How Are SNAP Benefits Spent?

Evidence from a Retail Panel

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Abstract

We use a novel retail panel with detailed transaction records to study the effect of the Supplemental Nutrition Assistance Program (SNAP) on household spending. We use administrative data to motivate three approaches to causal inference. 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 because their benefits are below their food spending. Using a semiparametric framework, we reject the hypothesis that households respect the fungibility of money. A model with mental accounting can match the facts.

Keywords: in-kind transfers, mental accounting, fungibility
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1 Introduction

This paper studies how the Supplemental Nutrition Assistance Program (SNAP) affects household spending. SNAP, the successor to the Food Stamp Program, provides recipient households with a monthly electronic benefit that can only be spent on groceries. 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 the average month of fiscal 2014.[1]

The program's design reflects its stated goal of increasing recipient households' food purchases.[2] Yet for the large majority of recipient households who spend more on food than they receive in benefits,[3] SNAP benefits are economically equivalent to cash.[4] Recent estimates of low-income US households' marginal propensity to consume food (MPCF) out of cash income are at or below 0.1.[5] Thus, if households obey traditional demand theory, the program mainly increases nonfood spending.

This tension between program rhetoric and traditional economic theory matters both for evaluating SNAP and for the basic science of household decision-making. The fungibility of money is a fundamental prediction of traditional demand theory. It is challenged by the hypothesis of mental accounting (Thaler 1999), which posits that households treat different sources of income differently. If households treat SNAP benefits differently from cash, this would likely have important implications for the modeling of household behavior in this and other contexts.

[1] Over the months of fiscal 2014, the number of participating households ranged from 22,580,029 to 23,053,620, with an average of 22,744,054 (FNS 2016a). There were 116,211,092 households in the US on average from 2010-2014 (US Census Bureau 2016).

[2] On signing the bill to implement the 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 families put food on the table" (FNS 2012).

[3] Hoynes 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.

[4] To fix ideas, consider a household with monthly income y and SNAP benefits b. If the household spends f on SNAP-eligible food then it 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 $f^* = \arg\max_f U(f, y+b-f)$. See Mankiw (2000) and Browning and Zupan (2004) for a textbook treatment.

[5] Castner 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.

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, allowing us to infer SNAP participation.

We use three approaches to estimating the causal effect of SNAP on household 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 these three approaches with findings from administrative data. Rhode Island administrative data show that household circumstances change fairly smoothly around SNAP enrollment, motivating our panel event-study design. The administrative data also confirm our expectation that SNAP spell lengths are often divisible by six months because of the recertification process (Klerman and Danielson 2011; Mills et al. 2014; Scherpf and Cerf 2016), 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.

Panel event-study plots show that after adoption of SNAP, households in the retailer panel increase SNAP-eligible spending by about $110 a month, equivalent to more than half of their monthly SNAP benefit, thus implying an MPCF out of SNAP between 0.5 and 0.6. Plots motivated by our instrumental-variables and differences-in-differences designs also imply an MPCF out of SNAP in the range of 0.5 to 0.6. By contrast, we estimate small effects of SNAP on nonfood spending.

We exploit large swings in gasoline prices during our sample period to estimate the MPCF out of cash for the SNAP-recipient households in the retail panel, in a manner similar to Gelman et al. (2017). Data on retail panelists' gasoline purchases show that increases in gasoline prices lead to significant additional out-of-pocket fuel expenses for SNAP-recipient households, but little change in SNAP-eligible spending. We estimate a very low MPCF out of changes in fuel spending, consistent with other estimates of the MPCF out of cash for low-income populations.

Our findings indicate that the MPCF out of SNAP is greater than the MPCF out of other income sources. We show that this pattern holds even for households for whom SNAP benefits should be fungible with cash because their SNAP-eligible spending before SNAP receipt exceeds their SNAP benefits.

In our analysis, we consider several possible challenges to the internal and external validity of the estimated MPCF out of SNAP. These challenges follow from the fact that our retail panel includes only purchases by regular customers at a single retail chain.

The first challenge is that we may mismeasure transitions on to and off of SNAP, which we infer using data on mode of payment at the retailer. We use data on the universe of SNAP transactions in Rhode Island to develop and validate our approach to measuring program transitions.

The second challenge is that SNAP participation may impact households' choice of retailer, which would affect the conclusions from our first two research designs. Nationally representative survey data, and data from the Nielsen Homescan Consumer Panel, show that SNAP participation is only weakly related to a household's choice of retailer. We perform simulations to quantify the sensitivity of our estimates to assumptions about the relationship between SNAP participation and choice of retailer.

The third challenge is that household circumstances change in the period surrounding entry into the program, and we know relatively little about the households in the retail panel beyond their purchases. This challenge is especially important for our first research design. We argue that trends in food spending prior to adoption suggest a small role for confounding changes in circumstances, and use the instrumental variables approach proposed in Freyaldenhoven et al. (2018) to account directly for such confounds.

The fourth challenge is that our retail panel is not a nationally representative random sample, which limits the external validity of our estimates. We compare several features of our panel to nationally and locally representative statistics to gauge differences in the populations.

After laying out our evidence on the MPCF, we develop an economic model of monthly food spending by households for whom SNAP benefits are economically equivalent to 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 parametric structure. Our tests consistently reject the null hypothesis that households treat SNAP benefits as fungible with other income.

We turn next to the possible psychological reasons for departures from fungibility. We discuss responses to qualitative interviews conducted at a food pantry as part of a Rhode Island state pilot proposal to modify SNAP benefit timing. Interviewees often express different intentions when asked how they would spend additional SNAP benefits or additional cash. Using our retail panel, we show that SNAP receipt reduces shopping effort, measured as either the store-brand share of expenditures or the share of purchases on which coupons are redeemed, but only for SNAP-eligible items.

Motivated by these findings, we specify a parametric model of behavior in which households choose expenditures and shopping effort subject to short-run time preference (Laibson 1997) and mental accounting (Thaler 1999). Following Farhi and Gabaix (2015), we operationalize mental accounting as a linear cost of deviating from a default level of food spending that moves one-for-one with SNAP benefits. We fit some model parameters to our data and choose others based on past research. Short-run time preference alone cannot explain the observed MPCF because SNAP benefits are too small relative to desired food spending to constrain household spending even at the beginning of the month. Introducing mental accounting allows the fitted model to match the observed MPCF out of SNAP and the key qualitative patterns in shopping effort. The fitted model implies that a household would be willing to give up 0.84 percent (0.0084 log points) of monthly consumption to avoid the psychological cost of a $100 deviation from default spending.

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 Schanzenbach (2016). There are four strands to this literature. The first strand studies the effect of converting food stamp benefits to cash.[6] 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.[7] The third strand studies randomized evaluations of program extensions or additions.[8]

[6]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. Fox 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.

[7]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. Wilde 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.

[8]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 legislated benefit increases between 2007 and 2010 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.[9] 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.[10] We estimate an MPCF out of SNAP benefits of 0.5 to 0.6 with confidence interval radius as low as 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 Beshears 2009; Hastings and Shapiro 2013; Abeler and Marklein 2017). Important exceptions include Kooreman's (2000) study of a child tax credit in the Netherlands, Jacoby's (2002) study of a school nutrition program in the Philippines, Feldman's (2010) study of a change in US federal income tax withholding, Card and Ransom's (2011) and Kooreman et al.'s (2013) studies of employee savings programs in the US and the Netherlands, respectively, Beatty et al.'s (2014) study of a labeled cash transfer in the UK, and Benhassine et al.'s (2015) study of a labeled cash transfer in Morocco.[11]

[9]Studying the effect of the benefit increase arising from the 2009 American Recovery and Reinvestment Act (ARRA) using survey data, Tuttle (2016) estimates an MPCF out of SNAP of 0.53 with confidence interval radius of 0.38. Nord 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.

[10]Bruich (2014) does not report an MPCF out of cash income. Andreyeva 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.

[11]See also Islam and Hoddinott (2009), Afridi (2010), Shi (2012), and Aker (2017). A closely related literature on "flypaper effects" studies violations of fungibility by governments (Hines and Thaler 1995; Inman 2008).

This paper also shows how to test for the fungibility of money without assuming that the consumption function is linear or that the consumption function is identical for all households. Our approach nests Kooreman's (2000), but, like Kooreman et al.'s (2013), avoids the concern that a rejection of fungibility is due to misspecification of functional forms (Ketcham et al. 2016).

Our use of a parametric model to quantify the predictions of multiple psychological departures from the neoclassical benchmark is similar in spirit to DellaVigna et al. (2017). Hastings and Shapiro (2013), Ganong and Noel (2017), and Thakral and Tô (2017) also compare the predictions of alternative psychological models. The only other paper we are aware of that reports an estimate of a structural parameter of a model of mental accounting based on non-laboratory evidence is Farhi and Gabaix (2017), who calibrate parameters of their model to match our empirical findings. In this sense, our paper contributes to the growing literature on structural behavioral economics (DellaVigna 2017).

2 Motivating 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 Lab (RIIPL) 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.

The data include anonymized state SNAP records from October 2004 through June 2016, which indicate the months of benefit receipt and the collection of individuals associated with each household on SNAP in each month. We define a SNAP spell to be a contiguous period of benefit receipt. We assume that an individual belongs to the household of her most recent spell, does not change households between the end of any given spell and the start of the next spell, and belongs to the household of her first spell as of the start of the sample period. We determine each individual's age in each month, and we exclude from our analysis any household whose membership we cannot uniquely identify in every month,[12] or whose adult (over 18) composition changes during the sample period. The final sample consists of $184,308$ unique households. For each household and month, we compute the total number of children under five years old.

The data also include anonymized administrative records of the state's unemployment insurance system joined via anonymized identifiers to the individuals in the SNAP records over the same period. We compute, for each household and quarter,[13] the sum of total unemployment insurance benefits received from and total earnings reported for all individuals who are in the household as of the quarter's end.[14] We will sometimes refer to this total as "in-state earnings" for short, and we note that it excludes income sources such as social security benefits and out-of-state earnings.

Finally, the data include anonymized administrative records of all debits and credits to the SNAP Electronic Benefit Transfer (EBT) 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.

