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

CHASE S. ECK[*]

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The Supplemental Nutrition Assistance Program (SNAP) provides food assistance to nearly 44 million Americans each year. Empirical estimates of the program's ability to stimulate food consumption vary widely. Studies of SNAP recipients from the 1960s-1980s find much smaller marginal propensities to consume food (MPCf) out of SNAP than studies using samples of recipients from the late-2000s and 2010s. I provide some of the first evidence on the cause of this increase by investigating whether this difference can be explained by the shift from paper coupons ("food stamps") to Electronic Benefit Transfer (EBT) cards. I use a linear fixed effects model to exploit plausibly exogenous variation in the introduction of EBT across states and time to identify the effect of EBT introduction on the MPCf out of SNAP. I find that EBT introduction can partially explain the increase in the MPCf out of SNAP between earlier and later studies. Additionally, the transition to EBT caused an economically significant increase in food expenditures by SNAP households. This work documents a significant, though unintended, increase in the ability of SNAP to stimulate food consumption. It has significant implications for estimating the impact of SNAP benefits today as well as understanding the effects of changing how transfer payments are made.

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, transfers policymakers hope that recipients consume more food than they would with an equivalent cash transfer (Currie and Gahvari, 2008). However, economic 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 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, as theory predicted. 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, forthcoming).

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

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, forthcoming) to identify the causal effects of SNAP on food consumption. Moreover, different methodological approaches within the same sample period provide remarkably consistent estimates.

To shed light on why the MPCf out of SNAP increased, I investigate the adoption of Electronic Benefit Transfer (EBT) cards. In the past, benefits came in the form of physical coupons but now they are distributed via Electronic Benefit Transfer (EBT) cards. 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 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 the causal effects of EBT on 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, Forthcoming). 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 and the exact timing of the rollout is unrelated to funding levels since 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 (forthcoming) 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. Additionally, 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, forthcoming for review).

I find that the transition to EBT doubled the MPCf out of SNAP and can partially explain about 25 to 33 percent of the observed increase in the MPCf out of SNAP between 1990 and 2010. Given the size of the program, even small increases in the MPCf out of SNAP can lead to large increases in overall food consumption. The estimates imply that switching to EBT caused the average household to spend an additional $36 (2017 dollars) a month on food, about an 8 percent increase. To induce a similar increase in food consumption the government would have to spend an additional $14 billion to $86 billion on the SNAP program each year.[2] Such an increase is significant both for SNAP recipients as well as food

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[2] At the time of the EBT transition SNAP benefits probably had an MPCf near .1 so a $36 increase per month per household in food expenditures would require an additional $360 in SNAP funding for each

retailers. Anecdotal evidence suggests that increases in food expenditure driven by SNAP expansions can have large impacts on sales (see Hastings and Shapiro, forthcoming; Bruich, 2014).

The estimated effects of EBT on food consumption are consistent across groups and I find no evidence of heterogeneous effects of EBT adoption across important subpopulations. 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.

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, they 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 (forthcoming)'s preferred explanation for the high MPCf out of SNAP that they measure in a retail panel. Second, Kuhn (forthcoming) 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 power, saw larger increases in food consumption after EBT introduction.

I rule out two potential mechanisms that were part of the rationale for the program: 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

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household in each month. Today, the MPCf out of SNAP is likely near .6 implying that an additional $60 in monthly benefits would induce a $36 increase in food expenditures. There are about 20 million SNAP households each month.

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 consumption of the households most likely to be engaged in fraud.

This work highlights an unintended consequence of the transition to electronic benefit payments. In an era of welfare reform meant to make transfer programs more efficient and targeted, EBT was adopted for the sole purpose of reducing administrative costs but I show that it also had a strong positive effect on food consumption via its effect on SNAP. This paper is the first to provide evidence that the transition from paper food stamps to EBT affected the food consumption habits of recipients.

The observed increase in the MPCf out of SNAP suggests that there are important frictions that may make in-kind transfers better at steering benefits towards the intended consumption targets and may distort consumption choices more than equivalent cash transfers. 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 on health and economic outcomes of the recipients of increasing food consumption. 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 et al., 2001). 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.

I. 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.[3] 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, Hoynes, and Schanzenbach, 2011). Additionally, the rollout of the program has been associated with long-term improvements in health and economic outcomes (Hoynes, Schanzenbach, and Almond, 2016).

