The Effect of Electronic Benefit Transfer on the Marginal Propensity to Consume Food out of SNAP

Chase S. Eck*

This version: November 2019
First version: September 2018

Abstract

The Supplemental Nutrition Assistance Program (SNAP) provides food assistance to nearly 44 million Americans each year. I document a substantial increase in the program's ability to stimulate food consumption from 1990 to 2010, as measured by the marginal propensity to consume food (MPCf) out of SNAP. I provide the first evidence for a mechanism driving this increase: the transition from paper coupons to Electronic Benefit Transfer (EBT) cards. Using plausibly exogenous variation over states and time I estimate that the introduction of EBT doubles the MPCf out of SNAP and accounts for 25 percent of its observed increase.

---

*The University of Arizona, Department of Economics, 1130 E Helen St., McClelland Hall Rm 401, Tucson, AZ 85721 (email:eckcs1@email.arizona.edu). I would like to thank Martin Dufwenberg, Price Fishback, Hidehiko Ichimura, Michael Kuhn, Ashley Langer, Jessamyn Schaller, Gary Solon, Tiemen Woutersen, and seminar participants at The University of Arizona for their excellent comments and suggestions.

Many of the 43 million Americans living in poverty struggle to consume enough food. For children who grow up in poverty, malnutrition is associated with lower childhood and adult health (Cook et al., 2004). To address this problem, Congress created the Supplemental Nutrition Assistance Program (SNAP; formerly called the food stamp program) in 1964 to help people living at or below the poverty line gain access to a secure source of food. SNAP distributes money that can only be used on eligible food items at qualified retailers. Today, SNAP is the second largest in-kind transfer program in the United States and distributes $66.5 billion in food assistance to 44 million people (USDA, 2017).

By providing in-kind, rather than cash, benefits policymakers hope that SNAP recipients will consume more food than they would with an equivalent cash transfer (Currie and Gahvari, 2008). However, there is a long-standing debate over whether in-kind transfers, and SNAP benefits in particular, are better at steering consumption towards certain types of goods. Standard demand theory predicts that inframarginal households—those that would spend more on food than they receive in SNAP benefits—will treat SNAP benefits and cash as perfect substitutes (Southworth, 1945). This result implies that, for the majority of SNAP households, SNAP benefits and cash should lead to similar increases in food consumption.[1]

Despite the clear theoretical predictions, the empirical evidence on how much SNAP increases food consumption is mixed. Most studies have focused on estimating the marginal propensity to consume food (MPCf) out of SNAP to measure the effect of SNAP benefits on total food consumption. The MPCf out of SNAP indicates how much food expenditures rise in response to a $1 increase in SNAP benefits. As shown in Figure 1, from the beginning of the program until the early 1990s, the evidence suggests that the MPCf out of SNAP was near .1, which is in line with most estimates of the MPCf out of cash (Moffitt, 1989; Schanzenbach, 2002; Hoynes and Schanzenbach, 2009). In other words, inframarginal consumers treated a cash transfer and SNAP benefits in the same way. However, a new wave of studies focusing on consumers in the late 2000s and early 2010s finds the opposite result: SNAP induces much more food consumption than an equivalent cash transfer (Collins et al., 2016; Beatty and Tuttle, 2015; Bruich, 2014; Hastings and Shapiro, 2018).

Methodological differences between the two sets of studies are insufficient to explain the increase in estimated MPCfs over time. In each sample period researchers have used both experiments (Moffitt, 1989; Schanzenbach, 2002; Collins et al., 2016) and exogenous policy changes (Hoynes and Schanzenbach, 2009; Beatty and Tuttle, 2015; Bruich, 2014; Hastings and Shapiro, 2018) to identify the causal effects of SNAP on food consumption. Moreover,

[1] 76 percent of SNAP households are inframarginal in my sample and this proportion is relatively stable over the sample period.

different methodological approaches within the same sample period provide remarkably consistent estimates.

While there are a number of models that seek to explain this deviation from standard demand theory there is no direct evidence of what caused consumers to begin responding to SNAP benefits in a different way. To help resolve this puzzle I provide the first evidence for a cause of the increase in the MPCf out of SNAP: the adoption of Electronic Benefit Transfer (EBT) cards. In the past, SNAP benefits came in the form of physical coupons, but some states began issuing them via Electronic Benefit Transfer (EBT) cards starting in the 1990s. EBT cards function like a standard debit card with benefits loaded into a recipient's account and redeemed at participating retailers. The transition to EBT cards from paper food stamps occurred from 1993-2005, in between the two periods for which we have reliable estimates of the MPCf out of SNAP.

I use the Current Population Survey's (CPS) Food Security Supplement (FSS) to estimate the effect of EBT cards on the food consumption of SNAP recipients. The FSS is a nationally representative survey with detailed information on household food spending and SNAP benefits. To track EBT adoption, I use the fraction of SNAP recipients using EBT in each state, as reported by the USDA's SNAP policy database.

To identify how much EBT changes the MPCf out of SNAP I exploit plausibly random variation in EBT adoption over states and time. This rollout approach is a common method for studying the effects of transfer programs (e.g., Bailey, 2012; Hoynes and Schanzenbach, 2009; Hoynes, Schanzenbach and Almond, 2016) and has been used by recent studies of EBT (Wright et al., 2017; Kuhn, 2018). EBT adoption is likely to be exogenous because it was driven by federal mandates and administrative constraints and not implemented in response to food consumption trends in individual states. Additionally, since every state eventually adopted EBT there is no unobserved selection into the program. Moveover, the exact timing of the rollout is unrelated to funding levels because benefits are set nationally each year by the federal government. Finally, the transition to EBT occurred during a broader period of welfare reform. I adjust for these changes by excluding populations whose eligibility changed substantially (e.g., non-citizens) and adjust for the level of non-SNAP federal transfers to each state. Additionally, Kuhn (2018) finds no evidence of sharp changes in TANF enrollment that coincide with EBT adoption.

To test for possible policy endogeneity directly I examine whether state characteristics at the time of the mandate (1996) are related to the timing of EBT adoption. Specifically, I regress either the first year with EBT coverage and the time it takes the state to attain cover every

SNAP recipient on a variety of state demographic characteristics in 1996, the level of non-SNAP transfers to the state, SNAP participation, the average level of food consumption, and region fixed effects. I find that none of these characteristics have a statistically significant relationship with adoption and their estimated impacts are small. Placebo tests find no evidence that other changes coinciding with EBT adoption or differential pre-trends are driving the results.

While the introduction of EBT is likely exogenous, it may endogenously change selection into the program by reducing stigma or administrative barriers. In this case, estimates of the effect of EBT may be picking up changes in the composition of the SNAP population rather than EBT's causal effect on behavior. To address this issue, I present evidence that EBT adoption is not associated with any changes in several observable characteristics of the SNAP population nor changes in the probability a household participates, state-level participation rates, and state-level take-up rates. Moreover, the broader literature on the effects of EBT adoption on selection into the SNAP program is mixed (see Kuhn, 2018 for review).

It is important to note that this identification strategy does not provide exogenous variation in the level of benefits. Consequently, it cannot identify the MPCf out of SNAP benefits. However, it does identify changes in the MPCf out of SNAP due to EBT. This approach can shed light on why the MPCf out of SNAP is increasing over this time period. Additionally, even though the main effect of SNAP benefits is not identified, the estimated effect is close to more identified estimates that use pre-EBT samples.

I find that the transition to EBT substantially increased the MPCf out of SNAP. I estimate that the introduction of EBT increased the MPCf out of SNAP by .124. In other words a $1 increase in SNAP benefits for an EBT household would translate into an additional $.13 of food expenditures relative to a household using paper coupons. This increase nearly doubled the MPCf out of SNAP based on a pre-EBT MPCf out of SNAP of .1. Moreover it can explain about 25 to 30 percent of the observed increase in the estimated MPCf out of SNAP between 1990 and 2010.

The estimated effects of EBT on the MPCf out of SNAP are consistent across groups and I find no evidence of heterogeneous effects of EBT adoption across important subpopulations. In particular, I cannot reject equality of effects for all nonelderly SNAP households, those with no college education, non-white headed households, female-headed households, or households with children. These results are unrelated to changes in other transfer programs and are robust to including state linear time trends, and a rich set of state and household characteristics.

In addition to the effects on the MPCf out of SNAP, which is the main focus on the literature, I also present results on EBT's effect on overall food consumption. While it's effect on the MPCf implies an increase the total effect is reduced complicated by a negative, but not statistically significantly different from 0, level effect of EBT on food expenditures. The point estimate of -48 implies that EBT had reduced total food expenditures for recipients with benefits below the 75^{th} percentile. However, the estimate of the level effect is very imprecise likely due to the relative lack of observations with near 0 benefits. While changes in the effects on the MPCf are the focus of this paper, identifying whether and why EBT may lower overall food expenditures for low-benefit households may be an important avenue for understanding the relationship between payment methods and transfer programs.

There are several potential mechanisms by which the shift from paper food stamps to EBT cards may have increased the MPCf out of SNAP. First, the introduction of EBT may have increased the complexity of household budgeting, leading consumers to adopt a mental accounting heuristic which makes it easier to budget but impairs fungibility. This explanation is consistent with Hastings and Shapiro (2018)'s preferred explanation for the high MPCf out of SNAP that they measure in a retail panel. Second, Kuhn (2018) argues that EBT cards more clearly define property rights over SNAP benefits, which gives the main recipient more leverage in household bargaining. I find some evidence that large households, in which the primary recipient likely holds less bargaining power, saw larger increases in the MPCf out of SNAP after EBT introduction.

I rule out two alternative explanations for the effect of EBT: a reduction in stigma and rates of fraud. EBT cards may have reduced the stigma of the program because the cards are more similar to credit and debit cards than paper stamps are to cash. If the reduction in stigma causes households with a greater propensity to purchase food to select into the program, then the average MPCf out of SNAP will increase. However, I find no evidence that EBT changed selection into the program or the composition of the SNAP population. It is also possible that EBT made SNAP fraud harder. Households that spend less on food than they receive in SNAP benefits may try to convert their benefits into cash through fraudulent activities. If EBT cards reduce fraud, then these households may use their benefits to purchase more food. However, prior studies of EBT rollouts find it does not meaningfully reduce fraud (e.g., ABT, 2002). Additionally, I find no effect of EBT on the food expenditures of the households most likely to be engaged in fraud.

This work highlights an unintended consequence of the transition to electronic benefit payments. EBT was adopted to reduce administrative costs but it also made recipients more responsive to benefit increases. This paper is the first to provide evidence that the transition from paper food stamps to EBT affected the relationship between SNAP benefits and food expenditures.

