Curing the Calorie Crunch:
The Effect of EBT on Household Present-Bias

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Abstract

Intra-month cycles in household consumption and expenditures are often considered hallmarks of dynamically-inconsistent planning. I find that food-stamp households with more children and gender-balanced adult populations experience this phenomenon more severely prior to the introduction of Electronic Benefit Transfer (EBT), and that the introduction of EBT eliminates this gap. I propose an explanation based on collective dynamic inconsistency –present bias generated by the aggregation of differing time preferences within households– and that EBT affects this aggregation process by establishing more dictatorial control over the food stamp resources. This implies that policies that improve the property rights of a single recipient over a transfer disbursement and shortening the frequency of disbursement would help improve budgeting, especially for families with young children.

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1 Introduction

Following the recent economic crises, food security in the United States is at the forefront of welfare policy. The number of individuals classified as "food insecure" by the USDA[1] grew by 14 million (roughly 28%) from 2007 to 2011, with food stamp enrollment nearly doubling from 26 to 46 million.[2] Food Stamps, now known offically as the Supplementary Nutrition Assistance Program or SNAP, is the largest government program dedicated specifically to this issue. As of 2012, one in eleven Americans received SNAP benefits in any given month.[3] Economists have highlighted the program's impact on child health (see Almond, Hoynes, and Schanzenbach (2011) for a review of this literature), food insecurity (Bhattacharya and Currie, 2000) and many types of nutritional intake (Devaney and Moffitt, 1991).[4]

There is little definitive work on how the introduction of Electronic Benefit Transfer (EBT) has affected SNAP.[5] Previous efforts analyze data on the same monthly frequency as the transfer distribution, or longer. Given the nature of food necessities, it may be that the effects of EBT need to be examined on a daily basis. I do so, using the program implementation as a tool for testing testing a theory of non-unitary household behavior and its affect on time preference. As a motivating anecdote, consider this quotation reported in a 2013 International Human Rights Clinic study. It features Tiffany, a mother of three on food stamps with no labor income due to disability:

> 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. (IHRC p. 20)

This downward-sloping intra-month trend in food expenditures and consumption is termed the "calorie crunch". Reconciling it with a dynamic model of consumer behavior is troubling, yet it

[1] Prior to 2006, the USDA's blanket definition of food insecurity was, "consistent access to adequate food is limited by a lack of money and other resources at times during the year." In 2006, they introduced two levels of food insecurity, low food security ("reports of reduced quality, variety, or desirability of diet. Little or no indication of reduced food intake") and very low food security ("Reports of multiple indications of disrupted eating patterns and reduced food intake"). Classifications are made yearly, at the household level: http://www.ers.usda.gov/topics/food-nutrition-assistance/food-security-in-the-us.

[2] IHRC, p. 8.

[3] IHRC, p. 12

[4] For a more comprehensive review of food assistance programs generally, see Currie (2003).

[5] Currie and Grogger (2001) and Kaushal and Gao (2011) find some positive effects on enrollment. Atasoy, Mills, and Parmeter (2010) find that it decreased enrollment. Bednar (2011) finds no significant effect.

is observed frequently.[6] Why not budget the food stamps to achieve a steadier, but lower level of consumption throughout the entire month? Shapiro (2005) attributes this behavior to present-biased time preferences; decision makers over consume in the present, without internalizing that they will fail to resist the temptation to do so again in the future. More severe present bias means a more severe calorie crunch.

Groups can exhibit present bias even if none of the individuals within the group are, on their own, present-biased. There are a variety of mechanisms through which this "collective present bias" can occur, and I focus on the framework of Jackson and Yariv (2012a). They show that under very general conditions, heterogeneity in exponential discount rates generates collective present bias when preferences are aggregated. For example, parents and toddlers have very different time preferences. Similarly, aggregation through household bargaining can lead to intertemporal trades or resource scrambles that manifest as present-biased representative preferences. While they are unlikely to bargain directly, the parent is surely aware of their child's impatience. Thus, the temptation to appease that impatience is a feature of the parent's household time preference even if it isn't a part of their personal time preference. This model has testable implications for which types of households should be subject to the strongest bias, and how this comparative static should be affected by shocks to household property rights over resources. EBT, I argue, is such a shock. By unifying control over the food-stamp resources in a variety of ways, the EBT card it could reduce household present bias to the degree that it was generated through collective means.

Specifically, I build on the empirical work of Shapiro (2005), by exploring across-household heterogeneity in the severity of the calorie crunch and determining the effect of the introduction of EBT on the degree of this heterogeneity. This is a novel approach to analyzing this policy with direct implications for other welfare and transfer programs. I consider the consistency of the results with Jackson and Yariv (2012a) and others, providing the first direct test of the theory outside the laboratory. Additionally, I use the results (both reduced-form and structural) to asses how much empirically observed present-bias can be attributed to collective rather than individual sources: a contribution to the broader literature on dynamic inconsistency.

The results indicate substantial heterogeneity in the severity of the calorie crunch prior to the

[6] Stephens Jr. (2003) and Mastrobuoni and Weinberg (2009) with Social Security disbursements, Stephens Jr. (2006) with paycheck receipt, Shapiro (2005) and Hastings and Washington (2010) with Food Stamp transfers.

introduction of EBT, in a manner consistent with collective present bias. This finding is clearer in expenditure than consumption data. Moreover, the introduction of EBT has the predicted effect. Families with young children and gender-balanced adult populations, which exhibit the most severe pre-EBT crunches, experience the greatest reductions in severity due to EBT. The overall effect of the policy is to substantially reduce heterogeneity in crunch severity.

I compare the magnitude of the effect of EBT to other food-subsidy policies that have been shown to have positive effects on child health. While post-EBT data still exhibit a pattern consistent with meaningful individual present-bias, the portion of the pre-EBT trends attributable to collective present bias is substantial. I show that EBT affects preferences over goods as well as time, consistent with the hypothesis that its introduction has changed the preference expression balance. Additionally, I consider empirical specifications that cast doubt on the role that stigma or fraud could play in explaining the primary results.

Section 2 discusses the foundational empirical work in labor economics and relevant theory. Section 3 presents the results and Section 4 concludes.

2 Empirical and Theoretical Foundations

From a daily perspective, the majority of income receipts by households are lumpy. Paychecks and unemployment insurance usually arrive biweekly, food stamps (the largest non-elderly safety net program, by expenses, as of 2010) and disability benefits arrive monthly, and the Earned Income Tax Credit (the largest cash-transfer program, by expenses, as of 2010, for non-elderly individuals) arrives on a yearly frequency (Bitler and Hoynes, 2013). It is crucial for the evaluation of these programs to understand how income is meted out by recipients over the relevant interval, especially if recipients are likely to be cash-constrained.

Stephens Jr. (2003, 2006) demonstrates jumps in non-durable expenditures in response to social security payments and paychecks, respectively.[7] Shapiro (2005) goes a step further by identifying the decline in daily caloric intake over the course of the month following food-stamp receipt.[8]

[7] Stephens Jr. (2003) uses the same data source as the present study: the Consumer Expenditure Survey. However, the limited nature of heterogeneity across recipients of social security in terms of household structure makes the group less relevant for the issue at hand.

[8] Mastrobuoni and Weinberg (2009) extends the Stephens Jr. (2003) finding to caloric intake and Hastings and Washington (2010) extends Shapiro (2005) using grocery store scanner data rather than self reports.

Explorations of heterogeneous trends within this literature are generally limited to identifying effects stratified by likelihood of liquidity constraint. As expected, constrained households exhibit more severe fluctuations (Mastrobuoni and Weinberg, 2009). For this reason, dynamic responses to programs targeted at distressed households, such as SNAP, should be particularly suspect. Shapiro (2005) briefly addresses whether his findings could be generated by a scramble for resources within a household. He finds no economically or statistically significant effect of household size on cycle severity. I revisit this issue with a broader approach to classifying households as subject to detrimental bargaining processes, an exogenous shifter of weights in the bargaining process and with data that offer more statistical power.

2.1 How Can Household Bargaining Affect Expenditure Cycles?

Hyperbolic and quasi-hyperbolic time preferences (Laibson, 1997; O'Donoghue and Rabin, 1999) feature higher discount rates in the short run than in the long run. Harris and Laibson (2001) show that individuals solving a standard budgeting problem with these preferences will not save precautionarily and exhibit a "comovement between income and consumption" (p. 937). Shapiro (2005) recognizes an empirical manifestation of this in downward-sloping intra-month consumption profiles. A contribution of the present work is to suggest that the theory of dynamic inconsistency generated by collective decision making, as explained by Jackson and Yariv (2012a), can justify the use of hyperbolic preferences to explain this phenomenon without necessitating non-standard time preferences on the individual level. Additionally, the theory of Jackson and Yariv (2012a) has direct implications for which households, all-else-equal, should be subject to collective decision processes that generate these cycles.