A. 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

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[3] 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

made cross-sectional comparisons of participants and non-participants (for review see Fox et al., 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. 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 et al., 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, a recent study by Hastings and Shapiro (forthcoming) uses 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.

B. 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 Abt, 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.[4] 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.

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[4] 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.

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, 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.[5] 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.

Kuhn (forthcoming) 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 (forthcoming) finds that EBT

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

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 (forthcoming) 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 more precise treatment variable, described below.

II. 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 (Currie, 2003).

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

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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, forthcoming). 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[6] percentile of weekly food expenditures[6] as these observations are probably due to measurement error. I also remove observations that are missing any of the variables used in the analysis.

My final sample consists of 406,129 household-level observations taken from 1996-2010, with about 20,000 households per year. The average household spends $593.95 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. SNAP households receive an average benefit amount of $282.30 and spend $461.64 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 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).

III. Model

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

$$food_{ist} = \beta_0 + \beta_1 EBT_{st} + \beta_2 Benefits_{ist} + \beta_3 EBT_{st} * Benefits_{ist} + X_{ist}\gamma + A_{st}\rho + \delta_s + \tau_t + \varepsilon_{ist} \qquad (1)$$

where $food_{ist}$ 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, and $Benefits_{ist}$ is the level of real monthly SNAP benefits each household receives. X_{ist} is a

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[6] In my sample, this restriction eliminates households who spend more than $590 on food each week. This amount is about the average *monthly* food expenditure by households in my sample.

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. Since SNAP benefits and food expenditures are in levels β_1 represents the effect of EBT on households with no SNAP benefits, β_2 represents the MPCf out of SNAP before EBT is implemented, and β_3 represents the change in MPCf out of SNAP associated with EBT adoption and is 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.

This rollout design is a common approach used to evaluate the impact of social programs, including SNAP and Medicaid (Hoynes and Schanzenbach, 2009; Almond et al., 2006). This linear fixed effect model can be thought of as a generalized differences-in-differences (DD) strategy with the restriction that the main effect of the policy variable is zero (Wooldridge, 2007). In this DD 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,

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this approach helps control for time trends across the sample period and selection into the program.[7] Since my policy variation is at the state-level, I cluster the standard errors by state (Bertrand, Duflo, and Mullainathan, 2004; Abadie et al., 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.

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 (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.

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[7] 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.

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.

IV. Results

I begin by estimating the model on the entire sample of SNAP households with non-elderly heads and report results in Table II.[8] 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.

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[8] 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).

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 increase in the MPCf out of SNAP caused by the transition to EBT. It implies that the transition to EBT increased food spending by 12.9 cents for every dollar in SNAP benefits. To put this in context, the average monthly benefit amount was $282. Consequently, the EBT increased monthly food expenditures among SNAP households by about $36, on average.

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 $0.134 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.

Finally, by restricting the sample to only SNAP recipients there are no observations that have zero SNAP benefits. Consequently, the main effect of EBT is only identified by the linearity assumption in the model and, so, I do not report it. While the linearity assumption may be a good approximation away from zero, it likely performs poorly near zero. To identify this parameter, I estimate the main specification on non-SNAP recipients as a placebo test in Table IV and find that there is no statistically significant main 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.

A. *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 .129. 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 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 (forthcoming)'s estimate of the MPCf out of SNAP of .58.[9] 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 27 percent of the observed increase. Using Hoynes and Schanzenbach (2009)'s estimates, the increase due to EBT explains 31 percent of the increase in the low-education sample.

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

B. Economic impact of the increase in MPCf out of SNAP

This paper primarily focuses on how the introduction to EBT can help explain the recent rise in the MPCf out of SNAP. However, the increases in the MPCf out of SNAP caused by EBT are also economically significant themselves. By increasing the MPCf out of SNAP the shift to EBT increased the average SNAP household's food spending by about $36 a month. To achieve a similar boost in food spending, the government would have to increase the average monthly benefit amount by $360 if SNAP benefits and cash are truly fungible. Since there are about 20 million SNAP households each month[10], total program expenditures would have to increase by about $7 billion a month, more than doubling current SNAP spending.[11]

My estimates imply that altering the distribution method of SNAP benefits, and behavioral nudges more generally, can be efficient ways to achieve SNAP's main policy goals. By altering the MPCf out of SNAP they can produce relatively large increases in total food expenditure, leading to increased food security among the target population and price support for food suppliers. This underexplored policy lever is especially important given the renewed debate about SNAP funding and increased interest in finding ways to achieve the program's goals while reducing work disincentive effects. However, it is unclear what impact increasing the MPCf out of SNAP will have on non-food outcomes. If the positive health and economic outcomes associated with SNAP come from the non-food items that SNAP households purchase by substituting SNAP benefits for cash, then increasing the MPCf out of SNAP may harm recipients.

