Cyclical Food Insecurity and Electronic Benefit Transfer*

Michael A. Kuhn†
University of Oregon

April 20, 2018

Abstract

Intra-month cycles in consumption and expenditure are often present amongst participants in transfer programs, and a sizable literature documents that they have harmful consequences. Little is known about how these cycles interact with disbursement technology. I find that households with children experienced more extreme cycles than those without, prior to the implementation of Electronic Benefit Transfer (EBT) for Supplemental Nutrition Assistance Program participants. The implementation of EBT eliminated this differential severity. The results are not consistent with stigma reduction, security improvements, other welfare programs, or reduced salience of benefits as mechanisms. Household bargaining is a more plausible explanation. The findings have implications for the design of transfer disbursements and for why some households fail to budget successfully.

JEL Classifications: D13; D91; I38
Keywords: SNAP; Electronic benefits; Food insecurity

*I owe sincere thanks to James Andreoni, Julie Cullen, Gordon Dahl, Uri Gneezy, Yuval Rottenstreich, Jeffrey Clemens, Prashant Bharadwaj, Lawrence Schmidt, Douglas Bernheim, Charles Sprenger, Jesse Shapiro, Peter Kuhn, Dallas Dotter, Laura Gee, Matthew Niedzwiecki, Paul Smeets, Julian Jamison, Benjamin Hansen, Glen Waddell, and David Figlio for helpful comments.

†University of Oregon, Department of Economics, 1285 University of Oregon, Eugene, OR 97403, USA. E-mail: mkuhn@uoregon.edu. Web page: http://www.pages.uoregon.edu/mkuhn.

1 Introduction

> My food stamps are depleted after maybe two and a half weeks. That's when our cupboards become bare and there isn't anything left in the deep freezer. I start to worry about where our next meal is coming from.
>
> –Tiffany, mother of three (Narula et al. 2013, p. 20)

Many households receiving food assistance do not smooth their expenditure and consumption between disbursement dates. In the U.S., food spending and calories consumed jump upon SNAP (Supplemental Nutrition Assistance Program) disbursement and then decay over the course of the month. This is often called the 'calorie crunch', and it is well documented.[1] Thus, the average consumption of a participating household masks variance in food insecurity.[2] For example, a cross-section of SNAP households in 2011 and 2012 found that 61% were food-insecure at the time of the survey (Mabli et al., 2013).

Crime, health and education outcomes are all related to the SNAP cycle. Carr and Packham (2017) find that grocery store theft cycles with the SNAP schedule. Financially motivated crimes increases as the benefit month progresses and resources run out (Foley, 2011). Then, the sharp transition from scarcity to plenty increases intimate partner violence (Hsu, 2017) and drug use (Dobkin and Puller, 2007) when benefits arrive. According to Seligman et al. (2014), hypoglycemia hospital admissions are 27% higher in the last week of the month than the first week of the month for low-income households (with no difference for high-income households). Children's test scores are lower at the end of the benefit month (Cotti et al., 2017), and they are more likely to misbehave in school Gennetian et al. (2015). Economists and policy makers should have an acute interest in what determines the severity of the calorie crunch that a household experiences.

As access to digital and mobile financial tools expands, researchers should seek an understanding of how payment and transfer-disbursement techniques impact the nature and timing of spending and consumption. The introduction of Electronic Benefit Transfer (EBT) is a useful case study in this regard. EBT is the current disbursement technique for multiple transfer programs in the U.S., but it was first introduced as a replacement for cash-similar SNAP coupons (literal 'food stamps').

[1] Wilde and Ranney (2000); Shapiro (2005); Hastings and Washington (2010); Castner and Henke (2011); Todd (2015); Smith et al. (2016).

[2] Food insecurity is defined as experiencing ”reduced quality, variety, or desirability of diet” (http://www.ers.usda.gov/topics/food-nutrition-assistance/food-security-in-the-us/definitions-of-food-security).

Users have an individual-specific debit card controlled by a Personal Identification Number (PIN). From the user's point of view, the introduction of EBT represented a change in procedure, security and store of value for their SNAP benefits. During the roughly 15-year rollout period, nearly 10% of Americans were affected by this change.[3]

There is limited empirical research on the behavioral impacts of such technological changes, which is unfortunate because there are a number of theoretical reasons why disbursement mechanisms might affect the calorie crunch. For example, if stigma associated with using visually-identifiable welfare led participants to use their benefits all at once, switching to a discreet store of value could allow them to spread out their shopping trips out. Or, if EBT consolidated decision-making power in the hands of a single, patient individual, its introduction could have decreased effective household discount rates.

I find that the impact of EBT was to substantially reduce the severity of the calorie crunch in households with the largest pre-EBT calorie crunches: those with more children. Before EBT, an additional child under 18 was associated with a 33% faster decline in food expenditure over the benefit month.[4] EBT eliminated this differential decline. Another way to frame the magnitude of this effect is to consider how much food expenditure was increased at the very end of the benefit month as a result of EBT. For a household with one adult and two children, food expenditure during a shopping trip in the fourth week of the benefit month was $19 higher after EBT was implemented (in 2017 $). This increase was enabled by a reduction in the large spending spike that occurs in the first week of the benefit month.

I examine the results for evidence of mechanisms that can explain the heterogeneous impact of EBT. Two of the explanations I consider are natural candidates as they relate directly to explicit goals of the EBT program: reduced stigma and reduced risk of benefit theft. Both were factors that motivated the implementation of EBT. However, the nature of the observed changes in expenditure patterns is not consistent with either mechanism as the driver of the heterogeneous effect of EBT. These mechanisms predict changes to the extensive margin of shopping behavior over the benefit month, which I do not observe.

I also consider a mechanism related to changes in other welfare policy. EBT rollout occurred

[3]https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

[4]From column (4) of Table 4. Household size and a variety of other characteristics are held constant.

state by state and over many years. As such, other policy changes, most notably the 1996 Welfare Reform, occurred within my sample. My identification approach and a variety of robustness checks are designed to ensure that other policy changes do not influence my estimates of the impact of EBT. However, one program is worthy of particular attention because it was expanded gradually over a similar time horizon to EBT. Expansions in the SNAP-Education (SNAP-Ed) outreach program may have helped teach households to budget more effectively, and households with kids might face a more difficult budgeting problem with less time to solve it. I use SNAP-Ed program funding data as controls to show that SNAP-Ed expansions are not confounders that explain away the impact of EBT. I also estimate whether the combination of EBT and SNAP-Ed could explain the heterogeneous impact of EBT. While this mitigates the un-interacted effect of EBT somewhat, the interaction term is not statistically significant.

The other two mechanisms I consider are motivated by recent work in behavioral economics on budgeting: imperfect salience, and household bargaining and discounting. Concerning the first, reduced salience of benefit arrival –EBT cards are automatically recharged at midnight each month, whereas food stamp coupons arrived in the mail– could have smoothed shopping behavior at the beginning of the month. However, I find no impact of EBT on the likelihood of a shopping trip soon after benefit disbursement. This holds in general, and for households with kids.

The second behavioral mechanism I consider is a modification of a common explanation for the calorie crunch: present-biased discounting. Shapiro (2005) draws a link between the calorie crunch and present bias, and Jackson and Yariv (2014a) show that within-group differences in preferences lead to collective present bias. Combining these two ideas produces a possible mechanism for the heterogeneous impact of EBT.[5] More preference-diverse and unruly households are more present-biased and thus experience a more severe calorie crunch. The switch from cash-similar coupons to an individual-specific EBT card likely strengthened the primary recipient's control over how the funds were spent. This could have reduced present bias and therefore the calorie crunch as well. I find considerable support for the reallocation of bargaining power: EBT shifted the nature of purchases as well as the timing. There was also no significant impact of EBT for households where I would not expect a bargaining problem to exist prior to EBT: single-individual households, and

[5]In the interest of full disclosure, the study of this specific mechanism was my original motivation for examining the heterogeneous impact of EBT on the calorie crunch, and writing this paper.

single-adult households with only infant children. On the other hand, I do not observe a stronger impact of EBT for households with more adults, holding the number of children fixed, which would be consistent with this mechanism.

There are at least two significant policy takeaways from these findings. First, in conjunction with the considerable previous literature on the calorie crunch, the magnitude of the expenditure cycle I find –even after EBT implementation– suggests that increasing the frequency of SNAP disbursements could be a low-cost way to make SNAP more effective.[6] Second, benefit security issues are often debated on the merits of fraud protection versus user stigma and flexibility. Consistent with Laibson (1997) in a different context, my findings suggest that the intra-household value of clear benefit property rights may be high. For cash welfare programs like the Earned Income Tax Credit, which is very large and disbursed once a year, policy makers need to consider to whom the transfer is going and how the recipient will access it. These features of program design will influence both the nature and dynamics of transfer spending, with potentially significant consequences for economic welfare.

Section 2 discusses foundational empirical work and the details of the EBT program. Section 3 presents the primary results and robustness checks while Section 4 evaluates potential mechanisms. Section 5 concludes.

2 Background

In this section I provide additional detail on food insecurity in the U.S., and its negative associations, to motivate this study. I also review the literature on expenditure and consumption cycles, which is foundational to my modeling approach. Last, I discuss the program details associated with the introduction of EBT, especially as they relate to causal identification of the program's impact.

[6] EBT implementation has dramatically reduced the costs of such a project, relative to the figures in the cost-benefit analysis of Shapiro (2005).

2.1 Food Insecurity

Food insecurity is common in the U.S., especially during economic downturns. The number of individuals classified as such by the USDA grew by 14 million (roughly 28%) from 2007 to 2011.[7] Food insecurity has numerous deleterious associations, most notably for young children and expectant mothers. These are documented in a large literature, mostly outside economics. Food insecurity is associated with low birth weight deliveries (Borders et al., 2007) as well as preterm births and retarded fetal growth (King, 2003). In children 0-3, it is associated with a higher likelihood of hospitalization and lower overall reports of health status (Cook et al., 2004). Obesity is also associated with food insecurity through a quality-quantity food tradeoff; experiencing food insecurity at any point during the toddler years is a stronger predictor of obesity at 4.5 years old than having one obese (or overweight) parent (Dubois et al., 2006).[8] Food insecurity has important behavioral correlates as well. Children from food insecure households lag in ability by the time they enter kindergarten, and learn less during the year (Winicki and Jemison, 2003). They exhibit worse behavior throughout their schooling (Alaimo et al., 2001). Recent work in economics has addressed the impact of food availability on school outcomes. Howard (2011) shows negative effects of food insecurity and transitions into food insecurity on classroom behavior in a elementary school panel. Dotter (2013) shows that directly providing breakfast to students in the classroom leads to persistent math and reading test score gains, and behavior improvements. While the bulk of the research focuses on children, Seligman et al. (2010) shows an association between food insecurity and various chronic cardiovascular diseases in adults.

There is a more substantial economic literature on the impact of SNAP on households, and the associated long-run benefits. Economists have highlighted the program's impact on food insecurity (Bhattacharya and Currie 2000, Hoynes and Schanzenbach 2009), many types of nutritional intake (Devaney and Moffitt, 1991), and child health (Almond, Hoynes, and Schanzenbach 2011, Currie and Cole 1991, Currie and Moretti 2008).[9] Recent work by Hoynes et al. (2016) shows that participation as a child reduces the incidence of metabolic syndrome as an adult, and even increases economic self-sufficiency for women.

[7] Narula et al. (2013), p. 8.
[8] This is a brief summary of a detailed report prepared by Cook and Jeng (2009).
[9] For a more comprehensive review of food assistance programs generally, see Currie (2003).

This paper is focused specifically on within-month variance of food insecurity. Is the literature on the importance of food insecurity in general relevant for the end-of-month food insecurity induced by the SNAP benefit cycle? I argue that it is. First, it has behavioral consequences for children; school disciplinary events increase (Gennetian et al., 2015) and test scores decrease (Cotti et al., 2017) for children in SNAP households at the end of the benefit month. Second, the way food insecurity is measured seeks out specific episodes of food shortfall. The U.S. Food Security Scale, administered in the Current Population Survey, ascertains a household's food security status retrospectively. One example is, "In the last 12 months, did any of the children ever not eat for a whole day because there wasn't enough money for food?" Thus, households that experience only intermittent food shortages are a part of the group classified as food insecure in the existing literature. Additionally, Mabli et al. (2013) find that SNAP participants are an important part of the food insecure population in the U.S. Yet, on the day that monthly benefits arrive, there are surely sufficient food resources in most participating households. A better understanding of what causes and alleviates the calorie crunch is an important part of developing policy to combat food insecurity in general.

2.2 Expenditure and Consumption Cycles

According to the Permanent Income Hypothesis, the arrival of anticipated income should not trigger consumption changes among unconstrained agents. Even constrained agents should approximately smooth consumption between disbursements if the gap is short and there is a low intertemporal elasticity of substitution (as would be expected with food). A sizable empirical literature has established that this prediction does not hold for non-durables on a monthly frequency. Instead, consumption and expenditure exhibit a strong dependence on payment dates even when payment is anticipated. SNAP payments generate cycles in caloric intake (Wilde and Ranney, 2000; Shapiro, 2005; Todd, 2015), meals consumed (Kuhn, 2017) and grocery expenditures (Hastings and Washington, 2010; Smith et al., 2016). Social security payments generate cycles in both general non-durable consumption (Stephens, 2003) and caloric intake specifically (Mastrobuoni and Weinberg, 2009). Paychecks also generate cycles in non-durable consumption (Stephens, 2006).

There is some work on heterogeneous expenditure and consumption cycling within this literature. Liquidity constrained households exhibit more severe fluctuations (Mastrobuoni and Weinberg, 2009). Programs targeted at low-income households should thus be particularly susceptible to cycling. Shapiro (2005) estimates heterogeneity in the calorie crunch by household size. He finds no economically or statistically significant relationship. Closely related to this paper, Todd (2015) finds that households with and without children experience similar diet cycles using single-day dietary intake data from the 2007-2010 National Health and Nutrition Examination Survey.

2.3 The Introduction of EBT

In 1989 Maryland was the first state to begin implementing EBT statewide, with completion in April of 1993.[10] A number of states implemented the program of their own accord until 1996, when a welfare reform bill mandated the full implementation of EBT across the country by October of 2002.[11] It took the median state just over one year to fully implement the program after the initial pilot. I use the month of statewide completion as the policy change date. The results are robust to the exclusion of the rollout period from the sample.

Two issues with SNAP prior to the implementation of EBT were the stigma associated with identifiable coupons and the lack of clear property rights that allowed them to disperse (either voluntarily or involuntarily). EBT cards addressed both of these issues. They work and look like standard debit cards, with a PIN required for use. PINs can be changed with minimal transaction cost over the phone. Benefits are loaded onto the cardholder's account on a monthly frequency, but the disbursement dates vary by state. A household's card is issued to the primary benefit recipient.[12] Most states put names on the card, and in Massachusetts, Missouri and New York (on a voluntary basis) the cards feature a photo of the primary recipient.[13] Compared to the cash-similar coupons that were used prior, the primary recipient is more clearly delineated following the policy change.

There is a literature that examines the impact of the introduction of EBT on SNAP participation. Results are mixed: Atasoy et al. (2010) find a decrease in enrollment, Currie and Grogger (2001),

[10] http://www.fns.usda.gov/ebt/electronic-benefits-transfer-ebt-status-report-state

[11] A number of states were unable to comply until 2003, and California and Guam did not complete implementation until 2004 (http://www.fns.usda.gov/snap/short-history-snap).

[12] The primary recipient is the individual that fills out an application (in most states, this can now be done online, but it can still be done by mail or in person) and participates in the follow-up interview (either by phone or in person).

[13] New York cards also feature name, gender, birthdate and a signature. The exact rules for EBT cards and their usage vary slightly from state to state.

Kornfeld (2002), Kabbani and Wilde (2003), Danielson and Klerman (2006) and Kaushal and Gao (2011) find and increase and Ziliak et al. (2000), McKernan and Ratcliffe (2003) and Bednar (2011) find no impact. Researchers use across-state differences in the timing of implementation for identification, with state-specific time trends used where data quality permits. This literature is important because it speaks to whether EBT induces compositional changes in SNAP participation that could affect the calorie crunch. Bednar (2011) tests whether state characteristics from the 1990 census predict when a state will implement EBT, and finds no evidence thereof. Beyond examining SNAP participation, Wright et al. (2014) find that the implementation of EBT in Missouri had a negative impact on crime rates. Within Missouri, their identification technique is similar; they use the across-county variation in timing to estimate a generalized difference-in-difference model.

3 Data and Results

In the first subsection I describe the primary data for my analysis, the Consumer Expenditure Survey (CES). This includes construction of the sample and baseline calorie crunch estimates. In Section 3.2, I present the main result of the paper: the heterogeneous impact of EBT. Section 3.3 offers several robustness checks and a more in-depth discussion of the policy landscape surrounding EBT. Section 3.4 explores what the expenditure cycles that I identify imply for consumption.