2.2 Changes in household circumstances around SNAP adoption

Because SNAP is a means-tested program and its eligibility rules incorporate a poverty line standard, household income and household size are major determinants of SNAP eligibility (FNS 2016b). We therefore hypothesize that entry into SNAP is associated with a decrease in in-state earnings and an increase in the number of children. Figure 1 shows panel event-study plots of in-state earnings and number of children as a function of time relative to SNAP adoption, which we define to occur in the first quarter or month, respectively, of a household's first SNAP spell. In the

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

[13]The quarterly level is the most granular at which earnings data are available. We use data only on household-quarters in which the household is observed for all three months of the quarter. Data on earnings are missing from our database for the fourth quarter of 2004 and the second quarter of 2011.

[14]We 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 individual weekly benefit of $707 (Rhode Island Department of Labor and Training 2016).

period of SNAP adoption, in-state earnings decline and the number of children rises, on average.[15]

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).[16] 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 look for sharp changes in spending around SNAP enrollment, and use trends in spending prior to SNAP adoption to diagnose the direction and plausible magnitude of confounds.

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.[17] 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. The online appendix reports that the change in in-state earnings is economically similar between quarters that do and do

[15]An analogous plot in the online appendix shows that in-state earnings rise in the quarters leading up to program exit. The online appendix also includes a plot of trends in income and number of children around SNAP adoption constructed from the Survey of Income and Program Participation.

[16]Past research also finds that unemployment—a likely cause of the decline in income associated with SNAP adoption—is associated with a small decline in at-home food expenditure. 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.

[17]Federal 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).

not contain a spell month divisible by 6.

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

The online appendix 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 large 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.[18] These facts motivate 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 (FNS 2012). Because we will conduct our analysis of household spending using data from a single retail chain, we are at risk of mistaking changes in a household's choice of retailer for program entry or exit. We use Rhode Island EBT records to determine how best to infer program transitions in single-retailer data.

For each $K \in \{1, ..., 12\}$ and for each household in the EBT records, 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

[18]Two smaller jumps, in October of 2006 and 2007, coincide with the annual cost of living adjustments to SNAP payments (FNS 2017b). We do not exploit these smaller changes in our analysis as we expect more precise inference from larger changes in benefits.

with K. For $K = 6$, the fraction constituting new enrollments is 87 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 regular customers in our retail panel—this fraction rises to 96 percent.

Motivated by figure 3, our main analysis of SNAP adoption in the retailer data will use transitions with $K = 6$ and above, and we will present sensitivity analysis using larger minimum values of K.

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.[19] 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.

Appendix A and appendix table 1 present the results of a longitudinal analysis of the relationship between SNAP participation and choice of retailer using data from the Nielsen Homescan Consumer Panel. For the full sample of households, we find that SNAP participation is associated with a statistically insignificant increase of 0.4 percentage points (or 0.7 percent of the mean) in the share of spending devoted to the primary retailer. For households whose primary retailer is a grocery store, we find a statistically significant increase of 1.1 percentage points (2.2 percent). Section

[19] Administrative data show that 84 percent of SNAP benefits are redeemed at supercenters or supermarkets (Castner and Henke 2011).

4.4 shows that our main conclusions are not sensitive to allowing for these estimated changes in retail choice.

3 Retailer data and definitions

3.1 Household purchases and characteristics

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.[20] We refer to these customers as households.

The loyalty card is used to deliver and track promotions (Holmes 2011). Communication with the retailer indicates that at least 90 percent of purchases involve the use of a loyalty card, consistent with the magnitude reported by Andreyeva et al. (2013), who also conduct research using loyalty-card data from a grocery retailer.

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, the retailer provided us with characteristics including the age and gender of adult household members, the median years of schooling of adult household members, an indicator for the presence of children, a categorical measure of household income, and ZIP code. These are based on a combination of sources, including information supplied by the household when obtaining the loyalty card, information purchased from third parties, and information imputed from Census statistics for the local area. We use these data in robustness checks and to explore heterogeneity in our estimates. We match ZIP codes to counties and states using federal data files.[21]

[20]The 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.

[21]We assign ZIP codes to counties and states using the crosswalk for the first quarter of 2010 from US Department of Housing and Urban Development (2017). We assign each ZIP code to the county that contains the largest fraction of the ZIP code's residential population, breaking ties at random.

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 a classification of the main payment method used for the purchase, defined as the payment method accounting for the greatest share of expenditure. The main payment method categories include cash, check, credit, debit, and a government benefit category that consists of SNAP, WIC, cash benefits (e.g., TANF) delivered by EBT card, and a number of other, smaller government programs.

For purchase occasions in March 2009 and later, we further observe the exact breakdown of spending according to a more detailed classification that itemizes specific government programs. These data indicate that, excluding WIC transactions, SNAP accounts for 99.3 percent of expenditures classified as a government benefit.

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. Appendix table 2 shows that our key results are not sensitive to excluding from the sample any household that ever uses WIC.

We define a *SNAP month* as any household-month with positive total spending across SNAP purchase occasions.[22] Of the household-months in our panel, 7.7 percent are SNAP months. Of the households in our panel, 42.9 percent experience at least one SNAP month, and 21.6 percent experience at least two consecutive SNAP months.[23]

SNAP penetration is lower in the retail panel than in the US as a whole. Calculations in the online appendix show that an average of 14.5 percent of US households were on SNAP during the months of the retail sample period. The fraction of households on SNAP is similar when focusing only on the counties of residence of the households in the retail panel. A possible explanation is

[22]Using 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.

[23]Calculations in the online appendix show that in the 2008 Survey of Income and Program Participation, households are on SNAP in 9.0 to 11.9 percent of survey months, and 17.2 to 21.7 percent of households are on SNAP at some point during the panel.

that households in the retail panel have higher-than-average income. Consistent with this, we show in the online appendix that retail panelists live in ZIP codes with higher income than the average in their counties of residence.

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.[24] 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.[25]

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.

3.3 Shopping effort

For each household and month we compute the store-brand share of expenditures and the share of purchases for which coupons are redeemed for both SNAP-eligible and SNAP-ineligible purchases.[26] We adjust these measures for the composition of purchases as follows. For each item

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

[25]Using the Nielsen Homescan Consumer Panel data that we describe in appendix A, we calculate that the share of SNAP-eligible spending among all classified non-fuel spending is at the 15th percentile of the top 20 grocery retail chains by total sales.

[26]We treat these shares as undefined whenever the household has a nonpositive SNAP-eligible or SNAP-ineligible expenditure in a given month. In the small number of cases in which product returns lead to shares above one or below zero, we truncate the relevant share to lie between zero and one.

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 purchases by other households buying the same item in the same retailer division, month, and week in which a coupon is redeemed, 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*.

Nevo and Wong (2015) find that the store-brand share and rate of coupon redemption rose along with other measures of shopping effort during the Great Recession, reflecting households' greater willingness to trade time for money. The store brand is comparable to the national brand in many categories (Bronnenberg et al. 2015), but comparison shopping requires time and effort. Likewise, redeeming coupons requires keeping track of them and bringing them to the store if they have been mailed to the home. As in Nevo and Wong (2015), we use these measures as proxies for the overall level of shopping effort, which we do not observe directly.

In SNAP-eligible product categories, the average store-brand price is \$0.63 below the average non-store-brand price of \$3.34. In SNAP-ineligible product categories, the average store-brand price is \$1.21 below the average non-store-brand price of \$8.07. The average coupon redeemed delivers savings of \$1.01 in SNAP-eligible categories and \$1.53 in SNAP-ineligible categories.[27]

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.[28] The online appendix compares the distribution of SNAP benefits between the retail panel

[27]These calculations are performed at the level of the store division, product category and week, weighting by total expenditures, and excluding the top and bottom 0.1 percent of observations for each respective calculation.

[28]Our concept of total SNAP benefits has a correlation of 0.99 with the exact amount of SNAP spending calculated using detailed payment information in SNAP months March 2009 and later.

and the administrative data for the Rhode Island Retailer.

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 94 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. Appendix table 2 reports estimates of key parameters for the subset of households for whom, according to various 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.[29]

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 reports that the average SNAP benefit among SNAP adopters is 82 percent of the average benefit among demographically similar households in publicly available administrative records. The online appendix also compares the distribution of benefits between the two sources.

We conduct the bulk of our analysis using the sample of SNAP adopters. Appendix table 2 presents our key results for a broader sample and for a more stringent definition of SNAP adoption.

[29]To 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.

3.6 Retailer share of wallet

Spending patterns suggest that panelists buy a large fraction of their groceries at the retailer. Mabli 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.[30] 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. Bee et al. (2015) estimate a gap of 14 percent for expenditures on food-at-home. In the six months following a SNAP adoption, average monthly SNAP-eligible spending in our data is \$469. Likewise, Mabli and Malsberger (2013) estimate average 2010 spending on food at home by eligible nonparticipants of \$292, and we find that average monthly SNAP-eligible spending in our data is \$355 in the six months prior to a SNAP adoption.

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 Mabli and Malsberger's (2013) estimate of \$115 for SNAP recipients in 2010.

Survey data from the retailer do not suggest that SNAP use is associated with an increase 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).[31]

In appendix table 2 we verify that our results are robust to restricting attention to households

[30] Our own calculations from the data used by Ver Ploeg et al. (2015) imply average monthly food-at-home spending of \$379 for SNAP recipients and \$371 for SNAP recipients shopping primarily at supermarkets.

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

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

4.1.1 Trends in spending before and after SNAP adoption

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 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, and that SNAP-eligible expenditures decline significantly on exit from the program. The online appendix also presents a decomposition exercise showing that the increase in spending at adoption is due both to an increase in the frequency of shopping trips and to an increase in the amount of spending per trip.

To address the possibility that the increase in spending is due to short-term stockpiling of non-perishables, the online appendix 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. Consistent with an important role for SNAP, the online appendix also shows that the increase in spending at adoption is concentrated in the early weeks of the month, when SNAP benefits are typically spent.

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.

The trends in SNAP-eligible spending prior to SNAP adoption documented in figure 5 appear quantitatively reasonable given the trends in in-state earnings and number of children documented in figure 1. The estimates underlying panel A of figure 1 imply a decline in in-state earnings of \$95.91 between the fourth and first quarter prior to SNAP adoption. The estimates underlying panel A of figure 5 imply a decline in SNAP-eligible spending of \$3.24 between the first three pre-adoption months and the last three pre-adoption months. The ratio of these values implies an MPCF out of in-state earnings of 0.034, on the low end of the range for the MPCF out of cash reported by Hoynes and Schanzenbach (2009). Moreover, the estimates underlying panel B of figure 1 imply an increase in the number of children under five years old of 0.025 between the first and last three pre-adoption months. If, as a rough guide, we take Lino's (2017) estimate that an additional child aged 0 to 2 costs \$134 in monthly food expenditures for a low-income, single-parent household, the trends are mutually consistent with an MPCF out of in-state earnings of 0.069, well within the range for the MPCF out of cash reported by Hoynes and Schanzenbach (2009).