C. 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

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

[11] If the MPCf out of SNAP is actually .58, as in Hastings and Shapiro (2018) monthly benefit levels would have to rise by $62. This increase would lead to an increase of $1.2 billion (22 percent) a month in total program spending.

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.

V. Potential Mechanisms

Below I discuss several possible mechanisms by which EBT can influence the MPCf out of SNAP. First, I discuss how EBT may relate to the mental accounting model in Hastings and Shapiro (forthcoming). I then provide some evidence that suggests EBT influences the outcome of intra-household bargaining, in line with Kuhn (forthcoming)'s findings. Finally, I test whether the results are driven by changes in selection into the program or the costs of participating or a reduction in fraud rates. I find no evidence of a selection or fraud mechanism.

A. Mental accounting

In line with standard demand theory, the Southworth model predicts that inframarginal households—those that would spend more on food than they receive in SNAP benefits—treat SNAP benefits and cash as fungible. Fungibility implies that the MPCf out of SNAP should be close to the MPCf out of cash.

Hastings and Shapiro (forthcoming) propose that the violations of fungibility that they observe are due to mental accounting. As described in Thaler (1999), mental accounting is the idea that individuals designate certain types of income for certain types of expenditures. For example, people tend to save bonuses but spend salary raises (Ishikawa and Ueda, 1984). If SNAP recipients engage in mental accounting, individuals would spend more money on food because SNAP is specifically designated for food.

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 (forthcoming)'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. The higher the effort involved in optimizing, the more people will stick to the planned allocation.

EBT cards increase the effort involved in optimizing in several ways. These effects make it harder to properly substitute between cash and SNAP benefits. First, they 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. Second, it is 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.

B. Household bargaining

If households arrive at joint consumption decisions through a bargaining process, then EBT may alter consumption decisions by increasing the bargaining power of the primary recipient. 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 (forthcoming) 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.

To test whether this mechanism can explain the relationship between EBT and the MPCf out of SNAP 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.

C. 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 Abt, 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. [12] 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 and Grogger,

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

2001; Kornfeld, 2002; Kabbani and Wilde 2003; Danielson and Klerman, 2006; and Kaushal and Gao 2011) others find no impact (McKernan and Ratcliffe 2003; Bednar 2011) or a negative impact (Atasoy et al., 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 Abt, 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 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.

D. 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 (Abt, 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 (Abt, 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).

VI. Conclusion

In this paper I investigate a novel SNAP policy lever: the digitization of SNAP benefits via the EBT system. 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 SNAP's main goal: food consumption. EBT cards significantly increase the ability of the SNAP program to raise food consumption among eligible households. SNAP recipients in EBT states increase their spending on food even when real benefits remain constant. This effect is 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, EBT's effect on food consumption helps explain the observed increase in the MPCf out of SNAP. 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. 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. I provide novel evidence from a real-world policy that changes in the type of payment can induce consumers to alter their behavior.

Moreover, 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, additional work is needed to evaluate the welfare results of doing so. On the one hand, there is a clear welfare loss for the decision-maker in the household that previously chose to use fungible SNAP benefits on non-food items. On the other hand, it may be a welfare gain for children or other dependents if the household decision-maker is underinvesting in nutrition. More research is needed to explore how this change may influence the economic and health benefits of SNAP.