I illustrate this with an example based on Jackson and Yariv (2012a). Consider two individuals, a and b who are jointly evaluating a time-stamped consumption stream. They are exponential discounters with different rates of positive discounting, $\delta_a \neq \delta_b, \delta_a, \delta_b \in [0, 1]$, with preferences

$$V_a = \sum_{t=0}^{T} \delta_a^t u(c_t) \quad \text{and} \quad V_b = \sum_{t=0}^{T} \delta_b^t u(c_t). \tag{1}$$

Their preferences are equally weighted in the collective decision process such that

$$V_{a,b} = 0.5V_a + 0.5V_b = \sum_{t=0}^{T} \frac{\delta_a^t + \delta_b^t}{2} u(c_t) = \sum_{t=0}^{T} \Delta_t u(c_t), \tag{2}$$

where $V_{a,b}$ is the collective utility function and Δ_t is the collective discount factor for period t. The first 3 discount factors are

$$\Delta_0 = 1, \quad \Delta_1 = \frac{\delta_a + \delta_b}{2} \quad \text{and} \quad \Delta_2 = \frac{\delta_a^2 + \delta_b^2}{2}. \tag{3}$$

What determines the group's relative preferences for one period over another is the ratio of the corresponding discount factors. For example,

$$\frac{\Delta_0}{\Delta_1} = \frac{2}{\delta_a + \delta_b} \geq \frac{\Delta_1}{\Delta_2} = \frac{\delta_a + \delta_b}{\delta_a^2 + \delta_b^2}, \tag{4}$$

meaning that relative preferences for consumption in adjacent periods is non constant. Specifically, the relative preference for consumption sooner is greater when the sooner period is closer to the present, holding the gap between the sooner and later periods fixed.[9] In other words, the collective preferences are present-biased. Most generally, Jackson and Yariv (2012a) prove that any non-dictatorial, unanimity respecting aggregation rule will generate group preferences that are dynamically inconsistent, provided differing constituent discount factors.[10]

However, if the exponential discount factors, δ_a and δ_b are very similar, the degree of collective present bias will be very small. Bigger differences in preferences will generate more dynamic inconsistency and thus bigger budget shortfalls.

2.2 Evidence on Individual Time Preference in a Low-income Sample

I take advantage of a unique dataset collected by Andreoni et al. (2011) from the Griffin Early Childhood Center (GECC) in Chicago Heights, Illinois. The area is a low-income suburb of

[9]This follows algebraically: $\frac{2}{\delta_a+\delta_b} \geq \frac{\delta_a+\delta_b}{\delta_a^2+\delta_b^2} \Rightarrow 2(\delta_a^2 + \delta_b^2) \geq \delta_a^2 + \delta_b^2 + 2\delta_a\delta_b \Rightarrow \delta_a^2 + \delta_b^2 - 2\delta_a\delta_b \geq 0 \Rightarrow (\delta_a - \delta_b)^2 \geq 0$, which must be true.

[10]Others, including Hertzberg (2012), Zuber (2011) and Bernheim (1999) allude to similar effects of preference aggregation under different circumstances, most notably, separate consumption streams and identical discount factors.

Chicago, and the preschool services families that would be of interest for food stamp policy.

Preschoolers' time preferences were elicited by having them make a series of choices between two plates of candy. One plate could be received on the current day and the other could be received the next day. Four candies were always on the "today" plate, while the number on the "tomorrow" plate ranged from five to eight as the choices progressed. 75% of children chose the "today" plate when the opportunity cost was 1.25 candies tomorrow, and this only decreased to 61% as the opportunity cost reached 2 candies tomorrow. The data showcase extraordinary levels of discounting that most parents would find completely unsurprising. Additionally, the well-known marshmallow experiment conducted by Mischel, Ebbesen, and Raskoff Zeiss (1972) finds that 3-5 year olds resiting temptation in front of potential rewards (in my context, in a supermarket) last less than 30 seconds when attempting to delay gratification to double their reward. More recently, Levit et al. (2012), shows that delayed rewards (in contrast to immediate rewards) have no motivational effect on primary and secondary school students. Bettinger and Slonim (2007) shows the same with direct measures of time preference, but also shows that discounting converges quickly to adult levels during adolescence.

The effect of highly impatient individuals on household time preferences can be very large. Consider a household consisting of a parent (the decision maker) and their infinitely impatient ($\delta_c = 0$) child. The parent has standard exponential preferences with discount factor, δ_h, for the household as a whole[11] and the child has preferences for itself alone. If household preferences are a weighted sum of the two sets, with weight B assigned by the decision maker to their own preferences, two-period preferences for the household, $V(\cdot)$, are

$$V(c_t, c_{t+1}) = \begin{cases} u(c_t) + B\delta u(c_{t+k}) & \text{if } t = 0 \\ u(c_t) + \delta u(c_{t+k}) & \text{if } t > 0 \end{cases}, \tag{5}$$

where a common factor of $(1 - B)$ has been removed from the case in which $t > 0$. This is exactly the quasi-hyperbolic model of time discounting. Adding more children has a natural interpretation of decreasing B, thus increasing the expressed present-bias. Moreover, it takes little appeasement to cause budgeting problems; a value of 0.95 for B represents meaningful present-bias.

Andreoni et al. (2011) also elicited parental discounting. Their time preferences are estimated

[11] This seems more reasonable than to suggest the parent has preferences only for themselves.

using a Convex Time Budget (Andreoni and Sprenger, 2012) in the specific form of Andreoni, Kuhn, and Sprenger (2013). At least over monetary disbursements, the average individual does not exhibit present-biased preferences[12] and discounts at about 92% over a four-week period.[13] While the annualized rate associated with this estimate is very large by some standards, an interest rate of about 180% is actually below some payday loan rates and other credit relevant for this population. Importantly, women and men exhibit different short-run and long-run discounting in this sample.[14]

A key factor that determines whether gender differences in preferences lead to collective present bias is whether decisions are made jointly. Parents in the Andreoni et al. (2011) sample were asked, "Thinking back over the past month, how involved were you in your family's financial decisions?" Answering that they made about half of the decisions (indicating multiple decision makers) is correlated with poor spending plan development, shifting the median report from "I made a spending plan and followed it some of the time" to "I have not made a spending plan in the past month" ($p < 0.05$).

Given this evidence, I explore not just the immediate implication of Jackson and Yariv (2012a), that households consisting of multiple individuals are exposed to the potential for inconsistency via preference aggregation while unitary households are not, but also direct predictors of collective present bias: adding individuals to a group that have different preferences. This is why the number of children in a household will be the key variable of interest. The decision makers are sure to consider their preferences, at least to some minor degree, and their time preferences are vastly different from adults'. Gender differences in preferences only translate into dynamic inconsistency when one group cannot overrule the other.[15] Therefore, I classify households based on whether their adults are gender-balanced, and thus have equal-sized coalitions to represent the different sets of preferences.[16]

[12] There is a discussion in the literature as to whether present-bias should be observed over receipts of monetary income. Augenblick, Niederle, and Sprenger (2013) show experimentally that the same subjects who do not exhibit a disproportionate demand for income today do display a disproportionate propensity to procrastinate when scheduling labor effort if it can be allocated to the present.

[13] That is, for the utility function $U(c_t, c_{t+28}) = u(c_t) + \delta u(c_{t+28}), \delta = 0.92$.

[14] Men discount significantly higher in the short run, but significantly lower in the long run.

[15] The language 'dictatorial' could be misleading in this context. What matters is not whether only one person is making the decisions, but whether only one set of preferences determines them. In the example above, the single parent is the solitary decision maker, but because they aggregate their preferences with their child's preferences, the result is still dynamically inconsistent.

[16] A particularly interesting way to characterize households would be by income-earning structure. A number of papers, including Hoddinott and Haddad (1994) and Andreoni, Brown, and Rischall (2003) identify relative income

2.3 Empirical Application: The Introduction of EBT

Without a shock to the bargaining process, it would be nearly impossible to draw a causal link between household composition and the severity of decline. I argue that the introduction of EBT is a shock to the bargaining process, in the form of an improvement of the property rights over the transfer for the recipient, which has the unintended consequence of changing the level of household present bias. This puts the work squarely at the intersection of behavioral economics and public finance, an area which has seen substantial growth recently.

