first SNAP adoption, with the month prior to SNAP adoption (“-1”) as the omitted category. The unit of observation for each regression is the household-month and the sample is the set of SNAP adopters. Error bars are ± 2 coefficient standard errors. Standard errors are clustered by household. Each regression includes calendar month fixed effects, household fixed effects, and two indicators for observations before and after 12 months of SNAP adoption. The dotted lines show the sample mean of household monthly expenditure across observations within 12 months of SNAP adoption. Each coefficient series is shifted by a constant so that the observation-count-weighted mean of the regression coefficients is equal to the sample mean of the corresponding dependent variable.

Figure 6: Participation, benefits, and spending over the six-month SNAP clock

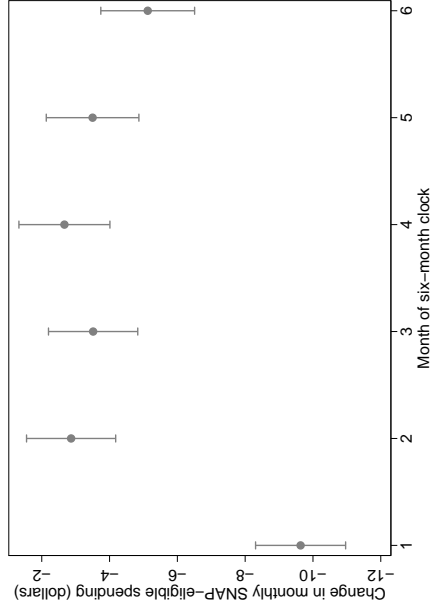
Panel A: SNAP use



Panel B: SNAP benefits

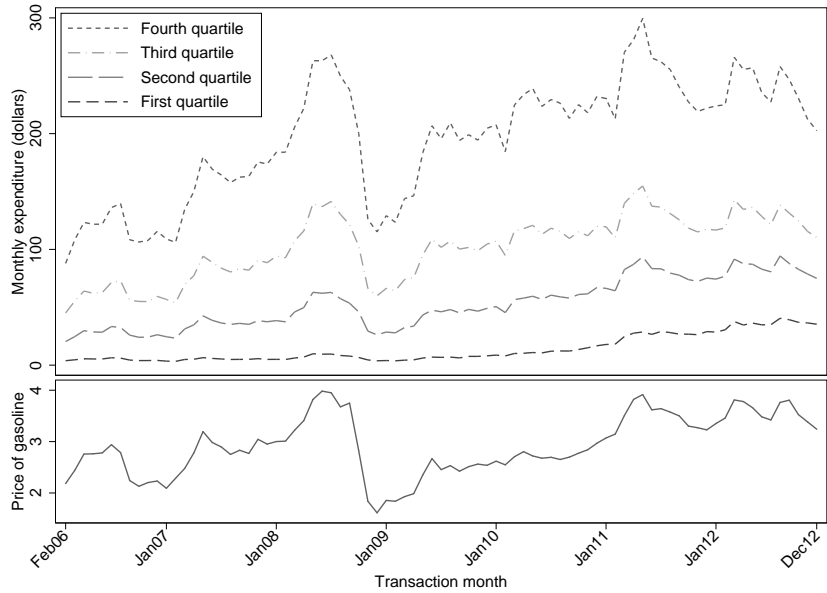


Panel C: SNAP-eligible spending

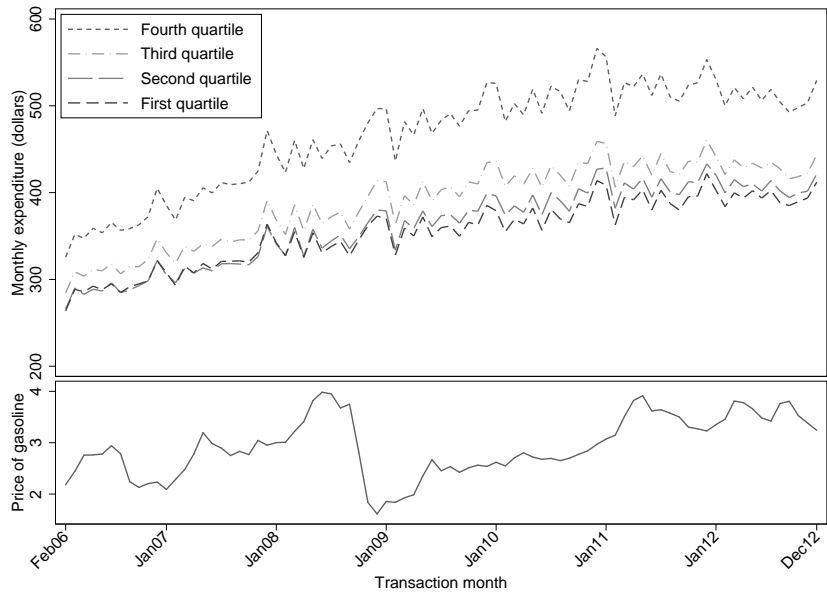


Notes: Each figure plots coefficients from a regression of the dependent variable on a vector of indicators for the position of the current month in a monthly clock that begins in the most recent adoption month and resets every six months or at the next SNAP adoption, whichever comes first. The unit of observation for each regression is the household-month. The sample is the set of SNAP adopters. Error bars are ± 2 coefficient standard errors. Standard errors are clustered by household. Each regression includes calendar month fixed effects. The omitted category consists of the first six months (inclusive of the adoption month) after the household's most recent SNAP adoption, all months after the first 24 months (inclusive of the adoption month) following the household's most recent adoption, and all months for which there is no preceding adoption. In Panel A, the dependent variable is the change in an indicator for whether the household-month is a SNAP month. In Panel B, the dependent variable is the change in monthly SNAP benefits. In Panel C, the dependent variable is the change in monthly SNAP-eligible spending.