3.1 CES Data and Methodology

The Consumer Expenditure Survey (CES) diaries are published by the U.S. Bureau of Labor Statistics. These are self-reported expenditure logs that cover 14 consecutive days. They are collected every year, all throughout the year, and consist of two back-to-back, week-long logs formatted to keep item-specific records of all purchases. The diaries are linked to a broader, one-time survey of households. Item-level data are coded with a Universal Classification Code (UCC), which identifies expenditures on narrow food categories. Purchases are not coded as individual-specific.

The usable set of CES diaries ranges from 1994 to 2003. Prior to and following this period, the CES did not ask for the exact date of the most recent SNAP disbursement. Household i is observed over 14 consecutive diary days, j. j is transformed into a variable that indicates the number of days since the last SNAP disbursement, t, with $t = 0$ on the exact date of reported arrival. For

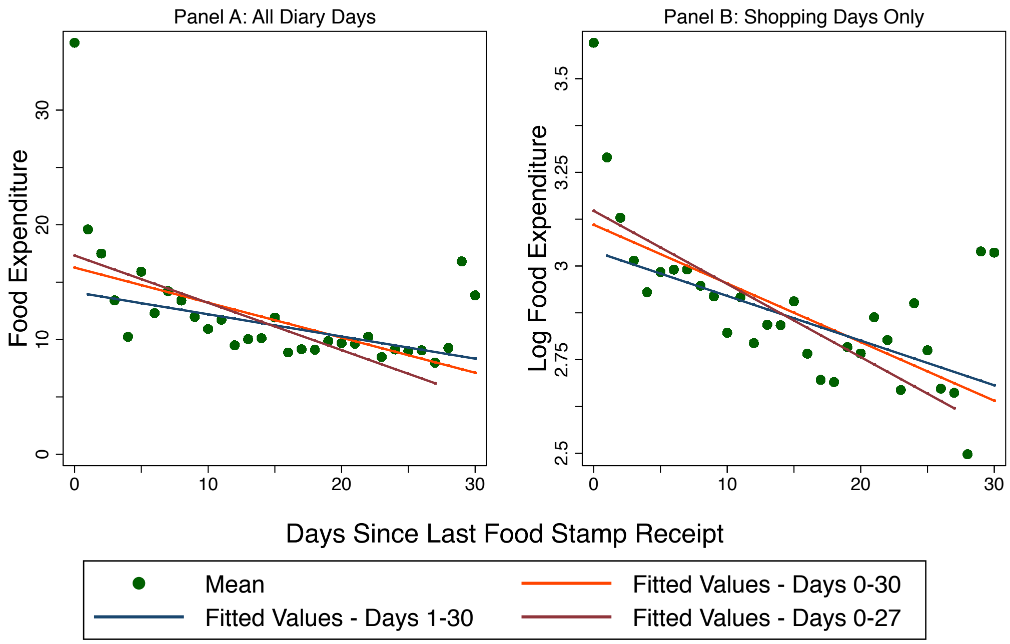

Figure 1: Food Expenditure Cycles in the CES

example, if $t = 0$ corresponds to $j = 1$ for a particular household, all 14 days of the diary are used as the first two weeks of expenditures. Panel A of Figure 1 shows the time path of mean household SNAP-eligible food expenditure (measured in 2017 $) of the course of the benefit month. There is large spike on the day of disbursement, however, the fitted value plots show that the calorie crunch is robust to excluding that point. Average food expenditures on the day of SNAP disbursement ($t = 0$) are $35.87 ($82.66, conditional on shopping, with 44% of households shopping). After four weeks, expenditures decline to $9.26 on average ($29.08, conditional on shopping, with 33% of households shopping).[14] As shown in Figure 1, the decline from day zero to day one of the benefit month is very large. After that, expenditures decline more slowly and steadily over the course of the month.

I do not use data after the fourth week of the benefit month (past $t = 28$). As shown in Figure 1, there is a spike there that may correspond to other income at the beginning of a new calendar or benefit month.[15] Also, I do not use diary observations that fall outside of the SNAP period

[14] If households shop weekly, budgeting for four trips a month with a small gap to bridge at the end, a better comparison for day zero of the month is day 21. On this day, average expenditures are $9.63 ($31.34 conditional on shopping, with 31% of households shopping.

[15] While most states vary the day of disbursement across individuals, there is often bunching of the potential dates near the beginning of the month. Also, if the day of last SNAP disbursement is reported with some noise, this spike could

corresponding to the reported disbursement because of uncertainty over whether the implied next disbursement will occur. Recent data from the USDA indicate that "churning" in and out of SNAP is very common: across six states in a study, 17-28% of households exited and re-entered within a four month period, with about a third of those households only leaving for one month (Mills et al. 2014). So long as $j = 1$ corresponds to $0 \leq t \leq 14$, all 14 diary days are potential shopping days. Overall, there are 17,665 household-days in the sample.[16] At least \$1 of SNAP-eligible purchases are made on 32% of these days. I call these days 'shopping' days. Panel B in Figure 1 shows the time path of log SNAP-eligible for expenditures on shopping days only. The large spike on day zero remains, but the calorie crunch is robust to excluding that point.

The baseline fixed-effect specification for estimating expenditure trends, without any heterogeneity or policy impact, is

$$e_{it} = \alpha_i + \gamma_1 t + Y_{it}\Theta_1 + \epsilon_{it} \quad , \tag{1}$$

where e_{it} are the total SNAP-eligible food expenditures of household i on days since SNAP receipt t, in 2017 dollars.[17] Y_{it} are other characteristics of the day in question to be controlled for: a weekend indicator variable, a week of calendar month variable and a week of diary variable.[18] e_{it} is constructed as the sum of all SNAP-eligible expenditures on a given diary day (all non-prepared food and beverage expenditures, besides alcohol).

I consider e_{it}, e_{it} on shopping days only (intensive margin), and the likelihood of $e_{it} \geq 1$ (extensive margin).[19] Examining the margin by which EBT impacts heterogeneity in expenditure

---

be due to a new disbursement.

[16] I remove households with incomplete information on size or children, missing SNAP benefit amount, SNAP benefit amount reported under \$10, and unit size larger than twelve members from this baseline.

[17] A notable difference between this specification and that of Shapiro (2005) is the use of a fixed effect. This is enabled by the long household diaries in the CES offering 14 observations per household. The advantage is that fixed effects offer some robustness against specification error in this case. Because α_i represents household expenditures on day zero of the benefit month for all households in an OLS/Random Effects model, it is an out-of-sample projection for households observed late in the month. Specification error could produce systematically bad projections and thus a correlation between t and α_i, which necessitates the fixed-effects model.

[18] These variables are also indexed by i because the mapping from t to calendar day varies across households.

[19] Angrist and Pischke (2009) caution against using conditional-on-positive models to evaluate treatment effects because selection into positive values that is correlated with treatment will bias the estimates. I am able to examine that selection directly using the extensive margin estimates. In addition, using a conditional-on-positive model is less problematic in a fixed-effects panel framework because the diary captures the same households at different times of the month. In the language of Angrist and Pischke (2009) on the analysis of experimental treatments using conditional-on-positive models, rather than calculating $E[y_{1,i}|y_{1,i} > 0] - E[y_{0,i}|y_{0,i} > 0]$, I calculate $E[y_{1,i} - y_{0,i}|y_{1,i} > 0, y_{0,i} > 0]$ (p. 99). In general, while OLS is an inconsistent estimator of the latent-determinant γ when the mass at $e_{it} = 0$ is the result of a latent process with censoring (Wooldridge 2002, p.524), it is a consistent

Table 1: Food Expenditure Cycle Estimates

Dep. Var.	e_{it}		e_{it} per capita		e_{it} per SNAP $		$ln(e_{it})$	$1(e_{it} \geq 1)$
Sample Rest.	None	$e_{i,t} \geq 1$	None	$e_{i,t} \geq 1$	None	$e_{i,t} \geq 1$	$e_{i,t} \geq 1$	None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
t	-2.73***	-2.71***	-0.97***	-0.80***	-0.02***	-0.02***	-0.06***	-0.03***
	(0.18)	(0.36)	(0.08)	(0.10)	(0.00)	(0.00)	(0.01)	(0.00)
Weekend	0.20	2.10	0.05	0.88*	-0.01	0.01	0.03	-0.02**
	(0.65)	(1.30)	(0.23)	(0.50)	(0.01)	(0.02)	(0.03)	(0.01)
Week of month	-0.91**	-1.34*	-0.30**	-0.35	0.00	-0.01	-0.03	-0.01
	(0.35)	(0.73)	(0.13)	(0.22)	(0.00)	(0.01)	(0.02)	(0.01)
Diary week	16.47***	13.90***	5.81***	4.10***	0.13***	0.10***	0.35***	0.21***
	(1.35)	(2.76)	(0.62)	(0.87)	(0.01)	(0.03)	(0.07)	(0.02)
Clusters	41	41	41	41	41	41	41	41
N	17,665	5596	17,665	5596	17,665	5596	5596	17,665

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses are clustered at the state level. All specifications feature household fixed effects.

patterns will help shed light on the mechanism behind the effects in Section 4.

Table 1 presents estimates of the daily decline in food expenditures, γ_1. Pooled across all households, average SNAP-eligible food expenditures decline roughly $2.73 per day over the benefit month. Limited to shopping days only, the estimate is very similar: $2.71 per day. Per-capita, I find a decline of $0.97 per day per person ($0.80 limited to shopping days only). As a fraction of SNAP benefits, I find a decline of 2.0% per day (1.8% limited to shopping days only). A $ln(e_{i,t})$ model on shopping days only indicates a decline of 6.2% per day.

Variables that don't vary within a household's diary, X_i, are added as interaction terms with t. These include household size (minus one) and the number of children (under 18). I also use SNAP benefit amount and gross annual income (both in 2017 $) as control variables to try and account for any mechanical relationship between household composition and the expenditure trend.

To consider a heterogeneous policy effect, I add an indicator for whether EBT rollout has been completed in a household's state, EBT_i as an interaction with t, and all X_i variables are included in a triple interaction with both t and EBT_i. The main specification is

$$e_{it} = \alpha_i + \gamma_1 t + \gamma_2(t \cdot EBT_i) + Y_{it}\Theta_1 + (t \cdot X_i)\Theta_2 + (t \cdot EBT_i \cdot X_i)\Theta_3 + \epsilon_{it} \quad . \tag{2}$$

estimator of the conditional expectation of e_{it}, when the zeros are genuine data (Angrist and Pischke 2009, p.96).

Table 2: Year of EBT Completion for States in Sample

Year	States	Month, Respectively
1993	Maryland	4
1995	Texas, South Carolina	11, 12
1996	Utah	4
	Welfare Reform Bill Passed - EBT Mandated	
1997	Kansas, Connecticut, Massachusetts, Alabama, Illinois, Louisiana	3, 10, 10, 11, 11, 12
1998	Oklahoma, Colorado, Idaho, Arkansas, Missouri, Oregon, Alaska, Hawaii, Pennsylvania, District of Columbia, Florida, Minnesota, Vermont, Georgia	1, 2, 2, 4, 5, 5, 6, 8, 9, 10, 10, 10, 10, 11
1999	New Hampshire, New Jersey, North Carolina, Arizona, Tennessee, Ohio, Kentucky, Washington	1, 6, 6, 8, 8, 10, 11, 11
2000	Wisconsin	10
2001	New York, Michigan	2, 7
2002	Indiana, Nevada, Virginia, Nebraska	3, 7, 7, 9
	EBT Implementation Deadline	
2003	Iowa	10
2004	California	6

Source: http://www.fns.usda.gov/ebt/electronic-benefits-transfer-ebt-status-report-state

In some specifications, I use year fixed effects, state fixed effects, state-specific linear time trends, and year fixed effects interacted with X_i, all interacted with t. In the absence of the fixed effects, γ_1 is an estimate of the per-day change in food expenditures for a single individual with \$200 in SNAP benefits, and a gross annual income of \$20,000, prior to EBT. Θ_2 contains coefficients that represent the conditional correlations of the fixed household characteristics with the per-day change in food expenditures. γ_2 is the general effect of EBT on the expenditure trend and Θ_3 contains the heterogeneous EBT effect coefficients. The error term, ϵ_{it} is clustered at the state level, with 41 states in the sample.

While the specifications all include a household fixed effect, the identification of the policy impact relies on across-household differences in within-household time trends. Therefore, stability of the sample composition across the EBT implementation is important. A critical feature of this policy change for causal identification is that EBT rollout occurred at different times in different states over roughly a ten-year period (see Table 2 for the schedule). Thus, my estimates compare households within the same state over time and households at the same time across states (in the models without a state times t fixed-effect).

Table 3: Household Characteristics and EBT Status

Dep. Var.:	SNAP ben. ('17 $)	HH Size	# children	# female adults	Median adult age	# earners	Gross ann. inc. ('17 $)	# HS Deg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Unconditional Means								
EBT	-8.53	-0.05	-0.10	0.01	0.31	0.08	32.84	0.29***
	(11.51)	(0.21)	(0.17)	(0.03)	(1.23)	(0.06)	(1,410.29)	(0.06)
Constant	272.59	3.36	1.67	1.14	40.61	0.59	20,323.62	0.74
	(8.47)	(0.21)	(0.16)	(0.03)	(1.22)	(0.06)	(1,297.75)	(0.05)
Panel B: Means with Year Fixed Effects								
EBT	17.28	-0.03	-0.12	0.04	-0.27	0.11	-186.81	0.01
	(27.62)	(0.47)	(0.34)	(0.07)	(1.88)	(0.13)	(2,025.83)	(0.05)
Constant	318.37	3.51	1.80	1.16	39.05	0.63	20,718.17	0.35
	(16.42)	(0.21)	(0.14)	(0.05)	(1.53)	(0.08)	(1,674.14)	(0.04)
Panel C: Means with Year and State Fixed Effects, and State-specific Linear Time Trends								
EBT	-0.54	-0.21	-0.28	0.03	0.61	-0.02	2122.06	0.08
	(22.52)	(0.29)	(0.27)	(0.09)	(2.49)	(0.12)	(2972.10)	(0.13)
Constant	333.04	4.18	2.30	1.22	31.80	0.59	26,445.67	0.60
	(14.82)	(0.12)	(0.12)	(0.03)	(0.93)	(0.04)	(1702.19)	(0.04)
Clusters	41	41	41	41	41	41	41	41
N	1578	1578	1578	1578	1575	1578	1578	1578

*** $\Rightarrow p < 0.01$. Estimates are from OLS regressions of the dependent variable on EBT completion status. Standard errors in parentheses are clustered at the state level. All specifications in Panel B feature year fixed effects, with 1994 as the excluded year. All specifications in Panel C feature year fixed effects, with 1994 as the excluded year, state fixed effects with California as the excluded state, and state-specific linear time trends.

This generalized diff-in-diff framework is a typical approach, but the broad welfare reform legislation that passed in 1996 is a specific event of concern. EBT was implemented at different times over a range of years, but the policy variable only ever turns on over time, not off. Thus, there should be some correlation between the EBT indicator and the impacts of reform. If SNAP households after welfare reform are very different from pre-reform households, or if EBT itself induces differential SNAP selection, the sample will not be balanced across the policy change. Given the year fixed effects, state fixed effects and state-specific linear time trends, selection effects that sharply coincide with EBT are of primary concern. Table 3 shows the balance of household observables across the implementation of EBT, with and without year fixed effects, state fixed effects and state-specific linear time trends. Adding year fixed effects eliminates the significant difference in educational attainment I find across the policy change.