The observed increase in the MPCf out of SNAP suggests that there are important frictions that may enable in-kind transfers to alter consumption choices more than previously thought. Understanding why the MPCf out of SNAP changed can inform the design of transfer programs to increase food consumption without increasing program costs. For example, the design of future payment systems via apps or smartphones can directly impact the purchasing behavior of SNAP recipients. In addition to food assistance, this work applies to the large variety of in-kind transfer programs including housing assistance, education, and health care.

However, additional research is needed to determine the effect of increasing food consumption on health and economic outcomes. On the one hand, better nutrition is associated with a wide variety of beneficial outcomes including better infant health and higher educational attainment (e.g., James et al., 1997; Glewwe, Jacoby and King, 2001) and exposure to SNAP in childhood increases health and economic outcomes in adulthood (Hoynes, Schanzenbach and Almond, 2016; Bitler and Figinski, 2019; Bailey et al., 2019). On the other hand, by raising the MPCf out of SNAP you are, by definition, lowering the amount of other goods the household purchases. Doing so may mitigate the beneficial effects of expanded nutrition, especially if households are already optimizing their purchases. In other words, raising the MPCf out of SNAP may be welfare enhancing if households systematically underinvest in food. They may do so if the benefits of increased food consumption are only realized over the long-term and, consequently, these benefits are not known to the household. However, if households are not underinvesting in the benefits of food consumption then raising the MPCf out of SNAP will reduce welfare by pushing households away from the optimal consumption bundle. Moreover, understanding how and when the MPCf is changing and its impacts on the effect of the program will inform how to best extrapolate past estimates of the program's effects to today.

1 Background

SNAP began as the Food Stamp Program in the mid-1960s to provide a baseline level of nutrition for the poor and to indirectly subsidize farmers.[2] SNAP has grown from a few pilot counties to the nation's second-largest in-kind transfer program. Recipients receive a monthly allotment of benefits that can be used to purchase food at retailers for a market rate. The USDA sets benefit levels based on the cost of the Thrifty Food Plan (TFP), adjusting for family size. The TFP is a meal plan designed to be the cheapest way for a family to meet their basic nutritional needs. Benefit amounts are adjusted for inflation each October and the TFP is updated periodically.

SNAP is one of the only federal safety net programs not restricted to a given demographic group: anyone who satisfies the income, asset, and work requirements are eligible for benefits. Eligibility and benefit levels are determined at the household level, with some exceptions for elderly household members. A household receives SNAP benefits equal to the TFP minus 30 percent of household income net of certain deductions. Deductions adjust for family income that is not available to be spent on food. For example, in addition to a standard 20 percent deduction, some states allow you to deduct dependent care expenses and shelter costs.

SNAP has been tied to a variety of beneficial outcomes. For instance, the expansion of SNAP to a given county lowered infant mortality rates (Currie and Moretti, 2008; Almond, 2011). Additionally, the rollout of the program has been associated with long-term improvements in health and economic outcomes (Hoynes and Schanzenbach, 2016).

1.1 Prior estimates of the MPCf out of SNAP

SNAP's inclusive eligibility requirements coupled with the lack of state-level variation in benefit levels, makes it hard to identify the MPCf out of SNAP. Early studies of the program made cross-sectional comparisons of participants and non-participants (for review see Fox, Hamilton and Lin, 2004), while controlling for observable characteristics. Since it is likely that there is unobservable selection into the program that is positively correlated with food consumption, these studies provide upward biased estimates of the program's impact on food consumption (Currie, 2006).

Another group of studies uses field experiments to estimate the impact of SNAP on food consumption. In the 1980s and 1990s officials conducted four cashout experiments in which they randomly gave some SNAP recipients the cash value of their usual SNAP benefits.

[2]Much of this section on the history of EBT and SNAP comes from the USDA Food and Nutrition Service: https://www.fns.usda.gov/snap/short-history-snap

Studies of these field experiments typically find no statistically significant difference between the MPCf out of cash and the MPCf out of food stamps for inframarginal households (Fox, Hamilton and Lin, 2004; Moffitt, 1989; Schanzenbach, 2002). In 2016, the USDA conducted a randomized evaluation of the Summer Electronic Benefit Transfer for Children. They found that the average family in the treatment group had an MPCf out of SNAP of .58, substantially higher than the MPCf out of cash (Collins et al., 2016).

To solve the selection problem, other studies use plausibly exogenous variation in SNAP availability or benefit levels. Hoynes and Schanzenbach (2009) study the rollout of the program across counties in the late 1960s and 1970s. They find a similar MPCf out of cash and SNAP and cannot reject their fungibility. More recent studies investigate benefit expansions and reductions associated with the American Recovery and Reinvestment Act (ARRA) and the Great Recession. These studies find larger MPCfs out of SNAP (around .5-.6) and can reject fungibility between cash and SNAP benefits.

Finally, Hastings and Shapiro (2018) use retail data from 2007-2013. They estimate the MPCf out of SNAP by exploiting plausibly exogenous variation in SNAP spell lengths and an IV approach. They then estimate a variety of behavioral structural models and find that the mental accounting model best fits the data. In each case they find an MPCf out of SNAP of .5-.6 and can reject fungibility between cash and SNAP.

As depicted in Figure 1, studies that focus on the program before the 1990s estimate a substantially lower MPCf of SNAP than those that focus on the 2000s and 2010s, regardless of methodology. For example, experimental evaluations in the early period typically find that the MPCf out of SNAP is 0 to .15 points larger than the MPCf out of cash, though the difference is not statistically significant. Since estimates of the MPCf out of cash during this period are around .1, it is reasonable to assume that these studies estimate an MPCf out of SNAP of .1-.25. Collins et al. (2016) perform a randomized experiment in the later period and find that the MPCf out of SNAP is .58.

1.2 EBT rollout

The Hunger Prevention Act of 1988 established several pilot projects to test EBT systems for the delivery of SNAP benefits. EBT was an attractive system for states and the federal government as it reduced expected administrative costs and losses due to fraud. A study of the Maryland pilot programs in 1994 found that switching to EBT reduced administrative costs, losses during the administration of the benefits (e.g., coupons stolen in the mail), and benefit diversion (Kirlin and Inc., 1994). The 1993 National Performance Review Report estimated that much of the $400 million in annual SNAP administrative costs could be saved by no longer printing "3 billion food stamps... [and distributing] them to more than 10 million households" only to have them eventually destroyed by the Federal Reserve. The same review estimated that savings to the Federal government from EBT would be $1 billion over the next five years (Gore, 1993, see pages 112-120).

Recognizing that EBT would reduce federal administrative costs and benefit diversion, Congress mandated the adoption of EBT systems in the 1996 welfare reform bill: The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). All states were required by PRWORA to implement EBT systems by 2002 unless granted a waiver. Four states, Maryland, Texas, South Carolina, and Utah adopted EBT before PRWORA was passed and by 2005 all states had finished rolling out their EBT systems.[3] This rollout period occurred between the two sets of estimates of the MPCf, highlighted in Figure 1. This convenient timing makes it a possible explanation for the increase in the MPCf over this time period.

After the mandate, states had the ability to choose both when to start implementing their EBT programs and how long they would take to cover their full SNAP population. Figure 2 plots the year in which each state achieved full EBT coverage, using data from SNAP policy database, which tracks the fraction of SNAP recipients using EBT in each state. In many states every SNAP recipient used an EBT card within three years of the Federal mandate. However, as seen in Figure 3, a third of SNAP recipients did not use EBT until after 2000. Complete coverage nationwide was not achieved until 2005, three years after the original deadline.

There was also considerable within state variation in EBT coverage. Some states opted to roll out the program to their entire SNAP population within a year, while others took almost eight years to do so. Wright et al., 2017 study the rollout of EBT in Missouri at the county level. They find that the delay between the initial rollout and full coverage was due to the fact that Missouri first ran a pilot program in larger counties and then rolled out the program to smaller ones over the next year. While most states do not provide county level data, it appears that other states followed a similar strategy. Figure 4 plots EBT coverage over time for all states and figure 5 provides a closer look at four states with different rollout patterns. While the rates of EBT expansion differ, states typically followed a similar pattern to Missouri: in the first year they expand coverage to a portion of SNAP recipients and then in the next few years expand to cover everyone in the state. Other states, such as Iowa,

[3] Iowa and California were granted waivers that extended the implementation deadline. In five other states nearly all SNAP recipients used EBT in 2002, but they didn't achieve 100 percent usage until 2005.

rolled out EBT to only a small fraction of SNAP recipients for several years before rapidly achieving full coverage.

Having many implementation dates both across and within states makes it less likely that EBT coverage occurred at the same time as other large shocks to food consumption in a systematic manner. Moreover, as the example in Missouri demonstrates, within state rollouts reflected administrative considerations such as population size and the ability to run a pilot program.

To explicitly test whether differences in state adoption patterns are related to food consumption I regress the year a state started its EBT rollout on state characteristics at the time of the mandate (1996) and report results in Table I.[4] The start year is the first year with any SNAP recipients in a state using EBT. I run the same regression using the time it took to achieve full coverage as the dependent variable. In each regression I include a number of state characteristics which might be related to food consumption and the EBT rollout patterns including, the log of state population, the percent of the state that is non-white, over 65 years old, less than 5 years old, and on SNAP, as well as the unemployment rate and the average real monthly food expenditure per household. Each state is weighted by its population in 1996 and I exclude the few states that began their EBT program before 1996.

I find that state characteristics at the time of the mandate do not predict either start year nor implementation time. I present results on whether state characteristics in 1996 are related to a state's start year in columns 1 and 2 of Table I. None of the state characteristics are significantly related to the start year and the model explains very little of the underlying variation even after adding region fixed effects in column 2. Columns 3 and 4 present results on whether state characteristics in 1996 are related to how long it takes a state to finish rolling out the program once they have started. The results suggest that states which had shorter implementation times had populations that were more nonwhite, older, and consumed more food. These relationships are no longer statistically significant at conventional levels once I adjust for regional fixed effects in column 4.

Two existing papers have found beneficial outcomes associated with EBT adoption using a rollout strategy. Wright et al., 2017 examine the impact of EBT adoption on crime. They track EBT adoption across counties in Missouri and compare outcomes to neighboring counties in nearby states. They find that EBT adoption caused significant reductions in overall crime rates and attribute this effect to people carrying around less cash.

[4] This is a common method of testing the exogeneity of rollout timing (e.g., Hoynes and Schanzenbach, 2009; Bednar, 2011)

Kuhn, 2018 investigates how EBT cards affect consumption smoothing across the benefit cycle using Diary information from the Consumer Expenditure Survey (CES). Many SNAP households spend a large amount of their benefits right after receiving them and then face a "calorie crunch" at the end of the benefit month. Kuhn, 2018 finds that EBT adoption causes larger households to spread their food expenditures out more evenly across the benefit cycle. Additionally, he concludes that EBT provides a clearer delineation of property rights over SNAP benefits which mitigates the present-bias that is generated by aggregating preferences across household members. Kuhn, 2018 also uses plausibly exogenous variation across states and time to identify the effects of EBT but does not have the statistical power to find evidence of an increase in overall food consumption. I estimate the effects of EBT on the level of food consumption more precisely by using a survey with a larger sample size and a continuous treatment variable, described below.