The preceding analysis of trends is informal and does not account for the evolution of program participation before and after SNAP adoption. The online appendix presents a formal analysis using the approach proposed in Freyaldenhoven et al. (2018) to control explicitly for confounding trends in in-state earnings and number of children. This approach yields an estimated MPCF out of SNAP of 0.49. The confidence intervals on the MPCF out of in-state earnings and the effect of children on food spending are wide and include reasonable values.

4.1.2 Timing of program exit due to certification period lengths

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. The online appendix presents analogues of these plots constructed using administrative data for the Rhode Island Retailer.

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.[32]

4.1.3 Legislated benefit changes

Figure 7 plots the evolution of SNAP benefits and SNAP-eligible spending around the legislated benefit changes described in section 2.4. Panel A shows the evolution of SNAP benefits in administrative data. Panel B shows the evolution of SNAP benefits and SNAP-eligible spending in the retail data, comparing likely SNAP recipients to likely non-recipients. Both plots show increases in benefits corresponding to the implementation dates of the Farm Bill and ARRA, respectively.[33] Panel B further shows that the SNAP-eligible spending of likely SNAP recipients 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 benefit increases 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. The online appendix reports on the fit of the estimated differences-in-differences model to the plot in panel B

[32]The online appendix shows that patterns similar to those in figure 6 obtain for those SNAP adopters who exhibit a period of six consecutive non-SNAP months after initial exit from SNAP, for whom short-run "churn" off of and back on to SNAP (Mills et al. 2014) is less likely to be a factor.

[33]The increase in benefits in September 2008, before the implementation of the Farm Bill, appears to be due to emergency SNAP benefits issued in response to Hurricane Ike. See, for example, Center for Public Policy Priorities (2008).

of figure 7, and on the effect of aggregating the data to the store-month level on the precision of the estimates.

4.2 Marginal propensity to consume food out of cash

Two existing pieces of evidence suggest that SNAP recipients' 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 existing 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 to 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 on the MPCF out of cash for the SNAP recipients in our sample, we study the effect on spending of the large changes in gasoline prices during our sample period. These changes provide an attractive source of variation in disposable income because gasoline prices vary at high frequency and the demand for gasoline is relatively price-inelastic in the short run (Hughes et al. 2008). Disadvantages are that fuel prices may affect the relative price of goods, including food (e.g., Esmaeili and Shokoohi 2011), may affect shopping behavior directly through effects on transportation costs (Ma et al. 2011), and may have a different psychological status than other income shocks (Hastings and Shapiro 2013).

Panel A of figure 8 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 8 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 their 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 SNAP benefits and the change in the additive inverse of fuel spending. The coefficients on these endogenous regressors can be interpreted as MPCs out of SNAP and cash, respectively. 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 household's sample-period 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 8.

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. We also reject the null hypothesis that the MPCF out of SNAP is equal to the budget share of food for SNAP households of 18 percent estimated in Mabli and Malsberger (2013).

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.

Appendix table 2 shows that the main conclusion from table 1, that the MPCF out of SNAP exceeds the MPCF out of cash, is robust to a number of changes in sample and specification, such as excluding households for whom SNAP benefits may not be economically equivalent to cash, excluding households with low pre-adoption SNAP-eligible spending, 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 SNAP 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.

4.4 Sensitivity to assumptions about retailer share of spending

Table 2 presents estimates of the regression model in column (1) of table 1 under alternative assumptions about the share of SNAP-eligible spending that each household devotes to the retailer. In column (1) of table 2 we assume that each household devotes all SNAP-eligible spending to the retailer in all months. Here the estimates are identical to those in column (1) of table 1.

In column (2) we assume that each household devotes 82 percent of spending to the retailer in all months. This value is obtained as the ratio of average SNAP benefits in the retail sample to average SNAP benefits in demographically similar households in administrative data, as presented in the online appendix. The MPCF out of SNAP is unchanged because both SNAP benefits and SNAP-eligible spending are scaled in proportion. (The MPCF out of cash increases in absolute value because we do not similarly scale fuel expenditures.)

In columns (3) and (4) we assume that the retailer's share of SNAP-eligible spending is greater by 1.1 percentage points and 2.0 percentage points, respectively, when a household is on SNAP than when it is not. These values are obtained as the point estimate and upper bound of the 95% confidence interval, respectively, from a panel regression of the primary retailer's share of wallet on SNAP status in the Neilson Homescan Consumer Panel data, as reported in column (2) of appendix table 1. The estimated MPCF out of SNAP falls to 0.56 and 0.54, respectively, and remains easily distinguishable from the MPCF out of cash, both statistically and economically.

In column (5) we ask how strong a relationship between SNAP participation and retailer market share is needed to maintain the hypothesis of fungibility. Specifically, we assume that households devote 82 percent of SNAP-eligible spending to the retailer when on SNAP, and set the corresponding share for households not on SNAP to be the largest value such that we can no longer reject the null hypothesis that the MPCF out of SNAP is equal to the MPCF out of cash. This implies a change of 14.9 percentage points in the retailer's share of SNAP-eligible spending.

In the online appendix, we show that the estimated MPCF out of SNAP based on the legislative benefit changes illustrated in figure 7 is not sensitive to assumptions about the effect of SNAP participation on the retailer's share of spending analogous to those in table 2. The reason is that the research design illustrated in figure 7 exploits variation in benefits rather than participation.

5 Model and tests of fungibility

Table 1 may be thought of informally as testing the hypothesis of fungibility while maintaining that all households share a common, linear consumption function. Here we show formally how to test for fungibility under weaker assumptions on the consumption function.

5.1 Model

In each month $t \in \{1,...,T\}$, 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

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

where ξ_{it} is a preference shock and $U_i()$ 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*

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

where $f_i()$ 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 $\nu_{it} = (y_{it} + b_{it}) - \mathrm{E}(y_{it} + b_{it}|z_{it})$. For each household i, the instruments z_{it} satisfy*

$$(\xi_{it}, \nu_{it}) \perp z_{it}. \tag{3}$$

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

$$\mathrm{E}\left(f_{it}|z_{it}\right) = \varphi_i\left(\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right)\right) \tag{4}$$

for some function $\varphi_i\left(\right)$.

Proof. Let P_i denote the CDF of $\left(\xi_{it},\nu_{it}\right)$. Then

$$\begin{aligned}\mathrm{E}\left(f_{it}|z_{it}\right) &= \int_{\Omega_i} f_i\left(\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right)+\nu_{it},\xi_{it}\right)dP_i\left(\xi_{it},\nu_{it}|z_{it}\right)\\ &= \int_{\Omega_i} f_i\left(\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right)+\nu_{it},\xi_{it}\right)dP_i\left(\xi_{it},\nu_{it}\right)\\ &= \varphi_i\left(\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right)\right),\end{aligned}$$

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

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

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

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

$$f_i\left(y_{it}+b_{it},\xi_{it}\right) = \theta_i\left(y_{it}+b_{it}\right)+\xi_{it}. \tag{6}$$

and, under assumption 2, proposition 1 applies with

$$\varphi_i\left(\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right)\right) = \alpha_i+\theta_i\,\mathrm{E}\left(y_{it}+b_{it}|z_{it}\right) \tag{7}$$

for $\alpha_i \equiv \mathrm{E}\left(\xi_{it}\right)$.

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, θ_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

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

for $f_i()$ 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 $Y_{it} = \mathrm{E}(y_{it} + b_{it}|z_{it})$ and $B_{it} = \mathrm{E}(b_{it}|z_{it})$ and observe that, following proposition 1,

$$f_{it}^{\gamma} - \mathrm{E}\left(f_{it}^{\gamma}|Y_{it}\right) = \gamma(B_{it} - \mathrm{E}(B_{it}|Y_{it})) + e_{it}, \tag{9}$$

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

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 $\mu()$, unknown function $\lambda_{it}()$, and unobserved measurement error η_{it} independent of z_{it} we have that measured food spending $\hat{f}_{it}$ follows

$$\mu\left(\hat{f}_{it}\right) = \mu(f_{it}) + \lambda_{it}(y_{it} + b_{it}, \eta_{it}). \tag{10}$$

Then under perturbations $\mu\left(f_{it}^{\gamma}\right) = \mu(f_{it}) + \gamma b_{it}$ an analogue of (9) holds, replacing f_{it}^{γ} with $\mu\left(f_{it}^{\gamma}\right)$. Examples include additive measurement error, where $\mu()$ is the identity function, and multiplicative measurement error, where $\mu()$ 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 $\exp(\lambda_{it}(y_{it} + b_{it}, \eta_{it}))$. Appendix table 2 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 $\left(\hat{Y}_{it}, \hat{B}_{it}\right)$.

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

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

Table 3 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. Appendix table 2 presents a range of robustness checks for the test in the fourth column of table 3, including one in which we deseasonalize the dependent variable.

The estimated value of γ of 0.58 in column (1) of table 3 is similar to the difference between the estimated MPCF out of SNAP and the estimated MPCF out of cash of 0.59 in column (1) of table 1, where we also impose a linear, homogeneous consumption function.[34] Allowing for heterogeneous, linear consumption functions in column (2) leads to a similar estimate that is slightly larger in magnitude. Allowing for heterogeneous, nonlinear consumption functions in columns (3) and (4) leads to meaningfully larger estimates of γ, possibly indicating that the effect of SNAP on food spending is greater at points on the consumption function where the effect of cash income is smaller.

6 Interpretation

We hypothesize that households treat SNAP benefits as part of a separate mental account that is psychologically earmarked for spending on groceries. 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 roles of mental accounting and short-run time preference in explaining our findings.