Other aspects of SNAP and the safety net should also be constant for proper sample bal-

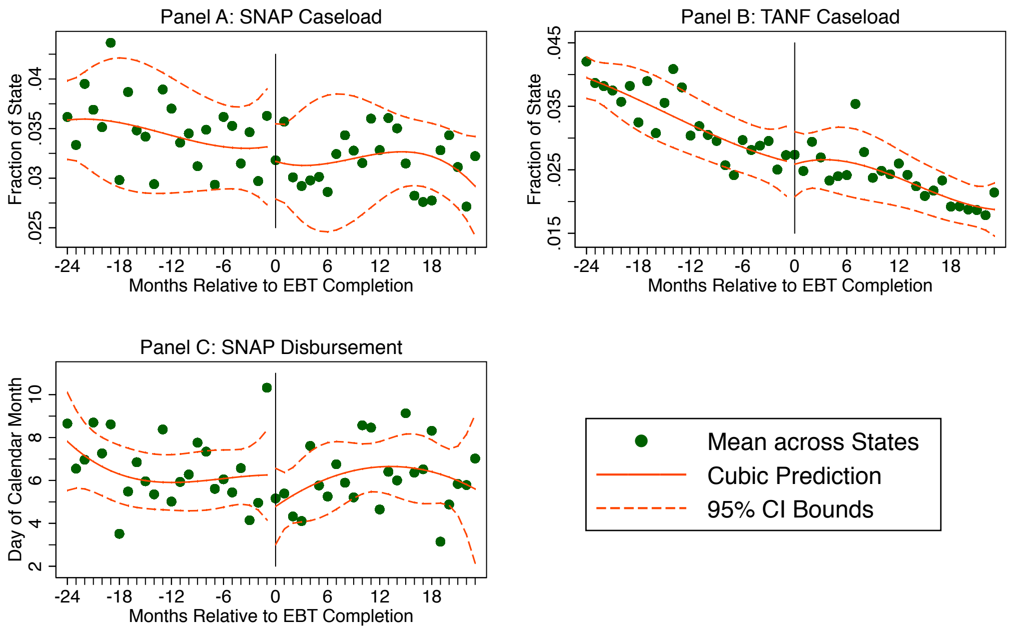

Figure 2: Welfare Program Characteristics through EBT Implementation

ance. Figure 2 shows that SNAP caseloads (Panel A), Temporary Assistance for Needy Families (TANF) caseloads (Panel B) and the average SNAP disbursement calendar day (Panel C) are smooth through the completion of EBT.[20] Carr and Packham (2017) find effects of recent changes to disbursement dates on crime, possibly due to increased consumption smoothing. Therefore, it is important that disbursement dates don't change along with the implementation of EBT. Notably, TANF caseloads are steadily declining over time, which was a primary impact of welfare reform.[21] Year fixed effects and state-specific linear time trends will help to capture this change.

3.2 Main Estimates

Table 4 shows results from estimating equation (2) with a variety of specifications of control variables, fixed effects and time trends. The sample is limited to shopping days only. For space, only coefficients of direct interest are presented, with the full set of estimates in Appendix Table A1. While household size is not a strong predictor of how the calorie crunch reacts to EBT, the number

[20]TANF data are from the U.S. Department of Health and Human Services Office of Family Assistance: https://www.acf.hhs.gov/ofa/programs/tanf/data-reports

[21]Some states also used EBT cards for TANF disbursements, so it is important that nothing sudden happens with TANF caseloads around EBT implementation.

of children in the household is. Prior to EBT, an additional child is correlated with a 33% larger decline in expenditures on shopping days over the benefit month (from column (4)). The implementation of EBT more than fully counteracts this correlation. In columns (3)-(6), the pre-EBT coefficient on the number of children is less precise than in column (2), however, the sum of the number of children and household size coefficients is always statistically significant ($p < 0.01$ in all cases). While the household size interacted with EBT coefficient is negative, the sum of that coefficient and the number of children interacted with EBT coefficient is always positive and statistically significant ($p < 0.01$ in all cases). For a household with one adult and two children, EBT flattens the slope of the calorie crunch from \$3.71 per day to \$2.81 per day (estimates from column (4), all else besides household size and the number of children held equal). Put into a more tangible metric, EBT increases the amount spent in a shopping trip at the beginning of week four of the benefit month by \$19, mitigating roughly 25% of the calorie crunch.[22]

Specifications with alternative dependent variables are presented in the Appendix: household per-capita expenditures in Appendix Table A2, per-SNAP \$ expenditures in Appendix Table A3 and log expenditures in Appendix Table A4. The positive, precise coefficient on the interaction of EBT and the number of children is robust across all models, while the corresponding negative pre-EBT coefficient is less so. I return to this issue using consumption data in Section 3.3.

In Table 4 there is a substantial but imprecise negative level effect of EBT in columns (2)-(6). It is not robust to alternative specifications of the dependent variable; EBT actually has a positive level impact in the case of expenditures per SNAP \$. Nonetheless, the correct interpretation of the results in Table 4 is that a notable positive impact of EBT was experienced mainly for households with two or more children. For the average household (3.32 members, 1.62 children), the impact of EBT is to flatten the slope of calorie crunch by \$0.71 per day ($p = 0.04$, 21% of the pre-EBT slope).[23] Figure 3 presents a non-parametric comparison of the impact of EBT on single adults versus single parents with two children. In Panel A the moderate calorie crunch for single adults is the same before and with EBT. The level effects of EBT are not statistically significant. In Panel B, the very pronounced calorie crunch for single parents with two children before EBT is replaced

[22]This assumes no level effect off EBT because benefit amounts did not change. I find a level increase of \$2.14 (S.E. = 1.82, clustered at the state level) of eligible expenditures due to EBT.

[23]Estimates from column (4) of Table 4. When calculating this estimate, it is important to recall that one is subtracted from the household size variable in the regressions.

Table 4: Impact of EBT on Food Expenditure Cycles, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-2.87***	-1.58***	-1.43**	-2.06***	-2.05**	-2.65***
	(0.38)	(0.42)	(0.54)	(0.76)	(0.80)	(0.79)
t X EBT	0.41	-0.71*	-0.58	-0.42	-1.10	-1.03
	(0.28)	(0.37)	(0.57)	(0.71)	(0.68)	(0.76)
t X # children		-0.70***	-0.61	-0.68*	-0.55	-0.46
		(0.17)	(0.41)	(0.39)	(0.39)	(0.40)
t X # children X EBT		0.62***	1.16***	1.34***	1.35***	1.49***
		(0.19)	(0.43)	(0.41)	(0.48)	(0.53)
t X HH size			-0.17	-0.11	-0.22	-0.27
			(0.42)	(0.41)	(0.40)	(0.42)
t X HH size X EBT			-0.31	-0.44	-0.46	-0.40
			(0.47)	(0.46)	(0.47)	(0.48)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. I subtract one from household size. A weekend indicator, week of month trend and diary week indicator are included as day-specific controls in all specifications. SNAP benefit amount and household income interacted with t, and their triple interactions with EBT as well are included as controls in columns (3)-(6). Full results available in Appendix Table A1.

with steady spending for the first two weeks of the benefit month and then a smaller decline. For this type of household, the level effect of EBT is positive and marginally statistically significant ($p = 0.06$) over weeks 2-4 of the benefit month. It is negative and not significant at conventional levels ($p = 0.13$) in the first week.[24]

Appendix Tables A5 and A6 present results for the extensive margin impact of EBT on food shopping and the total effect on expenditures. There is no evidence that EBT affected anything but the time trend in the intensive margin –the amount purchased conditional on shopping. Appendix Table A7 disaggregates the number of children into the number by age group: pre-school age (0-5) primary/middle school age (6-12) and teenagers (13-17). Qualitatively, there are similar patterns for each group. However, the effects appear most strongly for primary/middle school age children.

[24] Estimates are obtained from random effects regressions of food expenditure, conditional on shopping, on EBT, limited to the relevant benefit week and household type. There are no control variables in this specification; it is meant as a direct test of the data in the figure as a contrast with the more structured approach I take elsewhere. The need for fixed effects to reduce specification error is not present when t is not an independent variable.

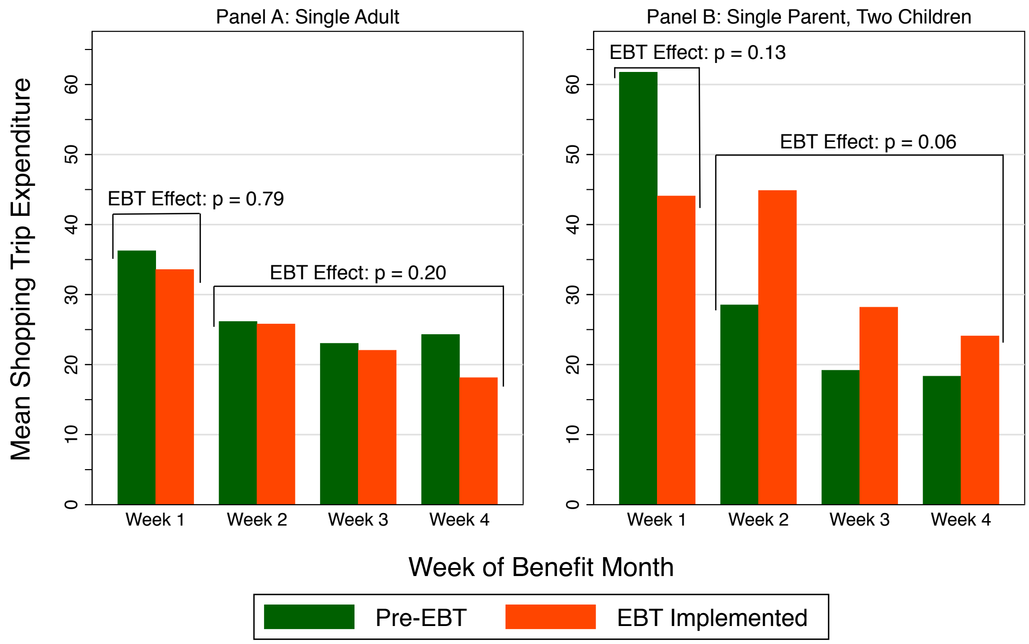

Figure 3: Impact of EBT on Food Expenditure by Week of Benefit Month, Shopping Days Only

Appendix Table A8 shows that the heterogeneous impact of EBT is slightly larger in magnitude when the data from during EBT rollout are excluded.

3.3 Robustness

An important concern with the analysis of any state-by-state program implementation is whether states selected in based on conditions related to the variables of interest. I argue that this study is not at high risk for this problem. First and foremost, the implementation of EBT was mandated by the federal government as a part of the 1996 welfare reform legislation. As shown in Table 2, the majority of states in the sample implemented EBT after that mandate was passed. Bednar (2011) shows that implementation dates for both voluntary and mandatory adopters cannot be predicted using 1990 state characteristics. Second, I observe nearly every state both before and after the implementation of EBT. I re-estimate the main specifications in Table 4 with all voluntary adoption states excluded, and states I only observe with or without EBT excluded. This removes the voluntary adopters of Maryland, Texas, South Carolina and Utah, and the late adopters of Iowa and California.[25] Results are presented in Appendix Table A9 and are similar to those in Table 4: the main result is not driven by states that voluntarily adopted EBT.

The most substantial threat to identification is the changing nature of households enrolled in SNAP over time. As a part of its 1993 budget, Congress authorized the Mickey Leland Childhood Hunger Relief Act. Its main impact was to expand SNAP by adding and increasing deductions that households could use to determine eligibility.[26] As a result, the period prior to the 1996 welfare reform act is a period of relative program generosity with large enrollment. The Personal Responsibility and Work Opportunities Reconciliation Act of 1996 introduced new restrictions. A time limit of three months of benefits within any three-year period was imposed for able-bodied adults without dependents (ABAWDs) with less than 20 hours per week of work, and states were given more power to disqualify SNAP recipients based on disqualification from other assistance programs.[27] These changes to the program came during a time of falling enrollment due to economic factors. From a high of 27.5 million households in 1994, enrollment fell to 17.2 million households in 2000.[28] However, almost immediately following the passage of welfare reform, Congress began to undo some of its changes. By 1998, states were allowed to exempt 15% of the excluded underemployed ABAWDs.[29] The 2001 agriculture appropriations bill increased the shelter deduction cap and pegged it to inflation moving forward. The Food Security and Rural Investment Act of 2002 offered an expanded standard deduction for larger households and indexed it to inflation, in addition to restoring eligibility for some non-citizen households.[30] Enrollment grew to 21.3 million in 2003 at the end of my sample period.

[25] Arkansas and Idaho also removed from this sample, but due to small samples that all happen to fall before EBT (in the case of Arkansas) or after implementation (in the case of Idaho).

[26] Most notably, the cap on deductions for excess (above half of income) shelter expenses was scheduled for gradual elimination, Earned Income Tax Credit receipts in the previous year were made deductible, the age limit for the deduction of students' income was raised to 21, child support payments were made deductible, and certain vehicles were excluded from the asset test. https://www.govtrack.us/congress/bills/103/hr529

[27] Other reductions in benefits/eligibility included: reducing the cost of living adjustments for benefits, freezing the growth some important deductions (including the shelter deduction cap), and removing legal immigrants from eligibility (https://www.govtrack.us/congress/bills/104/hr3734). School meal programs and the Women, Infants and Children (WIC) program were not altered by welfare reform, although WIC eligibility is tied to SNAP eligibility. The use of EBT for WIC did not begin long after the sample period with the exception of Wyoming in 2002 (http://www.fns.usda.gov/wic/wic-ebt-activities). Some changes were made to these programs in the William F. Goodling Child Nutrition Reauthorization Act of 1998, which made more federal money available for after-school program meals and snacks (Martin and Oakley, 2008).

[28] http://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap

[29] http://www.fns.usda.gov/snap/short-history-snap

[30] https://www.govtrack.us/congress/bills/107/hr2646

With so many changes occurring throughout the sample period, and a range of EBT implementation dates, it is hard to simply characterize which compositional shifts are most strongly correlated with EBT.[31] However, the 1996 welfare reform that mandated EBT is by far the most notable policy change during the sample, and there is work on the impact of welfare reform on SNAP caseloads. Both Wallace and Blank (1999) and Ziliak et al. (2000) find that falling enrollment in the period after welfare reform was most substantially driven by strong economic growth rather than legislative changes. Ziliak et al. (2000) conclude that "the major policy changes affecting the Food Stamp Program (that is, introduction of EBT and ABAWD waivers) did not appear to have major effects on the food stamp caseload." (p. 636).

Regardless of the source of compositional shifts, the large, non-linear fluctuations in SNAP enrollment over the sample demand caution. While the estimates of the heterogeneous impact of EBT in the previous section survive year and state fixed effects, and state specific linear time trends, state-specific non-linearities in the unobservable characteristics of participating households could be problematic. I take an event-study approach in Figure 4 that puts no restrictions on the shape of time trends. The time trend of expenditures over the benefit month (the t coefficient), and the correlation between the number of children and the time trend (the t X # children coefficient) appear on the vertical axes. Panels A and B show a five-year window before the month of EBT inception and after EBT completion, with data pooled over twelve month periods. Panels C and D zoom in to show a 2.5-year window, with data pooled over six month periods. In each time period I regress food shopping expenditures on days since benefit disbursement and its interaction with the number of children in the household.[32] Panels A and C present the t coefficients from these regressions, and Panels B and D present the t X # children coefficients. I also show the 95% confidence intervals, and the coefficients from regressions pooled across the periods after EBT completion, during the median EBT rollout time of roughly one year, and prior to the start of median EBT rollout.[33] The change in the correlation between the number of children and the calorie crunch begins during EBT implementation and then holds steady in the post period. Isolating the closest

[31] This is further complicated by the fact that states implemented other aspects of welfare reform at different points in time, not coincident with EBT.

[32] These regressions contain household fixed effects, and control variables for the interaction between household size and the expenditure trend, a weekend indicator, a week of month trend and a diary week variable. Standard errors are clustered at the state level.

[33] Because the programs were rolled out starting with operational pilots and then expanded, many households were using the new technology well prior to the date of reported statewide completion.

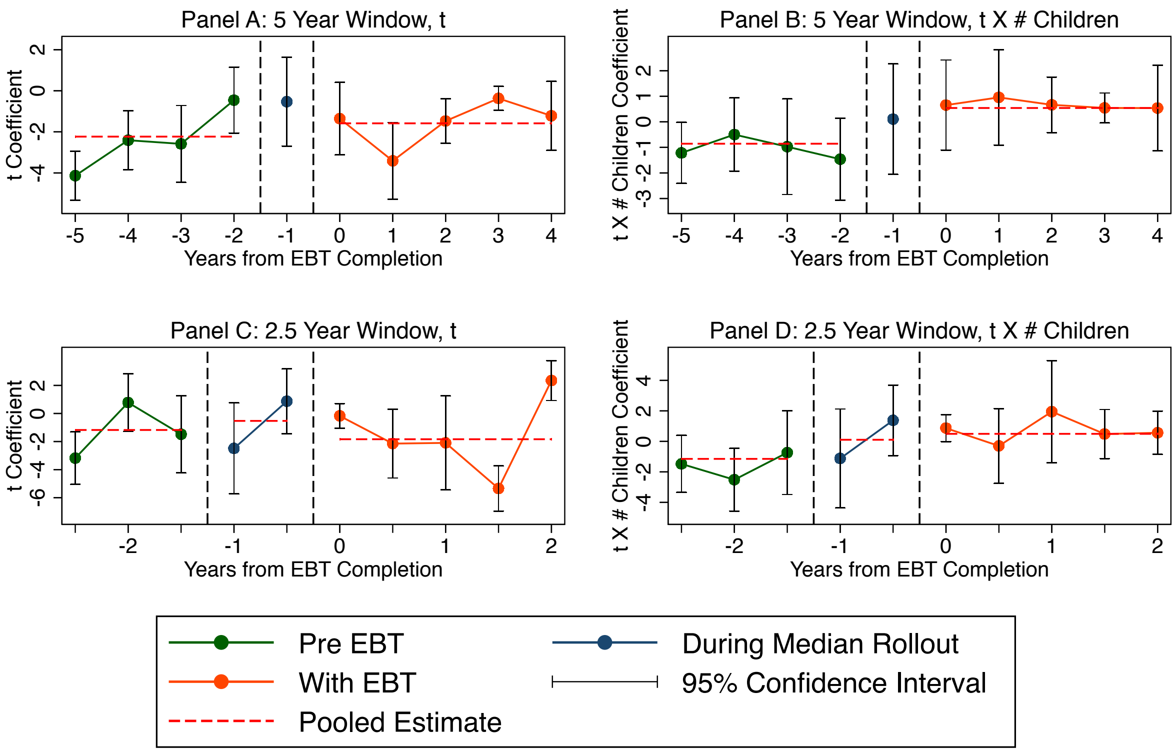

Figure 4: Event Study Analysis of the Impact of EBT

pre- and post-periods, the change from 24-12 months prior to EBT to the first twelve months with EBT completed in Panel B is statistically significant ($p = 0.04$), while change from from 18-12 months prior to EBT to the first six months with EBT completed in Panel D is not ($p = 0.14$). The t coefficient is noisy, especially in the six-month period data in Panel C. However, there are no clear or significant changes that coincide with EBT rollout or completion. Neither the change from 24-12 months prior to EBT to the first twelve months with EBT completed in Panel A, nor the change from from 18-12 months prior to EBT to the first six months with EBT completed in Panel C, is statistically significant ($p = 0.32$, and $p = 0.27$, respectively).