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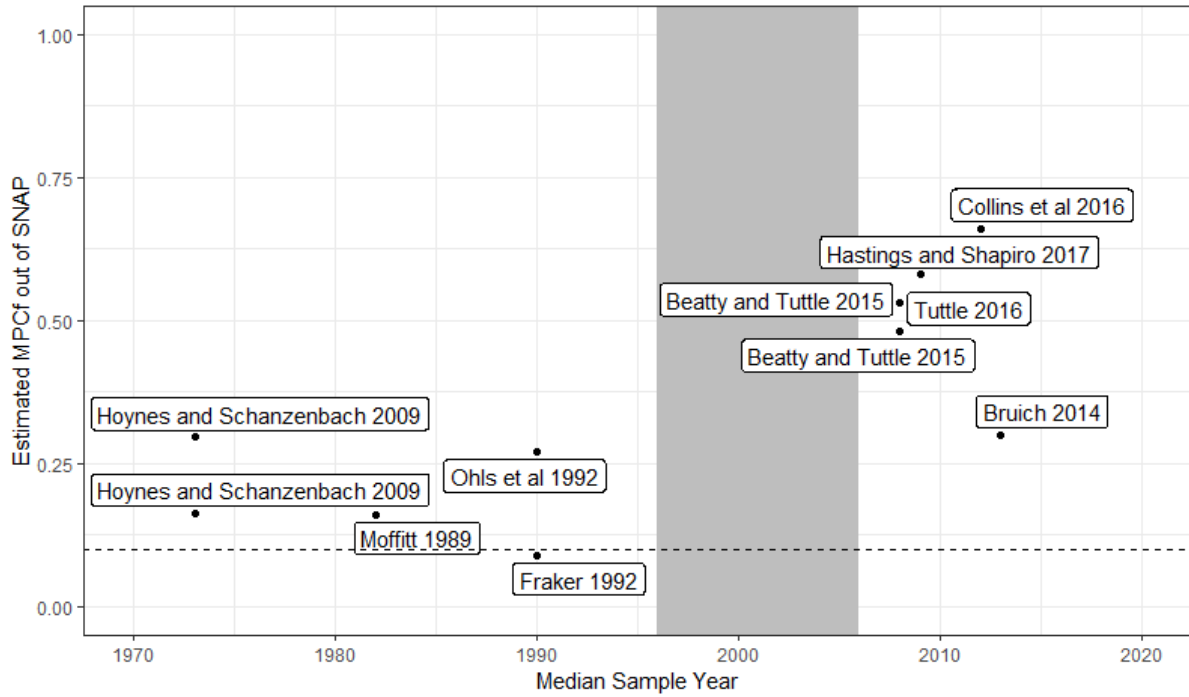
FIGURE 1. ESTIMATES OF THE MPCF OUT OF SNAP

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.
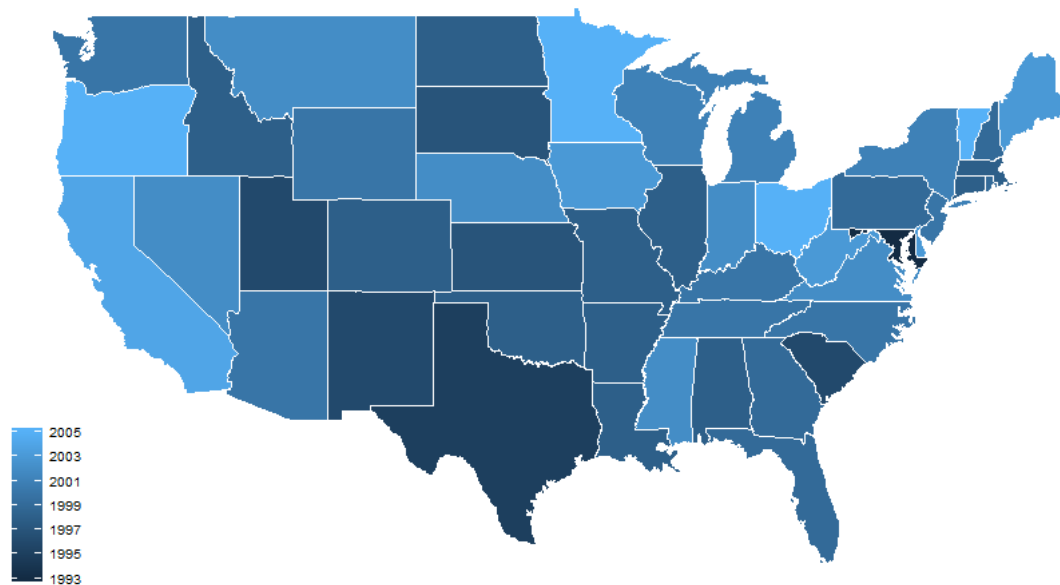
FIGURE 2. YEAR EACH STATE ACHIEVED FULL EBT COVERAGE.