Figure 7: Monthly expenditure and the price of gasoline
Panel A: Fuel spending



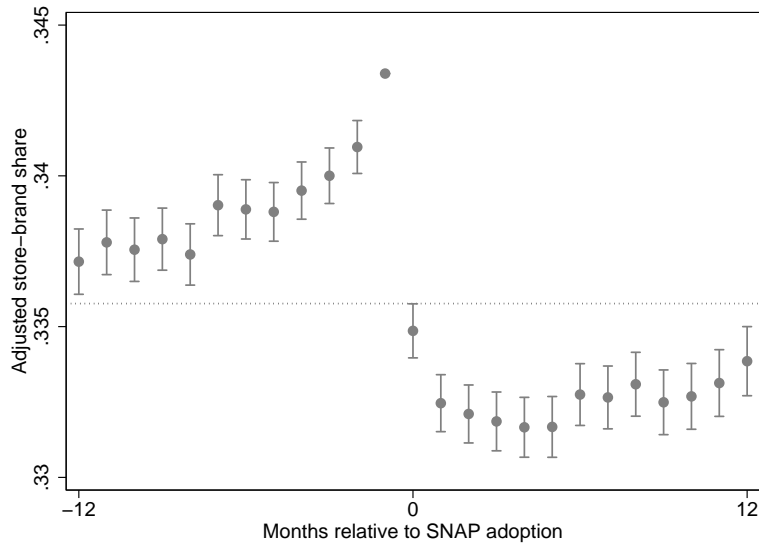
Panel B: SNAP-eligible spending



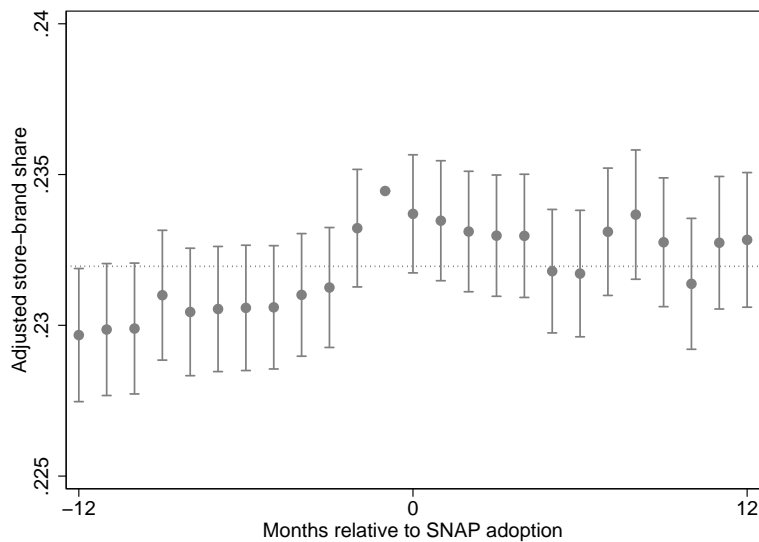
Notes: Panel A plots average monthly fuel spending by quartile of average monthly fuel spending. Panel B plots average monthly SNAP-eligible spending by quartile of average monthly fuel spending. The unit of observation is the household-month and the sample is the set of SNAP adopters who ever purchase fuel. The lower portion of both plots shows the price of gasoline, computed as the quantity-weighted average spending per gallon on regular grade gasoline among all households before any discounts or coupons.

Figure 8: Store-brand share before and after SNAP adoption, by SNAP eligibility of product

Panel A: SNAP-eligible products



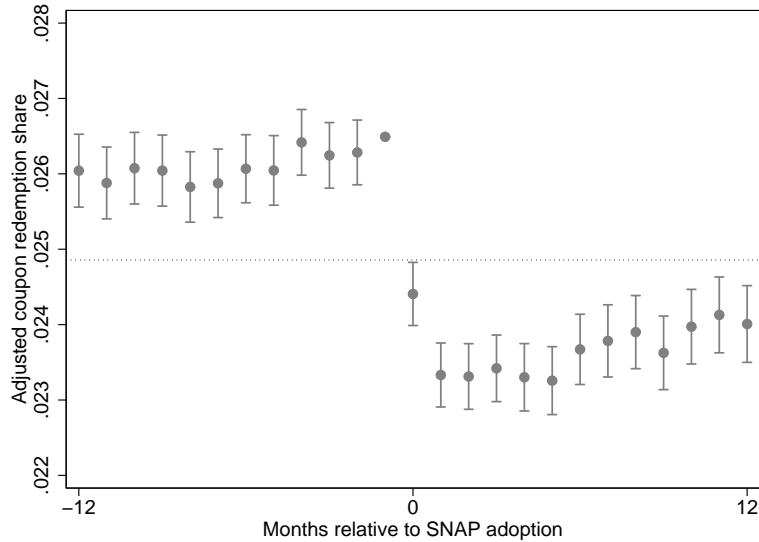
Panel B: SNAP-ineligible products



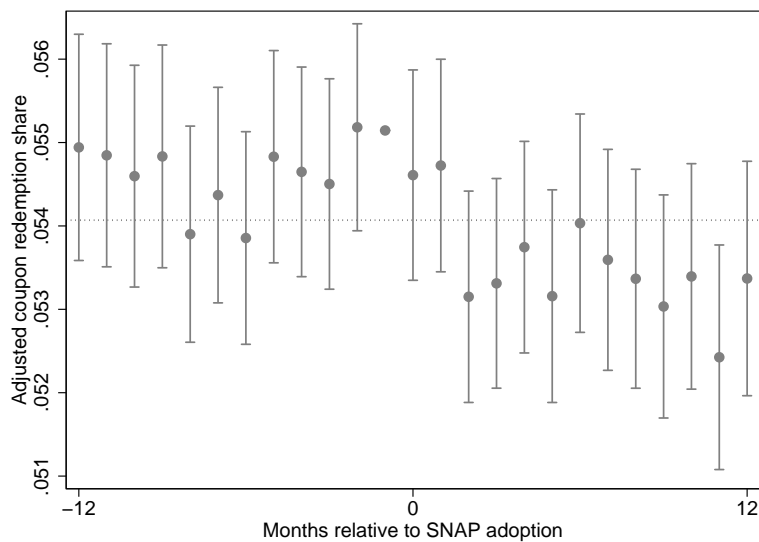
Notes: Each figure plots coefficients from a regression of adjusted store-brand share of expenditures on a vector of lead and lagged indicators for month relative to the household’s first SNAP adoption, with the month prior to SNAP adoption (“-1”) as the omitted category. The unit of observation for each regression is the household-month and the sample is the set of SNAP adopters. Error bars are ± 2 coefficient standard errors. Standard errors are clustered by household. Each regression includes calendar month fixed effects, household fixed effects, and two indicators for observations before and after 12 months of SNAP adoption. The dotted line shows the sample mean of the store-brand share of expenditure across observations within 12 months of SNAP adoption. Each coefficient series is shifted by a constant so that the observation-count-weighted mean of the regression coefficients is equal to the sample mean of the store-brand share of expenditure in the given SNAP eligibility group.