While the estimates in Figure 4 come from a sparse specification, I take a second event-study style approach based on the main specifications presented in Table 4. I include interaction terms between t, year fixed effects, and the number of children in the household. This yields exclusively within-year estimates of the heterogeneous impact of the calorie crunch, which offer additional robustness to heterogeneous impacts of welfare reform. The estimates of both the pre-EBT heterogeneity and the heterogeneous impact of EBT by the number of children are slightly larger (as are the imprecise negative level impacts of EBT) when these terms are included. Results are presented in Appendix Table A10.

3.4 Consumption

Even if EBT changed expenditure patterns for some households, that does not necessarily imply that consumption patterns changed as well. For example, Aguiar and Hurst (2005) find that households smooth consumption, but not expenditure, through the anticipated income change at retirement. There are a number of reasons why I argue that EBT likely had a heterogeneous impact on consumption in addition to expenditures. First, when I limit my sample to include expenditures on perishable foods only, I find similar results (see Appendix Table A11).[34] Second, previous literature has identified both consumption and expenditure cycles amongst SNAP participants, and Kuhn (2017) shows that these are correlated within households. Third, I use the 1989-1991 Continuing Survey of Food Intake by Individuals (CSFII) to show that prior to EBT, the heterogeneity in the consumption calorie crunch matches the heterogeneity in the expenditure calorie crunch identified in Section 3.2.[35]

I modify equations (1) and (2) slightly to estimate the consumption calorie crunch. I replace e_{it} with c_{it}, a household's total caloric consumption on day since SNAP disbursement t. Because the CSFII only offers 3 days of observation for a household (as opposed to 14 in the CES), purely within-household estimates of the time trend access a very limited fraction of the variance in t. I therefore adopt a random-effects specification, and add day-of-month fixed effects. Household size, SNAP benefit amount, household income, and their interactions with t remain as control variables. I add household Women, Infants and Children (WIC) supplemental SNAP participation and the number of children getting free or reduced-price school meals as control variables.

The sample construction is similar to that in the CES, with a couple exceptions. While the data contain a report of the overall household size, not all members contribute diaries, and there is limited information on the characteristics of those who do not. Therefore, I restrict my attention to households with a consistent number of diaries each day and the same age profile across survey

[34] I classify fresh fruits and vegetables, non-frozen dairy items and non-frozen meat/seafood as perishables.

[35] The 1994-1996, 1998 wave of the CSFII did not record the date of SNAP arrival, and the survey was then discontinued. This means I cannot estimate the impact of EBT with these data.

Table 5: Pre-EBT Consumption Trend Heterogeneity

Dep. Var.:	kCal			ln(kCal)		
	(1)	(2)	(3)	(4)	(5)	(6)
t	-31.276**	-8.948	-13.183	-0.007**	-0.001	-0.004
	(12.463)	(8.646)	(19.465)	(0.004)	(0.003)	(0.005)
# children		1822.988***	1461.716***		0.476***	0.436***
		(124.166)	(183.696)		(0.028)	(0.046)
t X # children		-10.823	-22.601**		-0.004**	-0.008***
		(6.631)	(11.393)		(0.002)	(0.003)
HH size			66.767			-0.027
			(152.448)			(0.043)
t X HH size			5.477			0.002
			(9.827)			(0.002)
Day of Month FE	Y	Y	Y	Y	Y	Y
Clusters	757	757	757	757	757	757
N	1864	1864	1864	1864	1864	1864

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the household level. All specifications feature household random effects and day-of-month fixed effects, with the first of the month excluded. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. I subtract one from household size. Columns (3) and (6) feature controls for SNAP benefit amount, household income, WIC participation, and free/reduced price school breakfast and lunch participation and all of their interactions with t. Full results available in Appendix Table A12.

days. Additionally, there are some households with a consistent number of diaries each day, but that contain days with a reported zero for caloric intake. I exclude these observations. I follow the convention of only including observations that occur in the four weeks following a reported SNAP disbursement and avoid inferring the the date of other disbursements. This leaves me with 757 households and 1864 household-days with full information. Results for the coefficients of interest are presented in Table 5 for both the level and log of household caloric consumption. The full list of coefficient estimates is in Appendix Table A12.

Columns (1) and (4) show a significant calorie crunch for all households pooled: a decline of roughly 31 kCal, or 0.7%, per-day. By the end of the fourth week of the benefit month, caloric consumption is more than 800 kCal lower than on day zero, which is between one-third and one-half of the USDA-recommended daily caloric intake for a typical adult.[36] The cumulative size of the calorie crunch over the entire benefit month, measured as a difference from day-of-disbursement-consumption, is thus very large.[37] This pooled estimate masks heterogeneity by the number of

[36]https://www.cnpp.usda.gov/USDAFoodPatterns

[37]Because these estimates are linear fitted values, bingeing behavior on day zero is not solely responsible for the large magnitudes.

children in the household. In fact, the un-interacted t coefficient estimates in columns (2), (3), (5) and (6) are not significantly different from zero. As in the CES prior to EBT, having more children predicts a more severe calorie crunch. In column (6), the sum of the household size and number of children coefficients is significantly from zero ($p = 0.04$), although this is not true in the equivalent level specification in column (3) ($p = 0.17$).

An additional piece of evidence that these changes over the course of the benefit month are unplanned and suboptimal is that households' reports of food security also show evidence of a decline over the course of the benefit month. I use an ordered-logit specification to model responses to a food-sufficiency question in the CSFII.[38] The likelihood of reporting "enough of the kind of foods we want to eat" falls by 0.6 percentage points per day ($p = 0.01$). The likelihoods of reporting "enough but not always what we want to eat", "sometimes not enough to eat" and "often not enough to eat" grow by 0.3, 0.2 and 0.1 percentage points per day, respectively ($p = 0.01$, 0.01 and 0.02, respectively).

4 Potential Explanations

In this section, I explore potential explanations for the heterogeneous impact of EBT. Two of the explanations come from the design goals of the EBT program: reduced stigma and increased benefit security. The other three are motivated by recent work in behavioral economics on pitfalls in household budgeting: household bargaining and discounting, time poverty and decision-making quality, and imperfect salience.

4.1 Stigma and Security

I group stigma and security because they make similar primary predictions about how EBT should impact spending over the benefit cycle. If there is stigma associated with using visually identifiable food coupons, SNAP users with coupons should try to minimize the number of SNAP-financed

[38]There is only one observation per household, on the first day of the survey. I include a weekend indicator variable and day-of-month fixed effect variables. Standard errors are heteroskedasticity robust. The coefficient estimates from the regression represent the impact of the regressors on a latent variable that determines food security reports. The coefficient on t is 0.03 (S.E. = 0.01) and the coefficient on the weekend indicator variable is -0.10 (S.E. = 0.17). Estimates presented in the text are marginal effects of t on the probability of each outcome.

shopping trips they take each month. EBT allows them to spread expenditures out more evenly over more trips over the course of the month. Concerns about safely storing food coupons produces a similar incentive –spend them quickly after they arrive– that EBT mitigates. And perhaps households with more children experience more stigma or more concern about benefit theft.

The impact of EBT on the daily likelihood of SNAP-eligible spending is presented in Table 6. Coefficients of direct interest are presented, with the full estimates in Appendix Table A5. I estimate models identical to those in Table 4 with an indicator variable for $e_{it} \geq 1$ as the dependent variable. The likelihood of shopping does decline over the course of the month. However, EBT has no level impact on shopping behavior over the month, nor do I find any heterogeneous impact of EBT according to the number of children in the household. Additionally, EBT does not significantly reduce the fraction of households with children that quickly exhaust their benefits. This is important to test because households may always shop the same number of times each month, but based on stigma or theft concerns, choose to use all benefits during the first shopping trip. Appendix Table A13 shows an estimate of the heterogeneous impact of EBT on the fraction of households that have spent more on SNAP-eligible food (cumulatively) than their reported benefit amount for each day of the first week of the month. EBT appears to have no impact, heterogeneous or otherwise, on benefit exhaustion very early in the month.[39]

There are other reasons to be skeptical of these two explanations. Theoretically, heterogeneity in stigma and theft concerns could vary in either direction with the number of children in a household. Researchers have taken SNAP participation as one measure of stigma, but the literature on the impact of EBT on participation shows no clear effects (see Section 2.3). Currie and Grogger (2001) estimate a heterogeneous impact of EBT on participation and find enrollment gains only for rural households and married couples with no children. This suggests that having more children makes a household more deserving of assistance and less susceptible to stigma.

Reports of theft are very rare. A 1995 report from the Government Accounting Office stated that "losses to the [Food Stamp] Program due to counterfeiting of food stamp coupons and mail theft are not significant". 0.4% of the value of mailed benefits in 1993 was reported lost or stolen from the mail. In that year, only 79 criminal investigations were opened into the matter (Robinson

[39] To keep the sample constant for this exercise, I limit it to household I observe on every day of the first week of the benefit month.

Table 6: Impact of EBT on Food Expenditure Cycles, Likelihood of Shopping

	(1)	(2)	(3)	(4)	(5)	(6)
t	-0.033***	-0.033***	-0.035***	-0.032***	-0.032***	-0.034***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
t X EBT	0.001	0.000	0.001	0.005	0.005	0.007
	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)
t X # children		-0.000	-0.000	-0.000	0.000	0.001
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
t X # children X EBT		0.000	-0.000	-0.000	-0.000	-0.001
		(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
t X HH size			0.001	0.001	0.001	0.001
			(0.001)	(0.001)	(0.001)	(0.001)
t X HH size X EBT			-0.001	-0.001	-0.001	0.000
			(0.002)	(0.002)	(0.002)	(0.003)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	17,665	17,665	17,665	17,665	17,665	17,665

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. I subtract one from household size. A weekend indicator, week of month trend and diary week indicator are included as day-specific controls in all specifications. SNAP benefit amount and household income interacted with t, and their triple interactions with EBT as well are included as controls in columns (3)-(6). Full results available in Appendix Table A5.

et al., 1995). However, theft that encourages an expenditure cycle occurs after receipt, so mail theft is only informative insofar as it is indicative of food coupon theft in general. An analysis of the 1994-2003 National Crime Victimization Survey (NCVS) shows that among low-income households, more young children predicts less exposure to theft crimes, but more teenagers predicts more exposure. Despite these differences, I find qualitatively similar estimates of the heterogeneous impact of EBT by children of different age ranges (see Appendix Table A7). Fewer adults in a household predicts more theft concern, as proxied by security devices, yet single-adult households experience no EBT impact in the main specifications. Households with more children do appear more concerned about security. Results from the NCVS are in Appendix Table A14.

4.2 Time Poverty and Decision-making Quality

Budgeting SNAP funds to last a month can be a time-consuming, cognitively difficult task. Household decision makers have to consider current needs and income, and uncertain future needs and income. Having more children to care for and manage exacerbates this difficulty. Perhaps budgeting education through the SNAP-Ed program helped these households smooth their expenditures.

SNAP-Ed, the nutrition education component of SNAP, was first funded in 1992 on a voluntary state-by-state basis.[40] Some of the trainings and information they provide involves budgeting, with an emphasis on setting specific weekly goals for food.[41] By 2004, all states had some form of SNAP-Ed program. Unfortunately, the fully voluntary nature of SNAP-Ed take-up means that estimates of the program's impact are confounded by selection. With that caution in mind, I estimate the specification from column (4) of Table 4 with a control for the existence of state-level SNAP-Ed programs and its interaction with the number of children in a household.[42] Coefficients of interest are presented in column (1) of Table 7, with full results from these models are presented in Appendix Table A18. There is no evidence that the implementation of SNAP-Ed programs, rather than the implementation of EBT, explains the heterogeneous change in the calorie crunch I identify.[43]

[40] http://www.ers.usda.gov/topics/food-nutrition-assistance/supplemental-nutrition-assistance-program-(snap)/nutrition-education.aspx

[41] https://snaped.fns.usda.gov/resource-library/handouts-and-web-sites/meal-planning-shopping-and-budgeting

[42] Data are available from the USDA SNAP-Ed Connection program: https://snaped.fns.usda.gov/administration/funding-allocations.

[43] The same is true when I use the level of SNAP-Ed funding rather than the existence of SNAP-Ed, with results in column (3).

Table 7: Impact of EBT on Food Expenditure Cycles, Shopping Days Only, with SNAP-Ed

	(1)	(2)	(3)	(4)
t	-1.82**	-1.98**	-1.88**	-2.00**
	(0.75)	(0.80)	(0.76)	(0.80)
t X EBT	-0.31	0.18	-0.31	0.01
	(0.69)	(0.95)	(0.69)	(0.98)
t X # children	-0.82*	-0.71*	-0.80*	-0.72*
	(0.43)	(0.40)	(0.42)	(0.40)
t X # children X EBT	1.27***	0.40	1.28***	0.68
	(0.42)	(0.64)	(0.42)	(0.69)
t X Any SNAP-Ed	-0.38	-0.24		
	(0.48)	(0.62)		
t X Any SNAP-Ed X EBT		-0.66		
		(0.86)		
t X Any SNAP-Ed X # children	0.22	0.04		
	(0.33)	(0.35)		
t X Any SNAP-Ed X # children X EBT		0.95		
		(0.64)		
t X SNAP-Ed Funding ($ln(\$'17 + 1)$)			-0.01	-0.01
			(0.03)	(0.04)
t X SNAP-Ed Funding X EBT				-0.03
				(0.06)
t X SNAP-Ed Funding X # children			0.01	0.00
			(0.02)	(0.02)
t X SNAP-Ed Funding X # children X EBT				0.04
				(0.05)
t X HH size	-0.10	-0.11	-0.10	-0.11
	(0.40)	(0.40)	(0.40)	(0.40)
t X HH size X EBT	-0.45	-0.44	-0.45	-0.44
	(0.45)	(0.46)	(0.46)	(0.46)
Year FE	Y	Y	Y	Y
Clusters	41	41	41	41
N	5596	5596	5596	5596

** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least \$10. All specifications feature household fixed effects. 1994 is the excluded year. I subtract one from household size. A weekend indicator, week of month trend and diary week indicator are included as day-specific controls in all specifications. SNAP benefit amount and household income interacted with t, and their triple interactions with EBT as well are included as controls in columns (3)-(6). Full results available in Appendix Table A18.

I also consider the possibility that EBT and SNAP-Ed worked in conjunction. As funding for SNAP-Ed increased while states were implementing EBT, perhaps states used the point-of-contact established by EBT rollout to deploy educational interventions. For example, when EBT was first implemented in New Mexico, card pickup was accompanied by an in-person training session on the EBT system (Robinson et al., 1995). I again estimate the specification from column (4) of Table 4, this time including SNAP-Ed variables interacted with the number of children, EBT and both simultaneously. Results are in columns (2) and (4) of Table 7. The estimates shows a mitigated heterogeneous impact of EBT without SNAP-Ed, and in the case of column (2), that there may be a stronger effect of EBT in states with SNAP-Ed programs. However, the quadruple-interaction specifications produce imprecise estimates that are not statistically significant.