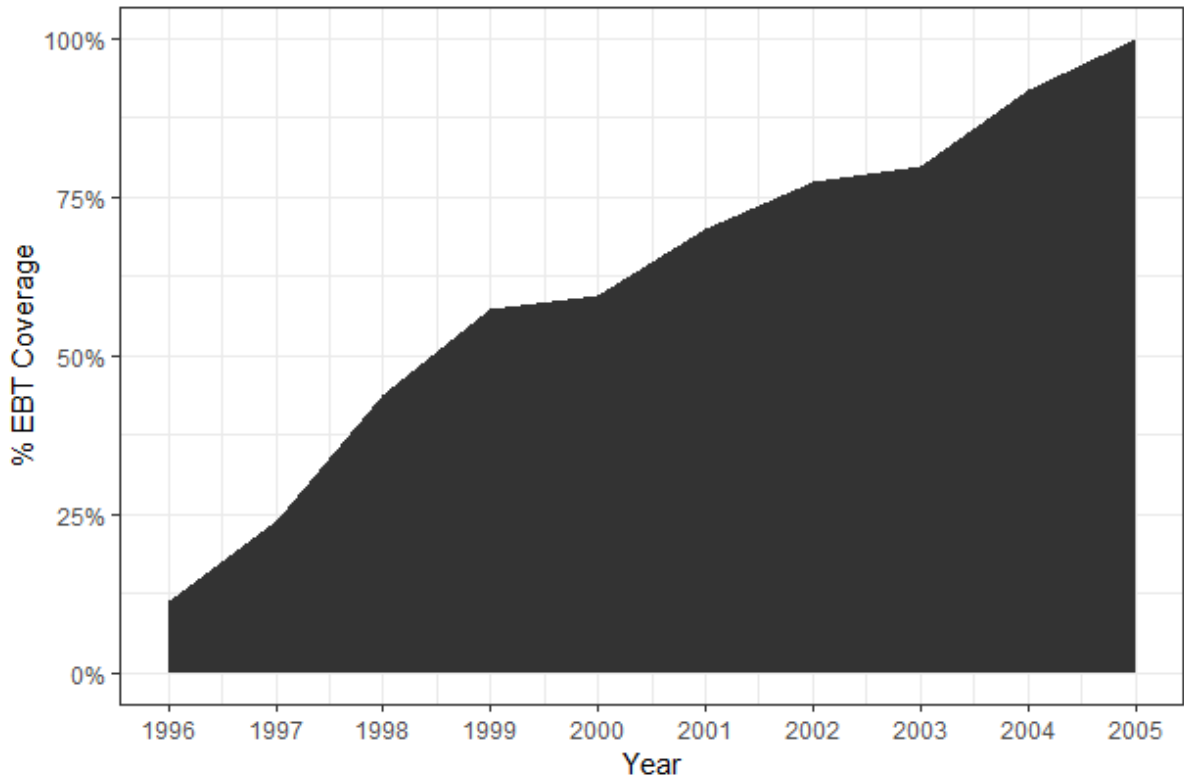*Source*: Author's calculations based on the USDA's SNAP policy database.

FIGURE 3. CUMULATIVE PERCENT OF SNAP HOUSEHOLDS USING EBT.