Figure 9: Coupon use before and after SNAP adoption, by SNAP eligibility of product

Panel A: SNAP-eligible products



Panel B: SNAP-ineligible products



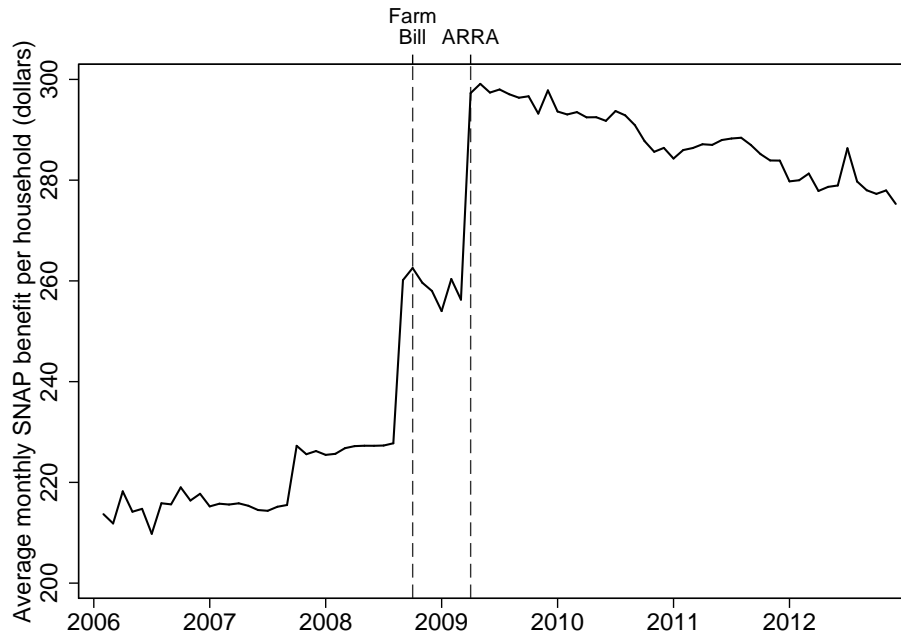
Notes: Each figure plots coefficients from a regression of the adjusted coupon redemption share on a vector of lead and lagged indicators for month relative to the household’s first SNAP adoption, with the month prior to SNAP adoption (“-1”) as the omitted category. The unit of observation for each regression is the household-month and the sample is the set of SNAP adopters. Error bars are ± 2 coefficient standard errors. Standard errors are clustered by household. Each regression includes calendar month fixed effects, household fixed effects, and two indicators for observations before and after 12 months of SNAP adoption. The dotted line shows the sample mean of the share of purchases using a coupon across observations within 12 months of SNAP adoption. Each coefficient series is shifted by a constant so that the observation-count-weighted mean of the regression coefficients is equal to the sample mean of the share of purchases using a coupon in the given SNAP eligibility group.