Two long-standing issues with the Food Stamp program prior to the implementation of Electronic Benefit Transfer were the stigma associated with the use of the visually identifiable stamps and the lack of property rights that allowed them to disperse or be sold. The EBT cards were designed with these issues in mind. They work and look like standard debit cards (see Figure A1 in the Appendix), such that a Personal Identification Number (PIN) is needed to use them. Food stamp benefits are loaded onto the cardholder's account on a monthly frequency, but the disbursement dates vary by state.[17] When comparing this to the cash-similar coupons that were in place before (see Figure A2 in the Appendix), I argue that the recipient is more clearly delineated as the owner and controller of the benefits following the policy change.

What could this delineation of ownership mean for the households of interest? In the case of gender balance, it is clear that the property rights of the direct recipient will be improved. This works both explicitly via the choosing of a PIN number, and implicitly via the codification of the food stamps as belonging to one person. Households can have more than one card, *but only with the permission of the original recipient*. If this constitutes a shock in the direction of dictatorial decision making, EBT should counteract the calorie crunch for these households. Considering the number of children, if they are old enough to go to a store themselves, the coupons could be disbursed more easily than the EBT card, and EBT thus consolidates bargaining power. For EBT to have an effect on families with younger children via this mechanism, as there turns out to be, it must be that the implicit codification of the food stamps as belonging to mom or dad matters for

as an important determinant of whose preferences are expressed in expenditure data. This would get at the issue of income property rights influencing bargaining weights in the aggregation process. However, the diversity in earnings structure is very low in the CEX sample; only 12% of household have 2 or more earners.

[17] Some disburse on the first of the month, but many disburse on different days based on last names, EBT identification numbers or social security numbers.

how they bargain with their kids or, for the youngest children, to resist the temptation to overspend for them.[18]

A broad-strokes outline of the the hypothesis I test is that *the less homogeneous the constituent preferences of a household are and the less dictatorial the aggregation process is, the more present-biased household preferences will be, which will translate into more severe expenditure and consumption cycles. Policies that allow the transfer recipient to exert more dictatorial control over income will work against this heterogeneity.*

This paper is relevant to a larger literature in labor economics on non-unitary household behavior and transfer efficacy. A well known example is given by Lundberg, Pollak, and Wales (1997), who observe that shifting child allowance transfers to wives in the U.K. increased household expenses on women's (and children's) clothing.[19] While recipient effects are important for all sources of income, it is of particular importance for transfer income because of the associated policy objectives. Food stamps are, ideally, used to help provide low-income families with a *steady* flow of *nutritious* food. Studies that examine purchases on the same frequency as the transfer miss out on the first of the two goals. If intra-household preference aggregation can help explain both the stylized facts of intra-month expenditure patterns and the types of items demanded, then there is a clear advantage to dealing with both issues simultaneously.

There is some experimental work that analyzes the relationship between group preferences and the preferences of the constituents. Results are mixed. Abdellaoui, l'Haridon, and Paraschiv (2013) find that joint discounting decisions of real couples cannot be explained as a mix of the individual preferences. However, Carlsson et al. (2013) find that for risk preferences, couples decisions end up somewhere in the range between individual decisions. Additionally, Jackson and Yariv (2012b) find that subjects in the lab acting as social planners exhibit considerable time inconsistency. This paper is, to the author's knowledge, the first test of collective present bias outside of the lab.

[18] I do not wish this to be construed as a circular justification of the theory. I merely wish to point out that *if* the theory of collective present-bias explains the data, it seems that some of the effect of EBT must operate through a strengthening a willpower that affects bargaining with even very young children.

[19] Especially in the low-income and developing context, and number of studies indicate differing 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, Battistin, and Mesnard (2012) and Wang (2012). It is straightforward to embed a "wallet-to-purse" model within the collective present bias explanation of my results.

3 Results

I build up to examining the effect of the EBT introduction in three parts. First, I replicate previous literature by demonstrating a significant and robust decline in food expenditure and consumption. Second, I identify heterogeneity in the severity of this decline across households of different types prior to EBT. Third, I show that EBT significantly reduces (and often eliminates) the heterogeneity. At the end of the section, I use a structural model to characterize the magnitude of the heterogeneity and policy effects in the context of quasi-hyperbolic discounting and explore an alternative explanation for the results.

3.1 Intra-month Expenditure Profiles

The first data I use to identify downward-sloping intra-month expenditure profiles are the Consumer Expenditure Survey (CEX) Diaries. These are self-reported expenditure logs that cover 14 consecutive days. It is collected every year, all throughout the year. The diaries consist of two back-to-back, week-long logs formatted for respondents to keep item-specific records of all purchases. These diaries are linked to a broader, one-time survey of household demographics, composition and income. Food purchases are separated from other purchases. Following collection, the item-level data are coded with a Universal Classification Code (UCC), which permits the identification of expenditures on food alone. Purchases are not coded as individual-specific.

The usable set of CEX diaries ranges from 1994 to 2003. Prior to and following this sample period, the CEX did not record the exact date of the most recent food stamp arrival. Conditioning on households with recorded expenditures within 4 weeks of their recorded food stamp disbursement yields 1302 households over 14,809 days.[20]

I follow the panel approach of Shapiro (2005) with households i and 14 diary days j. Given the date of most recent food stamp receipt, j is transformed into a variable that indicates the number of days since then t, with $t = 0$ on the exact date of reported arrival. If $t = 0$ corresponds with $j = 1$ for a particular household, all 14 days of the diary are used as the first 2 weeks of expenditures. So long as $j = 1$ corresponds to $0 \leq t \leq 14$, all 14 diary days are potential shopping days.

[20] FS eligible purchases are made on slightly less than 35% of days in the sample. Households consist of an average of 3.2 members, with 1.5 of them younger than 18 and 1.1 of them under 13.

I do not use diary observations that fall outside of the food stamp period corresponding to the reported receipt because of the uncertainty as to whether the disbursement was identical the month prior to or following the reported one. This setup implies the basic fixed-effect specification for expenditure trends,

$$exp_{it} = \alpha_i + \beta t + \Gamma Y_t + \epsilon_{it}, \tag{6}$$

where exp_{it} are the food expenditures of household i on days since food stamp receipt t and Y_t are other characteristics of the day in question to be controlled for: a weekend dummy, a week of month variable and a week of diary variable. The identifying assumption for equation (6) is that day-to-day *changes* in expenditures are conditionally uncorrelated with the error term. This might fail if different types of households inform different parts of the food-stamp month. I establish in appendix Table A1 that important observables are uncorrelated with when, in the food-stamp month, households are observed.

Table 1 presents a variety of specifications that correspond to different sample restrictions and periods of observation. I strongly corroborate the existence of expenditure cycles with downward sloping intra-month profiles. The most basic specification in column (1) estimates a decline of \$1.85 per day including all potential shopping days, dropping to \$1.52 or roughly 6% per day conditional on non-trivial expenditures in columns (2) and (3).[21] Collapsing the data by week since arrival of food stamps reduces the gap between the conditional and unconditional samples, yielding an estimate of \$13.15 or roughly 18% per week. Both the finding that most of the effect has to do with amount purchased rather than frequency of purchase and the 18% per week estimate correspond closely to the estimate Hastings and Washington (2010) obtain from grocery store scanner data.

[21] This implies that the use of the log specification to approximate percent changes is only valid for a couple days at the beginning of the month. This will occur repeatedly throughout this section.

Table 1: Expenses on Food Decrease throughout the Food-Stamp Month

Dep. Var.	$ / Day		ln($) / Day	$ / Week	ln($) / Week
	(1)	(2)	(3)	(4)	(5)
Periods since receipt	-1.851***	-1.520***	-0.060***	-13.147***	-0.176***
	(0.122)	(0.247)	(0.009)	(1.213)	(0.041)
Expenses $\geq$ \$1 Only	N	Y	Y	N	Y
Observations	14,809	5000	5000	2918	2285
Clusters	1302	1296	1296	1302	1296

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effects at the household level. Columns (1)-(3) have controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. Household weights change from month to month in the sample, so the specifications presented above are using unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results as do OLS or random-effect specifications that utilize the changing weights properly.

While there are a variety of ways to model this trend, the raw data speak for themselves in this case. Figure 1 shows the raw data, conditional on expenditures of more than $1. Mean daily expenditures, conditional on the restriction, fall from $54.88 to $17.24 and unconditional expenditures from $27.30 to $5.77 over the month. Figure 1 also demonstrates that the negative slope of the fitted trend (using OLS this time for simplicity) remains negative and significant even when the large peaks on days 0 and 1 are removed. Extending this exercise indicates that the conditional decrease drops only from 6% per day to 5.1% per day when the entire first week is removed.