Source: Author's calculations based on the USDA's SNAP policy database and SEER.
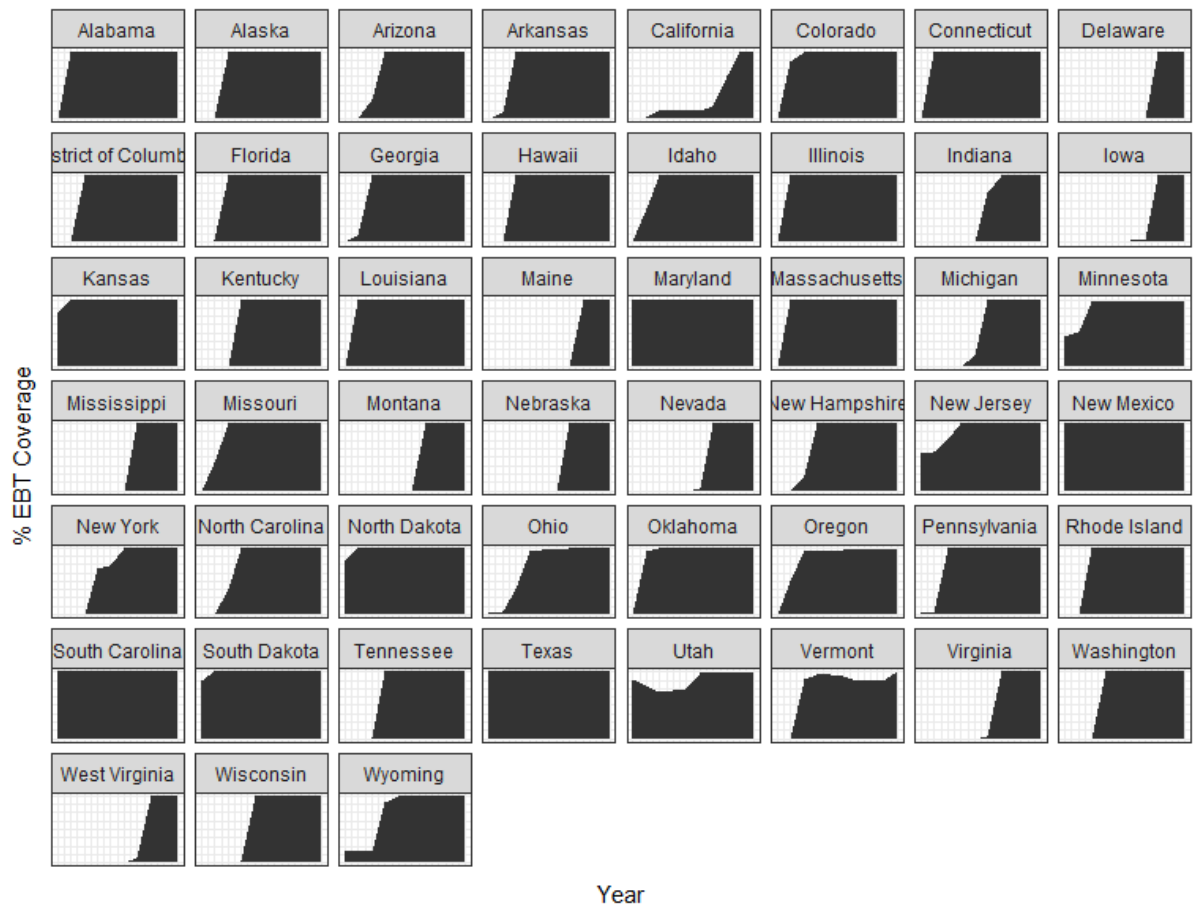
FIGURE 4. CUMULATIVE PERCENT OF SNAP HOUSEHOLDS USING EBT IN EACH STATE

Source: Author's calculations based on the USDA's SNAP policy database.
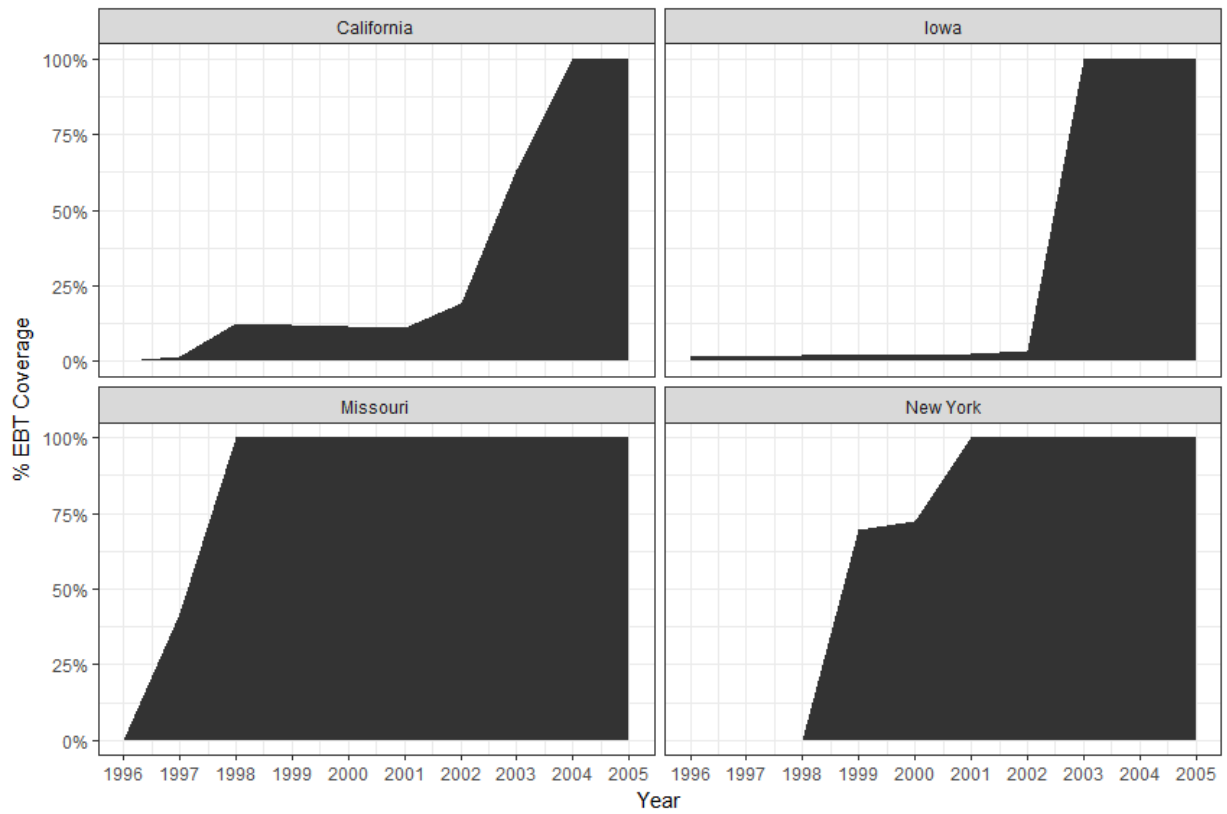
FIGURE 5. CUMULATIVE PERCENT OF SNAP HOUSEHOLDS USING EBT IN SELECT STATES