Appendix Table: Results for alternative samples and specifications

	MPCF out of SNAP benefits	cash	p-values for tests of fungibility	Number of household-months (households)
(1) Baseline	0.588 (0.007)	-0.002 (0.049)	0.0000 0.0000	2005392 (24456)
(2) All households with at least 2 consecutive SNAP months	0.589 (0.007)	0.018 (0.049)	0.0000 0.0000	8586958 (104719)
(3) 9-month SNAP adoption definition	0.571 (0.011)	0.009 (0.055)	0.0000 0.0000	1079284 (13162)
(4) 12-month SNAP adoption definition	0.565 (0.014)	0.008 (0.053)	0.0000 0.0000	668710 (8155)
(5) SNAP exit instead of SNAP adoption	0.594 (0.010)	0.011 (0.047)	0.0000 0.0017	1329056 (16208)
(6) Below-median number of supermarkets in county	0.602 (0.009)	0.011 (0.052)	0.0000 0.0000	1002450 (12225)
(7) Average SNAP-eligible spending exceeds average SNAP benefit	0.575 (0.007)	-0.000 (0.048)	0.0000 0.0000	1664272 (20296)
(8) Average SNAP-eligible spending exceeds average SNAP benefit by at least \$100	0.584 (0.009)	-0.001 (0.046)	0.0000 0.0000	1162842 (14181)
(9) Households with only one adult	0.579 (0.016)	0.010 (0.064)	0.0000 0.0000	350058 (4269)
(10) Households never on WIC	0.545 (0.008)	0.008 (0.043)	0.0000 0.0000	1174732 (14326)
(11) Exclude recession adopters	0.579 (0.006)	-0.000 (0.052)	0.0000 0.0000	1425160 (17380)
(12) Dependent variable in natural logarithm	0.584 (0.008)	0.003 (0.015)	0.0000 0.0000	2003876 (24456)
(13) Dependent variable deseasonalized	0.503 0.007	0.006 0.026	0.0000 0.0000	2005392 (24456)