4.3 Household Bargaining and Discounting

Shapiro (2005) posits that the calorie crunch may be a result of present-biased time preferences: households over-consume in the present, justifying that action in the moment with a myopic plan to cut back in the near future. They fail to execute that plan, and over-consume again (Laibson, 1997; O'Donoghue and Rabin, 1999). Recent theoretical work on group discounting by Hertzberg (2012) and Jackson and Yariv (2014a) indicates that preference aggregation can cause present-biased behavior, even when no group members are present biased themselves.[44] In general, whenever members of the group have at least partially independent consumption streams, or different discount rates, non-dictatorial preference aggregation leads to Collective Present Bias (CPB). One implication of CPB is that changing the bargaining process within a household changes that household's discounting behavior. If EBT unified control over SNAP benefits within households, it could have had two effects on discounting: 1) discounting would become more representative of the primary recipient's preferences, and 2) CPB should diminish.[45]

There are a number of ways that the implementation of EBT may have strengthened the bargaining power of the primary SNAP recipient. With EBT, the primary recipient controls access to benefits with a PIN. The PIN can be changed quickly at their request.[46] Benefits arrive silently to the primary recipient's card each month, rather than in the mail. The primary recipient's information is featured on the EBT card. I consider a gender-based test of the impact of EBT on

[44] Others, including Bernheim (1999), Gollier and Zeckhauser (2005), Jamison and Jamison (2011) and Zuber (2011) allude to similar effects of preference aggregation. Jackson and Yariv (2014b) find laboratory evidence of this, and Schaner (2015, 2016) finds evidence using lab-in-the-field experiments with savings accounts in Kenya.

[45] Shapiro (2005) argues that the month-long benefit cycle is too short for any significant decay of consumption over time to be explained by high exponential discount rates. With that view, the entire calorie crunch is attributable to present-bias.

[46] Additional EBT cards for a household need to be authorized by the primary recipient.

bargaining power. At the end of my sample period in 2003, just over two-thirds of adult participants were women (Cunnyngham and Brown, 2003). Work in labor economics on non-unitary household behavior has focused on gender as an indicator for separate consumption streams and preference disagreement. A well known example is Lundberg et al. (1997), who observe that shifting child allowance transfers to women in the U.K. increased household expenses on women's and children's clothing.[47] Consistent with this evidence, I find that prior to EBT, the number of male adults in a household with a child under two years old is negatively correlated with expenditures on baby food. EBT counteracts this correlation. Results are in Appendix Table A15.[48]

None of these changes should matter for a single-individual household, which is consistent with the estimates in Table 4. However, I would expect these changes to matter for multiple-adult households, which is inconsistent with the estimates in Table 4. I consider two pathways by which these changes in bargaining power could interact with the number of children in a household. One potential pathway is direct: perhaps parents have strong disagreements with their children over spending at the grocery store, and making resources private helps parents win or avoid that fight. It may be very hard to say no to children asking for treats in the grocery store, especially at the beginning of the benefit month. At that time, food has been scarce for the past week or two. Before EBT the children may have been aware that the food stamps had just arrived in the mail. If this were the case, I would expect no impact –or at least a smaller impact– of EBT for the youngest children. Another pathway is through disagreements and coordination failure amongst adults over how much to indulge children's preferences. If this were the case, I would expect the presence of other adults to exacerbate the heterogeneous impact of EBT. At the intersection of both pathways, single-adult households with only very young children should be the least impacted by EBT.

I estimate the model from column (4) of Table 4 for different groups of households: (1) single-adult households with either no children or infants (children under two) only (2) all single-adult

[47] Especially in the low-income and developing context, a number of studies indicate different expenditures between men and women in the same household, depending on income recipient. Examples include Browning et al. (1994), Browning and Chiappori (1999), Duflo (2003), Bobonis (2009), Attanasio and Lechene (2011), Attanasio et al. (2012) and Wang (2012).

[48] Specifically, I report regressions of expenditures on baby food as a fraction of total SNAP-eligible expenditures on an EBT indicator, the number of male adults in a household, and their interaction. I control for household size, SNAP benefit amount, income and their interactions with EBT. I also include year fixed effects and controls for diary week, week of calendar month and a weekend indicator. WIC nutrition risk eligibility criteria were standardized across states by the USDA in 1999. In columns (3) and (4) I add WIC reform and WIC reform interacted with the number of male adults as control variables.

households, (3) multiple-adult households with no children or infants only, (4) all multiple-adult households, and (5) multiple-adult households with adult dependents.[49] The estimates are noisy given the demands of estimating the triple interaction of interest on small subsamples. Results are in Appendix Table A16. I find no heterogeneous impact of EBT for single-adult households with either no children or infants only. When older children or other adults are present in the household, the household exhibits a heterogeneous response to EBT based on the number of children. This is especially true if the other adults are themselves dependents. These results support the hypothesis that EBT impacted disagreements both amongst adults and between parents and (non-infant) children. On the other hand, the similar size of the pre-EBT correlation between the calorie crunch and the number of children is similar across household types.

If EBT matters for bargaining power within the household, it should also impact the types of items purchased. For example, EBT should decrease (increase) purchases of indulgent (repugnant) food for children. This is due to either their preferences or their allies' influence being minimized. I find suggestive evidence of this. In Table 8 I report regressions of junk food and vegetable purchases on the number of children in a household, EBT status, and their interaction. Junk expenditures consist of cookies, ice cream and candy, and vegetable expenditures consist of fresh vegetables and tomatos. In columns (1) and (2), the dependent variable is the value of items purchased in either category (in 2017 $). In columns (3) and (4), the dependent variable is an indicator variable for at least $1 in spending on either category. The sample is limited to shopping days. Household size, SNAP benefit amount, income and their interactions with EBT, year fixed effects, and the value of total expenditures on a day are also included in all specifications. Prior to EBT, having more children is correlated with more spending on junk food. EBT counteracts this correlation. Holding household size fixed, more children is associated with a lower likelihood of purchasing vegetables prior to EBT, but not with EBT in place.

4.4 Imperfect Salience of Benefit Arrival

Sahm et al. (2012) hypothesize that the reduced visibility of stimulus payments delivered as reduced withholdings led to a lower propensity to consume than equivalent payments delivered as a one-time check in the mail. Perhaps EBT made the moment of benefit arrival less salient, and led

[49] Adult children are not included in the number of children variable.

Table 8: Impact of EBT on Food Expenditure Types, Shopping Days Only

Dep. Var.:	Amount Spent ($ '17)		Any Spending	
Food Type:	Junk	Vegetables	Junk	Vegetables
	(1)	(2)	(3)	(4)
Total Grocery Spending (100s '17 $)	3.907***	2.978***	0.328***	0.371***
	(0.227)	(0.160)	(0.018)	(0.013)
EBT	0.136	-0.034	-0.004	-0.010
	(0.195)	(0.201)	(0.024)	(0.031)
# children	0.283**	-0.076	0.029**	-0.021*
	(0.137)	(0.104)	(0.013)	(0.012)
# children X EBT	-0.315*	0.012	-0.020	0.017
	(0.182)	(0.114)	(0.019)	(0.014)
HH size	-0.144	0.118	-0.017	0.018
	(0.104)	(0.071)	(0.010)	(0.011)
HH size X EBT	0.080	-0.008	0.008	-0.015
	(0.147)	(0.082)	(0.015)	(0.013)
Year FE	Y	Y	Y	Y
Clusters	41	41	41	41
N	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year. I subtract one from household size. A weekend indicator, week of month trend and diary week indicator are included as day-specific controls in all specifications. SNAP benefit amount and household income, and their interactions with EBT are included as well. Full results available in Appendix Table A17.

to a lower immediate propensity to consume. This might be more likely in households with kids because of the time and attention demands of parenting. There are a couple pieces of evidence against a general lack of salience. The first is anecdotal: midnight queues at grocery stores for SNAP disbursements are common according to (Maestri and Baertlein, 2009). In the article, the CFO of Wal-Mart is quoted as saying, "Once the clock strikes midnight and EBT cards are charged, you can see our results start to tick up." Additionally, even though benefits roll over across months, benefit exhaustion is rapid and near universal. J.P. Morgan (which administered EBT in 20 states at the time of their report) reports that 85% of SNAP funds are depleted within three days of disbursement (Maestri and Baertlein, 2009). Finally, I find no evidence that EBT changes the likelihood of shopping on the first day of the benefit month, or that it does so in a way that depends on the number of children in a household. I regress a shopping indicator variable on an EBT indicator, the interaction between EBT and kids, with year and state fixed effects, state-specific linear time trends, and control variables for the weekend, week of month and week of diary. Results for select

Table 9: Impact of EBT on Shopping Likelihood at the Beginning on the Benefit Month

Sample Rest.:	$t = 0$			$t < 2$		
	(1)	(2)	(3)	(4)	(5)	(6)
EBT	0.15	-0.01	0.14	0.04	-0.07	0.01
	(0.10)	(0.08)	(0.16)	(0.08)	(0.05)	(0.11)
# kids		0.01	-0.02		-0.01	-0.02
		(0.04)	(0.05)		(0.03)	(0.03)
# kids X EBT		-0.00	0.03		-0.04	-0.01
		(0.07)	(0.09)		(0.05)	(0.05)
HH size		-0.01	0.01		0.03	0.03
		(0.03)	(0.04)		(0.02)	(0.03)
HH size X EBT		0.03	-0.01		0.05	0.03
		(0.06)	(0.08)		(0.05)	(0.06)
Year FE X t	N	N	Y	N	N	Y
State FE X t	N	N	Y	N	N	Y
State-Year Time Trend X t	N	N	Y	N	N	Y
Clusters	38	38	38	39	39	39
N	438	438	438	915	915	915

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first day of the benefit month in columns (1)-(3), and the first two days of the benefit month in columns (4)-(6). It is also limited to households with twelve or fewer members, and a reported SNAP disbursement of at least $10. I subtract one from household size. A weekend indicator, week of month trend and diary week indicator are included as day-specific controls in all specifications. SNAP benefit amount and household income, and their interactions with EBT are included as well. Full results available in Appendix Table A19.

coefficients are presented in Table 9, with full results in Appendix Table A19. The coefficient on EBT is generally positive and is never statistically different from zero across specifications. The coefficient on the interaction between EBT and the number of children is very close to zero. For the primary recipient at least, it appears unlikely that SNAP reduced the salience of benefit arrival.

On the other hand, EBT may have made benefit arrival less salient only for people other than the primary recipient. In this way, reduced benefit salience to non-decision-making individuals can be thought of as a part of both the bargaining mechanism. Quiet arrival is part of what allows for more unitary resource control, and may allow the decision maker more flexibility to make a budgeting plan before going shopping. This could have big benefits specifically for households with children, where the SNAP decision-maker likely faces substantial childcare stress. Mullainathan and Shafir (2013) argue that the time costs of being in poverty impede cognitive function and planning. As put by Shafir, “When so many moments of the day require your full attention, there’s very little of it left to worry about things that are not right in front of your eyes. ... You don’t anticipate things that are going to happen tomorrow." (Starecheski, 2014).

5 Conclusion

Failures to smooth monthly expenditures on food and caloric consumption were more severe for SNAP families with more children prior to EBT. EBT smoothed the expenditure profiles of those households. If EBT affected consumption as well, diminishing marginal utility implies that there were direct, positive welfare consequences of EBT from reallocating consumption. I perform a simple calibration exercise to determine for a single-parent with two children, how much larger would the pre-EBT food budget need to be to make up for the additional variance in expenditures? I take mean shopping trip expenditures for the first four weeks of the benefit month before EBT ($61, $29, $19, $18) and with EBT ($44, $45, $28, $24) and adjust them to sum to one in each policy period (these come from Panel B of Figure 3). I assume no discounting, and square-root utility over expenditures. I make this choice because Shapiro (2005) assumes log-utility over calories consumed in its model of consumption, but expenditures are more durable. I equate utility with EBT to utility before EBT, where each pre-EBT expenditure amount is multiplied by a common factor. Solving for the common factor yields 1.05. In other words, the rough model implies a welfare gain from purely the smoothing impact of EBT that is equivalent to increasing food expenditures in each pre-EBT weekly shopping trip by 5%.[50]

A second source of potential welfare gains from EBT are the indirect consequences of smoother consumption. As mentioned in the introduction, crime, health and education outcomes are all related to the SNAP cycle. Smoothing spending more effectively across the month, as EBT did for many households, could reduce the incidence of negative outcomes associated specifically with variance in consumption. For example, in states with two TANF disbursements per month, there is no spike in domestic violence upon benefit arrival (Hsu, 2017). Also, Missouri experienced an overall decrease in crime when it implemented EBT (Wright et al., 2014).

I explore potential mechanisms for the heterogeneous impact of EBT: stigma, theft reduction, household bargaining and discounting, time poverty and decision-making quality, and salience.

[50]Assuming log utility, consistent with Shapiro (2005) if all purchases are consumed in the week following the shopping trip, increases the common factor to 1.21 and the gain from smoothing to a 21% increase in the pre-EBT budget.

The first two mechanisms do not explain the data well. There was no pre-EBT heterogeneity or heterogeneous effects of EBT on the extensive margin of food shopping. Also, these explanations do not directly explain the heterogeneous consumption declines I find in the CSFII data. Though not definitive, I find some support for the bargaining and discounting, and time poverty and decision-making mechanisms. Some measures of revealed preferences over goods move in response to EBT: baby food, junk food and fresh vegetables. There is also no impact of EBT for single-individual households or households with only infant children (nor is there a heterogeneous impact by the number of infants). However, I do not see an impact of EBT for households with multiple adults, holding the number of children constant, which I would expect with this mechanism. The evidence for decision-making quality is weak: the interactive effect of SNAP-Ed and EBT is not statistically significant. The lack of an impact of EBT on shopping at the beginning of the benefit month is inconsistent with the salience mechanism. More research would be needed to pinpoint the exact mechanism behind the heterogeneous impact of EBT.

Even after the implementation of EBT, there remains a significant, homogeneous calorie crunch as measured by food expenditure in the CES. Todd (2015) and Kuhn (2017) show that this remains true for consumption in the EBT era. Other interventions are necessary if policy makers wish to further mitigate the calorie crunch and its consequences. Todd (2015) shows that increased benefit amounts from the American Recovery and Reinvestment Act reduced the severity of the calorie crunch. Parsons and Van Wesep (2013) suggest increasing disbursement frequency as a cost-neutral fix for volatile consumption. This paper suggests that efforts to empower and educate transfer recipients may also be an effective cost-neutral approach. Future work should examine the interaction between disbursement method and household structure for other types of income.

References

Mark Aguiar and Erik Hurst. Consumption versus expenditure. *Journal of Political Economy*, 113 (5):919–948, 2005.

Katherine Alaimo, Christine M. Olson, and Edward A. Frongillo Jr. Food insufficiency and american school-aged children's cognitive, academic and psychosocial development. *Pediatrics*, 108 (1):44–53, 2001.

Douglas Almond, Hilary W. Hoynes, and Diane Whitmore Schanzenbach. Inside the war on poverty: The impact of food stamps on birth outcomes. *Review of Economics and Statistics*, 93(2):387–403, 2011.

Joshua D. Angrist and Jörn-Steffen Pischke. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, 2009.

Sibel Atasoy, Bradford F. Mills, and Christopher F. Parmeter. The dynamics of food stamp program participation: A lagged dependent variable approach. Working Paper, 2010.

Orazio Attanasio and Valerie Lechene. Efficient responses to targeted cash transfers. Forthcoming, *Journal of Political Economy*, 2011.

Orazio Attanasio, Erich Battistin, and Alice Mesnard. Food and cash transfers: Evidence from colombia. *Economic Journal*, 122(559):92–124, 2012.

Steven Bednar. Would you like paper or plastic? food stamps disbursement method and take-up. Working Paper, 2011.

B. Douglas Bernheim. Comment on 'family bargaining and retirement behavior'. In Henry Aaron, editor, *Behavioral Economics and Retirement Policy*. Brookings Institution Press, 1999.

Jayanta Bhattacharya and Janet Currie. Youths at nutritional risk: Malnourished or misnourished. In Jonathan Gruber, editor, *Risky Behavior among Youths: An Economic Analysis*. University of Chicago Press, 2000.

Gustavo J. Bobonis. Is the allocation of resources within the household efficient? new evidence from a randomized experiment. *Journal of Political Economy*, 117(3):453–503, 2009.

Ann E. Bryant Borders, William A. Grobman, Laura B. Amsden, and Jane L. Holl. Chronic stress and low birth weight neonates in a low-income population of women. *Obstetrics and Gynecology*, 109(2):331–338, 2007.

Martin Browning and Pierre-André Chiappori. Efficient intra-household allocations: A general characterization and empirical tests. *Econometrica*, 66(6):1241–1278, 1999.

Martin Browning, François Bourguignon, Pierre-André Chiappori, and Lechene. Income and outcomes: A structural model of intrahousehold allocation. *Journal of Political Economy*, 102(6): 1067–1096, 1994.

Jillian B. Carr and Analisa Packham. Snap benefits and crime: Evidence from changing disbursement schedules. Working paper, 2017.

Laura Castner and Juliette Henke. Benefit redemption patterns in the supplemental nutrition assistance program. Technical report, USDA Food and Nutrition Service Office of Research and Analysis, 2011.

John Cook and Karen Jeng. Child food insecurity: The economic impact on our nation. Technical report, Feeding America, 2009.