Source: Author's calculations based on the USDA's SNAP policy database.
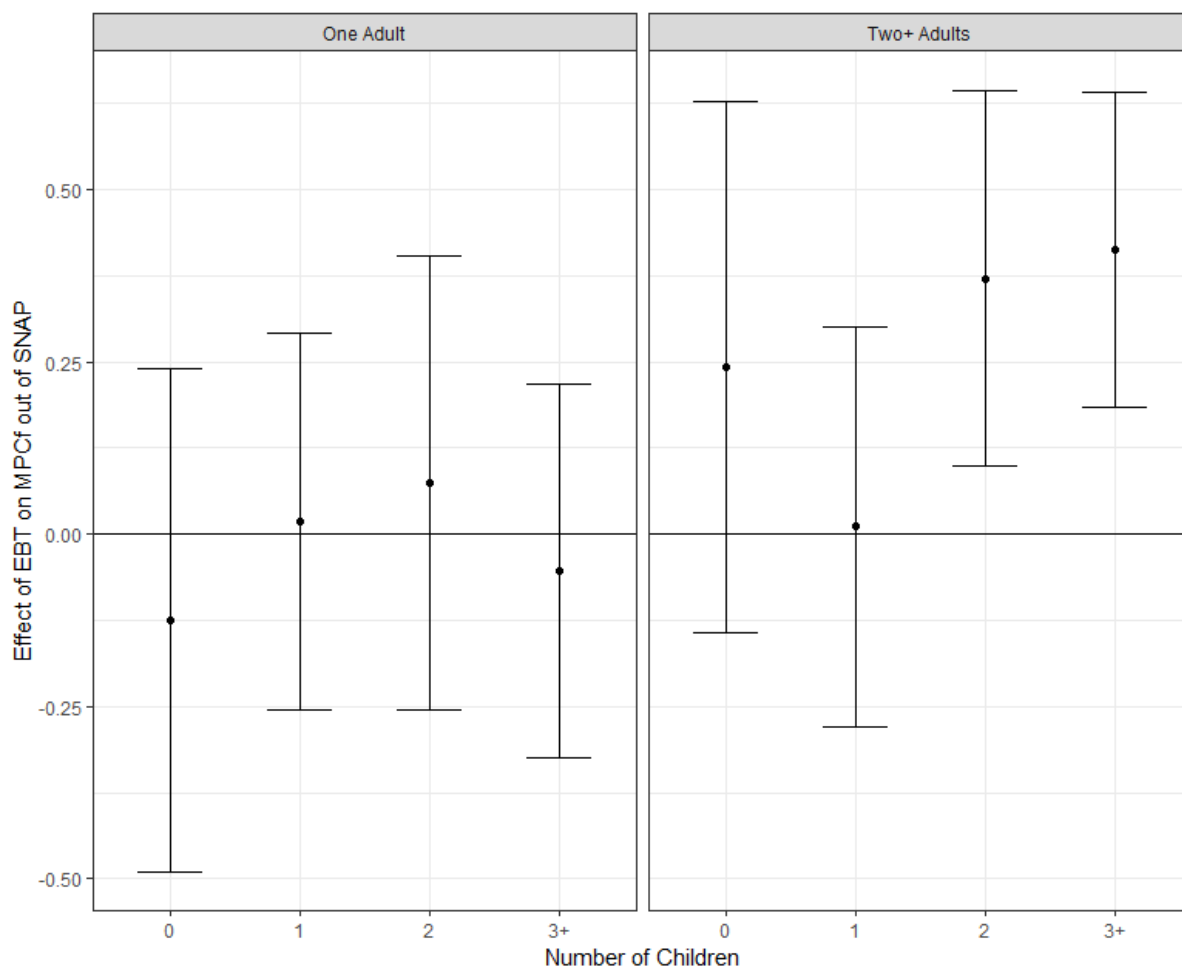
FIGURE 6. THE EFFECT OF EBT ON THE MPCf OUT OF SNAP BY HOUSEHOLD STRUCTURE.