2 Expected effects of EBT on the MPCf out of SNAP

I present a version of the model found in Hastings and Shapiro (2018) to help describe how EBT may alter the effects of SNAP benefits on food spending.

In each month t let household i choose food spending f_{it} and non-food spending n_{it} to solve the following maximization problem

$$\max_{f_{it}, n_{it}} U_i(f, n; \eta_{it}) \tag{1}$$

subject to

$$n \leq y_{it} - \max(0, f - b_{it}) \tag{2}$$

where y_{it} is monthly income and b_{it} is monthly SNAP benefits. η_{it} is a preference shock and $U(\dot{})$ is a utility function that is strictly increasing in f and n.

For simplicity, let U_i be the Cobb-Douglas utility function

$$U_i(f, n, \eta) = (f - \eta)^{\theta_i}(n + \eta)^{1-\theta_i} f \geq \eta \geq -n \tag{3}$$

Then the household's food expenditure is:

$$f_{it}(y_{it} + b_{it}, \eta_{it}) = \theta_i(y_{it} + b_{it}) + \eta_{it} \tag{4}$$

To allow for the possibility that SNAP benefits have an additional impact on food expenditures we can add an excess sensitivity parameter:

$$f_{it}(y_{it} + b_{it}, \eta_{it}) = \theta_i(y_{it} + b_{it}) + \gamma b_{it} + \eta_{it} \tag{5}$$

In this formulation, γ represents the "excess sensitivity" of food expenditures to SNAP benefits. The MPCf out of SNAP is $\theta + \gamma$ and the MPCf out of cash is θ. In the Southworth model $\gamma = 0$ for all inframarginal households. This implies that the MPCf out of cash is the same as the MPCf out of SNAP. Studies which cannot reject equality of these two MCPfs implicitly estimate a $\gamma = 0$. In this framework, the rise of the MPCf out of SNAP over time is captured by an increasing γ.

The adoption of the EBT system may be increasing γ, and the MPCf out of SNAP, either by inducing households to adopt a mental accounting heuristic or by changing the outcome of the household's bargaining process. First, EBT adoption may induce consumers to adopt a mental accounting heuristic by increasing the complexity of household budgeting. Mental accounting can be thought of as a decision-making heuristic that reduces budgeting effort but fails to achieve the optimal consumption bundle. In Hastings and Shapiro (2018)'s mental accounting model each consumer has a distaste for deviating from the planned allocation. One interpretation is that this distaste represents the disutility from the effort involved to further optimize. This distaste produces a wedge between the optimal allocation and the one chosen using mental accounting. Hastings and Shapiro (2018) estimate a γ that is around .5-.6. and reject the null hypothesis that it is 0. They find that a model which includes mental accounting rationalizes their results.

EBT cards may plausibly increase the effort involved in optimizing; making it harder to properly substitute between cash and SNAP benefits. EBT cards increase the complexity of a recipient's financial resources. Instead of having paper coupons in their wallet next to paper dollars, recipients now have a separate food stamp account in addition to their cash reserves and other accounts. The cards also make it substantially harder to keep track of the amount of SNAP benefits you have while shopping. In an EBT system, recipients can only check their balance via a website, phone, at the register after a purchase, or at an ATM depending on the state. This requirement was especially burdensome around the time of the initial rollout when home computer use was low, especially for low-income individuals, and internet-connected smartphones had yet to be invented.

Finally, EBT cards have lower payment transparency which makes it substantially more difficult to stick to your planned budget. Payment transparency describes the salience associated with different payment types. Cash has the highest payment transparency, whereas electronic payments, such as credit cards or EBT cards have much lower payment transparency. In experiments, participants using less transparent forms of payment spent more than they had planned (Soman, 2003; Raghubir and Srivastava, 2008). The reduced transparency makes it more difficult to stick to the optimal bundle, further increasing the effort required to perfectly substitute cash and SNAP benefits.

Second, EBT may increase γ because it provides the primary recipient more control over the benefits. Compared with paper coupons, EBT-based benefits are more strongly tied to the primary recipient. In an EBT system the primary recipient's name is on the card itself and benefits are loaded directly onto the card without notifying other family members. Moreover, the primary recipient must authorize access to the benefits by sharing a PIN, which they can easily change, or by ordering additional EBT cards. Kuhn (2018) finds evidence that EBT's consumption smoothing effects operate by increasing the bargaining power of the primary recipient. Greater bargaining power allows the primary recipient to steer consumption towards their preferences. For example, Lundberg, Pollak and Wales (1997) find that when U.K. child benefits are distributed to the mother, expenditures on women's and children's clothing increases substantially, even after holding total family income fixed. In a similar way, EBT may increase household food spending if the primary recipient has a greater preference for food spending than other household members.

2.1 Welfare effects

The introduction of EBT raises food expenditures above the neoclassical optimal level by increasing γ. However, the welfare implications of increasing γ are ambiguous. To demonstrate this, let $V(f)$ be the consumer's experienced utility, the utility she actually receives from a given level of food consumption and let $U(f)$ indicate the consumer's decision utility, the utility the consumer thinks she will get. Following Chetty (2015) we can write the experienced utility as:

$$V(\gamma) = v(f(\gamma)) = U(\gamma) + v(f(\gamma)) - u(f(\gamma)) \tag{6}$$

Here experienced utility is the decision utility plus the difference between the decision utility and the experienced utility. The change in utility as the policymaker changes γ is then:

$$V'(\gamma) = U'(\gamma) + \frac{df}{d\gamma}(v_f - u_f) \tag{7}$$

The first term represents standard welfare and the second term is the change due to changes in the consumer's decision-making induced by the policy. Since γ increases food expenditures above the optimal point for the consumer $U'(\gamma)$ is likely negative. The effect of increasing food expenditures on the second term depends on the relative magnitudes of v_f and u_f. If consumers under-value their experienced utility, then $v_f > u_f$ and the increase in food consumption raises welfare. If consumers over-value experienced utility, then $v_f < u_f$ and it lowers welfare.

The neoclassical model assumes that consumer's are maximizing experienced utility, that is, $U(\gamma) = V(\gamma)$. Consequently, increasing food expenditures by increasing γ lowers overall welfare. In this case it is plausible that households systematically undervalue their experienced utility since benefits from increased nutrition may take years to be realized. Recent work finds that childhood exposure to SNAP has substantial effects on health and economic outcomes 20-30 years later (Hoynes, Schanzenbach and Almond, 2016; Bitler and Figinski, 2019; Bailey et al., 2019). However, it is an open question whether the benefits from SNAP exposure are due to the increase in food consumption or an increase in other types of consumption. If food consumption is not driving these benefits then consumers may not be undervaluing experienced utility. In this case, raising the MPCf out of SNAP will distort decisions away from the optimal bundle and lower overall welfare.

3 Data

To investigate how the introduction of EBT affects food consumption I use the Food Security Supplement (FSS) of the Current Population Survey (CPS) to measure monthly food expenditures and SNAP participation from 1996-2010. The CPS is a nationally representative survey administered each month. The FSS is administered once a year starting in 1995. The exact survey month varied before 2001 but it has been administered in December consistently since 2001. The FSS asks respondents to report their usual weekly expenditures on food, including food purchased with SNAP benefits, in the last week, and their monthly SNAP benefit amount. To create a consistent measure of food expenditure and SNAP benefits, I convert the usual weekly expenses to monthly expenses by multiplying by 4. Additionally, I use the Consumer Price Index for food from the Bureau of Labor Statistics (BLS) to convert all nominal measures to into 2017 dollars. Finally, I use the FSS weights supplied by the CPS to take into account the sampling design of the survey.

In addition to the food expenditure variables, the CPS contains a variety of information on the household characteristics of the family. I can control for household demographics reported in the CPS that may be correlated with SNAP receipt and food consumption. I adjust for the head of household's years of education, employment status, and race. To non-parametrically control for family food needs, I include fixed effects for the number of children and adults in the household.

SNAP benefits are determined by household income but the FSS only reports the family income of the head of household within pre-defined categories. In 95 percent of my sample the household consists of only one family, so the household and family income measures are the same and the results are unchanged when I restrict the sample to single-family households. The CPS-FSS only reports income brackets for the full sample. To account for inflation, I control for family income by interacting income bracket dummies with a vector of year dummies. This method allows for a non-linear relationship between food and income that adjusts flexibly over time.

Prior papers studying the relationship between SNAP and food consumption track EBT adoption using state-specific county rollouts (Wright et al., 2017) or the month and year in which the state reports its EBT program is operational statewide (Kuhn, 2018). The first method can only look at the few states that implemented and reported county rollout patterns. The second method uses national data but ignores the sometimes-lengthy period in which some, but not all, SNAP recipients in a state are using EBT cards. While most states reach full EBT coverage within one year of starting the program, substantial portions of the population receive EBT cards in the months preceding complete coverage. Additionally, several states take more than one year to reach state-wide adoption and three states take up to nine years to do so. Ignoring the ramp-up in EBT usage introduces additional measurement error into the main treatment variable. To avoid these issues, I use the SNAP Policy Database maintained by the USDA. This database contains information on state-level policy variation in SNAP administration from 1996 to 2011, including the percent of SNAP recipients using EBT cards at the state-year-month level.

Finally, the bulk of the EBT rollout occurred alongside a broader effort to reform the safety net in the U.S. I check for the potentially confounding effects of welfare reform by adding gross federal transfers to each state, including Social Security, Medicare, and TANF payments from the Regional Economic Information System (REIS) administered by the Bureau of Economic

Analysis (BEA). Additionally, I control for demographic shifts within states over time that may be correlated with EBT rollout and food consumption by including the log of total population, the unemployment rate, and the percent of the state that is under the age of 6, over the age of 65, and non-white using information from the National Institutes of Health Surveillance, Epidemiology, and End Results Program (SEER) and the Bureau of Labor Statistics.

I eliminate non-citizens from my sample since their eligibility for SNAP changed substantially during the study period. I also restrict my analysis to the contiguous 48 states because Alaska and Hawaii have higher food prices as well as different SNAP benefit formulas. Finally, I remove households who report spending less than \$2 per week or are in the 99^{th} percentile of weekly food expenditures as these observations are probably due to measurement error.[5] I also remove observations that are missing any of the variables used in the analysis.