John T. Cook, Deborah A. Frank, Carol Berkowitz, Maureen M. Black, Patrick H. Casey, Diana B. Cutts, Alan F. Meyers, Nieves Zaldivar, Anne Skalicky, Suzette Levenson Tim Heeren, and Mark Nord. Food insecurity is associated with adverse health outcomes among human infants and toddlers. *Journal of Nutrition*, 134(6):1432–1438, 2004.

Chad D. Cotti, John Gordanier, and Orgul D. Ozturk. When does it count? the timing of food stamp receipt and educational performance. Working paper, 2017.

Karen Cunnyngham and Beth Brown. Characteristics of food stamps households: Fiscal year 2003. Technical report, USDA Food and Nutrition Service Office of Analysis, Nutrition and Evaluation, 2003.

Janet Currie. U.s. food and nutrition programs. In Robert A. Moffitt, editor, *Means-Tested Transfer Programs in the United States*. University of Chicago Press, 2003.

Janet Currie and Nancy Cole. Does participation in transfer programs during pregnancy improve birth weight? NBER Working Paper 3832, 1991.

Janet Currie and Jeffrey Grogger. Explaining recent declines in food stamp program participation. In William Gale and Janet Rothenberg-Pack, editors, *Brookings-Wharton Papers on Urban Affairs*. Brookings Institution Press, 2001.

Janet Currie and Enrico Moretti. Did the introduction of food stamps affect birth outcomes in california? In R. Schoeni, J. House, G. Kaplan, and H. Pollack, editors, *Making Americans Healthier: Social and Economic Policy as Health Policy*. Russell Sage Press, 2008.

Caroline Danielson and Jacob Alex Klerman. why did the food stamp caseload decline (and rise)? RAND Labor and Population Working Paper WR-386, 2006.

Barbara Devaney and Robert A. Moffitt. Dietary effects of the food stamp program. *American Journal of Agricultural Economics*, 73:202–211, 1991.

Carlos Dobkin and Steven L. Puller. The effects of government transfers on monthly cycles in drug abuse, hospitalization and mortality. *Journal of Public Economics*, 91:2137–2157, 2007.

Dallas D. Dotter. Breakfast at the desk: The impact of universal breakfast programs on academic performance. Working Paper, 2013.

Lise Dubois, Anna Farmer, Manon Girard, and Marion Porcherie. Family food insufficiency is related to overweight among preschoolers. *Social Science and Medicine*, 63(6):1503–1516, 2006.

Esther Duflo. Grandmothers and granddaughters: Old-age pensions and intrahousehold allocation in south africa. *World Bank Economic Review*, 17(1):1–25, 2003.

William N. Evans and Timothy J. Moore. Liquidity, activity, mortality. NBER Working Paper 15310, 2009.

C. Fritz Foley. Welfare payments and crime. *Review of Economics and Statistics*, 93(1):97–112, 2011.

Lisa Gennetian, Roopa Sephardi, Nathan Hess, Aaron Winn, and Robert George. Food stamp benefit cycles and student disciplinary infractions. Working Paper, 2015.

Christian Gollier and Richard Zeckhauser. Aggregation of heterogeneous time preferences. *Journal of Political Economy*, 113(4):878–896, 2005.

Justine Hastings and Ebonya Washington. The first of the month effect: Consumer behavior and store responses. *American Economic Journal: Economic Policy*, 2:142–162, 2010.

Andrew Hertzberg. Exponential individuals, hyperbolic households. Working Paper, 2012.

Larry L. Howard. Does food insecurity at home affect non-cognitive performance at school? a longitudinal analysis of elementary student classroom behavior. *Economics of Education Review*, 30(1):157–176, 2011.

Hilary W. Hoynes and Diane Whitmore Schanzenbach. Consumption responses to in-kind transfers: Evidence from the introduction of the food stamp program. *American Economic Journal: Applied Economics*, 1:109–139, 2009.

Hilary W. Hoynes, Diane Whitmore Schanzenbach, and Douglas Almond. Long run impacts of childhood access to the safety net. *American Economic Review*, 106(4):903–934, 2016.

Lin-Chi Hsu. The timing of welfare payments and intimate partner violence. *Economic Inquiry*, 55(2):1017–1031, 2017.

Institute of Medicine. *Dietary Risk Assessment in the WIC Program*. The National Academies Press, 2002.

Matthew O. Jackson and Leeat Yariv. Collective dynamic choice: The necessity of time inconsistency. *American Economic Journal: Microeconomics*, 2014a. Forthcoming.

Matthew O. Jackson and Leeat Yariv. Present bias and collective dynamic choice in the lab. *American Economic Review*, 104(12):4184–4204, 2014b.

Dean T. Jamison and Julian Jamison. Characterizing the amount of speed of discounting procedures. *Journal of Benefit-Cost Analysis*, 2(2), 2011.

Nader S. Kabbani and Parke E. Wilde. short recertif ication periods in the u.s. food stamp program. *Journal of Public Economics*, 38:1112–1138, 2003.

Neeraj Kaushal and Qin Gao. Food stamp program and consumption choices. In Michael Grossman and Naci H. Mocan, editors, *Economic Aspects of Obesity*. University of Chicago Press, 2011.

Janet C. King. The risk of maternal nutritional depletion and poor outcomes increases in early or closely spaced pregnancies. *Journal of Nutrition*, 133(5):1732S–1736S, 2003.

Robert Kornfeld. Explaining recent trends in food stamp program caseloads. Technical report, USDA Economic Research Service, 2002.

Michael A. Kuhn. Who feels the calorie crunch and why? the impact of school meals on the incidence of cyclical food insecurity. Working paper, 2017.

David Laibson. Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics*, 112 (2):443–477, 1997.

Shelly J. Lundberg, Robert A. Pollak, and Terence J. Wales. Do husbands and wives pool their resources? evidence from the united kingdom child benefit. *Journal of Human Resources*, 32 (3):463–480, 1997.

James Mabli, Jim Ohls, Lisa Dragoset, Laura Castner, and Betsy Santon. Measuring the effect of supplemental nutrition assistance program (snap) participation on food security. Technical report, USDA Food and Nutrition Service Office of Policy Support, 2013.

Nicole Maestri and Lisa Baertlein. Midnight in the food-stamp economy. *Reuters*, 2009.

Josephine Martin and Charlotte Oakley. *Managing Child Nutrition Programs: Leadership for Excellence*. Jones & Bartlett Learning, 2008.

Giovanni Mastrobuoni and Matthew Weinberg. Heterogeneity in intra-monthly consumption patterns, self-control, and savings at retirement. *American Economic Journal: Economic Policy*, 1: 163–189, 2009.

Singe-Mary McKernan and Caroline Ratcliffe. employment factors influencing food stamp program participation. Technical report, USDA Economic Research Service, 2003.

Gregory Mills, Tracy Vericker, Heather Koball, Laura Wheaton, Key Lippold, and Sam Elkin. Understanding the rates, causes, and costs of churning in the supplemental nutrition assistance program (snap). Technical report, Urban Institute, 2014. http://www.fns.usda.gov/sites/default/files/ops/SNAPChurning-Summary.pdf.

Sendhil Mullainathan and Eldar Shafir. *Scarcity: Why Having So Little Means So Much*. Times Books, 2013.

Smita Narula, Jesica Scholes, Matthew Simon, and Alyson Zureick. Nourishing change: Fulfilling the right to food in the united states. Technical report, New York University School of Law, 2013.

Ted O'Donoghue and Matthew Rabin. Doing it now or later. *American Economic Review*, 89(1): 103–124, 1999.

Christopher A. Parsons and Edward D. Van Wesep. The timing of pay. *Journal of Financial Economics*, 103(2):373–397, 2013.

Robert A. Robinson, James A. Fowler, James G. Cooksey, Frederick D. Berry, Sally S. Moino, Mary L. White, and Thomas L. Sipes. Food assistance: potential impacts of alternative systems for delivering food stamp program benefits. Technical report, Government Accountability Office, 1995.

Claudia R. Sahm, Matthew D. Shapiro, and Joel Slemrod. Check in the mail or more in the paycheck: Does the effectiveness of fiscal stimulus depend on how it is delivered? *American Economic Journal: Economic Policy*, 4(3):216–250, 2012.

Simone Schaner. Do opposites detract? intrahousehold preference heterogeneity and inefficient strategic savings. *American Economic Journal: Applied Economics*, 7(2):135–174, 2015.

Simone Schaner. The cost of convenience? transactions costs, bargaining power, and savings account use in kenya. Working paper, 2016.

Hilary K. Seligman, Barbara A Laraia, and Margot B. Bushel. Food insecurity is associated with chronic disease among low-income nhanes participants. *Journal of Nutrition*, 140(2):304–310, 2010.

Hilary K. Seligman, Ann F. Bolger, David Guzman, Andrea Lopez, and Kirsten Bibbins-Domingo. Exhaustion of food budgets at month's end and hospital admissions for hypoglycemia. *Health Affairs*, 33(1):116–123, 2014.

Jesse Shapiro. Is there a daily discount rate? evidence from the food stamp nutrition cycle. *Journal of Public Economics*, 89:303–325, 2005.

Travis A. Smith, Joshua P. Benning, Xiaosi Yang, Gergory Colson, and Jerrfey H. Dorfman. The effects of benefit timing and income fungibility on food purchasing. *American Journal of Agricultural Economics*, 98(2):564–580, 2016.

Laura Starecheski. This is your stressed-out brain on scarcity. http://www.npr.org/sections/health-shots/2014/07/14/330434597/this-is-your-stressed-out-brain-on-scarcity, July 2014.

Melvin Stephens, Jr. '3rd of tha month': Do social security recipients smooth consumption between checks? *American Economic Review*, 93:406–422, 2003.

Melvin Stephens, Jr. Paycheque receipt and the timing of consumption. *Economic Journal*, 116: 680–701, 2006.

Jessica E. Todd. Revisitng the supplemental nutrition assistance program cycle of food intake: Investigating heterogeneity, diet quality, and a large boost in benefit amounts. *Applied Economic Perspectives and Policy*, 37(3):437–458, 2015.

Geoffrey Wallace and Rebecca M. Blank. What goes up must come down? explaining recent changes in public assistance caseloads. In Sheldon H. Danziger, editor, *Economic Conditions and Welfare Reform*. W.E. Upjohn Institute for Employment Research, 1999.

Shing-Yi Wang. Property rights and intra-household bargaining. Working Paper, 2012.

Parke E. Wilde and Christine K. Ranney. The monthly food stamp cycle: Shopping frequency and food intake decisions in an endogenous switching regression framework. *American Journal of Agricultural Economics*, 82:200–213, 2000.

Joshua Winicki and Kyle Jemison. Food insecurity and hunger in the kindergarten classroom: Its effect on learning and growth. *Contemporary Economic Policy*, 21(2):145–157, 2003.

Jeffrey M. Wooldridge. *Econometric Analysis of Cross-Section and Panel Data*. MIT Press, 2002.

Ricahrd Wright, Erdal Tekin, Volkan Topalli, Chandler McClellan, Timothy Dickinson, and Richard Rosenfeld. Less cash, less crime: Evidence from the electronic benefit transfer program. NBER Working Paper No. 19996, 2014.

James P. Ziliak, Craig Gundersen, and David N. Figlio. The effects of the macroeconomy and welfare reform on food stamp caseloads. *American Journal of Agricultural Economics*, 82(3): 635–641, 2000.

Stéphane Zuber. The aggregation of preferences: Can we ignore the past? *Theory and Decision*, 70(3):367–384, 2011.

A Appendix for Online Publication

Table A1: Impact of EBT on Food Expenditure Cycles, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-2.87***	-1.58***	-1.43**	-2.06***	-2.05**	-2.65***
	(0.38)	(0.42)	(0.54)	(0.76)	(0.80)	(0.79)
t X EBT	0.41	-0.71*	-0.58	-0.42	-1.10	-1.03
	(0.28)	(0.37)	(0.57)	(0.71)	(0.68)	(0.76)
t X # children		-0.70***	-0.61	-0.68*	-0.55	-0.46
		(0.17)	(0.41)	(0.39)	(0.39)	(0.40)
t X # children X EBT		0.62***	1.16***	1.34***	1.35***	1.49***
		(0.19)	(0.43)	(0.41)	(0.48)	(0.53)
t X HH size			-0.17	-0.11	-0.22	-0.27
			(0.42)	(0.41)	(0.40)	(0.42)
t X HH size X EBT			-0.31	-0.44	-0.46	-0.40
			(0.47)	(0.46)	(0.47)	(0.48)
t X SNAP benefit (100s '17 $)			0.09	0.13	0.14	0.15
			(0.24)	(0.24)	(0.24)	(0.24)
t X SNAP benefit X EBT			-0.26	-0.32	-0.35	-0.52
			(0.30)	(0.29)	(0.31)	(0.35)
t X income (1000s '17 $)			0.02	0.02	0.02	0.02
			(0.02)	(0.02)	(0.02)	(0.02)
t X income X EBT			-0.01	-0.01	-0.01	-0.02
			(0.02)	(0.02)	(0.02)	(0.02)
Weekend	2.10	2.23*	2.29*	2.25*	2.07	1.96
	(1.30)	(1.27)	(1.30)	(1.33)	(1.36)	(1.40)
Week of month	-1.36*	-1.49*	-1.50*	-1.64**	-1.26*	-1.32*
	(0.73)	(0.75)	(0.76)	(0.78)	(0.71)	(0.75)
Diary week	13.88***	13.86***	13.80***	13.91***	14.11***	14.12***
	(2.75)	(2.76)	(2.76)	(2.78)	(2.79)	(2.82)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A2: Impact of EBT on Food Expenditure per Capita Cycles, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-0.86***	-0.87***	-0.91***	-1.15***	-1.28***	-1.36***
	(0.12)	(0.15)	(0.17)	(0.27)	(0.25)	(0.20)
t X EBT	0.15	0.07	0.14	0.08	-0.09	-0.07
	(0.10)	(0.20)	(0.25)	(0.23)	(0.28)	(0.30)
t X # children		0.00	-0.12	-0.15*	-0.11	-0.09
		(0.04)	(0.08)	(0.08)	(0.08)	(0.08)
t X # children X EBT		0.04	0.26**	0.33***	0.33***	0.35***
		(0.06)	(0.11)	(0.11)	(0.11)	(0.12)
t X HH size			0.09	0.11	0.09	0.07
			(0.07)	(0.07)	(0.08)	(0.07)
t X HH size X EBT			-0.15	-0.21*	-0.21*	-0.18
			(0.12)	(0.12)	(0.12)	(0.11)
t X SNAP benefit (100s '17 $)			0.04	0.06	0.06	0.06
			(0.05)	(0.05)	(0.05)	(0.05)
t X SNAP benefit X EBT			-0.08	-0.10	-0.09	-0.12*
			(0.06)	(0.06)	(0.06)	(0.07)
t X income (1000s '17 $)			0.00	0.00	0.00	0.00
			(0.00)	(0.00)	(0.00)	(0.00)
t X income X EBT			-0.00	-0.00	-0.00	-0.00
			(0.00)	(0.00)	(0.00)	(0.00)
Weekend	0.88*	0.88*	0.87*	0.86*	0.81	0.80
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.51)
Week of month	-0.36	-0.36	-0.35	-0.44*	-0.33	-0.36
	(0.22)	(0.22)	(0.21)	(0.22)	(0.21)	(0.22)
Diary week	4.09***	4.11***	4.12***	4.14***	4.20***	4.22***
	(0.87)	(0.86)	(0.86)	(0.86)	(0.88)	(0.90)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A3: Impact of EBT on Food Expenditure per SNAP $, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-0.021***	-0.022***	-0.021**	-0.031**	-0.029**	-0.022**
	(0.004)	(0.007)	(0.008)	(0.014)	(0.014)	(0.010)
t X EBT	0.009***	0.013**	0.014**	0.010*	0.006	0.002
	(0.003)	(0.006)	(0.007)	(0.006)	(0.005)	(0.007)
t X # children		0.001	-0.005*	-0.006*	-0.005	-0.004
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
t X # children X EBT		-0.002	0.008**	0.010**	0.008**	0.009**
		(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
t X HH size			0.002	0.003	0.001	0.000
			(0.003)	(0.003)	(0.003)	(0.003)
t X HH size X EBT			-0.006	-0.007*	-0.005	-0.006
			(0.004)	(0.004)	(0.004)	(0.005)
t X SNAP benefit (100s '17 $)			0.006***	0.006***	0.007***	0.007***
			(0.002)	(0.002)	(0.002)	(0.002)
t X SNAP benefit X EBT			-0.006**	-0.007**	-0.007**	-0.007**
			(0.002)	(0.003)	(0.003)	(0.003)
t X income (1000s '17 $)			0.000	0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)	(0.000)
t X income X EBT			-0.000	-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)	(0.000)
Weekend	0.008	0.007	0.008	0.007	0.006	0.008
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.015)
Week of month	-0.007	-0.007	-0.006	-0.009	-0.007	-0.009
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Diary week	0.103***	0.103***	0.102***	0.103***	0.104***	0.105***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A4: Impact of EBT on Log Food Expenditure Cycles, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-0.067***	-0.056***	-0.062***	-0.067***	-0.072***	-0.068***
	(0.010)	(0.010)	(0.010)	(0.015)	(0.015)	(0.012)
t X EBT	0.012*	0.004	0.017	0.012	-0.004	-0.007
	(0.007)	(0.013)	(0.014)	(0.016)	(0.019)	(0.020)
t X # children		-0.006*	-0.021**	-0.022**	-0.020**	-0.017*
		(0.003)	(0.008)	(0.008)	(0.009)	(0.009)
t X # children X EBT		0.005	0.030**	0.033***	0.033***	0.032**
		(0.004)	(0.011)	(0.011)	(0.012)	(0.013)
t X HH size			0.011*	0.012*	0.010	0.009
			(0.006)	(0.007)	(0.007)	(0.007)
t X HH size X EBT			-0.021**	-0.023**	-0.024**	-0.021**
			(0.010)	(0.010)	(0.010)	(0.010)
t X SNAP benefit (100s '17 $)			0.004	0.005	0.006	0.005
			(0.004)	(0.004)	(0.004)	(0.004)
t X SNAP benefit X EBT			-0.004	-0.004	-0.005	-0.006
			(0.005)	(0.005)	(0.005)	(0.006)
t X income (1000s '17 $)			0.000	0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)	(0.000)
t X income X EBT			-0.000	-0.000	0.000	-0.000
			(0.000)	(0.000)	(0.000)	(0.000)
Weekend	0.032	0.034	0.033	0.033	0.027	0.026
	(0.032)	(0.032)	(0.032)	(0.033)	(0.034)	(0.034)
Week of month	-0.033	-0.035	-0.035	-0.039*	-0.033	-0.032
	(0.020)	(0.021)	(0.022)	(0.022)	(0.021)	(0.021)
Diary week	0.345***	0.345***	0.346***	0.347***	0.350***	0.351***
	(0.067)	(0.067)	(0.067)	(0.066)	(0.068)	(0.068)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A5: Impact of EBT on Food Expenditure Cycles, Likelihood of Shopping