Daily expenditure declines do not translate one-for-one into consumption declines. It is easier to stockpile certain types of food for a month than calories. If there were no trend for perishables, these trends in expenditure mean little for trends in consumption. However, replacing all food expenditures with perishable expenditures[22] in each specification still yields negative and significant trends. Importantly, this is true for the weekly specification, which won't predict an overall monthly decline if there exists only within-week declines based on shopping frequency. Results of this exercise are in appendix Table A2.

[22]I use fruit, vegetables and milk, taking a narrow definition to ensure perishability.

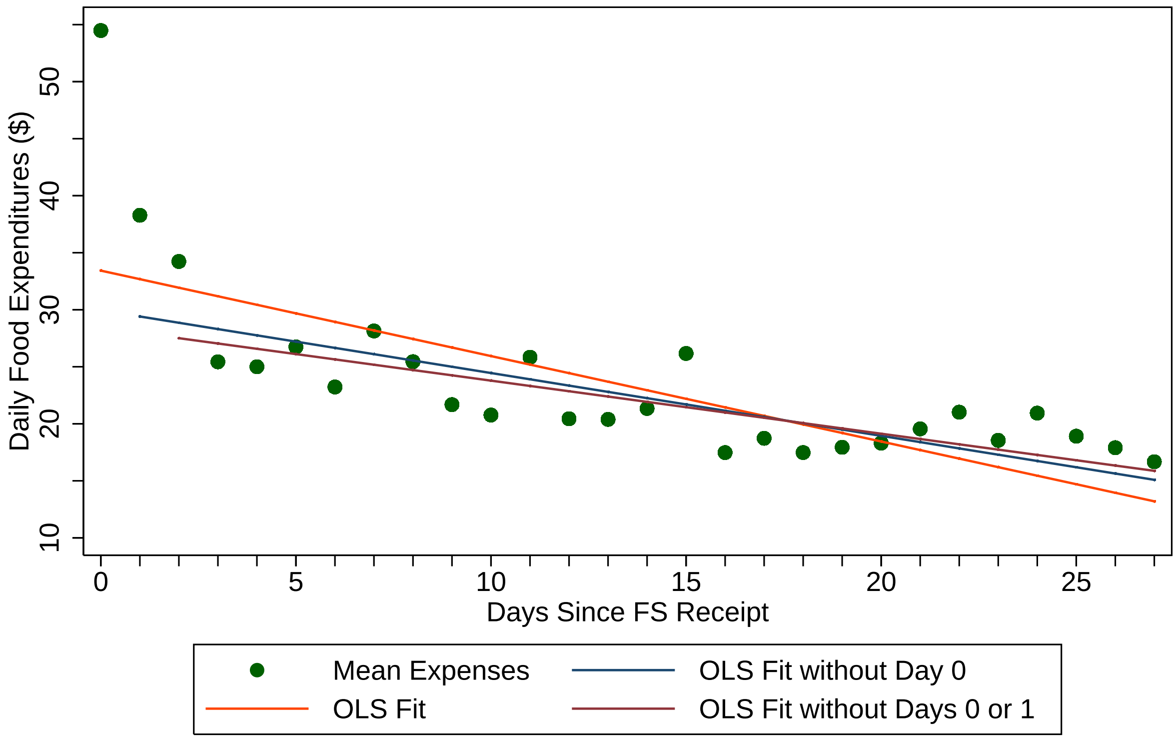

Figure 1: Raw Data Exhibits Strong Decline, Robust to Removal of Early Days

I use the Continuing Survey of Food Intake by Individuals (CSFII) as does Shapiro (2005), to illustrate the correspondence between consumption and expenditure. The CSFII is not collected annually, and the most recent version, from 1998, does not record the exact date that food stamps arrive to a household. The 1989-1992 collection does include this information, as well as demographic data. The dependent variable of food expenditures is replaced with total calories consumed by a household on a survey day. Unfortunately, the CSFII only offers up to 3 days of observations for any given household. I follow Shapiro (2005) by including dummies for survey day (1-3), day of week (1-7), month, year and calendar date rather than a fixed effect, because of the short length of each household's panel.[23] The identification assumption without the fixed effect is that the level of caloric intake is conditionally uncorrelated with the error term.

While the data contain a report of the household size, there is no information on the ages of individuals that do not contribute diaries. Therefore, I restrict my attention to households with a consistent age profile, as represented by the individuals with diaries. I do this to cut down on mea-

[23]There is no theoretical reason why a fixed effect cannot be used. Results are even stronger in specifications that match my CEX approach exactly.

surement error associated with incorrect characterizations of households.[24] I follow my convention of only including observations occur in the 4 weeks following a reported food-stamp disbursement, although my results are robust to defining the days since receipt variable less conservatively. This leaves me with 758 households and 1861 household-days. I estimate a decline of roughly 27 kCal per-day ($S.E. = 14.43$), per-household, or about 0.7% ($S.E. = 0.4\%$) in the log specification.

3.2 Pre-EBT Heterogeneity in the Severity of Intra-month Declines

To establish differential trends across households, I add interaction terms between household composition variables and the number of days since food stamp receipt to the regression specifications from the previous section. While these estimates will not establish a causal link between household structure and consumption or expenditure patterns (because the compositional variables are related to a host of unobservables) they provide context for the treatment effects of the EBT program that follow in the next section.

In Section 2, I identify household size, number of children and gender balance as variables that a preference aggregation theory would predict are correlated with an exacerbated decline in intra-month expenditure and consumption. I first interact household size with the number of days since food stamp receipt and add this to the specification in equation (6). I limit the sample to non-trivial expenditures to focus on value of food purchased rather than frequency of food purchase. Results are presented in Table 2. There is no evidence that profiles were steeper for larger households.

[24]Given that incorrect characterization is more likely in larger households with potentially different expenditure profiles, this measurement error is unlikely to be classical.

Table 2: Raw Household Size Not A Predictor of Pre-EBT Profile Slope

	Expenditures		Consumption	
	\$ / Day (1)	ln(\$) / Day (2)	kCal / Day (3)	ln(kCal) / Day (4)
Days since receipt	-1.494*** (0.319)	-0.082*** (0.013)	-43.133 (35.680)	-0.004 (0.008)
Days since receipt X HH Size	-0.111 (0.085)	0.004 (0.003)	8.971 (10.673)	0.001 (0.002)
Observations	3010	3010	1861	1861
Clusters	745	745	758	758

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. Expenditure models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. The household size variable is adjusted by subtracting 1, such that the terms non interacted with size apply to single-individual units. Expenses less than \$1 are trimmed to reduce noise, especially important in the log specifications. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results. Consumption models control for the amount of the food stamp disbursement and the level effect of household size.

Next, I consider different types of individuals in a household as a more direct proxy for variance in preferences. Table 3 presents the results from estimating profile heterogeneity by number of children, conditional on family size. I break children into 3 age groups: pre-speech fluency (ages 0-4), speech fluency, pre-adolescence (5-12) and adolescence (13-17). In this case, the results are more in line with the theoretical predictions. Prior to the implementation of EBT, replacing an adult member of a household with a child under the age of 13 exacerbates the downward trend in expenditures by 1.9% on top of a baseline decline of 8.3% (\$0.54 on a baseline of \$1.53 in the level model). The consumption estimates are not precise, but the magnitudes of the interaction correlations are clearly important, given the size of the baseline decline.

Table 3: Pre-EBT Profiles Are Steeper with More Young Kids

	Expenditures		Consumption	
	$ / Day (1)	ln($) / Day (2)	kCal / Day (3)	ln(kCal) / Day (4)
Days since receipt	-1.534*** (0.318)	-0.083*** (0.013)	-27.307 (38.244)	0.001 (0.007)
Days since receipt X Kids 0-5 Yrs.	-0.385* (0.230)	-0.016** (0.008)	-34.503 (27.943)	-0.005 (0.005)
Days since receipt X Kids 6-12 Yrs.	-0.538** (0.271)	-0.019* (0.010)	-15.767 (18.324)	-0.004 (0.004)
Days since receipt X Kids 13-17 Yrs.	0.108 (0.223)	-0.003 (0.009)	-34.208 (27.379)	-0.012** (0.005)
Observations	3002	3002	1861	1861
Clusters	737	737	758	758

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. Expenditure models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from, which week of the month it comes from, the interaction between household size and days since food stamp receipt and the interactions between the number of children over the age of 12 and days since food stamp receipt. The household size variable is adjusted by subtracting 1, such that the terms non interacted with size apply to single-individual units. Expenses less than $1 are trimmed to reduce noise, especially important in the log specifications. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results. Consumption models control for the amount of the food stamp disbursement, the level effect of household size, the interaction of size and days since receipt and the level effect of the number of children of each age type.