My final sample consists of 406,091 household-level observations taken from 1996-2010, with about 20,000 households per year. The average household spends \$593.23 on food each month. About 5 percent of households are receiving SNAP benefits during the month in which they are surveyed.

My main estimates use a SNAP-only sample consisting of 20,562 households.[6] SNAP households receive an average benefit amount of \$282.55 and spend \$462.64 on food each month. SNAP benefits account for about 61 percent of total food spending for the average SNAP household. Moreover, 24 percent of SNAP households spend less on cash food than they receive in SNAP benefits (i.e., they are not observed to be inframarginal). These estimates are similar to prior estimates of the fraction of SNAP households that are not inframarginal both in the 1970s and in later periods (Johnson et al., 2018).

4 Model

I estimate the following model using the CPS data described above from 1996-2010:

$$\begin{aligned}\text{Food}_{\text{ist}} \; = \; & \beta_0 + \beta_1 \text{EBT}_{\text{st}} + \beta_2 \text{Benefits}_{\text{ist}} + \beta_3 \text{EBT}_{\text{st}} * \text{Benefits}_{\text{ist}} + \\ & X_{\text{ist}}\gamma + A_{\text{st}}\rho \; + \delta_s + \tau_t + \varepsilon_{\text{ist}}\end{aligned} \tag{8}$$

[5] These restrictions are similar to those used in Hoynes and Schanzenbach (2009) and eliminate only a small proportion of the sample. The results are robust to varying the size of the lower cutoffs, as presented in Appendix Table A.I.

[6] The results are robust to including in the sample all families with low incomes, those with a high propensity to receive SNAP, or using the full sample after adjusting for the non-linear effect of moving from 0 to positive SNAP benefits on food consumption.

where $\text{food}_{\text{ist}}$is real monthly food expenditures in dollars for household i in state s in year t, EBT_{st} is the fraction of a state's SNAP recipients using EBT cards in a given year[7], and $\text{Benefits}_{\text{ist}}$ is the level of real monthly SNAP benefits each household receives. $X_{\text{ist}}$is a vector of household characteristics which account for changes in the composition of SNAP households over time. I include information on the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. X_{ist} also contains a vector of income bracket dummies equal to one if the household is in that income bracket and zero otherwise. To adjust for inflation, I also include interactions between each bracket dummy and year dummies τ_t. A_{st} is a vector of state characteristics including the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. δ_s is a vector of state dummies, which adjusts for time-invariant differences between states and ε_{ist} is an idiosyncratic error term.

In a level-level model the coefficients on income and SNAP represent the MPCf out of SNAP and cash, respectively. β_1 represents the level effect of EBT on food consumption, or the effects that operate through channels other than benefits. Since SNAP benefits and food expenditures are in levels, β_2 represents the MPCf out of SNAP before EBT is implemented (from the theoretical model: $\theta_{pre} + \gamma_{pre}$), and β_3 represents the change in the MPCf out of SNAP associated with EBT adoption ($\theta_{post} + gamma_{post} - \theta_{pre} + \gamma_{pre}$). If we assume that $\gamma_{pre} = 0$, as prior estimates suggest, and $\theta_{post} = \theta_{pre}$ (i.e., no effect on cash expenditures), then $\beta_3 = \gamma_{post}$, the primary parameter of interest. This specification captures the main features of the model: 1) it allows the MPCf out of cash and SNAP to differ and 2) it allows the MPCf out of SNAP to vary with EBT adoption.

Because the dependent variable, food expenditures, is non-negative it would have been natural to assume a specification that respects this restriction. In this case, however, the non-negativity restriction is less of a concern because the dependent variable does not cluster at 0, and, I eliminate households with food consumption close to zero because they are probably due to misreporting, following Hoynes and Schanzenbach, 2009.

[7] Rather than using a continuous treatment, treatment status could be assigned based on whether the state has started its rollout (start treatment) or whether it has achieved 100% coverage (end treatment, as in Kuhn (2018). I present results for each treatment type in Appendix Table A.II. The effect of EBT is no longer statistically significantly different from 0 when using the end treatment though none of the estimates are statistically distinguishable. The point estimates may differ for two reasons. First, by transforming the treatment into a binary variable some states that increase their EBT coverage will be in the "control" group leading to attenuation bias. Second, the start (end) treatment identifies a local average treatment effect (LATE) for those that receive EBT first (last). If SNAP recipients who receive EBT first have a larger response then the start treatment will yield a larger estimate compared to the continuous and end treatments. In this case, the continuous treatment provides a weighted average of the LATEs.

This rollout design is a common approach used to evaluate the impact of social programs, including SNAP and Medicaid. This linear fixed effect model can be thought of as a generalized differences-in-differences (DD) strategy. In this approach, I exploit the timing of EBT transitions to identify β_3. States that transition to EBT in a given year are the treatment group and those that do not transition, either because they already have full EBT or continue to have no EBT coverage, serve as the counterfactual control group. By comparing within state changes to changes in other states, this approach helps control for time trends across the sample period and selection into the program.[8] Since my policy variation is at the state-level, I cluster the standard errors by state (Bertrand, Duflo and Mullainathan, 2004; Abadie, Imbens and Wooldridge, 2017)

β_3 is identified if trends in real food expenditure for the treatment and control groups are the same before EBT adoption and there are no contemporaneous shocks that differentially affect the treatment or control groups. If the pre-trends were different then the control group trend will not be a good estimate of what would have happened to the treatment group in the absence of treatment. The no contemporaneous shocks assumption rules out any level shift that occurred at the same time as the switch to EBT. If another event occurred at the same time, then it is impossible to separate the two effects. Since there is no exogenous variation in benefit levels, β_2 is not identified. However, by controlling for benefit levels I am comparing households with EBT to those without EBT but have the same level of SNAP benefits. By including it in the model, I am able to adjust for an array of factors that influence benefits and food expenditures (e.g., assets).

The nature of the EBT rollout makes each assumption more likely. First, the rollout was mandatory for each state, which reduces the likelihood that policy adoption is related to underlying differences between treatment and controls. Second, idiosyncratic administrative factors likely drove the exact timing of adoption, not the characteristics of the state's SNAP population. To check these assumptions, I run a series of placebo tests by investigating how the transition to EBT affected untreated groups such as non-SNAP recipients and those that are not inframarginal. If the treatment has an effect on these control groups, then it is likely that something else is driving the observed effect of EBT. I also relax the common pre-trends assumption by introducing state-specific linear time trends. In each case I find evidence that the identification assumptions hold.

My identification strategy depends on comparing within-state changes in treatment states

[8] Using the panel structure of the CPS I also estimate individual fixed effects models. The results are consistent with those presented below. However, the timing of the FSS only permits linking individuals after 2000, eliminating nearly 2/3s of EBT transitions, which makes substantially reduces the precision of the estimates and makes it hard to compare them.

(those that increased their EBT coverage) to within-state changes in control states (those that did not). The control states will not be a valid counterfactual if the underlying SNAP population is changing due to EBT adoption.

EBT cards may alter the composition of the SNAP population by reducing the stigma associated with the program. This reduction in stigma may induce households to participate in SNAP that otherwise would not. If these new households have different consumption preferences, then the average MPCf out of SNAP may increase without changing the MPCf out of SNAP for households who would have participated anyway. I address this issue by including a variety of household characteristics in the main specification to adjust for any observable changes in the population. I also test for observable and unobservable changes in selection into the program due to EBT adoption in section 6. I find no evidence that EBT altered selection into the program either due to stigma or other effects.

5 Results

I begin by estimating the model on the entire sample of SNAP households with non-elderly heads and report results in Table II.[9] Limiting the sample to households that participate in SNAP ensures that the control variables, including the year and state fixed effects, are identified by similar observations and the estimates are not affected by unobserved factors that affect selection into the program. Comparing SNAP households to each other, rather than non-SNAP households, is particularly important given the relatively low rate of SNAP households in the sample and possible heterogeneity in the effect of economic shocks.

I first estimate the equation only adjusting for state and year fixed effects. In column 2, I add in the household characteristics, including household structure, to adjust for any changes in the composition of households over time. In column 3, I add the family income dummies, as family income partially determines benefit levels and is related to household food consumption. In column 4 I add the state characteristics and in column 5 I relax the common pre-trends assumption by including state-specific linear time trends. Since the results without state-specific time trends are similar to those in column 5, and have more straight-forward identification assumptions, I use the specification in column 4 as my main specification.

The estimates in column 4 indicate that EBT increased the MPCf out of SNAP. The coefficient on Monthly SNAP Benefits X Percent EBT represents the change in the MPCf out of

[9]The results are not significantly different when I restrict the sample to households that are observed to be inframarginal (i.e., they spend more on food than they receive in SNAP benefits).

SNAP caused by the transition to EBT, the main parameter of interest. It implies that the transition to EBT increased food spending by 12.7 cents for every dollar in SNAP benefits. Based on the assumptions above, this result implies that gamma, the excess sensitivity of SNAP benefits, increased from 0 to .127.

The coefficient on SNAP represents the MPCf out of SNAP before the switch to EBT. I estimate that an additional dollar of SNAP benefits is associated with $0.143 in additional food spending. This estimate is similar to other pre-EBT estimates of the MPCf out of SNAP. Moreover, consistent with fungibility between cash and SNAP benefits before the switch to EBT, I cannot reject the hypothesis that it is equal to common estimates of the MPCf out of cash. However, the estimate of the pre-EBT MPCf out of SNAP is not well-identified. First, it is probably driven by income variation within income bracket. Omitting more precise income measures may bias this coefficient in two ways. First, a household in the same bracket but with a higher income will receive fewer SNAP benefits but purchase more food. This relationship will lead to a downward bias on the estimate. Second, even if reported gross income is identical between households, their income net of deductions may be different. Since benefits are assigned based on net income, households with a greater need for food may boost their benefits by maximizing their deductions. Doing so may introduce a positive bias to the estimate.

The coefficient on EBT represents the level effect of EBT and it is strongly negative. While not statistically significant the point estimate indicates that EBT caused food consumption to drop by about $48. Taking into account both the level effect and the increase in the excess sensitivity of SNAP benefits implies that the overall effect of EBT on food expenditures is negative for those who receive less than $417 in monthly benefits (the 75$^{\text{th}}$ percentile of benefits). To put this effect into context, the average monthly benefit amount was $283. Consequently, while EBT increased the MPCf out of SNAP it decreased the average SNAP household's overall monthly food expenditures by about $16. This result should be interpreted with caution given the amount of noise in the estimate of the level effect.

I then estimate the model for different subgroups to explore heterogeneity in responses to the EBT transition. I investigate heterogeneous effects for households who have no college education, nonwhite headed households, female headed households, and households with children. I present the results in Table III. Across each sub-sample, I estimate that EBT increases the MPCf out of SNAP. I find little evidence of heterogeneous effects of EBT across these subsamples as I cannot reject that the effects are equal. The stability of the estimates provides additional evidence that the results aren't being driven by the sample selection criteria.