6.1 Qualitative interviews with SNAP-recipient households

As part of preparation related to a Rhode Island state proposal to pilot a change to SNAP benefit distribution, RIIPL 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 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 spend-

[34]The specifications in table 3 do not include any controls for calendar month whereas those in table 1 include calendar month indicators. This affects precision. Removing calendar month indicators from the specification in column (1) of table 1 causes the standard error on the estimated difference in MPCFs to increase from 0.05 to 0.15. The latter value is close to the standard error of 0.16 on the estimate of γ in column (1) of table 3.

ing 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]."[35]

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

[35]The 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).

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 shopping effort.

Figure 9 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 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 the 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 9 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. In regression analysis presented in the online appendix, we confidently reject the hypothesis that the change in adjusted store-brand share at SNAP adoption is equal between SNAP-eligible and SNAP-ineligible products.

Figure 10 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. In regression analysis presented in the online appendix, we 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.

The observed changes in shopping effort have a small effect on total spending. Recall that in SNAP-eligible product categories, the average store-brand price is \$0.63 below the average non-store-brand price of \$3.34. A one-percentage-point decrease in the store-brand share therefore increases the price of the average product by \$0.0063. Even aggregating up to the monthly level, the price impact likely amounts to only a few dollars. Similar reasoning implies that the observed decline in coupon redemptions has a small effect on total spending.

6.3 Quantitative model of psychological forces

We now specify a model to quantify the potential role of mental accounting and short-run time preference in explaining our findings. In the model, a household makes choices over the month about how to allocate spending between food and nonfood consumption. To connect the model to the evidence in section 6.2, we also allow that the household makes a choice about how much effort to devote to shopping. Greater effort carries a linear cost (which may be thought of as the value of time), but reduces the price paid per unit consumption. The household's intrinsic utility from consumption takes a log-linear form.

We model mental accounting, following Farhi and Gabaix (2015), as a disutility of spending an amount that differs from an exogenous default. We model short-run time preference, following Laibson (1997), as a present-bias that leads to meaningful time discounting even within the month. In the online appendix we further allow, following Liebman and Zeckhauser (2004), that receipt of an in-kind benefit may lead the household to misperceive the price of food.

Formally, consider a single household in a single month that is divided into two or more periods indexed by $w \in \{1, ..., W\}$. In each period the household chooses food consumption f_w, nonfood consumption n_w, and the effort s_w^f and s_w^n devoted, respectively, to shopping for food and nonfood purchases. Greater effort carries a linear cost of $c > 0$ per unit, and results in lower prices for a given amount of consumption, or, equivalently, greater effective consumption for a given amount of expenditure. 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

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

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

$$v\left(f_w, n_w, s_w^f, s_w^n\right) = \theta \ln(f_w) + (1-\theta)\ln(n_w) - c\left(s_w^f + s_w^n\right). \tag{12}$$

Maximization of the undiscounted sum of felicities in (12) subject to the constraints in (11) can be thought of as a neoclassical benchmark.[36] We depart from this benchmark in allowing, following Farhi and Gabaix (2015), that the household has a target level of food spending and that a gap between actual and target levels of spending causes a loss of utility of $\kappa \geq 0$ per dollar. We assume that the target level of food spending is given by $\theta W m + b_0$, which corresponds to spending the Cobb-Douglas share θ of cash income, plus all of SNAP benefits, on food. This specification is *post hoc* but captures the idea that households may think of SNAP as "food money."

We also allow, following Laibson (1997), that future felicity is discounted by a factor $\beta \in [0,1]$. Combining these two departures means that in any period w' the household acts to maximize the objective

$$\begin{aligned} & \sum_{w \geq w'} \beta^{\mathbf{1}_{w>w'}} v\left(f_w, n_w, s_w^f, s_w^n\right) \\ - \quad & \kappa \beta^{\mathbf{1}_{w>w'}} \left| \theta W m + b_0 - \sum_w d\left(\frac{s_w^f}{f_w}\right) f_w \right| \end{aligned} \tag{13}$$

[36] If $y_0 > 0$ 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 $U_i()$ as the maximum undiscounted sum of felicities attainable for given total monthly food and nonfood expenditure.

where the final term captures the role of mental accounting. The absolute value penalty for deviations from the target spending is chosen for analytical convenience. Note that, in principle, households are psychologically penalized for spending either more or less than their target. In practice, because the target is above the neoclassical benchmark, only the penalty for spending below the target is relevant for our calculations.

We set $W = 2$ so that cash payments arrive biweekly, which is the modal frequency reported in Burgess (2014). We set the discount factor β to match the daily decline in log caloric intake in Shapiro (2005, table 1, column 2), translated into a decline over two-week periods. The estimated value of β is 0.94.

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 $\overline{f}_1$ and choose m so that $\overline{f}_1/(2m+b_0) = 0.18$, 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 θ to equal the ratio of monthly SNAP-eligible spending prior to adoption $\overline{f}_0$ to total monthly cash income, i.e. $\theta = \overline{f}_0/2m$. We choose $\overline{f}_0$ so that the difference $\left(\overline{f}_1 - \overline{f}_0\right)$ 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 $\rho = 0.085$, 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 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.

The level of shopping effort in period w, $\left(d\left(\frac{s_w^f}{f_w}\right)^{-1}, d\left(\frac{s_w^n}{n_w}\right)^{-1}\right)$, corresponds to the effective consumption delivered by a given level of expenditure on food or nonfood categories. As we do not observe this directly, we follow Nevo and Wong (2015) in treating our observable measures of shopping effort as proxy variables. We assume, in particular, that the store-brand share in each period is proportional to the level of shopping effort. If this assumption fails to hold, we may think of the model's predictions for shopping effort as qualitative. The online appendix presents sensitivity analysis with respect to our choice of proxy for shopping effort and our choice of value for the parameter ρ.

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 $\overline{f}_1$. Implicitly, this means that the model's predictions will also match the observed MPCF out of SNAP. The estimated value of κ is 0.000174. At this value, the household would, at the start of the month, be willing to reduce food and nonfood consumption by 0.84 percent (0.0084 log points) in all periods in order to avoid the disutility associated with being \$100 further from her target food spending at the end of the month.

We solve the model as follows. If either there are no SNAP benefits or food expenditure is below the psychological default $\theta W m + b_0$, we can solve for shopping effort in each period in closed form by exploiting necessary conditions for a local optimum. We can then solve for food and nonfood consumption in the first and second periods numerically.

Table 4 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, short-run time preference and mental accounting.

The first row of table 4 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. In particular, because the Cobb-Douglas share θ of first-period resources is close to the value b_0 of SNAP benefits, even a household who saves nothing between periods will exhibit an MPCF out of SNAP that is close to the neoclassical benchmark. As a result, there is no value of β for which the short-run time preference model can account for the observed MPCF.

Finally, adding mental accounting in column (4) 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 and third rows of table 4 show, respectively, the percent change in effective shopping effort for food and nonfood. 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 9. Instead, these models predict equal declines in shopping effort for the two groups of products. By contrast, the mental accounting model in column (4) correctly predicts a greater decline in shopping effort for food than nonfood products. This finding is not mechanical, as we did not use the data on store-brand shares to fit model parameters.

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 mental accounting can fit the observed MPCF and also matches patterns in shopping effort that were 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. The online appendix also shows that the MPCF out of SNAP is slightly larger for households who concentrate a greater share of their spending in the early portion of the month.

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.

A large MPCF out of SNAP has potentially important aggregate implications. The 2009 American Recovery and Reinvestment Act (ARRA) increased SNAP benefits because these "are spent quickly and have a multiplicative effect on total economic activity" (Economic Research Service 2016). A large MPCF out of SNAP further implies that SNAP benefit increases have an especially large effect on spending in the food retail sector (Beatty and Tuttle 2015). Indeed, consistent with the evidence in Bruich (2014), the expiration of the ARRA benefit increases in 2013 was often cited in the business press as a factor in declining grocery sales during that period.[37]

The possibility that households treat SNAP benefits as part of a separate mental account from cash is relevant for recent policy proposals to exclude unhealthy items (such as sugar-sweetened beverages) from SNAP eligibility. Because most SNAP recipients spend some of their own money on food each month, traditional demand theory predicts that such exclusions will affect the mode of payment used to purchased different foods and beverages, but not the actual amounts of these foods and beverages that households consume (Schanzenbach 2013). By contrast, if excluding an item from SNAP eligibility causes the household to treat spending on the item as coming from a cash account rather than a SNAP account, such an exclusion could affect the household's choices (Tuttle 2016). How item exclusions and other policy changes affect the mental accounting of benefits and spending therefore seems an important topic for future empirical and theoretical research.

[37]Major (2014) cites "a drop in SNAP benefits for its low-income customers" as a factor in Walmart's 2013 sales drop, and Walmart's own annual report cites "the reduction in government food benefits" (Walmart 2014). Similar discussion can be found in the press surrounding smaller, regional chains (Driggs 2014) and discount retailers (Progressive Grocer 2015). Food retailers and manufacturers lobby regarding SNAP (Merlin 2012), including for benefit expansions (Tuttle 2012). See also the discussion in Bruich (2014, footnote 2).

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A SNAP and Retailer Choice: Additional Empirical Evidence from the Nielsen Homescan Consumer Panel

This appendix investigates the longitudinal relationship between retailer choice and SNAP participation using data from the Nielsen Homescan Consumer Panel (NHCP).[38] Our discussion of the NHCP draws heavily on Bronnenberg et al. (2015).

We obtained data from the NHCP from the Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business[39]. The data comprises more than 119 million shopping trips made by 158,830 households from January 2004 to June 2015. Panelist households are given an optical scanner and are asked to scan the barcode of every consumer packaged good they purchase, regardless of the store where it was purchased.[40] Einav et al. (2010) study the accuracy of the recorded data.

Nielsen recruits its panelists by direct mail and through internet advertising, and they provide incentives to recruit and keep consumers on the panel. Muth et al. (2007) and Kilts Center for

[38]Copyright 2017, the Nielsen Company. The conclusions drawn from Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for and was not involved in analyzing and preparing the results reported herein.

[39]Information on the data is available at `http://research.chicagobooth.edu/nielsen/`.

[40]Beginning in 2007, a subset of panelists are asked to itemize purchases of products (e.g., produce) that do not have a barcode. We exclude these products from all of our calculations.

Marketing (2016) describe the recruitment process in more detail. Lusk and Brooks (2011) study the representativeness of the panel.