	(1)	(2)	(3)	(4)	(5)	(6)
t	-0.033***	-0.033***	-0.035***	-0.032***	-0.032***	-0.034***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
t X EBT	0.001	0.000	0.001	0.005	0.005	0.007
	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)
t X # children		-0.000	-0.000	-0.000	0.000	0.001
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
t X # children X EBT		0.000	-0.000	-0.000	-0.000	-0.001
		(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
t X HH size			0.001	0.001	0.001	0.001
			(0.001)	(0.001)	(0.001)	(0.001)
t X HH size X EBT			-0.001	-0.001	-0.001	0.000
			(0.002)	(0.002)	(0.002)	(0.003)
t X SNAP benefit (100s '17 $)			-0.002*	-0.002**	-0.002**	-0.002**
			(0.001)	(0.001)	(0.001)	(0.001)
t X SNAP benefit X EBT			0.002	0.002	0.002	0.002
			(0.001)	(0.001)	(0.001)	(0.001)
t X income (1000s '17 $)			0.000	0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)	(0.000)
t X income X EBT			0.000	0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)	(0.000)
Weekend	-0.018**	-0.018**	-0.018**	-0.018**	-0.018**	-0.018**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Week of month	-0.006	-0.006	-0.006	-0.006	-0.006	-0.006
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Diary week	0.206***	0.206***	0.206***	0.206***	0.206***	0.206***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	17,665	17,665	17,665	17,665	17,665	17,665

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A6: Impact of EBT on Food Expenditure Cycles, Total Effect

	(1)	(2)	(3)	(4)	(5)	(6)
t	-2.77***	-2.29***	-2.21***	-2.35***	-2.18***	-2.50***
	(0.19)	(0.20)	(0.22)	(0.31)	(0.38)	(0.33)
t X EBT	0.10	-0.08	0.02	0.20	0.16	0.22
	(0.10)	(0.13)	(0.21)	(0.26)	(0.25)	(0.27)
t X # children		-0.29***	-0.11	-0.12	-0.07	-0.03
		(0.07)	(0.15)	(0.14)	(0.14)	(0.14)
t X # children X EBT		0.10	0.28	0.29	0.26	0.17
		(0.10)	(0.21)	(0.20)	(0.22)	(0.22)
t X HH size			-0.14	-0.13	-0.19	-0.22
			(0.14)	(0.14)	(0.13)	(0.14)
t X HH size X EBT			-0.17	-0.18	-0.15	-0.07
			(0.20)	(0.19)	(0.20)	(0.20)
t X SNAP benefit (100s '17 $)			-0.06	-0.05	-0.05	-0.05
			(0.09)	(0.09)	(0.10)	(0.10)
t X SNAP benefit X EBT			0.01	-0.01	-0.01	-0.03
			(0.15)	(0.15)	(0.16)	(0.17)
t X income (1000s '17 $)			0.01	0.01	0.01	0.01
			(0.01)	(0.01)	(0.01)	(0.01)
t X income X EBT			-0.00	-0.00	-0.00	-0.00
			(0.01)	(0.01)	(0.01)	(0.01)
Weekend	0.19	0.20	0.20	0.21	0.20	0.19
	(0.65)	(0.65)	(0.65)	(0.66)	(0.65)	(0.66)
Week of month	-0.90**	-0.89**	-0.90**	-0.90**	-0.85**	-0.84**
	(0.35)	(0.36)	(0.37)	(0.37)	(0.37)	(0.38)
Diary week	16.47***	16.47***	16.46***	16.46***	16.48***	16.47***
	(1.35)	(1.35)	(1.35)	(1.35)	(1.35)	(1.35)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	17,665	17,665	17,665	17,665	17,665	17,665

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A7: Impact of EBT on Food Expenditure Cycles, Shopping Days Only, Child Age Groups

	(1)	(2)	(3)	(4)	(5)
t	-1.57***	-1.41**	-2.03**	-2.06**	-2.53***
	(0.42)	(0.55)	(0.76)	(0.82)	(0.82)
t X EBT	-0.67*	-0.58	-0.41	-1.09	-1.06
	(0.39)	(0.57)	(0.68)	(0.68)	(0.78)
t X # children under 6	-0.71**	-0.66	-0.71*	-0.56	-0.59
	(0.27)	(0.43)	(0.41)	(0.39)	(0.40)
t X # children 6-12	-0.80**	-0.71	-0.74	-0.67	-0.46
	(0.35)	(0.60)	(0.58)	(0.59)	(0.61)
t X # children 13-17	-0.52	-0.43	-0.55	-0.37	-0.32
	(0.36)	(0.41)	(0.38)	(0.42)	(0.46)
t X # children under 6 X EBT	0.57	1.14**	1.27**	1.17**	1.49**
	(0.42)	(0.54)	(0.51)	(0.56)	(0.59)
t X # children 6-12 X EBT	0.94**	1.36**	1.50***	1.62**	1.60**
	(0.39)	(0.58)	(0.54)	(0.60)	(0.66)
t X # children 13-17 X EBT	0.09	0.65	0.93	0.81	1.02
	(0.51)	(0.60)	(0.58)	(0.67)	(0.76)
t X HH size		-0.17	-0.11	-0.22	-0.28
		(0.43)	(0.42)	(0.42)	(0.43)
t X HH size X EBT		-0.28	-0.41	-0.42	-0.35
		(0.48)	(0.47)	(0.49)	(0.50)
t X SNAP benefit (100s '17 $)		0.10	0.14	0.15	0.16
		(0.23)	(0.23)	(0.23)	(0.23)
t X SNAP benefit X EBT		-0.25	-0.30	-0.32	-0.50
		(0.27)	(0.26)	(0.28)	(0.31)
t X income (1000s '17 $)		0.02	0.02	0.02	0.02
		(0.02)	(0.02)	(0.02)	(0.02)
t X income X EBT		-0.01	-0.01	-0.01	-0.02
		(0.02)	(0.02)	(0.02)	(0.02)
Weekend	2.22*	2.29*	2.24*	2.06	1.95
	(1.25)	(1.28)	(1.32)	(1.35)	(1.38)
Week of month	-1.48*	-1.48*	-1.62**	-1.24*	-1.30*
	(0.73)	(0.73)	(0.76)	(0.69)	(0.73)
Diary week	13.84***	13.80***	13.90***	14.12***	14.12***
	(2.76)	(2.77)	(2.78)	(2.80)	(2.83)
Year FE X t	N	N	Y	Y	Y
State FE X t	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	Y
Clusters	41	41	41	41	41
N	5596	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A8: Impact of EBT on Food Expenditure Cycles, EBT Rollout Data Excluded, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-2.86***	-1.33***	-1.36**	-1.87**	-1.75*	-2.51***
	(0.44)	(0.48)	(0.60)	(0.85)	(0.87)	(0.89)
t X EBT	0.55*	-0.78*	-0.38	0.02	-0.80	-0.05
	(0.32)	(0.44)	(0.61)	(0.80)	(0.99)	(1.23)
t X # kids		-0.81***	-0.80*	-0.87*	-0.79*	-0.78*
		(0.19)	(0.47)	(0.45)	(0.44)	(0.44)
t X # kids X EBT		0.71***	1.41**	1.65***	1.66***	1.85***
		(0.24)	(0.53)	(0.53)	(0.56)	(0.61)
t X HH size			-0.01	0.05	-0.06	-0.02
			(0.46)	(0.45)	(0.45)	(0.47)
t X HH size X EBT			-0.56	-0.72	-0.68	-0.75
			(0.53)	(0.53)	(0.55)	(0.56)
t X SNAP benefit (100s '17 $)			-0.02	0.02	0.04	0.05
			(0.29)	(0.29)	(0.30)	(0.30)
t X SNAP benefit X EBT			-0.15	-0.22	-0.26	-0.39
			(0.35)	(0.35)	(0.36)	(0.40)
t X income (1000s '17 $)			0.02	0.01	0.02	0.02
			(0.02)	(0.02)	(0.02)	(0.02)
t X income X EBT			-0.01	-0.01	-0.01	-0.01
			(0.02)	(0.02)	(0.03)	(0.03)
Weekend	1.39	1.46	1.53	1.51	1.34	1.14
	(1.46)	(1.40)	(1.43)	(1.47)	(1.51)	(1.52)
Week of month	-1.33*	-1.11	-1.17	-1.29	-0.99	-1.12
	(0.70)	(0.75)	(0.75)	(0.79)	(0.80)	(0.83)
Diary week	13.15***	13.00***	12.97***	13.08***	13.31***	13.33***
	(3.24)	(3.22)	(3.22)	(3.22)	(3.29)	(3.33)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	4780	4780	4780	4780	4780	4780

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0. Observations from during the rollout of EBT are excluded.

Table A9: Impact of EBT on Food Expenditure Cycles for Mandatory Adopting States, with Observed Policy Change, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-3.13***	-1.92***	-1.78**	-2.45**	1.40	0.84
	(0.47)	(0.49)	(0.68)	(1.00)	(1.70)	(2.18)
t X EBT	0.22	-0.72	-0.52	-0.24	-0.12	-0.16
	(0.35)	(0.45)	(0.72)	(0.85)	(0.91)	(1.02)
t X # children		-0.76***	-0.52	-0.56	-0.55	-0.57
		(0.21)	(0.54)	(0.53)	(0.54)	(0.53)
t X # children X EBT		0.63**	1.45**	1.49**	1.66**	2.01***
		(0.27)	(0.61)	(0.61)	(0.66)	(0.70)
t X HH size			-0.21	-0.18	-0.15	-0.09
			(0.55)	(0.53)	(0.53)	(0.53)
t X HH size X EBT			-0.54	-0.55	-0.68	-0.77
			(0.67)	(0.66)	(0.70)	(0.70)
t X SNAP benefit (100s '17 $)			-0.05	0.01	0.01	0.03
			(0.26)	(0.25)	(0.27)	(0.27)
t X SNAP benefit X EBT			-0.40	-0.46	-0.50	-0.72**
			(0.29)	(0.31)	(0.30)	(0.32)
t X income (1000s '17 $)			0.01	0.01	0.00	0.00
			(0.01)	(0.01)	(0.01)	(0.01)
t X income X EBT			-0.00	-0.00	0.00	-0.00
			(0.02)	(0.02)	(0.02)	(0.02)
Weekend	2.44	2.50	2.53	2.53	2.32	2.11
	(1.67)	(1.66)	(1.67)	(1.69)	(1.73)	(1.80)
Week of month	-1.34	-1.55	-1.56	-1.74	-1.43	-1.52
	(0.96)	(0.92)	(0.95)	(1.06)	(0.97)	(1.00)
Diary week	16.45***	16.41***	16.33***	16.43***	16.67***	16.70***
	(3.56)	(3.57)	(3.57)	(3.58)	(3.58)	(3.62)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	33	33	33	33	33	33
N	4081	4081	4081	4081	4081	4081

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and New York is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A10: Impact of EBT on Food Expenditure Cycles with Children-Year Fixed Effects, Shopping Days Only

	(1)	(2)	(3)	(4)
t	-1.40	-1.25	-1.06	-1.81*
	(0.87)	(0.93)	(1.04)	(1.07)
t X EBT	-0.93*	-0.94	-1.38*	-1.47*
	(0.50)	(0.74)	(0.78)	(0.83)
t X # kids	-1.07**	-1.18**	-1.06*	-0.88
	(0.41)	(0.51)	(0.54)	(0.57)
t X # kids X EBT	0.72**	1.68***	1.51**	1.67**
	(0.28)	(0.57)	(0.67)	(0.73)
t X HH size		-0.09	-0.24	-0.30
		(0.38)	(0.38)	(0.42)
t X HH size X EBT		-0.47	-0.43	-0.35
		(0.46)	(0.48)	(0.50)
t X SNAP benefit (100s ‘17 $)		0.19	0.21	0.20
		(0.21)	(0.22)	(0.22)
t X SNAP benefit X EBT		-0.43	-0.44	-0.61
		(0.30)	(0.33)	(0.37)
t X income (1000s ‘17 $)		0.02	0.02	0.02
		(0.02)	(0.02)	(0.02)
t X income X EBT		-0.01	-0.01	-0.02
		(0.02)	(0.02)	(0.02)
Weekend	2.26*	2.31*	2.13	2.00
	(1.29)	(1.31)	(1.35)	(1.40)
Week of month	-1.59**	-1.57*	-1.19	-1.24
	(0.77)	(0.78)	(0.71)	(0.74)
Diary week	14.02***	13.99***	14.19***	14.22***
	(2.79)	(2.79)	(2.80)	(2.84)
Year FE X t	Y	Y	Y	Y
# children X Year FE X t	Y	Y	Y	Y
State FE X t	N	N	Y	Y
State-Year Time Trend X t	N	N	N	Y
Clusters	41	41	41	41
N	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A11: Impact of EBT on Perishable Food Expenditure Cycles, Shopping Days Only

	(1)	(2)	(3)	(4)	(5)	(6)
t	-1.20***	-0.79***	-0.79***	-1.04***	-1.23***	-1.36***
	(0.21)	(0.23)	(0.27)	(0.34)	(0.39)	(0.40)
t X EBT	0.07	-0.15	-0.07	0.06	-0.23	-0.23
	(0.15)	(0.19)	(0.29)	(0.35)	(0.36)	(0.38)
t X # children		-0.22**	-0.32*	-0.35**	-0.27*	-0.25
		(0.08)	(0.16)	(0.15)	(0.16)	(0.17)
t X # children X EBT		0.13	0.53***	0.64***	0.63***	0.70***
		(0.10)	(0.17)	(0.16)	(0.18)	(0.19)
t X HH size			0.04	0.07	0.01	-0.02
			(0.16)	(0.15)	(0.15)	(0.16)
t X HH size X EBT			-0.24	-0.31	-0.33	-0.30
			(0.22)	(0.21)	(0.20)	(0.20)
t X SNAP benefit (100s '17 $)			0.06	0.08	0.08	0.10
			(0.12)	(0.11)	(0.12)	(0.11)
t X SNAP benefit X EBT			-0.18	-0.22	-0.22	-0.31*
			(0.15)	(0.14)	(0.15)	(0.16)
t X income (1000s '17 $)			0.00	0.00	0.01	0.01
			(0.01)	(0.01)	(0.01)	(0.01)
t X income X EBT			-0.00	-0.00	-0.00	-0.00
			(0.01)	(0.01)	(0.01)	(0.01)
Weekend	0.82	0.86	0.87	0.84	0.66	0.63
	(0.65)	(0.63)	(0.63)	(0.66)	(0.67)	(0.68)
Week of month	-0.57**	-0.61**	-0.60**	-0.63**	-0.39	-0.37
	(0.27)	(0.27)	(0.28)	(0.29)	(0.28)	(0.27)
Diary week	6.30***	6.26***	6.24***	6.30***	6.44***	6.49***
	(1.51)	(1.52)	(1.52)	(1.53)	(1.55)	(1.58)
Year FE X t	N	N	N	Y	Y	Y
State FE X t	N	N	N	N	Y	Y
State-Year Time Trend X t	N	N	N	N	N	Y
Clusters	41	41	41	41	41	41
N	5596	5596	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year and California is the excluded state. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0. Perishables include fresh fruits and vegetables, non-frozen dairy items and non-frozen meat/seafood.