Finally, to test whether EBT influences food expenditures by altering the outcome a household's bargaining process I rerun the main model on samples of households with different numbers of adults and children and report the results in Figure 6. While imprecisely estimated, the results are broadly consistent with a household bargaining mechanism. First, the estimated effect of EBT is near zero for single-adult households. In these households the primary recipient is likely to already dominate decision-making. Second, the effects for households with more adults are larger. Finally, the effect size is generally increasing with the number of children with the largest effects of EBT concentrated in households with multiple adults and more than two children.

5.1 Relationship to prior estimates

I find that the estimated EBT-induced change is relatively large compared with well-identified estimate of the pre-EBT MPCf out of SNAP. As a starting point, I assume that SNAP and cash were perfectly fungible before the introduction of EBT, as in the Southworth model. In this case, the MPCf out of SNAP is the same as the MPCf out of cash, estimated to be about .1 (Castner and Mabli, 2010; Hoynes and Schanzenbach, 2009). In my main specification I estimate an EBT-induced increase in the MPCf out of SNAP of .127. These estimates imply that EBT increased the MPCf out of SNAP by 129 percent.

An alternative pre-EBT estimate is Hoynes and Schanzenbach, 2009's estimate of the MPCf out of SNAP for households whose head did not attend college. They find that the low-education sample has an MPCf out of SNAP of .163 (95% CI: -.01, .33). Using this estimate as the pre-EBT MPCf out of SNAP and my estimate of EBT's impact for the low-education sample (Table III, column 1) implies that EBT increased the MPCf out of SNAP by about 75 percent.

The EBT-induced change also accounts for a significant portion of the observed increase in the MPCf out of SNAP. I measure this increase by taking the difference between the pre-EBT benchmarks mentioned above and Hastings and Shapiro, 2018's estimate of the MPCf out of SNAP of .58.[10] The increase in the MPCf out of SNAP over time in the perfect fungibility case is .48 which implies that the introduction of EBT explains about 25 percent of the observed increase. Using Hoynes and Schanzenbach (2009)'s estimates, the increase due to EBT explains about 30 percent of the increase in the low-education sample.

[10] I use the same estimate across samples because none of the post-EBT studies estimate MPCfs out of SNAP by education level.

5.2 Placebo tests

To check the validity of the identification assumptions I present a series of placebo checks in Table IV. These tests examine whether the treatment had an impact on populations that shouldn't be affected. They provide evidence that the results are not driven by concurrent events that raised food expenditures overall nor differential pre-trends.

First, I test whether adopting EBT affected the food consumption of households that did not receive SNAP in the survey month. To do so, I estimate the main effect of EBT expansion on non-SNAP recipients in column 1 of Table IV. Consistent with the identifying assumptions there is no significant effect on non-SNAP recipients.

I implement another placebo test by restricting the sample to SNAP recipients who spend less on food than they receive in SNAP benefits (those that are not observed to be inframarginal). Since non-inframarginal households spend less on food than they receive in SNAP benefits their MPCf out of SNAP is substantially higher than .1. Consequently, the switch to EBT should have little to no effect on this group of SNAP recipients. Consistent with the theory, I find no statistically or economically significant effect of EBT on the MPCf out of SNAP for the constrained group. I also estimate a substantially higher pre-EBT MPCf out of SNAP for this population.

Finally, I restrict the sample to households with incomes greater than 185 percent of the Federal Poverty Line in column 3. This group is likely to be ineligible for SNAP, even after deductions. Again, I find no statistically or economically significant effects of EBT. Taken together, these placebo tests indicate that the effect of EBT is not driven by differences in pre-trends or a shock that occurred at the same time as EBT adoption.

6 Alternative Explanations

Below I discuss two alternative explanations for the effects EBT on the MPCf out of SNAP. First, I test whether the results are driven by changes in selection into the program due to a reduction in the stigma or using benefits. I also explore whether the effects of EBT cards is due to a reduction in fraud rates. I find no evidence for either a selection or fraud mechanism.

6.1 Stigma

One of the benefits of EBT cards is that they may reduce the stigma of using SNAP benefits. By design, EBT cards are much less identifiable than the paper coupons that they replaced.

A survey of SNAP recipients after Maryland's EBT rollout found that some SNAP recipients preferred EBT to receiving paper coupons because it "reduced embarrassment or stigma" (Kirlin and Inc., 1994, page 9). A decrease in the stigma surrounding the program may lead to new households participating in SNAP leading to a change in the composition of the SNAP population (Moffitt, 1983). If these households have different preferences for food, the average MPCf out of SNAP will change as well. Since this composition change would coincide with EBT introduction it would confound the causal effect of EBT.

If EBT changed the selection into the program, then there should be changes in the average characteristics of the SNAP population. To test for any changes in observable characteristics, I regress a variety of household characteristics on EBT adoption, state fixed effects, and year fixed effects and present the results in Table V. [11] The only statistically significant change in the characteristics of SNAP households associated with EBT is an increase in the number of children. This increase is small compared to the average number of children in SNAP households, about 1.4. Moreover, when I control for the number of children in Table II the results do not significantly change.

A reduction in stigma may also have changed the SNAP population in unobservable ways. One way to test for these changes is to look at whether EBT introduction influenced overall participation. The literature on how EBT affects participation is largely inconclusive. While some studies find an increase in participation after EBT introduction (Currie et al., 2001; Kornfeld, 2002; Kabbani and Wilde, 2003; Danielson and Klerman, 2006; Kaushal and Gao, 2011) others find no impact (Bednar, 2011; McKernan, Ratcliffe and Gibbs, 2003) or a negative impact (Atasoy, Mills and Parmeter, 2010). Indeed, a report commissioned by the government to analyze Maryland's EBT rollout found that "[while] it is possible that EBT may affect the participation decisions of a small number of clients, there is no evidence of a clear trend or strong effect" (Kirlin and Inc., 1994, page 10).

In Table VI, I examine whether EBT adoption is associated with changes in unobservable selection into the program. I first estimate the effect of EBT introduction on the likelihood that a household participates in SNAP. I then aggregate the data to the state-year level to see whether EBT has any impact on SNAP participation rates. Finally, I examine changes in the take-up rate, i.e., the fraction of eligible households who participate in the program. The FSS does not contain enough detail to precisely measure the eligible population for SNAP. Consequently, I take a conservative approach and only treat a household as eligible if their family income is below $15,000 a year. I find that there is no significant relationship

[11] Kuhn (2018) implements a similar test and finds no observable changes in the composition of SNAP into the program conditional on state and year fixed effects.

between EBT introduction and the SNAP participation rate nor the take-up rate. Taken together, I find no evidence of EBT causing changes in the selection into SNAP.

6.2 Fraud

Policymakers hoped that EBT would reduce overall SNAP fraud (Gore, 1993). SNAP fraud can encompass a variety of activities from using SNAP benefits to purchase ineligible items to selling SNAP benefits in exchange for cash. Reducing SNAP fraud may lead to an increase in the MPCf out of SNAP because households that would have engaged in fraud will now use their benefits to purchase food.

A reduction in fraud is unlikely to be driving the results because most forms of SNAP fraud are relatively rare. A study of the Ohio EBT pilot found non-trafficking fraud accounted for less than 0.5 percent of all benefits issued (Inc., 2002). Using a relatively broad definition of trafficking the Food and Nutrition Service of the United States Department of Agriculture found that the rate of SNAP trafficking was at most 3.8 percent in 1993 and 3.5 percent in 1996-1998 (Macaluso, 2000).

Additionally, switching to EBT did not reduce fraud rates overall. In a survey, SNAP experts reported that the new Ohio EBT system had a similar risk of fraud as the paper system previously in use (Inc., 2002). The same study reported that demonstration projects in Maryland and Dayton, OH only reduced SNAP trafficking by about 0.4 percent of total benefits.

Finally, if a reduction in SNAP fraud is the primary mechanism, then there should be a large effect on households that spend less on food than they receive in benefits. These households have the greatest incentive to commit fraud and the most to lose from fraud reduction measures. In fact, EBT introduction has almost no effect on these households (see Table IV, column 2).

7 Conclusion

In this paper I present the first evidence of a policy that changes the MPCf out of SNAP. While previous studies have examined EBT's impact on the SNAP population, crime, and consumption smoothing, I provide the first evidence of its impact on the responsiveness food expenditures to changes in SNAP benefits. I find that the transition to EBT more than doubles the MPCf out of SNAP. These results are consistent across a variety of subgroups that rely more heavily on the program. Finally, the estimated impact of EBT on food consumption is robust to different specifications and checks of the identifying assumptions. I also find no evidence that the results are driven by changes in selection into the program or a reduction in fraud.

Importantly, the introduction of EBT helps explain the observed increase in the MPCf out of SNAP from 1990-2010. More recent estimates of the MPCf out of SNAP are substantially higher than those that used participant samples from the early part of the program. It is critical to understand how and why the MPCf is changing over time as it has a direct impact on the welfare of the beneficiaries. Moreover, it may alter the health and economic benefits of the program which may change how we extrapolate prior estimates of the program's effect to the present day. Finally, by identifying the cause of this increase we can better understand the ways in which consumers deviate from standard economic models and what may induce them to do so.

A deeper understanding of how consumers respond to transfer programs can help policymakers design more efficient programs. By altering the MPCf out of SNAP policymakers can increase food consumption without increasing program costs. These results can help us understand the effects of further changes in payment structure, such as adopting mobile-based payments or spreading payments out over time. However, more research is needed to explore how this change may influence the economic and health benefits of SNAP.

References

Abadie, Alberto, Guido Imbens, and Jeffrey Wooldridge. 2017. "When should you adjust standard errors for clustering?" National Bureau of Economic Research, Working Paper w24003.

Almond, Douglas. 2011. "Inside the war on poverty: The impact of food stamps on birth outcomes." The Review of Economics and Statistics, 93(2): 387–403.

Atasoy, Sibel, Brandon F. Mills, and Christopher F. Parmeter. 2010. "Paperless food assistance: the impact of electronic benefits on program participation." Agricultural & Applied Economics Association 2010 Joint Annual Meeting.

Bailey, Martha J. 2012. "Reexamining the impact of family planning programs on US fertility: evidence from the War o Poverty and the early years of Title X." American Economic Journal: Applied Economics, 4(2): 62–97.

Bailey, Martha J., Hilary Hoynes, Maya Rossin-Slater, and Reed Walker. 2019. "Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence from the Food Stamps Program."