For each product purchased on each shopping trip, we observe the date, the universal product code (UPC) if it has one, the transaction price, the quantity of items purchased, an identifier for the store chain in which the purchase was made, and the total expenditure on the item. We classify products as SNAP-eligible or SNAP-ineligible based on a product taxonomy and the guidelines for eligibility published on the USDA website (FNS 2017a).

For each household, we identify a primary retailer, defined as the retail chain at which the household has the largest total expenditure on SNAP-eligible items across the entire period for which the household is observed in the data. We then compute, for each household and quarter, the share of SNAP-eligible expenditure devoted to the household's primary retailer in the average month.

We obtained from the Nielsen Company a quarterly supplement from the Homescan Panel Omnibus Survey. The supplement is available for the fourth quarter of 2010 and for every other quarter from the fourth quarter of 2011 to the second quarter of 2015. It contains panelists' answers to the following question:

> Are you or anyone in your household currently using or have you ever used food stamps, which includes food stamp card or voucher or cash grant from the state for food (also known as Supplemental Nutritional Assistance Program (SNAP), Electronic Debit Card (EBT card))?
>
> Please read all response options then select the one that best describes you.
>
> 1) Currently using food stamps
> 2) Have used food stamps, but not currently using them.
> 3) Have never used food stamps.

We compute an indicator for current SNAP participation equal to 1 if the household's answer is "currently using food stamps" and 0 otherwise.

Appendix table 1 presents estimates from a series of panel regressions. In each regression, the dependent variable is the share of SNAP-eligible spending devoted to the primary retailer. The independent variable of interest is the indicator for SNAP participation. All models include both household and calendar quarter fixed effects. In column (1), we use our full sample. In columns (2), (3), and (4) we restrict attention to households whose primary retailer is, respectively, a grocery store chain, a discount store or warehouse club chain, or something else.

Column (1) reports that, for the full sample, we estimate an economically small and statistically insignificant relationship between SNAP participation and the share of SNAP-eligible spending devoted to the primary retailer. Column (2) reports that, for households whose primary retailer is a grocery store chain, the share of SNAP-eligible expenditures going to the household's primary retailer is greater by 1.1 percentage points in quarters in which the household is on SNAP. Columns (3) and (4) reports that, for households whose primary retailer is a discount store or warehouse club chain, and for households whose retailer is neither a grocery store chain, nor a discount store or warehouse club chain, the share of SNAP-eligible expenditures going to the household's primary retailer is lower by 1.1 percentage points quarters in which the household is on SNAP. The coefficients in columns (2) through (4) are statistically significant.

Table 1: Estimated marginal propensities to consume

	(1) SNAP-eligible spending	(2) SNAP-eligible spending	(3) SNAP-eligible spending	(4) SNAP-ineligible spending
MPC out of				
SNAP benefits	0.5891	0.5495	0.5884	0.0230
	(0.0074)	(0.0360)	(0.0073)	(0.0043)
cash	-0.0019	-0.0013	-0.0020	0.0421
	(0.0494)	(0.0494)	(0.0494)	(0.0688)
p-value for equality of MPCs	0.0000	0.0000	0.0000	0.7764
Instruments:				
Change in price of regular gasoline	Yes	Yes	Yes	Yes
$\times$(Household average gallons per month)				
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: Sensitivity of estimated MPCF to assumptions about retailer share of spending

	(1)	(2)	(3)	(4)	(5)
	SNAP-eligible spending across all retailers				
MPCF out of					
SNAP benefits	0.5891	0.5891	0.5609	0.5380	0.1316
	(0.0074)	(0.0074)	(0.0075)	(0.0075)	(0.0089)
cash	-0.0019	-0.0023	-0.0018	-0.0014	0.0055
	(0.0494)	(0.0602)	(0.0606)	(0.0609)	(0.0661)
p-value for equality of MPCs	0.0000	0.0000	0.0000	0.0000	0.0532
Assumed retailer share of SNAP-eligible spending when household is:					
Not on SNAP	1.000	0.820	0.809	0.800	0.671
On SNAP	1.000	0.820	0.820	0.820	0.820
Basis for assumed effect of SNAP on retailer share of spending	No effect	No effect	Homescan point estimate	Homescan upper bound	Impose fungibility
Number of household-months	2005392	2005392	2005392	2005392	2005392
Number of households	24456	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. Excluded instruments are the product of the price of regular gasoline and household average gallons per month, and an indicator for SNAP adoption, as in column (1) of table 1. In each column, the dependent variable is total SNAP-eligible spending across all retailers, computed by dividing SNAP-eligible spending at the retailer by the "on SNAP" share in SNAP months and the "not on SNAP" share in other months. In column (1) we assume that all households devote all SNAP-eligible spending to the retailer in all months. In column (2) we assume that all households devote a constant share of SNAP-eligible spending to the retailer, with the share given by the ratio of average SNAP benefits between retailer and administrative data in online appendix table 3. In columns (3) through (5) we assume the same share of spending in SNAP months as in column (2). In columns (3) and (4) we assume that the difference in the share of spending between SNAP months and non-SNAP months is equal to the point estimate and upper bound of the 95 percent confidence interval, respectively, of the effect of SNAP participation on the share of spending devoted to the primary retailer in column (2) of appendix table 1. In column (5) we assume that the share of spending in non-SNAP months is the largest value such that we cannot reject the null hypothesis of an equal MPCF between SNAP and cash.

Table 3: Tests of fungibility

	Consumption function:			
	Linear, homogeneous	Linear, heterogeneous	Nonlinear, heterogeneous (Linear spline with knots at the quintiles)	Nonlinear, heterogeneous (Local regression)
Excess sensitivity to SNAP benefits ($\hat{\gamma}$)	0.5809	0.6166	0.7296	0.8819
	(0.1631)	(0.1809)	(0.1826)	(0.0824)
p-value for γ=0	0.0006	0.0010	0.0001	0.0000
[bootstrap $p-$value]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Number of household-months	1944056	1944056	1944056	1936594
Number of households	23708	23708	23708	23617

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 procedure 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 $Y_{it} = \mathrm{E}(y_{it} + b_{it}|z_{it})$ and $B_{it} = \mathrm{E}(b_{it}|z_{it})$ via pooled 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 $(\mathrm{E}(f_{it}^{\gamma}|Y_{it}), \mathrm{E}(B_{it}|Y_{it}))$ via four different methods, producing estimates $\left(\mathrm{E}\widehat{(f_{it}^{\gamma}|Y_{it})}, \mathrm{E}\widehat{(B_{it}|Y_{it})}\right)$. 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 $\left(f_{it}^{\gamma} - \mathrm{E}\widehat{(f_{it}^{\gamma}|Y_{it})}\right)$ on $\left(\hat{B}_{it} - \mathrm{E}\widehat{(B_{it}|Y_{it})}\right)$, 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. In each replicate, we sample households with replacement, to match the original sample size, and compute all three steps of the estimation procedure. Missing values in the fourth column are due to a small number of cases in which the rule-of-thumb bandwidth is ill-defined.

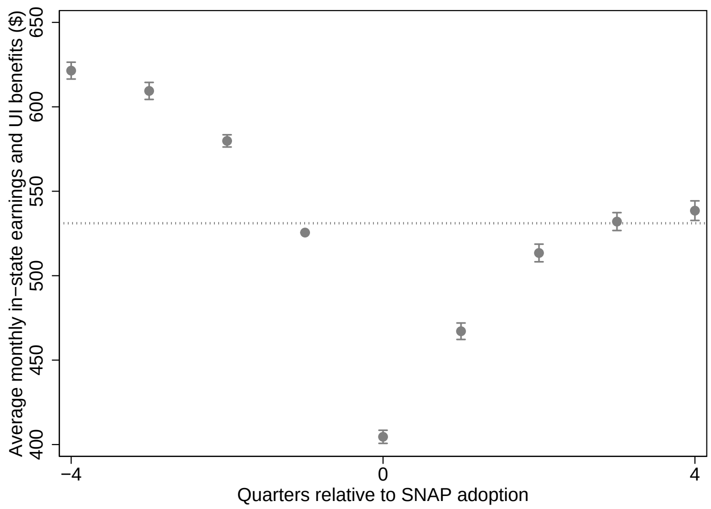
Table 4: Quantitative implications of psychological departures from fungibility

	(1)	(2)	(3)	(4)
	Observed		Simulated	
MPCF out of SNAP	0.5884	0.1454	0.1454	0.5912
	(0.0061)	(0.0006)	(0.0005)	(0.0065)
Relative change in effective shopping effort for				
Food	-0.0263	-0.0064	-0.0065	-0.0232
	(0.0013)	(0.0000)	(0.0000)	(0.0003)
Nonfood	-0.0008	-0.0064	-0.0064	-0.0032
	(0.0039)	(0.0000)	(0.0000)	(0.0001)
Short-run time preference (Laibson 1997)	-	No	Yes	Yes
Mental accounting (Farhi and Gabaix 2015)	-	No	No	Yes

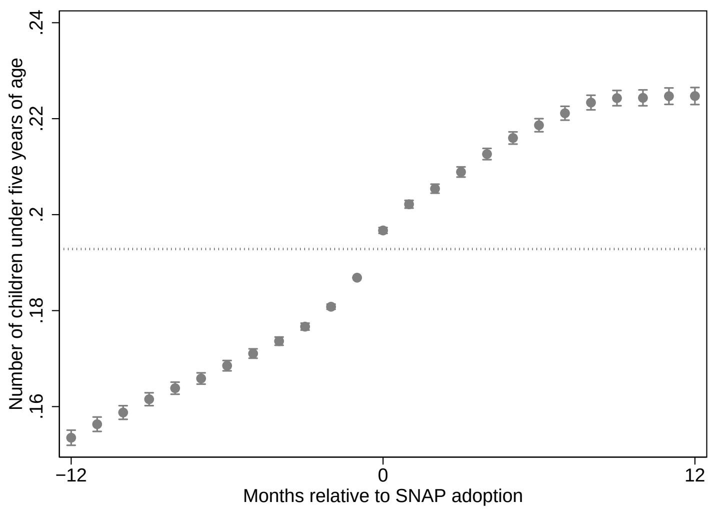
Notes: Column (1) shows empirical estimates for the sample of SNAP adopters. Columns (2) through (4) show analogues computed from the model described in section 6.3. In column (2) we set $\kappa = 0$ and $\beta = 1$. In column (3) we set $\kappa = 0$ and we set β so that the decline in food consumption between the first and second half of the month equals that implied by the estimated daily decline in caloric intake in table 1, column (2) of Shapiro (2005), assuming a constant daily rate of decline and that each period w consists of 15 days. In column (4) we set β as in column (3) and we set κ so that the MPCF out of SNAP equals the observed MPCF 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 $\sum_w f_w d\left(\frac{s_w^f}{f_w}\right)$ with and without SNAP, divided by the amount of SNAP benefits b_0 for SNAP recipients. The observed relative 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 relative change in effective shopping effort for food is the ratio of the effective shopping effort $\left(\sum_w f_w d\left(\frac{s_w^f}{f_w}\right)/\sum_w f_w\right)^{-1}$ with SNAP to the effective shopping effort without SNAP, less one. The simulated relative change in effective shopping effort for nonfood is defined analogously. Standard errors in parentheses are obtained via a nonparametric bootstrap over households with 30 replicates. In each bootstrap replicate, we draw households with replacement to match the size of the main sample, and recompute all empirical objects. We also draw a value of the daily decline in caloric intake from a Gaussian distribution with mean given by the point estimate in table 1, column (2) of Shapiro (2005) and standard deviation given by the standard error in table 1, column (2) of Shapiro (2005).