Moving to a measure of preference variance *and* non-dictatorial decision making, Table 4 presents the results from estimating heterogeneity by whether the adult population of a household is gender balanced, again conditional on family size.[25] The correlation is strong in all specifications except for the log-expenditure model in column (2).

[25] I do this because many households have 1 or 2 adults.

Table 4:Pre-EBT Profiles are Steeper with Gender Balance

	Expenditures		Consumption	
	$ / Day (1)	ln($) / Day (2)	kCal / Day (3)	ln(kCal) / Day (4)
Days since receipt	-1.422*** (0.322)	-0.081*** (0.013)	-50.370 (30.686)	-0.006 (0.008)
Days since receipt X Males = Females	-0.619* (0.348)	-0.006 (0.013)	-77.743** (35.071)	-0.016** (0.008)
Observations	3002	3002	1861	1861
Clusters	737	737	758	758

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. Expenditure models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from, which week of the month it comes from and the interaction between household size and days since food stamp receipt. The household size variable is adjusted by subtracting 1, such that the terms non interacted with size apply to single-individual units. Expenses less than $1 are trimmed to reduce noise, especially important in the log specifications. Consumption models control for the amount of the food stamp disbursement, the level effect of household size, the interaction of size and days since receipt and the level effect of gender balance. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results.

As stated earlier,estimates in this section cannot necessarily be interpreted as causal. The consumption estimates are especially suspect given the lack of a household fixed effect. However, if the differences across households have to do with the expression of different time preferences, there should be detectable differences in purchased bundles that stem from the expression of the corresponding different preferences over goods. This turns out to be true with the purchase of fresh vegetables. Aggregating the CEX diaries within households and taking advantage of the item-level data allows me to ask whether households with more young children purchase fresh vegetables less frequently (conditional on family size, food-stamp disbursement and year dummies) prior to EBT. Unsurprisingly, this is true. Replacing an adult with a child under 13 is correlated with about 0.2 fewer diary days in which fresh vegetables are purchased, from a baseline of 0.87 days on average. Adjusting for different diary lengths, this is roughly a decrease of 0.01 in the probability of purchasing fresh vegetables on any given day, from a baseline probability of 0.09.

3.3 Effect of EBT Implementation on Heterogeneity

In 1989 Maryland became the first state to begin the implementation of a statewide system, with completion occurring in April of 1993.[26] A number of states began implementing the program of their own accord until 1996, when the U.S. congress passed a welfare reform bill that mandated the full implementation of EBT across the country by October of 2002.[27] On average it took states about 15 months to institute the program. I use the month of official completion as the policy change date. All results are robust to the exclusion of the rollout period from the sample. See appendix Table A3 for a list of states and completion dates.

Because of data limitations, only the CEX data permits a study of the policy change. I amend the specification in equation (6) with an interaction between EBT implementation and days since food stamp receipt to establish a baseline effect of the program, an interaction between the household composition variable of interest and days since receipt to replicate the pre-EBT patterns from the previous section and a triple interaction between EBT, the household variable and days since receipt, which will be the variable of interest.

While the specifications all include a fixed effect, the identification of the policy effect is between-households. Therefore, it is important to establish stability of the sample composition across the EBT implementation. A useful feature of this particular policy change that greatly aids identification is that EBT rollout occurred at different times in different states over roughly a 10 year period. Thus, I am simultaneously comparing different cohorts within the same state and the same cohort across states, rather than one or the other.[28] Appendix Table A4 presents means of observable variables across the EBT implementation. It appears that families on food stamps in the CEX sample are slightly larger following the implementation of EBT. This corresponds to a small increase in the amount of the food stamp disbursement, one factor that could potentially affect the dynamics of spending. However, controlling for household size, there is no significant increase in the disbursement following the introduction of EBT. This indicates that families of similar compositions have similar food stamp endowments on either side of the policy change.

[26] http://www.fns.usda.gov/snap/ebt/ebt_status_report.htm

[27] A number of states were unable to comply until 2003. California and Guam did not complete implementation until 2004: http://www.fns.usda.gov/snap/rules/Legislation/about.htm.

[28] I limit the samples to states that switched during my period of observation so that state specific effects cannot be conflated with policy effects.

Table 5: EBT Doesn't Interact with Household Size

Food Stamp Delivery	Coupons	EBT	All		
	(1)	(2)	(3)	(4)	(5)
Days since receipt	-1.594*** (0.284)	-0.677 (0.432)	-1.239*** (0.136)	-1.274*** (0.306)	-0.071*** (0.013)
Days since receipt X HH Size	-0.128 (0.099)	-0.182* (0.106)	-0.128 (0.085)	-0.146 (0.098)	0.002 (0.004)
Days since receipt X EBT			0.183 (0.320)	0.087 (0.338)	0.022 (0.017)
Days since receipt X HH Size X EBT			-0.019 (0.132)	-0.000 (0.143)	-0.005 (0.005)
Policy Switch States Only	Y	Y	N	Y	Y
Log Expenses	N	N	N	N	Y
Observations	2415	1881	5000	4296	4296
Clusters	610	533	1296	1141	1141

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. The household size variable is adjusted by subtracting 1, such that the terms non interacted with size apply to single-individual units. Expenses less than $1 are trimmed to reduce noise, especially important in the log specifications. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results.

Following the structure of the previous section, I start by considering the effect of the EBT implementation on expenditure profile heterogeneity by raw household size. Results are in Table 5. Column (1) replicates the pre-EBT estimates for the limited sample of policy-switching states, and column (2) presents the corresponding post-EBT implementation estimates. Columns (3)-(5) combine the pre and post data to estimate the interaction terms of interest. There is no evidence of a significant effect of the interaction between the implementation of EBT and household size on the intra-month expenditure profile.

Table 6: EBT Erases Heterogeneity Associated with Young Children

Food Stamp Delivery	Coupons	EBT	All		
	(1)	(2)	(3)	(4)	(5)
Days since receipt	-1.616*** (0.355)	-0.477 (0.442)	-1.248*** (0.282)	-1.262*** (0.303)	-0.070*** (0.013)
Days since receipt X Kids 0-5 Yrs.	-0.378 (0.245)	0.446 (0.331)	-0.363 (0.229)	-0.356 (0.243)	-0.020** (0.008)
Days since receipt X Kids 6-12 Yrs,	-0.604* (0.307)	0.753** (0.337)	-0.502* (0.271)	-0.567* (0.311)	-0.020* (0.011)
Days since receipt X Kids 13-17 Yrs,	0.116 (0.256)	0.155 (0.302)	0.113 (0.225)	0.115 (0.258)	-0.001 (0.010)
Days since receipt X EBT			0.267 (0.338)	0.165 (0.359)	0.023 (0.018)
Days since receipt X Kids 0-5 Yrs. X EBT			0.640 (0.391)	0.728* (0.402)	0.032** (0.016)
Days since receipt X Kids 6-12 Yrs. X EBT			1.043** (0.412)	1.207*** (0.449)	0.031* (0.017)
Days since receipt X Kids 13-17 Yrs. X EBT			-0.050 (0.374)	0.011 (0.405)	0.020 (0.016)
Policy Switch States Only	Y	Y	N	Y	Y
Log Expenses	N	N	N	N	Y
Observations	2415	1881	5000	4296	4296
Clusters	610	533	1296	1141	1141

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. Additionally, I control for household size interacted with both pre- and post-EBT trends such that the estimates refer to the composition rather than the size of the household. Expenses less than $1 are trimmed to reduce noise, especially important in the log specifications. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results.