Beatty, Timothy K. M., and Charlotte J. Tuttle. 2015. "Expenditure response to increases in inkind transfers: Evidence from the Supplemental Nutrition Assitance Program." American Journal of Agricultural Economics, 97(2): 390–404.

Bednar, Steven. 2011. "Would You Like Paper or Plastic? Food Stamps Disbursement Method and Take-up." Working Paper, 28.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" The Quarterly Journal of Economics, 119(1): 249–275.

Bitler, Marianne P., and Theodore F. Figinski. 2019. "Long-Run Effects of Food Assistance: Evidence from the Food Stamp Program."

Bruich, Gregory A. 2014. "The effect of SNAP benefits on expenditures: New evidence from scanner data and the November 2013 benefit cuts." Havard University Mimeograph.

Castner, Laura, and James Mabli. 2010. "Low-income household spending patterns and measures of poverty." Mathematica Policy Research, Washington, D.C.

Chetty, Raj. 2015. "Behavioral Economics and Public Policy: A Pragmatic Perspective." American Economic Review, 105(5): 1–33.

Collins, Ann M., Ronette Briefel, Jacob A. Klerman, Anne Wolf, Gretchen Rowe, Chris Logan, Ayesha Enver, Syeda Fatima, Anne Gordon, and Julia Lyskawa. 2016. "Summer Electronic Benefit Transfer for Children (SEBCT) Demonstration: Summary Report." USDA, Washington, D.C.

Cook, John T., Deborah A. Frank, Carol Berkowitz, Maureen M. Black, Patrick H. Casey, Diana B. Cutts, and Alan F. Meyers. 2004. "Food insecurity is associated with adverse health outcomes among human infants and toddlers." The Journal of Nutrition, 134(6): 1432–1438.

Currie, Janet. 2006. "The Take-Up of Social Benefits." In Public Policy and the Income Distribution. , ed. Alan J. Auerbach, David Card and John M. Quigley, 80–148. New York:Russell Sage Foundation.

Currie, Janet, and Enrico Moretti. 2008. "Did the Introduction of Food Stamps Affect Birth Outcomes in California?" In Making Americans Healthier: Social and Economic Policy as Health Policy. , ed. Robert F. Schoeni, George A. Kaplan and Harold Pollack, 122–142. New York:Russell Sage Foundation.

Currie, Janet, and Firouz Gahvari. 2008. "Transfers in cash and in-kind: Theory meets the data." Journal of Economic Literature, 46(2): 333–383.

Currie, Janet, Jeffrey Grogger, Gary Burtless, and Robert F. Schoeni. 2001. "Explaining recent declines in food stamp program particiation." Brookings-Wharton papers on urban affairs.

Danielson, Caroline, and Jacob A. Klerman. 2006. "Why Did the Food Stamp Caseload Decline (and Rise?): Effects of Policies and the Economy." University of Wisconsin-Madison, Institute for Research on Poverty.

Fox, Mary Kay, William Hamilton, and Biing-Whan Lin. 2004. Effects of food assistance and nutrition programs on nutrition and health: Volume 3, literature review. USDA, Economic Research Service.

Glewwe, Paul, Hanan G. Jacoby, and Elizabeth M. King. 2001. "Early childhood nutrition and academic achievement: a longitudinal analysis." Journal of Public Economics, 81(3): 345–368.

Gore, Al. 1993. "National Performance Review."

Hastings, Justine, and Jesse M Shapiro. 2018. "How are SNAP benefits spent? Evidence from a retail panel." American Economic Review, 108(12): 3493–3540.

Hoynes, Hilary W., and Diane Whitmore Schanzenbach. 2009. "Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program." American Economic Journal: Applied Economics, 1(4): 109–139.

Hoynes, Hilary W., and Diane Whitmore Schanzenbach. 2016. "US food and nutrition programs." In Economics of Means-Tested Transfer Programs in the United States. Vol. 1, 219–302. Chicago;London:The University of Chicago Press.

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

Inc., ABT Associates. 2002. "Evaluation of the Exapnded Off Line EBT System in Ohio Volume 2: Assessment of Statewide Operations."

James, Philip T., Michael Nelson, Ann Ralph, and Suzi Leather. 1997. "Socioeconomic determinants of health: the contribution of nutrition to inequalities in health." BMJ, 314(7093).

Johnson, David, Robert F. Schoeni, Laura Tiehen, and Jennifer Cornman. 2018. "Assessing the Effectiveness of SNAP by Examining Extramarginal Participants." PSC Research Report No. 18-889.

Kabbani, Nader S., and Parke E. Wilde. 2003. "Short recertification periods in the US Food Stamp Program." Journal of Human Resources, 1112–1138.

Kaushal, Neeraj, and Qin Gao. 2011. "Food stamp program and consumption choices." In Economic Aspects of Obesity. 223–247. Chicago;London:University of Chicago Press.

Kirlin, John A, and ABT Associates Inc. 1994. "The Evaluation of the Expanded EBT Demonstration in Maryland: Summary of Findings."

Kornfeld, Robert. 2002. "Explaining recent trends in food stamp program caseloads." USDA, Economic Research Service, Washington, D.C.

Kuhn, Michael A. 2018. "Cyclical Food Insecurity and Electronic Benefit Transfer." Working Paper.

Lundberg, Shelly J., Robert A. Pollak, and Terence J. Wales. 1997. "Do husbands and wives pool their resources? Evidence from the United Kingdom child benefit." Journal of Human Resources, 463–480.

Macaluso, Theodore F. 2000. "The extent of trafficking in the Food Stamp Program: An update."

McKernan, Signe-Mary, Caroline Ratcliffe, and Robert Gibbs. 2003. "Employment factors influencing food stamp program participation." USDA, Economic Research Service.

Moffitt, Robert. 1983. "An Economic Model of Welfare Stigma." American Economic Review, 73(5): 1023–1035.

Moffitt, Robert. 1989. "Estimating the Value of an In-Kind Transfer: The Case of Food Stamps." Econometrica, 57(2): 385.

Raghubir, Priya, and Joydeep Srivastava. 2008. "Monopoly money: The effect of payment coupling and form on spending behavior." Journal of experimental psychlogy: Applied, 14(3): 213.

Schanzenbach, Diane Whitmore. 2002. "What are food stamps worth?" Princeton University Industrial Relations Section Working Paper No. 468.

Soman, Dilip. 2003. "The effect of payment transparency on consumption: Quasi-experiments from the field." Marketing Letters, 14(3): 173–183.

Southworth, Herman M. 1945. "The Economics of Public Measures to Subsidize Food Consumption." Journal of Farm Economics.

Wright, Richard, Erdal Tekin, Volkan Topalli, Chandler McClellan, Timothy Dickinson, and Richard Rosenfeld. 2017. "Less cash, less crime: Evidence from the electronic benefit transfer program." The Journal of Law and Economics, 60(2): 361–383.

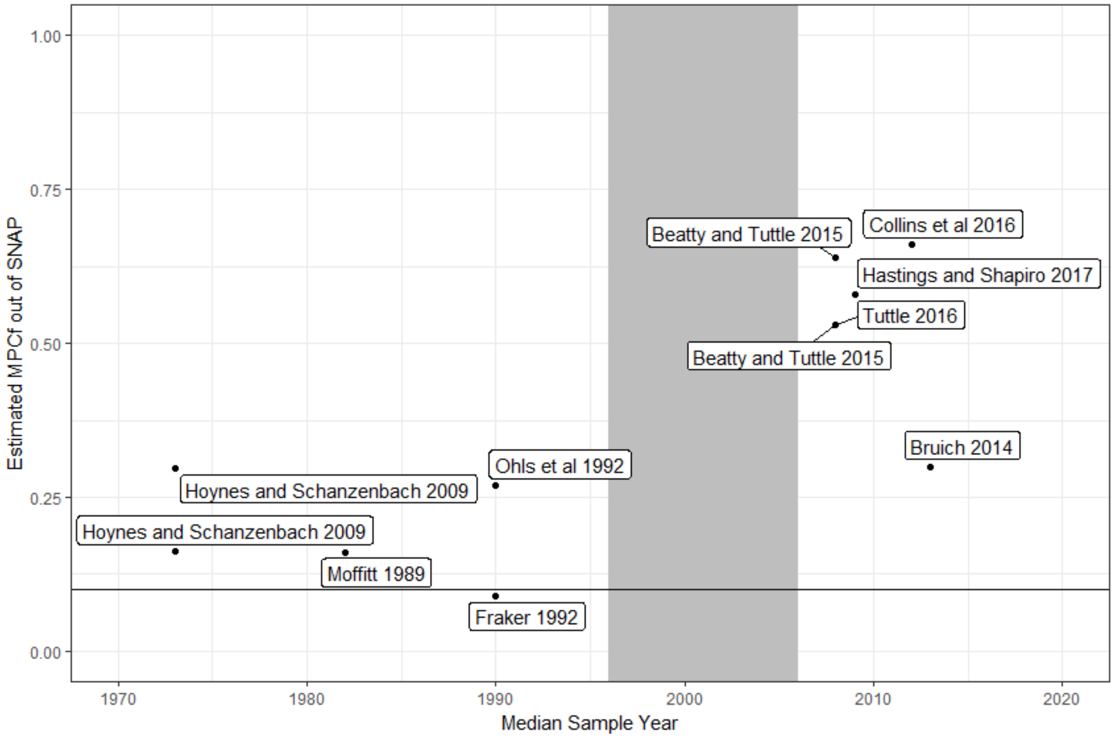

Figure 1: Estimates of the MPCf out of SNAP

Notes: Horizontal line is at .1, the estimated MPCf out of cash and the gray box indicates the EBT rollout period. For studies that only report the difference between the MPCf out of SNAP and cash I add the estimated difference to .1, the MPCf out of cash.

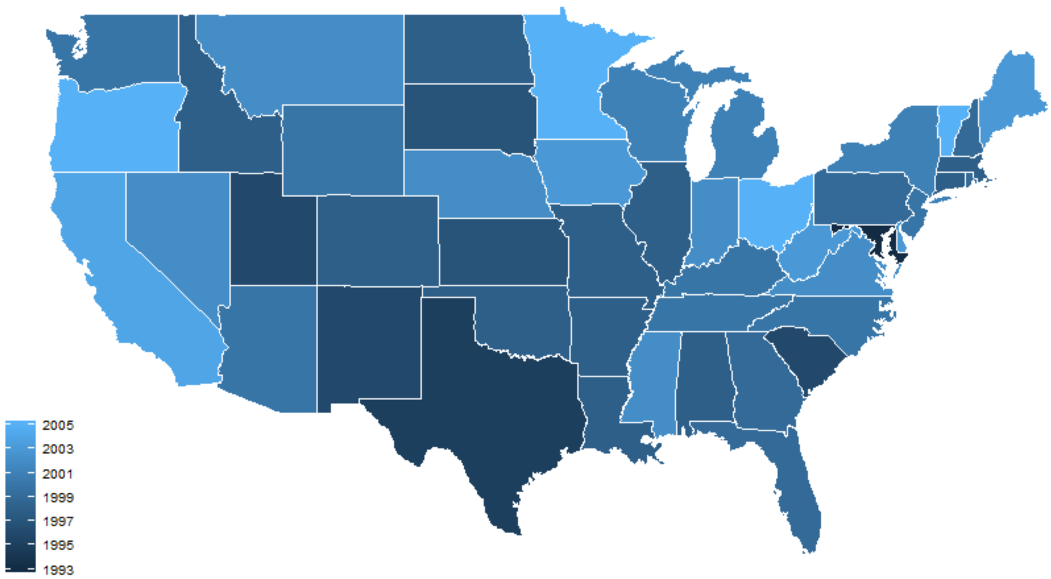

Figure 2: Year each state achieved full EBT coverage.