Figure 1: In-state earnings and number of children before and after SNAP adoption

Panel A: In-state earnings and unemployment insurance benefits

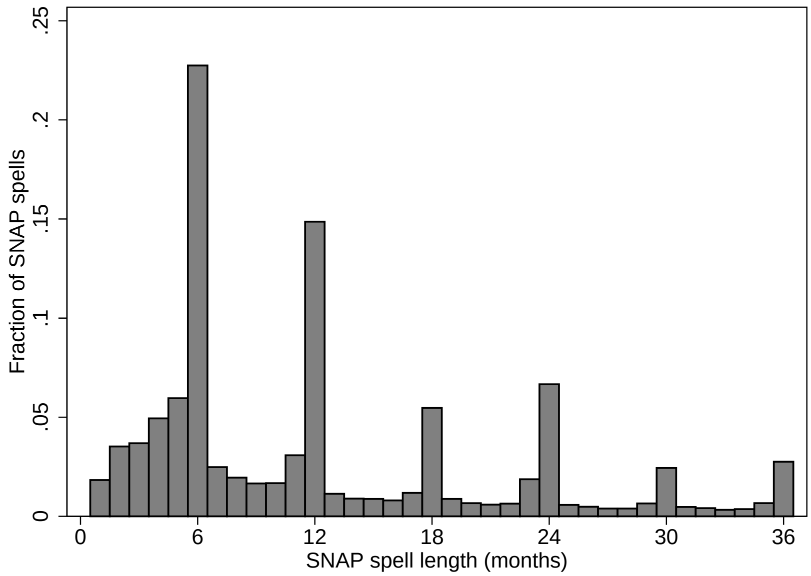


Panel B: Number of children under five years of age



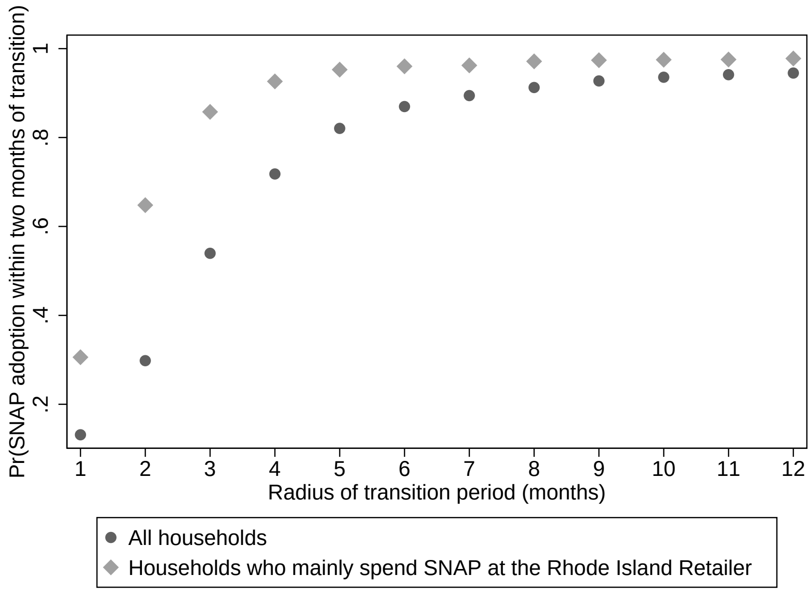
Notes: Data are from Rhode Island administrative records from October 2004 through June 2016. See section 2.1 for details on sample definition and variable construction. Each panel plots coefficients from a regression of the dependent variable on a vector of lead and lagged indicators for periods relative 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.

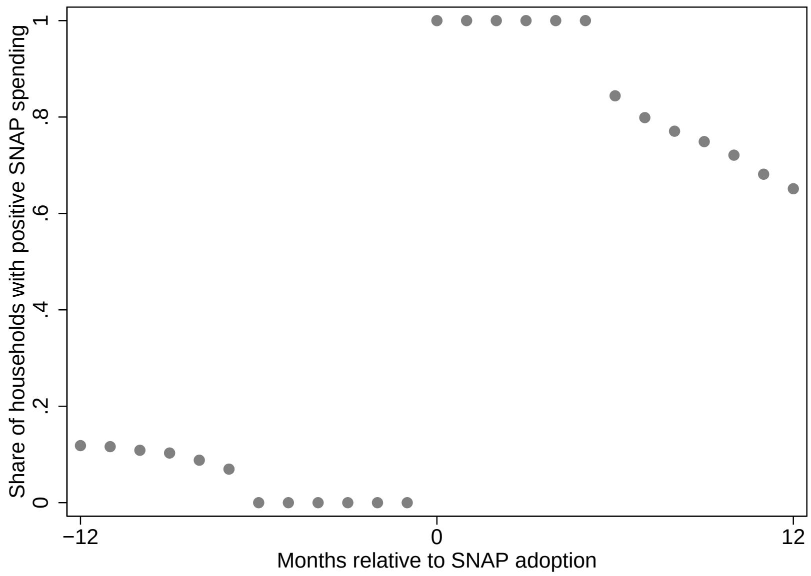
Figure 3: Inferring SNAP adoption from single-retailer data



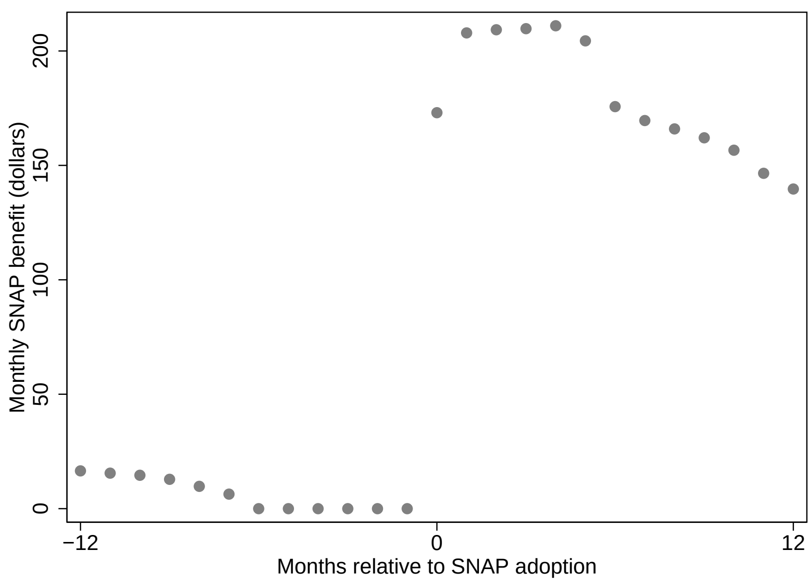
Notes: Data are from Rhode Island EBT transaction records from September 2012 through October 2015. See section 2.1 for details on sample definition and variable construction. The figure plots the fraction of transition periods of a given radius in which the household newly enrolled in SNAP within two 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



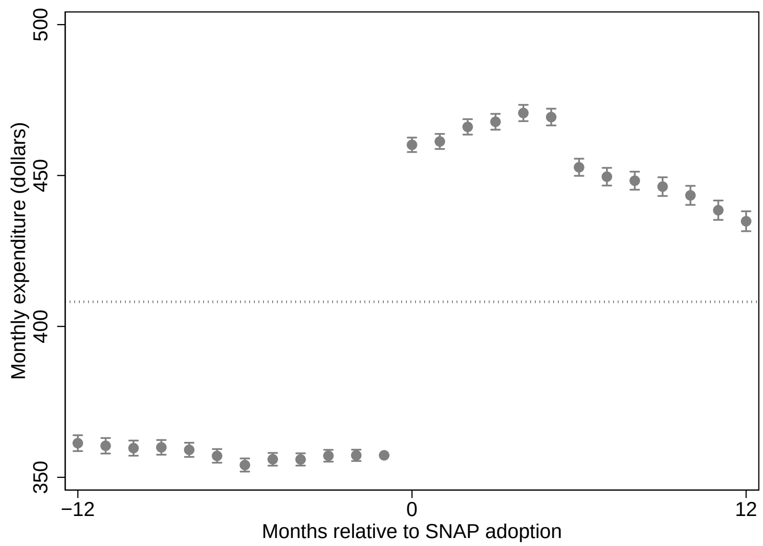
Panel B: SNAP benefits



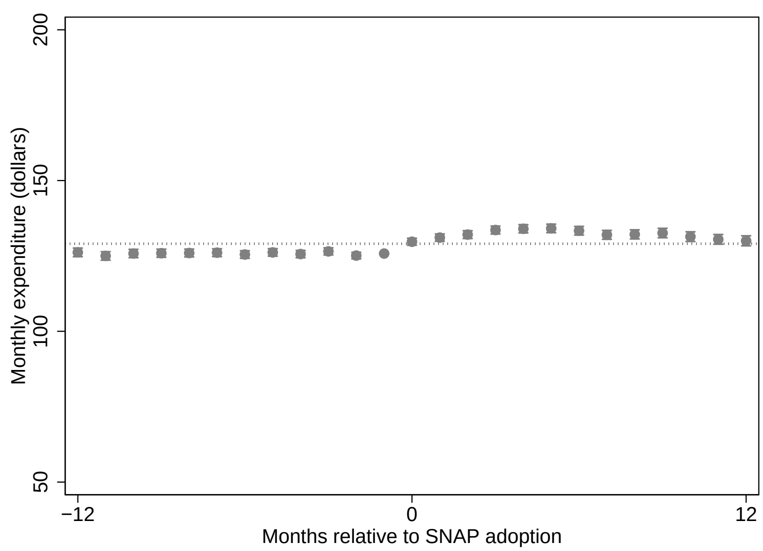
Notes: The sample is the set of SNAP adopters. Panel A plots the share of households with positive SNAP spending in each of the 12 months before and after the household's first SNAP adoption. Panel B plots the average SNAP benefit in each of the 12 months before and after the first SNAP adoption.