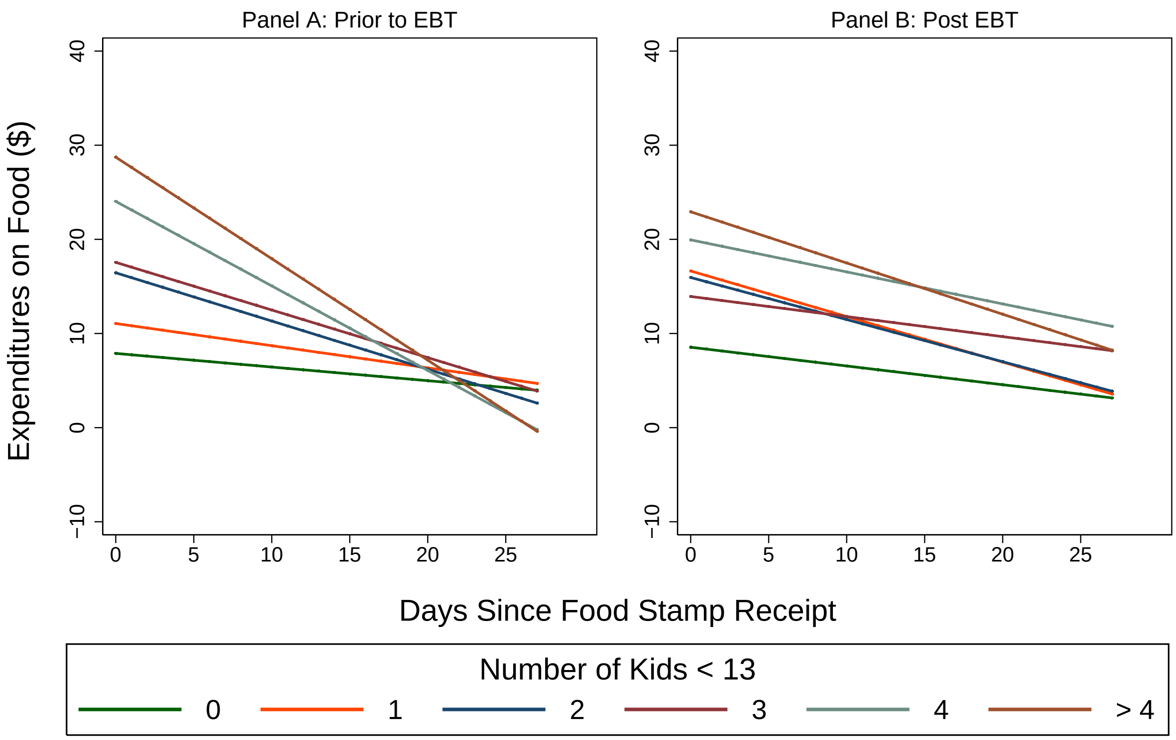

Figure 2: EBT Significantly Affects Relationship between Kids and Trend

Moving to a direct measure of preference variance, the number of children, produces a very different result in Table 6. Using the estimates from the log specification in column (5), households without children experience an expenditure decline of 7% per-day. This interpretation of the log coefficient holds only for the first couple days of the month, indicating that an alternative speficiation of the model may be preferable. See appendix Section A.5 for estimates that follow Hastings and Washington (2010) by using weekly dummies for expenditure levels. Pre-EBT, each young child correlates with an exacerbating of that decline by a little more than 2% per-day. EBT corrects that exacerbation, mitigating the slope by a little more than 3% for both of the younger age groups. EBT does not appear to have a significant baseline effect that can be applied to all households.[29]

[29]The large change in the baseline decline estimate between columns (1) and (2), which splits the pre- and post-EBT samples appear to be at odd with the direct estimates of the EBT-days-since-recepit interaction in columns (3), (4) and (5). This could be due to the change in sample composition, but it should not affect the estimation of the triple interactions conditional on the regular interactions.

Table 7: EBT Increases Fresh Vegetable Purchase for Households with Children

Food Stamp Delivery	Coupons	EBT	All	
	(1)	(2)	(3)	(4)
Kids $<$ 13	-0.196** (0.079)	-0.009 (0.064)	-0.174** (0.072)	-0.210*** (0.077)
Kids $>$ 12	-0.166* (0.098)	0.047 (0.088)	-0.163* (0.086)	-0.166* (0.099)
EBT			0.008 (0.166)	-0.099 (0.171)
Kids $<$ 13 X EBT			0.167* (0.091)	0.203** (0.097)
Kids $>$ 12 X EBT			0.203* (0.121)	0.217* (0.132)
Mean of DV	0.80 (1.02)	0.79 (0.97)	0.84 (1.07)	0.79 (1.00)
Policy Switch States Only	Y	Y	N	Y
Observations	602	527	1284	1131

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. The amount of the FS transfer, year dummies and family size are included as controls in all specifications. Family size must be explicitly controlled for in these specifications because shifting the unit of observation to the household level removes the fixed-effect.

Figure 2 uses all the data and much less statistical structure to give a visual representation of the effect of the EBT implementation for households with varying numbers of young children. I estimate pre- and post-policy change trends using OLS for each different type of household. Level differences in consumption are not absorbed, meaning that the convergence (and even overshoot) by households with many young children in Panel A is a legitimate budgeting failure that is much less serious post-EBT in Panel B.

Table 8: EBT Erases Heterogeneity Associated with Gender Balance

Food Stamp Delivery	Coupons	EBT	All		
	(1)	(2)	(3)	(4)	(5)
Days since receipt	-1.480*** (0.362)	-0.750* (0.443)	-1.168*** (0.286)	-1.161*** (0.310)	-0.068*** (0.013)
Days since receipt X Males = Females	-0.857** (0.413)	0.358 (0.382)	-0.632* (0.347)	-0.868** (0.411)	-0.021 (0.015)
Days since receipt X EBT			0.042 (0.321)	-0.093 (0.336)	0.020 (0.017)
Days since receipt X Males = Females X EBT			0.994** (0.495)	1.265** (0.556)	0.019 (0.021)
Policy Switch States Only	Y	Y	N	Y	Y
Log Expenses	N	N	N	N	Y
Observations	2415	1881	5000	4296	4296
Clusters	610	533	1296	1141	1141

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. Additionally, I control for household size interacted with both pre- and post-EBT trends such that the estimates refer to the composition rather than the size of the household. Characterization of gender balance is based solely on the adult (age 18 or greater) members of the household. Expenses less than \$1 are trimmed to reduce noise, especially important in the log specifications. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results.

If EBT is indeed affecting whose preferences are being expressed at the grocery store, there should be a heterogeneous effect of EBT on fresh vegetable purchasing. Controlling for the amount of the FS disbursement, family size and adding a year-specific dummy, I then regress number of days fresh vegetables were purchased during a diary on the presence of children, the EBT implementation dummy and the interaction between the two. Results are presented in Table 7. While the level of fresh vegetable purchasing is very low (the average household purchases fresh vegetables on about 8% of their shopping days), there is a relationship between children in the household and purchasing that corresponds to the predictions. Prior to EBT implementation, both age classes of children are associated with less frequent purchases of fresh vegetables. Relative to the small levels, the correlation is large. The implementation of EBT has a countervailing effect that almost exactly wipes out the differences in purchasing that come from children.

Moving to the measure of gender balance, the results are similar to those for young children, and consistent with the theory. While the coefficient on the triple interaction in the log specification in column (6) of Table 8 is not statistically significant, the point estimate almost fully counteracts the pre-EBT gender-balance interaction. It is worth noting that the magnitude of the un-interacted effect of EBT on the slope of the expenditure profile is non-negligible. One reason for this is that the baseline group includes many families with young children. From column (4), gender-balanced households experience intra-month declines \$0.87 per-day faster than unbalanced households from a baseline of \$1.16 prior to the EBT implementation (or 2.1% on a baseline of 6.8%). The differential is completely counteracted for by the implementation of the EBT program. Results are qualitatively very similar using a variable equal to the absolute value of the difference between the number of male and female adults in the household.

In sum, I find data consistent with the predictions of the preference aggregation model, though not always with statistical confidence. One puzzle is that the raw household size specification shows little evidence of an interaction with the EBT policy switch or an exacerbating pre-trend. Even though size is not a direct measure of preference variance, increasing it is directly related to whether the model should apply.

3.4 Structural Estimation

I perform a calibration of a quasi-hyperbolic model of time preferences in order to assess whether the data generate present-bias estimates in line with the literature, whether the magnitudes of the parametrically-measured heterogeneity in present-bias and EBT effects are meaningful, and then what bargaining weights would need to be in order to generate the observed patterns in the data. First, I make a series of assumptions that allows me to utilize a basic model of intertemporal choice. Consider a household whose budget for food is endowed every 4 weeks and is isolated from all other purchases. The budget is completely exhausted by the end of the 4 week period following receipt and prices do not change. Households derive utility from the log of expenditures on food. The advantage to this setup is that observed expenditures translate directly into value of food consumed and log utility captures fact that consuming no food is extraordinarily undesirable, but marginal utility decreases very quickly such that gourmet food is not worth starving for.

All the simplifying assumptions allow me to use the hyperbolic Euler equation from Harris and

Laibson (2001),

$$\frac{c_{t+1}}{c_t} = \beta\delta c'(x_{t+1}) + \delta(1 - c'(x_{t+1})), \tag{7}$$

where consumption levels, c_t are expressed as functions of the current wealth stock, x_t, and β and δ are the present-bias parameter and exponential discount factor from the quasi-hyperbolic model of discounting. Shapiro (2005) proves that this can be reduced to the recursive equation,

$$\alpha_t = \begin{cases} \frac{\alpha_{t+1}}{\alpha_{t+1}+(\delta(1-(1-\beta)\alpha_{t+1}))} & \text{if } t < T \\ 1 & \text{if } t = T \end{cases}, \tag{8}$$

where T represents the last period and α_t is the fraction of wealth spent on consumption in period t.