Source: Author's calculations based on the USDA's SNAP policy database.

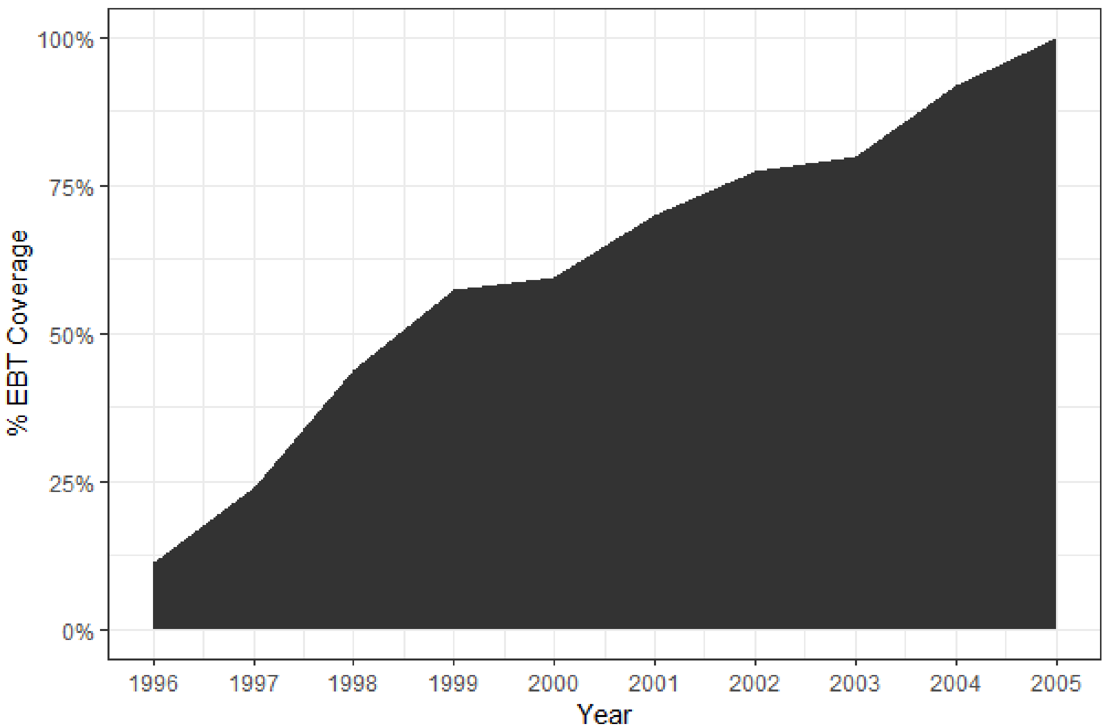

Figure 3: Cumulative percent of SNAP households using EBT.

Source: Author's calculations based on the USDA's SNAP policy database and SEER.

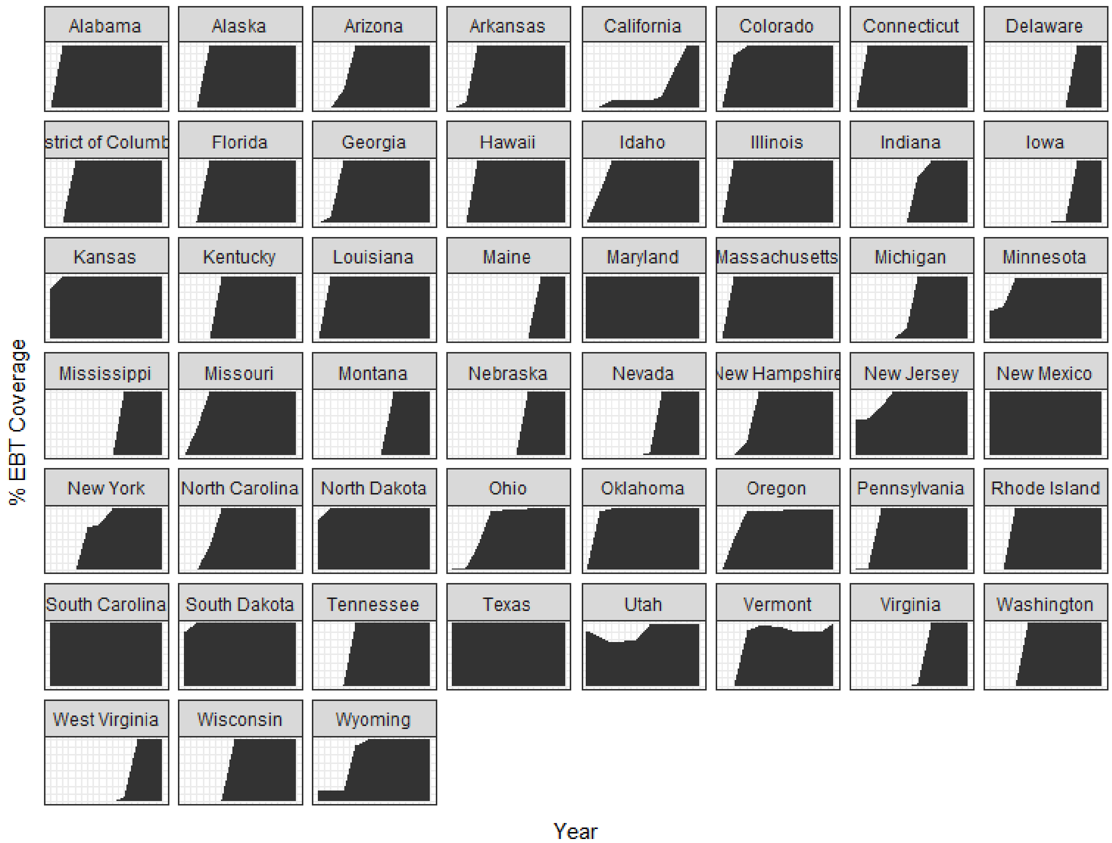

Figure 4: Cumulative percent of SNAP households using EBT in each state

Source: Author's calculations based on the USDA's SNAP policy database.

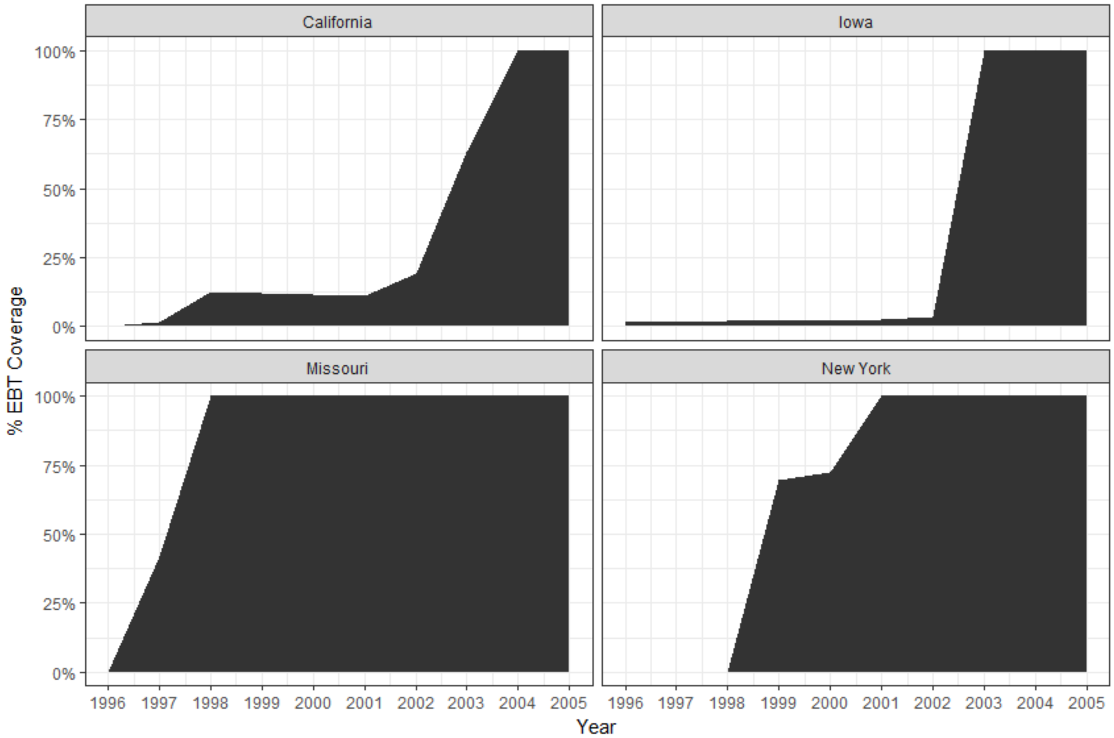

Figure 5: Cumulative percent of SNAP households using EBT in select states

Source: Author's calculations based on the USDA's SNAP policy database.