Figure 5: Monthly expenditure before and after SNAP adoption

Panel A: SNAP-eligible spending



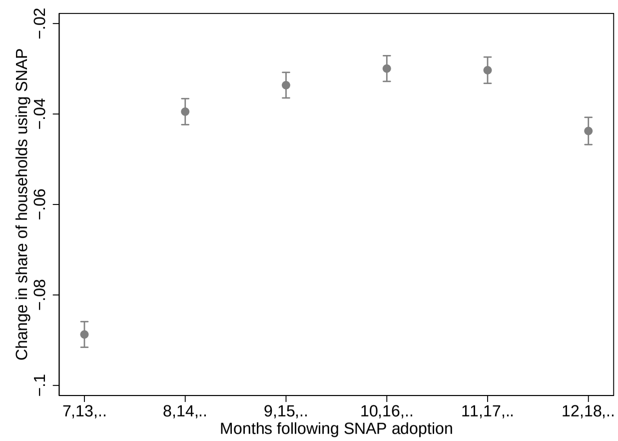
Panel B: SNAP-ineligible spending



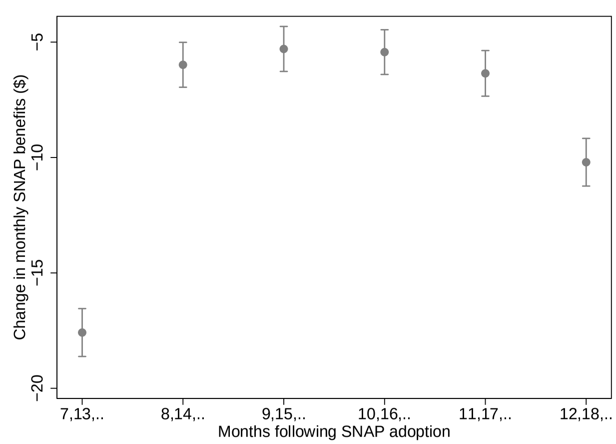
Notes: Each figure plots coefficients from a regression of SNAP-eligible or SNAP-ineligible spending on a vector of lead and lagged indicators for month relative to the household's first SNAP adoption, with the month prior to SNAP adoption ("-1") as the omitted category. The unit of observation for each 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

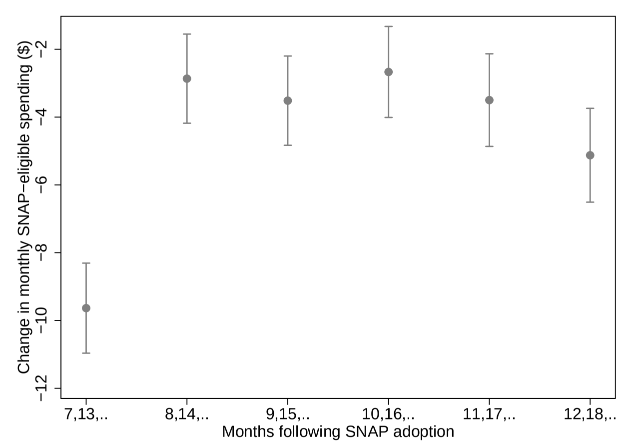
Panel A: SNAP use



Panel B: SNAP benefits



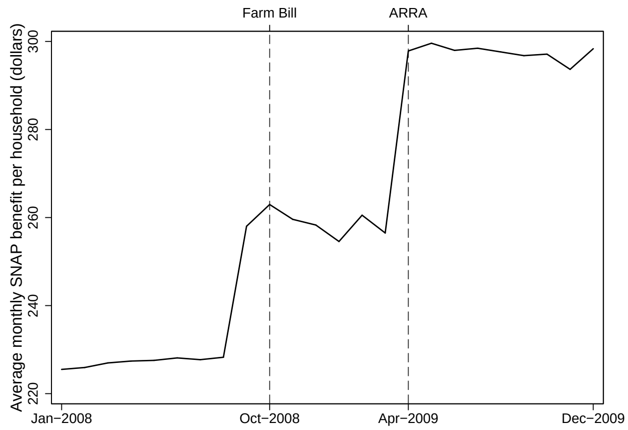
Panel C: SNAP-eligible spending



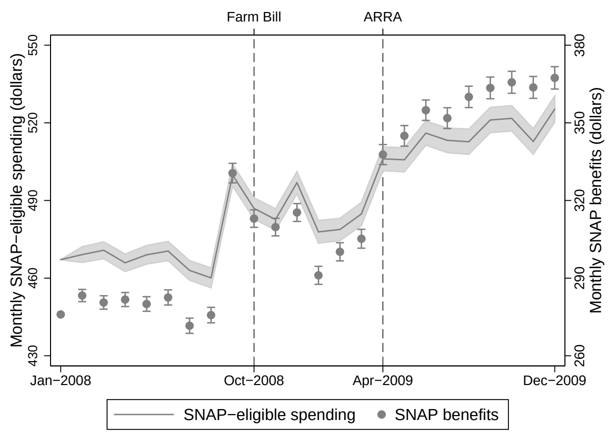
Notes: Each figure plots coefficients from a regression of the dependent variable on a vector of indicators for the position of the current month in a monthly clock that begins in the most recent adoption month and resets every six months or at the next SNAP adoption, whichever comes first. So, for example, the first month of the clock corresponds to months 7, 13, 19, etc. following SNAP adoption. 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 SNAP benefits and SNAP-eligible spending around benefit changes

Panel A: Administrative data for retailer states



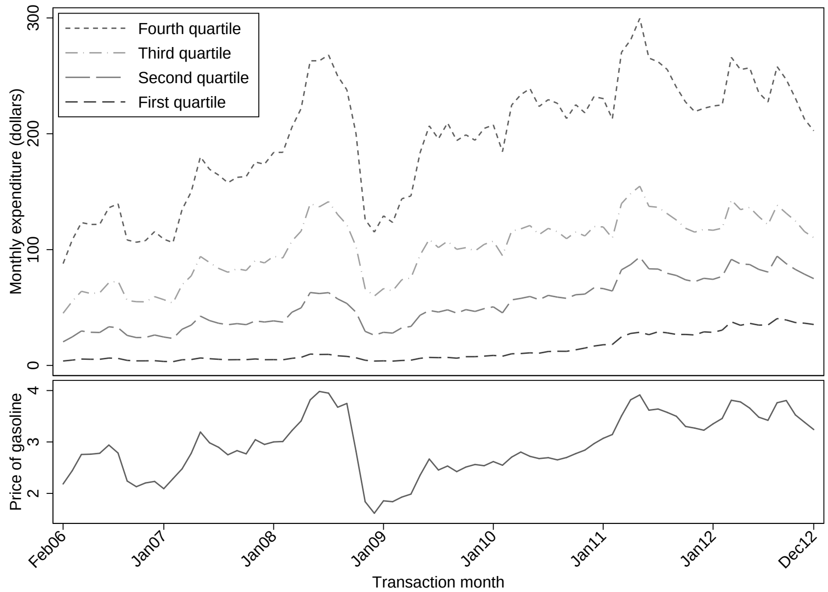
Panel B: Retailer data



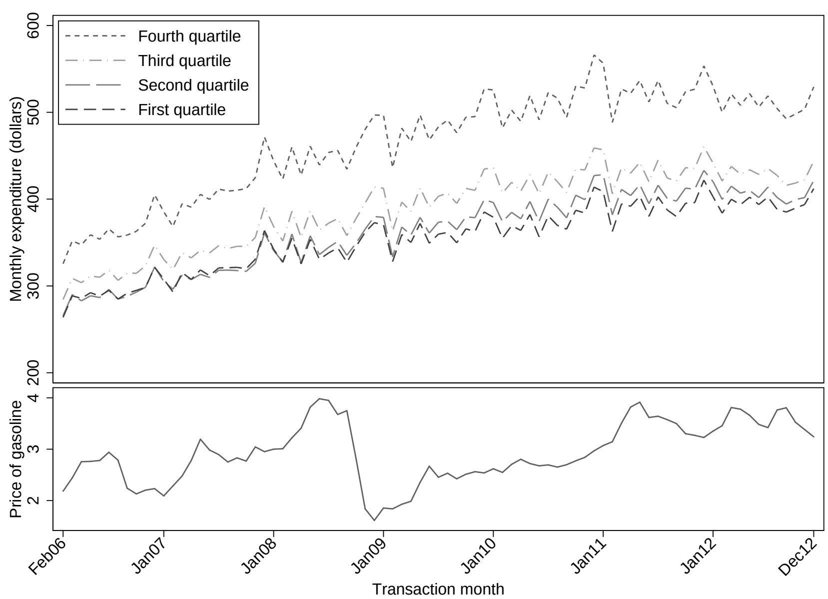
Notes: Panel A plots the average monthly SNAP benefit per household between January 2008 and December 2009 from administrative data. The series was obtained by weighing the average monthly SNAP benefit per household for each state (according to data from the United States Department of Agriculture Food and Nutrition Service via `http://www.fns.usda.gov/sites/default/files/pd/SNAPZip69throughCurrent.zip` as of May 2017) by the number of retailer households with at least one SNAP month. Panel B 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 sample includes all households in the retailer panel with at least one SNAP month. 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. In both panels, 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.