I borrow the estimated daily $\delta \approx 0.997$ from the Andreoni, List, Savikhin, and Sprenger (2011) data. While this rate is still relatively high, it comes from a context more likely to evaluate individuals' longer-run tradeoff rates rather than their short-run temptation to consume. Since the estimation depends on a recursive definition of consumption and expenditure, I use the predicted values from my fixed-effect regressions to generate complete monthly profiles of expenditures and then collapse by sample period and household characteristics. I include variables that shift the estimated parameter value in the recursive estimation that correspond roughly to the interactions of interest in the main fixed-effect specifications.

Looking at the pooled sample of all households after the introduction of EBT, I estimate $\beta = 0.945$. This is close to other estimates in the literature such as Shapiro (2005), Frederick, Loewenstein, and O'Donoghue (2002), Laibson, Repetto, and Tobacman (2003a, 2003b), Kuhn, Kuhn, and Villeval (2013) and Augenblick, Niederle, and Sprenger (2013). Forcing discounting to be purely exponential generates a yearly discount rate estimate of 51,423,440% (or a daily factor of 0.965). Focusing on single individuls actually produces a β further from 1. Thus, even in the period and population for which preference aggregation should not be a factor, a daily discount rate does not exist.

The estimate of β for a single person household prior to EBT is 0.942. Adding one child, aged 6-12, reduces that estimate all the way to 0.717. This is a substantial effect. After EBT, the single-person estimate is 0.982, or 0.999 with the added 6-12 year old. This gap implies an estimate of $1 - B$, the bargaining weight on the assumed-to-be infinitely impatient child's preferences of about 0.25. For a younger child, the pre-EBT estimate is 0.809 and the post-EBT estimate is 0.977, implying a smaller decision weight of about 0.15. This difference could reflect the fact that very young children cannot bargain as actively. For gender-unbalanced households prior to EBT, the estimate of β is 0.946. Making that household gender-balanced corresponds to reducing β to 0.636. Introducing EBT raises the estimate back up to 0.971.

While these structural estimates should be taken with considerable skepticism due to the many required abstractions and the amount of noise, they underscore two important points. First and foremost, preference aggregation exacerbates the dynamic inconsistency that exists even for single-person households. Second, the exacerbation empirically identified in this paper, regardless of the cause, is economically significant.

3.5 Could It be Heterogeneous Stigma?

To the author's knowledge, there is no precise theory or consensus as to how welfare stigma affects families and how it might do so heterogeneously. There are also few empirical facts to draw on. Currie and Grogger (2001) find that SNAP enrollment gains following EBT are limited to rural households and married couples with no children. The lack of gains for families with children suggests that experienced stigma could be based more on deservingness of benefits than parental responsibility. However, stigma from enrolling in SNAP and stigma from using SNAP benefits may not be the same. In the context of benefit usage, common sense dictates that stigma should affect how often benefits are used rather than the value used in any given visit. Thus, policies that reduce the stigma associated with benefit usage could affect shopping frequency in a way that allows for a smoother expenditure profile.

Table 9: Shopping Frequency Unrelated to EBT or Household Composition

hhvar	HH Size (1)	Kids 0-5 Yrs. (2)	Kids 6-12 Yrs. (3)	Kids 13-17 Yrs. (4)	Males = Females (5)
Days since receipt	-0.035***	-0.035***	-0.035***	-0.035***	-0.035***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Days since receipt	-0.002	-0.002	-0.002	-0.002	-0.002
X EBT	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Days since receipt	-0.000	0.001	0.000	0.001	0.003
X *hhvar*	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Days since receipt	0.001	0.002	0.003	-0.004	-0.003
X *hhvar* X EBT	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
Observations	14,809	14,809	14,809	14,809	14,809
Clusters	1302	1302	1302	1302	1302

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effects at the household level and controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. The household size variable is adjusted by subtracting 1, such that the terms non interacted with size apply to single-individual units. Days with expenses less than $1 are considered to be non-shopping days. Household weights change from month to month in the sample, so the specifications presented above use unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results.

To get directly at this issue, I analyze the frequency of shopping, whether it is affected by EBT and if this effect (or pre-existing trends) indicate heterogeneous stigma. Table 8 presents regressions of a dummy variable for whether a household made purchases totaling more than $1 on any given day in their diary on the number of days since stamp receipt and other variables of interest.

The uninteracted coefficient on days since receipt is steady across all specifications and none of the interactions are important either statistically or in terms of their magnitude. EBT appears to have no homogeneous or heterogeneous effect on shopping frequency. This analysis casts doubt on the idea that welfare stigma is experienced when benefit usage is observed by other individuals[30] and does not support stigma as an explanation of the EBT effects.

[30] This result has interesting self-signaling versus social-signaling ramifications, but an in-depth analysis is best left for future work.

4 Conclusion

I demonstrate that failures to use food stamps to smooth monthly expenditures on food are more severe for families with young children and gender-balanced homes. The introduction of the EBT program appears to have counterbalanced this discrepancy. I propose that the structure of the theoretical link drawn by Jackson and Yariv (2012a) between the literature on household bargaining and time inconsistency is a plausible explanation. The main analysis using expenditures from CEX data is supplemented with supporting evidence on calories consumed from the CSFII on caloric intake. I show that EBT affects bundles purchased in a manner consistent with this hypothesis and try to rule out stigma as an alternative. I view this as a contribution to a new literature at the intersection of behavioral economics and public finance regarding the consequences of program design that are best explained by non-traditional choice theory.

Thaler (1999), writing on mental accounting, describes different types of heuristic wealth, income and consumption accounting that violate the principle of fungibility. The best-known example is the envelope system of budgeting. He notes two purposes for categorical techniques like the envelope system: to facilitate rational tradeoffs between categories and as self-control devices. Thaler also illustrates the downside to the envelope system and similar techniques; when one account is near the limit, individuals will display excess price sensitivity in one category even when there is slack in another account. These distortions stem from a violation of basic principles, so it comes as little surprise that they get attention from economists. Indeed, Emily Oster writes in an opinion article for *Slate* (Oster, 2013) that this method of accounting is dangerous as it prevents households from reoptimizing to price changes and clearing bad debt.[31]

While Oster's wisdom applies generally, I argue that the allocation of resources to food in different time periods is an exception to the rule for distressed households. Food is a fundamental unit of daily consumption for which the need fluctuates minimally. The self-control value of the envelopes trumps the fungibility issues, *especially if one person can be put clearly in charge of them.* Treating a food budget as completely non-fungible over a week or two-week horizon could even offer the added benefit of encouraging the consumption of foods that perish on a similar schedule.

[31]This assumes that households don't reoptimize the size of the envelopes to match price changes. They have self-control value even in a completely static price-setting.

A policy implication that follows from this and previous literature on the smoothing of transfer income, is to enforce weekly or biweekly envelopes by shifting disbursement to a higher frequency. Even post-EBT, the uninteracted days since stamps variable is negative, significant and large in magnitude. Beyond that, the results here suggest that property rights play an important role in determining how well transfer income is budgeted over time. The lesson from this finding is broad and echoed by a large development literature on recipient effects. Property rights over other types of transfers should be investigated as well. The Earned Income Tax Credit (EITC) is of particular interest since it is only disbursed only once per year. Whether its use should be smoothed depends on motive: does the EITC break credit constraints for large investments and durable purchases, or does it help bring a family out of food insecurity over the course of the following year?

A significant caveat to the policy implications is the cost-benefit analysis performed by Shapiro (2005). He accounts for administrative costs and makes assumptions about caloric utility to asses the gains from increasing disbursement frequency and finds little net gain. While I do not disagree with his calculations, I suggest that modeling the impact of improving food stability using a caloric utility function may not be the best approach to quantifying the benefits. Numerous studies, including Hoynes, Page, and Stevens (2011), suggest a positive and significant impact of the Women Infants and Children (WIC) extension of SNAP on nutrient intake and subsequently birthweight even though benefits in 1999 (roughly the middle of my sample period) had an average monthly value of only $32.50.[32] My estimates indicate that EBT increased the amount spent in a week 4 shopping trip for a family with a young child by about that amount.