Table A12: Pre-EBT Consumption Trend Heterogeneity

Dep. Var.:	kCal			ln(kCal)		
	(1)	(2)	(3)	(4)	(5)	(6)
t	-31.276**	-8.948	-13.183	-0.007**	-0.001	-0.004
	(12.463)	(8.646)	(19.465)	(0.004)	(0.003)	(0.005)
# children		1822.988***	1461.716***		0.476***	0.436***
		(124.166)	(183.696)		(0.028)	(0.046)
t X # children		-10.823	-22.601**		-0.004**	-0.008***
		(6.631)	(11.393)		(0.002)	(0.003)
HH size			66.767			-0.027
			(152.448)			(0.043)
t X HH size			5.477			0.002
			(9.827)			(0.002)
SNAP benefit (100s '17 $)			90.474			0.031
			(110.499)			(0.031)
t X SNAP benefit			-0.711			0.001
			(7.059)			(0.002)
t X income (1000s '17 $)			58.333***			0.012**
			(21.570)			(0.005)
t X income			-0.415			-0.000
			(1.494)			(0.000)
WIC participant			-44.550			-0.022
			(374.690)			(0.088)
t X WIC participant			14.088			0.011**
			(21.867)			(0.005)
# FRP breakfasts			-197.464			-0.052
			(342.417)			(0.061)
t X # FRP breakfasts			17.014			0.004
			(17.228)			(0.003)
# FRP lunches			484.131*			0.082
			(254.192)			(0.052)
t X # FRP lunches			0.223			0.000
			(15.202)			(0.003)
Weekend	-197.582**	-169.162**	-163.098**	-0.038*	-0.031	-0.029
	(78.646)	(72.684)	(72.000)	(0.021)	(0.020)	(0.020)
Day of Month FE	Y	Y	Y	Y	Y	Y
Clusters	757	757	757	757	757	757
N	1864	1864	1864	1864	1864	1864

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the household level. All specifications feature household random effects and day-of-month fixed effects, with the first of the month excluded. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000.

Table A13: Impact of EBT on SNAP Benefit Exhaustion, First Week of Benefit Month

Day of SNAP Mth:	0	1	2	3	4	5	6
EBT	0.06	0.06	0.07	0.05	0.04	0.05	0.00
	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.07)
# children	0.03	0.03	0.01	0.00	0.02	0.02	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
# children X EBT	-0.00	0.00	0.04	0.06	0.02	0.00	-0.01
	(0.02)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
HH size	-0.02*	-0.02	0.01	0.00	-0.00	0.02	0.01
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
HH size X EBT	-0.01	-0.02	-0.06	-0.05	-0.02	-0.02	0.00
	(0.02)	(0.02)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
SNAP benefit (100s '17 $)	-0.01	-0.03**	-0.05***	-0.04***	-0.06***	-0.07***	-0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
SNAP benefit X EBT	-0.00	0.01	0.02	0.00	-0.01	0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Income (1000s '17 $)	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Income X EBT	-0.00	-0.00	-0.00	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Weekend	-0.01	0.03	0.03	-0.03	-0.01	-0.02	0.03
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Week of month	-0.01	-0.01	-0.02	-0.00	-0.01	-0.02	-0.03
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Diary week	0.01	0.02	-0.01	-0.04	0.01	0.05	0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
Constant	0.05	0.08	0.11	0.14	0.15	0.11	0.21
	(0.04)	(0.06)	(0.06)	(0.07)	(0.09)	(0.10)	(0.11)
Year FE	Y	Y	Y	Y	Y	Y	Y
Clusters	38	38	38	38	38	38	38
N	327	327	327	327	327	327	327

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to households observed for every day of the first week of a benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. 1994 is the excluded year. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A14: Marginal Effects of Household Characteristics on Theft Risk and Concern

Dep. Var.:	Theft (or Attempted)	Cash Theft (or Attempted)	Any Security Devices
	(1)	(2)	(3)
# children under 13	-0.0043***	-0.0019***	0.0377***
	(0.0008)	(0.0003)	(0.0022)
# children under 13-17	0.0197***	0.0041***	0.0071**
	(0.0012)	(0.0004)	(0.0031)
HH size	0.0198***	0.0041***	-0.0476***
	(0.0006)	(0.0003)	(0.0016)
Constant	0.1048	0.0200	0.5209
	(0.0023)	(0.0010)	(0.0049)
Year FE	Y	Y	Y
Income FE	Y	Y	Y
Clusters	193,540	193,540	156,105
N	499,679	499,679	377,625

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$. Coefficients are marginal effects from Probit regressions. Standard errors are clustered at the household level, with observations at the household-quarter level. Households are surveyed for six consecutive quarters. 1994 is the excluded year. Income fixed effects are a set of indicator variables for annual income levels between $0 and $50,000. Less than $5000 is the excluded income group. The sample is limited to households with less than $50,000 in annual income twelve or fewer members. The security devices variable is not recorded after the year 2000, second quarter. All regressions are weighted according to the household-quarter weights provided by the NCVS.

Table A15: Impact of EBT on Baby Food as a Fraction of Total Food Expenditures for Households with at Least One Child under Two, Shopping Days Only

	(1)	(2)	(3)	(4)
EBT	0.29*	0.25	0.33**	0.29*
	(0.15)	(0.18)	(0.13)	(0.16)
# male adults	-0.19***	-0.19**	-0.21**	-0.20**
	(0.06)	(0.07)	(0.08)	(0.08)
# male adults X EBT	0.18*	0.23*	0.15*	0.19*
	(0.10)	(0.12)	(0.08)	(0.10)
HH size	-0.01	-0.00	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
HH size X EBT	-0.05	-0.04	-0.05	-0.05
	(0.03)	(0.04)	(0.03)	(0.03)
SNAP benefit (100s '17 $)		-0.00		-0.00
		(0.02)		(0.02)
SNAP benefit X EBT		-0.24		-0.24
		(0.19)		(0.19)
Income (1000s '17 $)		-0.00		-0.00
		(0.00)		(0.00)
Income X EBT		0.00		0.00
		(0.00)		(0.00)
WIC reform			-0.12	-0.11
			(0.15)	(0.15)
# male adults X WIC reform			0.07	0.06
			(0.10)	(0.10)
Weekend	0.00	0.01	0.00	0.01
	(0.06)	(0.07)	(0.06)	(0.07)
Week of month	-0.00	0.00	-0.00	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)
Diary week	0.06	0.06*	0.06	0.06*
	(0.04)	(0.03)	(0.04)	(0.04)
Year FE	Y	Y	Y	Y
Clusters	27	27	27	27
N	152	152	152	152

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least \$10. 1994 is the excluded year. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from \$200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from \$20,000. One household with negative income is adjusted to \$0. WIC reform occurred on April 1, 1999; state-specific nutrition risk criteria were replaced with partially standardized criteria (Institute of Medicine, 2002).

Table A16: Impact of EBT on Food Expenditure Cycles by Household Structure, Shopping Days Only

# adults:	1		> 1		
Sample Rest.	Infants Only	All children	Infants Only	All children	Adult Dependents
	(1)	(2)	(3)	(4)	(5)
t	-3.27*	-1.44	-3.38**	-2.79	-2.71
	(1.86)	(0.96)	(1.59)	(1.73)	(2.71)
t X EBT	0.50	-1.49	-1.36	0.01	0.59
	(1.35)	(1.21)	(2.01)	(1.37)	(2.13)
t X # children	-1.55	-1.14***	-1.73	-1.25	-1.73*
	(2.05)	(0.40)	(1.33)	(0.75)	(1.00)
t X # children X EBT	-0.28	1.25**	2.78	1.57*	3.37*
	(1.95)	(0.49)	(3.45)	(0.88)	(1.73)
t X HH size			1.39	0.37	0.08
			(0.82)	(0.80)	(0.85)
t X HH size X EBT			-0.65	-0.56	-0.78
			(1.02)	(0.85)	(1.03)
t X SNAP benefit (100s ‘17 $)	-0.02	0.69**	0.73*	-0.09	0.84*
	(0.35)	(0.26)	(0.42)	(0.31)	(0.45)
t X SNAP benefit X EBT	0.32	-0.41	-2.12***	-0.26	-1.47*
	(0.54)	(0.40)	(0.58)	(0.38)	(0.84)
t X income (1000s ‘17 $)	-0.08	0.02	-0.01	0.02	0.03
	(0.10)	(0.04)	(0.01)	(0.02)	(0.02)
t X income X EBT	0.07	-0.04	0.00	-0.01	-0.04
	(0.11)	(0.08)	(0.02)	(0.02)	(0.03)
Weekend	4.14*	3.31*	4.53*	1.41	9.20**
	(2.18)	(1.78)	(2.31)	(1.88)	(3.44)
Week of month	-0.16	-2.15**	-2.32**	-1.42	0.67
	(1.36)	(0.91)	(1.13)	(1.32)	(1.77)
Diary week	5.62	10.92***	13.42***	16.28***	31.84***
	(4.77)	(2.68)	(4.78)	(4.75)	(8.39)
Year FE X t	Y	Y	Y	Y	Y
Clusters	37	40	35	40	37
N	914	2529	884	3048	1030

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A17: Impact of EBT on Food Expenditure Types, Shopping Days Only

Dep. Var.:	Amount Spent ($ '17)		Any Spending	
Food Type:	Junk	Vegetables	Junk	Vegetables
	(1)	(2)	(3)	(4)
Total Grocery Spending (100s '17 $)	3.907***	2.978***	0.328***	0.371***
	(0.227)	(0.160)	(0.018)	(0.013)
EBT	0.136	-0.034	-0.004	-0.010
	(0.195)	(0.201)	(0.024)	(0.031)
# children	0.283**	-0.076	0.029**	-0.021*
	(0.137)	(0.104)	(0.013)	(0.012)
# children X EBT	-0.315*	0.012	-0.020	0.017
	(0.182)	(0.114)	(0.019)	(0.014)
HH size	-0.144	0.118	-0.017	0.018
	(0.104)	(0.071)	(0.010)	(0.011)
HH size X EBT	0.080	-0.008	0.008	-0.015
	(0.147)	(0.082)	(0.015)	(0.013)
SNAP benefit (100s '17 $)	-0.007	-0.097	-0.003	-0.015***
	(0.069)	(0.063)	(0.007)	(0.004)
SNAP benefit X EBT	0.081	-0.028	0.008	0.007
	(0.098)	(0.060)	(0.011)	(0.007)
Income (1000s '17 $)	0.004	0.005	0.000	0.001
	(0.005)	(0.004)	(0.000)	(0.001)
Income X EBT	0.002	0.000	0.000	-0.000
	(0.007)	(0.004)	(0.000)	(0.001)
Weekend	0.125	-0.047	0.014	-0.014
	(0.101)	(0.065)	(0.014)	(0.011)
Week of month	-0.010	0.014	-0.002	0.008
	(0.047)	(0.037)	(0.005)	(0.006)
Diary week	0.165	0.027	0.007	-0.004
	(0.102)	(0.057)	(0.010)	(0.011)
Year FE	Y	Y	Y	Y
Clusters	41	41	41	41
N	5596	5596	5596	5596

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A18: Impact of EBT on Food Expenditure Cycles, Shopping Days Only, with SNAP-Ed

	(1)	(2)	(3)	(4)
t	-1.82**	-1.98**	-1.88**	-2.00**
	(0.75)	(0.80)	(0.76)	(0.80)
t X EBT	-0.31	0.18	-0.31	0.01
	(0.69)	(0.95)	(0.69)	(0.98)
t X # children	-0.82*	-0.71*	-0.80*	-0.72*
	(0.43)	(0.40)	(0.42)	(0.40)
t X # children X EBT	1.27***	0.40	1.28***	0.68
	(0.42)	(0.64)	(0.42)	(0.69)
t X Any SNAP-Ed	-0.38	-0.24		
	(0.48)	(0.62)		
t X Any SNAP-Ed X EBT		-0.66		
		(0.86)		
t X Any SNAP-Ed X # children	0.22	0.04		
	(0.33)	(0.35)		
t X Any SNAP-Ed X # children X EBT		0.95		
		(0.64)		
t X SNAP-Ed Funding ($ln(\$'17 + 1)$)			-0.01	-0.01
			(0.03)	(0.04)
t X SNAP-Ed Funding X EBT				-0.03
				(0.06)
t X SNAP-Ed Funding X # children			0.01	0.00
			(0.02)	(0.02)
t X SNAP-Ed Funding X # children X EBT				0.04
				(0.05)
t X HH size	-0.10	-0.11	-0.10	-0.11
	(0.40)	(0.40)	(0.40)	(0.40)
t X HH size X EBT	-0.45	-0.44	-0.45	-0.44
	(0.45)	(0.46)	(0.46)	(0.46)
t X SNAP benefit (100s '17 $)	0.13	0.14	0.13	0.13
	(0.24)	(0.23)	(0.24)	(0.24)
t X SNAP benefit X EBT	-0.31	-0.27	-0.31	-0.27
	(0.28)	(0.28)	(0.28)	(0.28)
t X income (1000s '17 $)	0.02	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
t X income X EBT	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Weekend	2.24*	2.25*	2.24*	2.26*
	(1.32)	(1.33)	(1.33)	(1.33)
Week of month	-1.69**	-1.70**	-1.68**	-1.68**
	(0.80)	(0.80)	(0.80)	(0.80)
Diary week	13.91***	13.99***	13.89***	13.95***
	(2.78)	(2.77)	(2.78)	(2.77)
Year FE	Y	Y	Y	Y
Clusters	41	41	41	41
N	5596	5596	5596	5596

** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first four weeks of the benefit month, households with twelve or fewer members, and a reported SNAP disbursement of at least $10. All specifications feature household fixed effects. 1994 is the excluded year. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000. One household with negative income is adjusted to $0.

Table A19: Impact of EBT on Shopping Likelihood at the Beginning on the Benefit Month

Sample Rest.:	$t = 0$			$t < 2$		
	(1)	(2)	(3)	(4)	(5)	(6)
EBT	0.15	-0.01	0.14	0.04	-0.07	0.01
	(0.10)	(0.08)	(0.16)	(0.08)	(0.05)	(0.11)
# kids		0.01	-0.02		-0.01	-0.02
		(0.04)	(0.05)		(0.03)	(0.03)
# kids X EBT		-0.00	0.03		-0.04	-0.01
		(0.07)	(0.09)		(0.05)	(0.05)
HH size		-0.01	0.01		0.03	0.03
		(0.03)	(0.04)		(0.02)	(0.03)
HH size X EBT		0.03	-0.01		0.05	0.03
		(0.06)	(0.08)		(0.05)	(0.06)
SNAP benefit (100s '17 $)		0.01	0.02		0.01	0.03
		(0.02)	(0.02)		(0.02)	(0.02)
SNAP benefit X EBT		-0.01	-0.02		-0.02	-0.03
		(0.03)	(0.03)		(0.02)	(0.03)
Income (1000s '17 $)		0.00^{*}	0.00		0.00	-0.00
		(0.00)	(0.00)		(0.00)	(0.00)
Income X EBT		-0.01^{**}	-0.00		-0.00^{**}	-0.00^{*}
		(0.00)	(0.00)		(0.00)	(0.00)
Weekend	-0.10	-0.11^{**}	-0.10	-0.01	-0.02	-0.00
	(0.06)	(0.05)	(0.07)	(0.04)	(0.03)	(0.04)
Week of month	-0.04	-0.03	-0.04	-0.02	-0.03	-0.02
	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Diary week	0.15^{***}	0.15^{***}	0.13^{**}	0.11^{***}	0.12^{***}	0.10^{**}
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Year FE X t	N	N	Y	N	N	Y
State FE X t	N	N	Y	N	N	Y
State-Year Time Trend X t	N	N	Y	N	N	Y
Clusters	38	38	38	39	39	39
N	438	438	438	915	915	915

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors in parentheses beneath the estimates, are clustered at the state level. The sample is limited to the first day of the benefit month in columns (1)-(3), and the first two days of the benefit month in columns (4)-(6). It is also limited to households with twelve or fewer members, and a reported SNAP disbursement of at least $10. SNAP benefits are measured in hundreds of 2017 dollars, as a difference from $200. Household income is gross annual, and measured in thousands of 2017 dollars, as a difference from $20,000.