Figure 8: Monthly expenditure and the price of gasoline

Panel A: Fuel spending



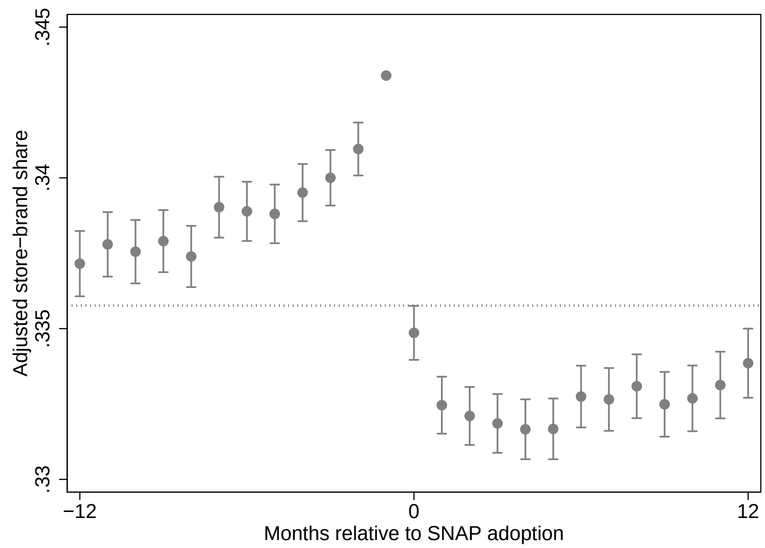
Panel B: SNAP-eligible spending



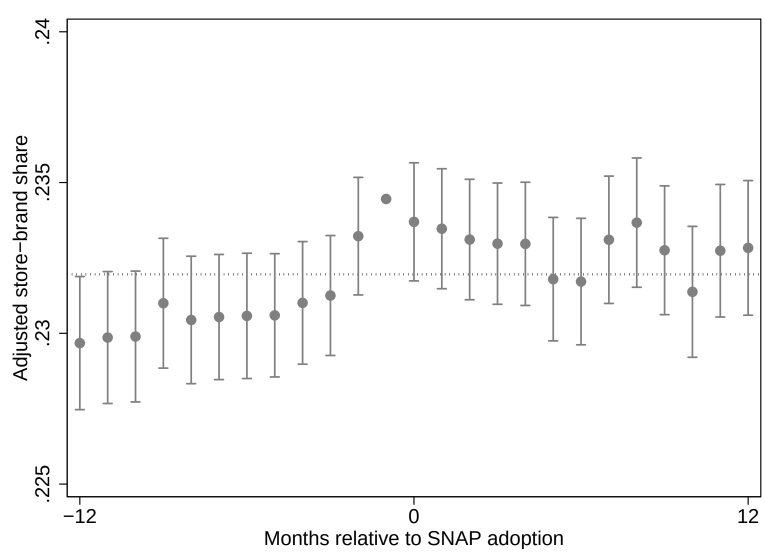
Notes: Panel A plots average monthly fuel spending by quartile of average monthly fuel spending. Panel B plots average monthly SNAP-eligible spending by quartile of average monthly fuel spending. The unit of observation is the household-month and the sample is the set of SNAP adopters who ever purchase 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 9: Store-brand share before and after SNAP adoption

Panel A: SNAP-eligible products



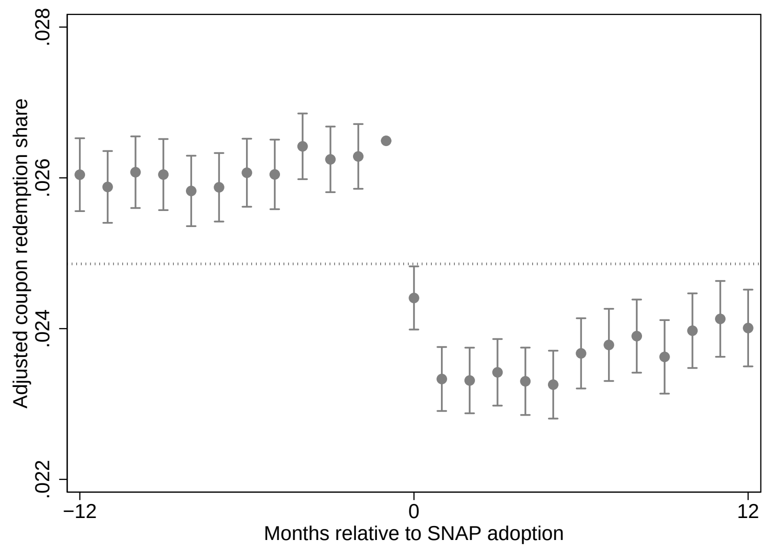
Panel B: SNAP-ineligible products



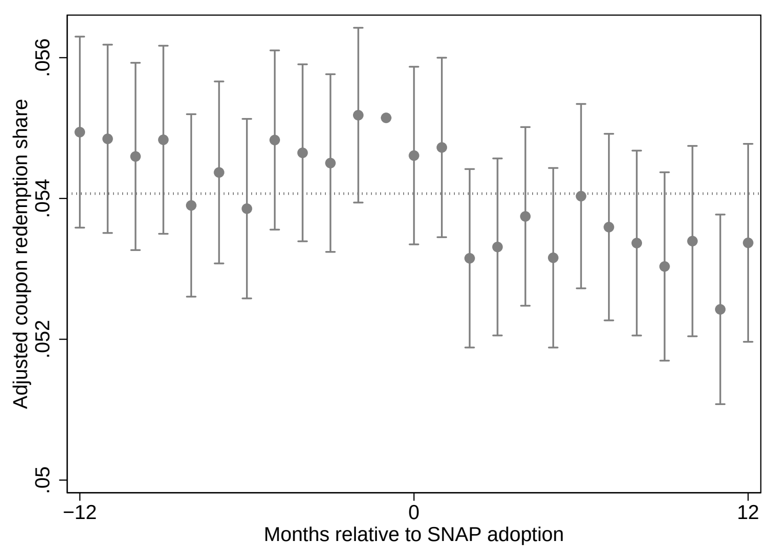
Notes: Each figure plots coefficients from a regression of adjusted store-brand share of expenditures on a vector of lead and lagged indicators for month relative to the household's first SNAP adoption, with the month prior to SNAP adoption ("-1") as the omitted category. The unit of observation for 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 10: Coupon use before and after SNAP adoption

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 1: Relationship between SNAP status and primary retailer share of expenditures in the Nielsen Homescan Consumer Panel

	Share of SNAP-eligible expenditure in primary retailer			
Primary retailer type	(1) All	(2) Grocery	(3) Discount store or warehouse club	(4) Other
On SNAP	0.0035 (0.0036)	0.0111 (0.0044)	-0.0106 (0.0063)	-0.0112 (0.0211)
Number of households	4407	2904	1345	158
Number of household-quarters	24198	16058	7309	831
Dependent variable:				
Mean	0.515	0.512	0.535	0.398
Within-household standard deviation	0.177	0.175	0.175	0.222

Notes: Data come from the Nielsen Homescan Consumer Panel and the Homescan Panel Omnibus Survey, as described in appendix A. The unit of observation is the household-quarter. Standard errors in parentheses are clustered by household. Each column reports results from a linear regression with household and calendar quarter fixed effects. The dependent variable, "Share of SNAP-eligible expenditure in primary retailer," is the share of SNAP-eligible spending at the household's primary retailer, defined as the retail chain in which the household spends the most on SNAP-eligible items across all quarters in which the household is observed in the Nielsen Homescan Consumer Panel. Column (1) includes all households regardless of the primary retailer. Columns (2) to (4) restrict the sample, respectively, to households whose primary retailer is a grocery store chain, a discount store or warehouse club chain, or another type of retailer. The key independent variable, "On SNAP", is an indicator for whether the household reports being on SNAP in the current quarter. Household-quarters with missing values of the "On SNAP" indicator are excluded from the sample. Households are included in the sample if they report being on SNAP in at least one survey response and report not being on SNAP in at least one survey response.

Appendix Table 2: Results for alternative samples and specifications

		MPCF out of SNAP benefits	MPCF out of cash	p-values for tests of fungibility	Number of household-months (households)
(1)	Baseline	0.588	-0.002	0.0000	2005392
		(0.007)	(0.049)	0.0000	(24456)
(2)	All households with at least 2 consecutive SNAP months	0.589	0.018	0.0000	8586958
		(0.007)	(0.049)	0.0000	(104719)
(3)	9-month SNAP adoption definition	0.571	0.009	0.0000	1079284
		(0.011)	(0.055)	0.0000	(13162)
(4)	12-month SNAP adoption definition	0.565	0.008	0.0000	668710
		(0.014)	(0.053)	0.0000	(8155)
(5)	SNAP exit instead of SNAP adoption	0.594	0.011	0.0000	1329056
		(0.010)	(0.047)	0.0017	(16208)
(6)	Below-median number of supermarkets in county	0.602	0.011	0.0000	984574
		(0.009)	(0.053)	0.0000	(12007)
(7)	Average SNAP-eligible spending exceeds average SNAP benefit	0.575	-0.000	0.0000	1664272
		(0.007)	(0.048)	0.0000	(20296)
(8)	Average SNAP-eligible spending exceeds average SNAP benefit by at least $100	0.584	-0.001	0.0000	1162842
		(0.009)	(0.046)	0.0000	(14181)
(9)	Average SNAP-eligible spending exceeds CEX average	0.523	-0.003	0.0000	1129304
		(0.008)	(0.047)	0.0000	(13772)
(10)	Households with only one adult	0.579	0.010	0.0000	350058
		(0.016)	(0.064)	0.0000	(4269)
(11)	Households never on WIC	0.544	0.004	0.0000	1159480
		(0.008)	(0.043)	0.0000	(14140)
(12)	Exclude recession adopters	0.579	-0.000	0.0000	1425160
		(0.006)	(0.052)	0.0000	(17380)
(13)	Dependent variable in natural logarithm	0.584	0.003	0.0000	2003876
		(0.008)	(0.015)	0.0000	(24456)
(14)	Dependent variable deseasonalized	0.503	0.006	0.0000	2005392
		0.007	0.026	0.0000	(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 3. 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 that have an average monthly SNAP-eligible spending of at least $292 in the six months before adoption, where $292 is the 2010 estimated average monthly spending on food at home by eligible nonparticipants in Mabli and Malsberger (2013). Specification (10) repeats specification (1) using the sample of SNAP adopters for which there is only one adult in the household. Specification (11) repeats specification (1) using the sample of SNAP adopters who never use WIC in any transaction. Specification (12) repeats specification (1) using the sample of SNAP adopters who did not adopt SNAP during the Great Recession (December 2007 - June 2009). Specification (13) 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 (14) 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).