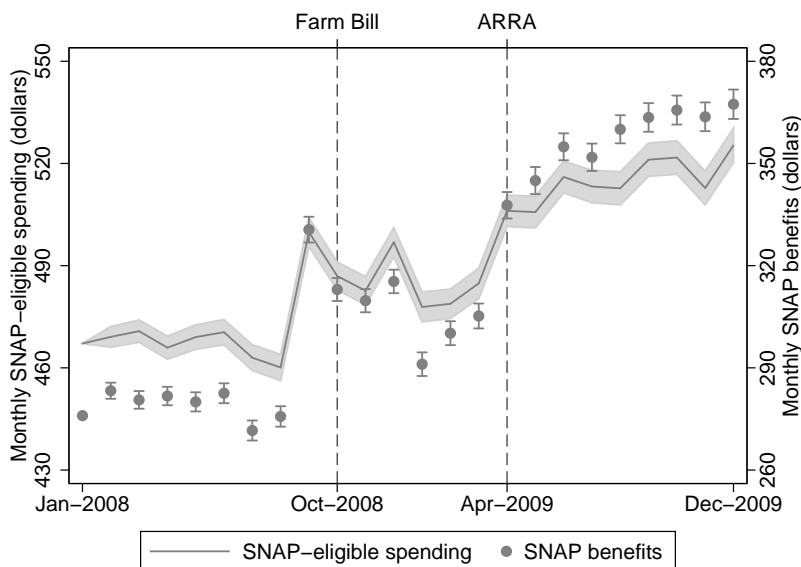
Notes: Specification (1) corresponds to baseline results presented in the body of the paper. The first and second columns of numbers report coefficients and standard errors from the third column of table 1. The third column of numbers reports two p -values. The upper p -value is for the test of the hypothesis that the MPCFs in the first two columns are equal. The lower p -value is for the test of the hypothesis that $\gamma = 0$ from the specification in the fourth column of table 2. The final column of numbers reports the sample size corresponding to the specifications in the first two columns. Specification (2) repeats specification (1) using the sample of all households with at least 2 consecutive SNAP months. Specification (3) repeats specification (1) defining SNAP adoption as a period of nine or more consecutive non-SNAP months followed by a period of nine or more consecutive SNAP months. Specification (4) repeats specification (1) defining SNAP adoption as a period of twelve or more consecutive non-SNAP months followed by a period of twelve or more consecutive SNAP months. Specification (5) repeats specification (1) but removes the “first month of SNAP clock” instrument and replaces the “SNAP adoption” instrument with an indicator for SNAP exit, where SNAP exit is defined as the first month of a period of six consecutive non-SNAP months that follow six consecutive SNAP months. The sample is the set of all households that exhibit such an exit. Specification (6) repeats specification (1) using the sample of SNAP adopters for whom the number of supermarkets in the county of residence is below the median for SNAP adopters. Data on the number of supermarkets come from US Census Bureau (2010). A supermarket is defined as a supermarket or other grocery store; the category excludes convenience stores. Specification (7) repeats specification (1) using the sample of SNAP adopters for whom average SNAP-eligible spending in non-SNAP months exceeds the average SNAP benefit in SNAP months. Specification (8) repeats specification (1) using the sample of SNAP adopters for whom average SNAP-eligible spending in non-SNAP months exceeds the average SNAP benefit in SNAP months by at least \$100. Specification (9) repeats specification (1) using the sample of SNAP adopters for which there is only one adult in the household. Specification (10) repeats specification (1) using the sample of SNAP adopters who never use WIC as the major payment method in any transaction. Specification (11) repeats specification (1) using the sample of SNAP adopters who did not adopt SNAP during the Great Recession (December 2007 - June 2009). Specification (12) repeats specification (1) except that the dependent variable is the natural logarithm of SNAP-eligible spending. The MPCFs are the average marginal effects implied by the estimates. Specification (13) repeats specification (1) except that the dependent variable is deseasonalized separately by each household. The season considered is the month. We adopt the additive classical decomposition using a centered moving average as in Kendall et al. (1983, pp. 485-493).

Appendix Figure 1: Legislated changes in SNAP benefits



Notes: This figure plots the average monthly SNAP benefit per US household between February 2006 and December 2012. The series was obtained directly from the United States Department of Agriculture Food and Nutrition Service via <http://www.fns.usda.gov/sites/default/files/pd/SNAPZip69throughCurrent.zip> as of May 2017. The vertical lines at October 2008 and April 2009 denote the implementation dates of changes in SNAP benefits due to the Farm Bill and American Recovery and Reinvestment Act (ARRA), respectively.

Appendix Figure 2: Monthly SNAP benefits and SNAP-eligible spending around benefit changes



Notes: The sample includes all households in the retailer panel that have at least two consecutive SNAP months during the panel. The figure plots coefficients from a regression of SNAP benefits and SNAP-eligible spending on interactions between the share of calendar months between February 2006 and December 2007 during which each household used SNAP and calendar month indicators, with the January 2008 interaction normalized to zero. The unit of observation is the household-month and only months from January 2008 to December 2009 are included in the regression. Error bars and shaded region represent ± 2 coefficient standard errors. Standard errors are clustered by household. Each regression includes household and calendar month fixed effects. Each coefficient series is seasonally adjusted by subtracting from each coefficient the corresponding coefficient from an auxiliary regression of the dependent variable on interactions between the share of months between February 2006 and December 2007 during which each household used SNAP and year and seasonal month indicators. The auxiliary regressions include household, year, and seasonal month fixed effects and are estimated using only data from January 2010 to December 2012. Each coefficient series is shifted by a constant so that the observation-count-weighted mean of the regression coefficients is equal to the sample mean of the corresponding dependent variable among households who used SNAP in every month between February 2006 and December 2007. Vertical lines at October 2008 and April 2009 denote the implementation dates of changes in SNAP benefits due to the Farm Bill and American Recovery and Reinvestment Act (ARRA), respectively.