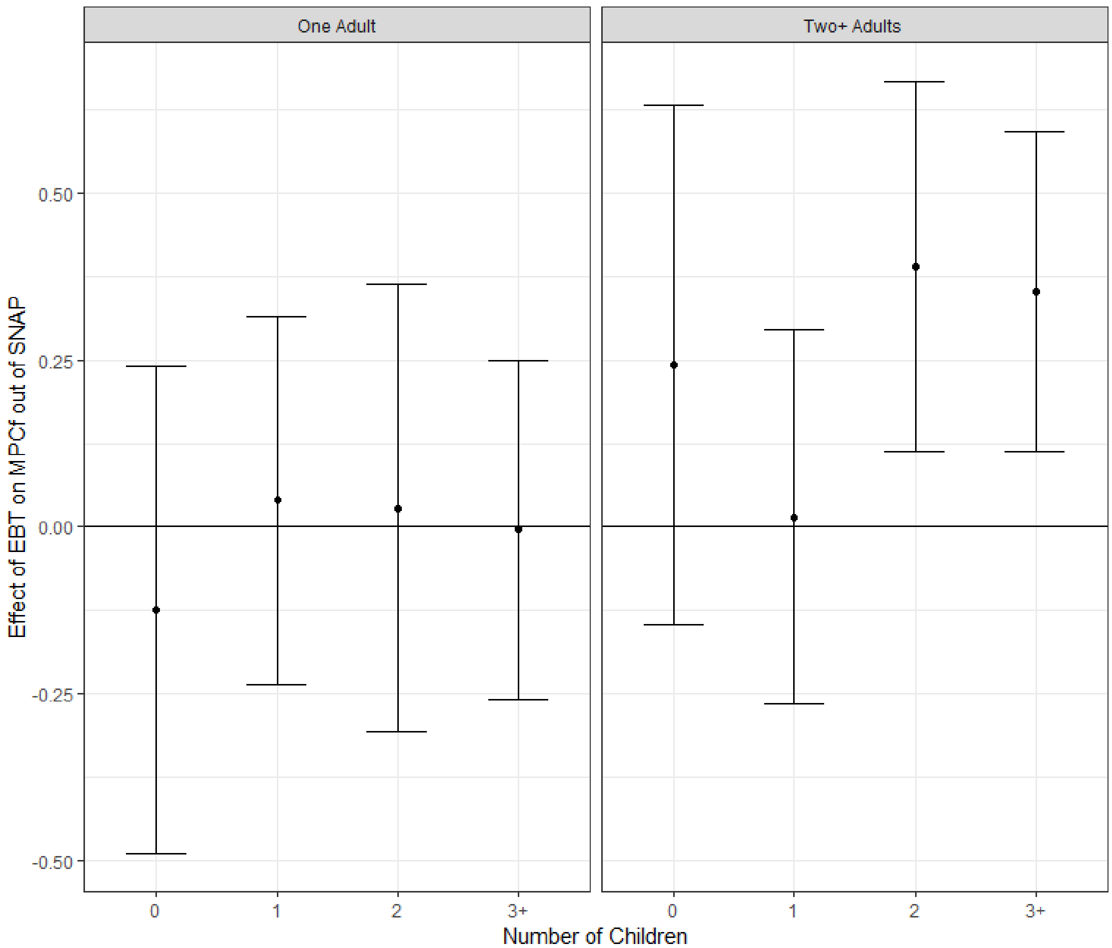

Figure 6: The effect of EBT on the MPCf out of SNAP by household structure.

Notes: Each point represents an estimate of the effect of EBT on the MPCf out of SNAP from a separate regression limited to just the sample with a given number of adults and children. In each regression the dependent variable is real monthly food expenditures. Income is incorporated via interactions between year fixed effects and income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Tables

Table 1: Determinants of state level EBT rollout

	Dependent variable:			
	Start Year		Implementation Time	
	(1)	(2)	(3)	(4)
Log Total non-SNAP transfers	2.957	6.866	−0.042	12.210
	(2.632)	(6.416)	(3.620)	(9.179)
Log Total Population	−3.017	−7.803	1.405	−12.340
	(2.788)	(6.677)	(3.834)	(9.553)
Unemployment rate	−0.518	−0.457	0.741	0.009
	(0.363)	(0.649)	(0.499)	(0.929)
Percent non-white	0.846	−1.513	−14.446**	−20.058*
	(3.907)	(7.286)	(5.372)	(10.423)
Percent <age 6	38.999	29.195	27.258	136.403
	(47.066)	(81.192)	(64.720)	(116.157)
Percent >age 65	−17.047	−38.046	−48.057*	−69.230
	(20.079)	(31.122)	(27.610)	(44.525)
Mean Food Consumption	−0.002	−0.006	−0.021*	−0.017
	(0.008)	(0.010)	(0.011)	(0.014)
SNAP Participation	20.016	6.726	−14.377	−28.683
	(15.331)	(21.581)	(21.082)	(30.875)
Region Fixed Effects?	No	Yes	No	Yes
Observations	46	46	46	46
R^2	0.099	0.335	0.372	0.499
Adjusted R^2	−0.091	−0.032	0.240	0.222

*p<0.1; **p<0.05; ***p<0.01

Notes: All variables measured at the time of the mandate in 1996. Excludes states that rolled out EBT before 1996. Each state weighted by its population.

Table 2: Effect of EBT Introduction on Real Monthly Food Expenditures

	(1)	(2)	(3)	(4)	(5)
Percent EBT x Monthly SNAP benefits	0.146^{***}	0.127^{***}	0.130^{***}	0.130^{***}	0.124^{***}
	(0.043)	(0.041)	(0.044)	(0.043)	(0.044)
Monthly SNAP benefits	0.531^{***}	0.133^{***}	0.139^{***}	0.140^{***}	0.147^{***}
	(0.036)	(0.036)	(0.037)	(0.036)	(0.036)
Percent EBT	-65.978^{***}	-58.484^{***}	-51.692^{**}	-47.743^{*}	-54.611^{*}
	(16.557)	(14.460)	(25.124)	(24.809)	(28.864)
Year and State FEs	X	X	X	X	X
Household Characteristics		X	X	X	X
Income			X	X	X
State Characteristics				X	X
State Time Trends					X
Observations	20,578	20,578	20,578	20,578	20,578

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on the MPCf out of SNAP. The dependent variable is real monthly food expenditures. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies and percent EBT interacted with income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Table 3: Heterogeneous effects of EBT

	≤ HS education	Nonwhite head	Female head	Has children
	(1)	(2)	(3)	(4)
Percent EBT x Monthly SNAP benefits	0.126***	0.151**	0.114**	0.185***
	(0.046)	(0.062)	(0.048)	(0.058)
Monthly SNAP benefits	0.149***	0.136***	0.135***	0.059
	(0.038)	(0.053)	(0.039)	(0.043)
Percent EBT	−49.058**	−66.939	−27.880	−65.396**
	(23.749)	(44.436)	(28.856)	(31.196)
Year and State FE	X	X	X	X
Household Characteristics	X	X	X	X
Income	X	X	X	X
State Characteristics	X	X	X	X
Observations	19,190	6,984	14,640	13,513

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on the MPCf out of SNAP on groups with high participation rates. The dependent variable is real monthly food expenditures. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies and percent EBT and income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Table 4: Placebo tests

	No SNAP	Unconstrained SNAP	Income >185% of FPL
	(1)	(2)	(3)
Percent EBT	-1.135	14.846	34.844
	(14.127)	(14.985)	(42.953)
Monthly SNAP benefits		0.624***	-0.116
	(0.000)	(0.036)	(0.273)
Percent EBT x Monthly SNAP benefits		-0.001	-0.082
	(0.000)	(0.033)	(0.271)
Year and State FEs	X	X	X
Household Characteristics	X	X	X
Income	X	X	X
State Characteristics	X	X	X
Observations	385,513	5,677	294,615

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on the MPCf out of SNAP on groups unlikely to be affected by EBT. The dependent variable is real monthly food expenditures. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies and percent EBT interacted with income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Table 5: Effect of EBT on the compostion of the SNAP households

	Dependent variable:									
	<HS Educ	Some College	College +	HS Only	Head Black	Head Female	# Adults	# Children	Urban	Income <50K
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Percent EBT	−0.005	0.010	0.008	−0.018	0.032	0.023	0.015	0.136***	−0.028	−0.002
	(0.007)	(0.015)	(0.011)	(0.016)	(0.021)	(0.024)	(0.029)	(0.043)	(0.028)	(0.004)
State FEs	X	X	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X	X	X	X	X
Observations	20,578	20,578	20,578	20,578	20,578	20,578	20,578	20,578	20,578	20,578

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on observable household characteristics. Each column is an OLS regression of the dependent variable on the percent EBT, year fixed effects, and state fixed effects. The sample consists of only households who report receiving SNAP benefits in the survey month. All standard errors are clustered at the state level.

Table 6: Effects of EBT on SNAP participation

	Dependent variable:		
	SNAP Participant	SNAP Participation Rate	SNAP Take-up Rate
	(1)	(2)	(3)
Percent EBT	0.003	-0.001	0.027^{*}
	(0.004)	(0.002)	(0.015)
Year and State FEs	X	X	X
Household Characteristics	X		
Income	X		
State Characteristics	X		
Observations	406,091	735	735

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the effect of EBT on program adoption. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Appendix Tables

Table A1: Testing Sample Restrictions

	$> \$0/wk$	$> \$1/wk$	$> \$2/wk$	$> 5\$/wk$
	(1)	(2)	(3)	(4)
Percent EBT x Monthly SNAP benefits	0.131^{***}	0.131^{***}	0.130^{***}	0.128^{***}
	(0.044)	(0.044)	(0.043)	(0.043)
Monthly SNAP benefits	0.144^{***}	0.144^{***}	0.140^{***}	0.138^{***}
	(0.037)	(0.037)	(0.036)	(0.036)
Percent EBT	-47.228^{*}	-47.228^{*}	-47.743^{*}	-47.558^{*}
	(24.688)	(24.688)	(24.809)	(24.691)
Year and State FEs	X	X	X	X
Household Characteristics	X	X	X	X
Income	X	X	X	X
State Characteristics	X	X	X	X
Observations	20,625	20,625	20,578	20,539

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on the MPCf out of SNAP on using different benefit cutoffs. The dependent variable is real monthly food expenditures. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies and percent EBT interacted with income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.

Table A2: Testing Alternative Treatment Definitions

	Continuous Treat	$EBT > 0\%$	$EBT = 100\%$
	(1)	(2)	(3)
Percent EBT x Monthly SNAP benefits	0.130***	0.157***	0.065*
	(0.043)	(0.049)	(0.039)
Monthly SNAP benefits	0.140***	0.107**	0.197***
	(0.036)	(0.045)	(0.033)
Percent EBT	−47.743*	−82.986***	−26.962
	(24.809)	(24.179)	(17.737)
Year and State FEs	X	X	X
Household Characteristics	X	X	X
Income	X	X	X
State Characteristics	X	X	X
Observations	20,578	20,578	20,578

*p<0.1; **p<0.05; ***p<0.01

Notes: Estimates of the impact of EBT on the MPCf out of SNAP on groups using alternative treatment definitions. The dependent variable is real monthly food expenditures. All models include year and state fixed effects. Month fixed effects are included to account for changes in the survey month before 2001. Income is incorporated via interactions between year fixed effects and income bracket dummies and percent EBT interacted with income bracket dummies. Household characteristics include the head's education level, employment status, race, and sex as well as dummies for whether they live in an urban area and the number of kids and adults in the family. State characteristics include the log of real non-SNAP Federal transfers to states, log of total population and the percent of the state that is under the age of 6, over the age of 65, and non-white. Standard errors are clustered at the state level.