Although one of the implications of the preference aggregation theory of dynamic inconsistency is that even time-consistent individuals can become households with present bias, the expenditure data are clear that even households consisting of only one individual display trends that cannot be reconciled with an exponential daily discount rate. The β associated with a quasi-hyperbolic single individual in the CEX sample is 0.927, and the post-EBT β for all households is 0.945. Both estimates represent economically important deviations from 1. Forcing the post-EBT β equal to one would imply a daily exponential discount factor of roughly 0.965, or an annual discount rate of about 51,423,440%. While some households may have a tougher time budgeting because of their structure, in the population of SNAP recipients, even dictatorial control over resources isn't

[32]http://www.fns.usda.gov/pd/wisummary.htm

sufficient to eliminate the calorie crunch.

A final takeaway from this work is that when it comes to programs dedicated to alleviating poverty, assessments made on the yearly or monthly basis may fail to capture important issues. An EITC disbursement in February may bring a household above the poverty line on a yearly-income basis, but one should asses the effectiveness of the program in alleviating poverty based on how many days of the year that household consumes value above the daily poverty threshold.[33] Especially given recent political inclinations to reduce funding for SNAP and other welfare programs, it is worth asking whether behavioral insights into short-run decision making can improve the efficacy of transfers.

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[33]Or at least under an assumption of less-than-perfect substitutability of consumption across periods.

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A Appendix

A.1 Examples of Food Stamp Coupons and EBT Cards

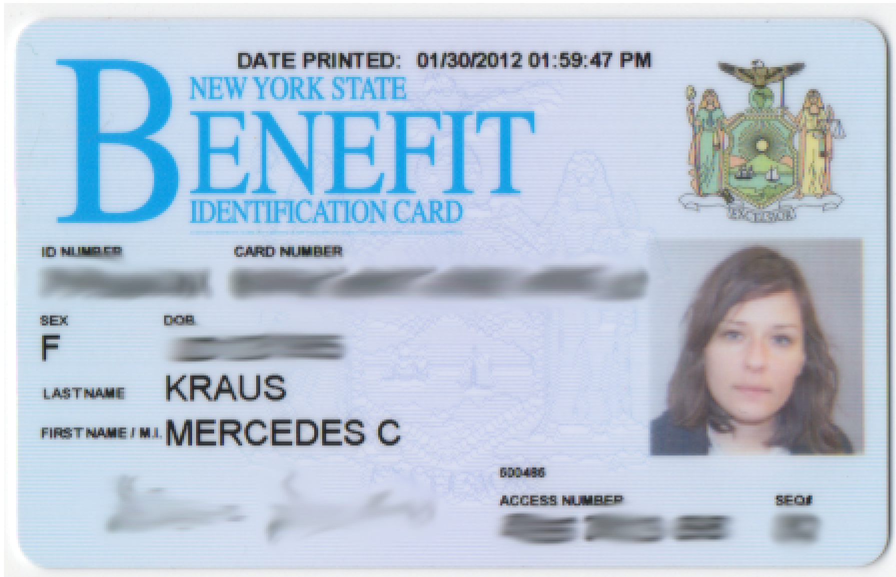

Figure A1: EBT Cards Establish the Identity of the Food Stamp Recipient Clearly

Figure A2: Food Stamp Coupons Were Cash Similar

A.2 Within-month Observable Balance

Table A1: Household Observable Means and Food Stamp Timing

Variable	Week 1	Week 2	Week 3	Week 4	H_0 : All Weeks Equal
Family Size	3.14	3.17	3.14	3.19	$p = 0.932$
Food Stamp Amount ($)	171.53	174.68	175.88	178.74	$p = 0.771$
Adults	1.67	1.64	1.64	1.66	$p = 0.883$
Female Adults	1.12	1.11	1.11	1.11	$p = 0.972$
Male Adults	0.54	0.53	0.53	0.55	$p = 0.899$
Kids 0-5 Yrs.	0.57	0.59	0.60	0.57	$p = 0.883$
Kids 6-12 Yrs.	0.54	0.57	0.56	0.61	$p = 0.536$
Kids 13-17 Yrs.	0.34	0.36	0.33	0.34	$p = 0.849$
Received FRP Lunches?	0.03	0.03	0.04	0.04	$p = 0.647$

A.3 Within-month Trends Using Perishables Only

Table A2: Expenses on Perishable Food Decrease throughout the Food-Stamp Month

Dep. Var.	$ / Day		ln($) / Day	$ / Week	ln($) / Week
	(1)	(2)	(3)	(4)	(5)
Periods since receipt	-0.668***	-0.590***	-0.046***	-4.851***	-0.176***
	(0.049)	(0.157)	(0.010)	(0.524)	(0.041)
Expenses $\geq$ \$1 Only	N	Y	Y	N	Y
Observations	14,809	3399	3399	2918	2004
Clusters	1302	1234	1234	1302	1234

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors clustered by household. All models feature fixed-effect at the household level. Columns (1)-(3) have controls for whether any particular day is a weekend, which diary week it comes from and which week of the month it comes from. Household weights change from month to month in the sample, so the specifications presented above are using unweighted data. Using the mean weight or the weight that characterizes the majority of the observations for a household produces very similar results as do OLS or random-effect specifications that utilize the changing weights properly.

A.4 EBT Policy Change

Table A3: Dates of EBT Completion for States in Sample

Year	States	Eligible for Policy Switch Sample?
1993	Maryland	No
1995	South Carolina, Texas	Yes
1996	Utah	Yes
1997	Alabama, Connecticut, Illinois, Kansas, Louisiana, Massachusetts	Yes
1998	Alaska, Arkansas, Colorado, District of Columbia, Florida, Georgia, Hawaii, Idaho Minnesota, Missouri, Oklahoma, Oregon Pennsylvania, Vermont	Yes
1999	Arizona, Kentucky, New Hampshire, New Jersey, North Carolina, Ohio, Tennessee, Washington	Yes
2000	Wisconsin	Yes
2001	Michigan, New York	Yes
2002	Indiana, Nebraska, Nevada, Virginia	Yes
2003	Iowa	No
2004	California	No

All states eligible for policy switch analysis appear contribute to the analysis except for Arkansas and Idaho.

A.5 Observable Balance across EBT Implementation

Table A4: Household Observable Means and EBT Timing

Variable	Food Stamp Coupons	EBT Foods Stamps	H_0 : Means Equal
Family Size	3.03	3.19	$p = 0.140$
Food Stamp Amount ($)	170.88	183.98	$p = 0.078$
Adults	1.60	1.67	$p = 0.157$
Female Adults	1.09	1.10	$p = 0.700$
Male Adults	0.51	0.57	$p = 0.136$
Kids 0-5 Yrs.	0.58	0.54	$p = 0.461$
Kids 6-12 Yrs.	0.54	0.59	$p = 0.390$
Kids 13-17 Yrs.	0.31	0.34	$p = 0.397$
Received FRP Lunches?	0.03	0.03	$p = 0.647$

Sample is limited to states that switched foos stamp disbursement methods during the observation period in the CEX data.

A.6 Weekly Dummies

Rather than enforce a linear, day-by-day trend over the course of the food stamp month, I can follow Hastings and Washington (2010) and allow for weekly dummies that will give some indication of when heterogeneity is most noticeable. I exclude the second week of the month because the overspending in the first week is of interest. Table A5 presents this specification for the number of children interaction.

Table A5: EBT Reallocates Expenditures from Week 1 to Later Weeks Heteregeneously

	Expenditure Difference from Week 2		
	Week 1	Week 3	Week 4
All Households	4.264 (2.901)	-3.751* (2.249)	-5.561** (2.740)
All Households Post EBT	-2.103 (5.210)	3.822 (3.524)	3.674 (4.738)
Per Kid 0-5 Yrs.	7.796* (4.322)	-2.177 (2.240)	-4.866 (3.537)
Per Kid 6-12 Yrs.	5.020 (3.863)	-2.862 (3.451)	-4.206 (4.396)
Per Kid 13-17 Yrs.	-0.399 (3.744)	1.357 (3.846)	-0.019 (4.314)
Per Kid 0-5 Yrs. Post EBT	-9.441* (5.702)	9.907** (4.548)	10.795* (6.048)
Per Kid 6-12 Yrs. Post EBT	-17.546** (8.416)	14.565*** (5.312)	11.248* (6.381)
Per Kid 13-17 Yrs. Post EBT	-4.514 (6.239)	2.796 (5.365)	3.637 (6.137)

$^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Sample consists of 4285 days of expenditures greater than $1 for 1141 households in states that switched disbursement policies in the sample period. Family size interactions with week dummies are controlled for as well as the triple interactions between raw size, EBT and week dummies. Standard errors clustered by household, household fixed effects included.

The effect of EBT is to shift expenditures from week 1 to later weeks in the food stamp month, such that in the post-EBT period families with more young children experience less of a monthly decline.