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.

TABLE I – DETERMINANTS OF STATE LEVEL EBT ROLLOUT

	Dependent variable:			
	Start Year		Implementation Time	
	(1)	(2)	(3)	(4)
Log Total non-SNAP transfers	3.144	6.982	0.018	12.100
	(2.624)	(6.395)	(3.625)	(9.117)
Log Total Population	-3.145	-7.920	1.328	-12.162
	(2.776)	(6.655)	(3.836)	(9.488)
Unemployment rate	-0.450	-0.464	0.877	0.042
	(0.382)	(0.652)	(0.528)	(0.929)
Percent non-white	0.608	-1.518	-14.618	-19.941
	(3.899)	(7.293)	(5.386)	(10.398)
Percent < age 6	39.587	31.636	16.803	148.045
	(46.001)	(81.945)	(63.554)	(116.828)
Percent > age 65	-17.676	-37.824	-52.123*	-66.705
	(19.934)	(31.274)	(27.541)	(44.587)
Mean Food Consumption	-0.005	-0.005	-0.019*	-0.017
	(0.008)	(0.009)	(0.010)	(0.013)
SNAP Participation	17.985	7.240	-14.711	-27.106
	(15.535)	(21.775)	(21.463)	(31.044)
Region Fixed Effects?	No	Yes	No	Yes
Observations	46	46	46	46

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 II – EFFECT OF EBT INTRODUCTION ON REAL MONTHLY FOOD
EXPENDITURES

	(1)	(2)	(3)	(4)	(5)
Percent EBT x Monthly SNAP benefits	0.148	0.128	0.129	0.129	0.123
	(0.041)	(0.038)	(0.041)	(0.041)	(0.041)
Monthly SNAP benefits	0.524	0.123	0.133	0.134	0.140
	(0.036)	(0.033)	(0.034)	(0.033)	(0.033)
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	20,562	20,562	20,562	20,562	20,562

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. 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 III – HETEROGENOUS EFFECTS OF EBT

	<= HS education	Nonwhite head	Female head	Has children
	(1)	(2)	(3)	(4)
Percent EBT x Monthly SNAP benefits	0.122	0.160	0.116	0.187
	(0.044)	(0.059)	(0.045)	(0.057)
Monthly SNAP benefits	0.15	0.122	0.128	0.050
	(0.037)	(0.050)	(0.034)	(0.042)
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	14,389	6,976	14,633	13,500

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. 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 IV – PLACEBO TESTS

	No SNAP	Unconstrained SNAP	Income > 185% of FPL
	(1)	(2)	(3)
Percent EBT	0.789	12.643	0.642
	(3.319)	(13.781)	(3.671)
Monthly SNAP benefits		0.630	-0.263
		(0.034)	(0.231)
Percent EBT x Monthly SNAP benefits		-0.009	0.072
		(0.032)	(0.231)
Year and State FEs	X	X	X
Household Characteristics	X	X	X
Income	X	X	X
State Characteristics	X	X	X
Observations	385,567	5,677	294,721

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. 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 V – EFFECT OF EBT ON THE COMPOSITION OF SNAP HOUSEHOLDS

	<= HS Educ	Some College	College or more	Nonwhite head	Female head	# Adult	# Children	Urban	Income < 50K
Dependent variable:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Percent EBT	-0.012	0.011	0.001	0.035	0.022	0.016	0.137	-0.029	-0.002
	(0.015)	(0.014)	(0.005)	(0.024)	(0.024)	(0.030)	(0.042)	(0.028)	(0.004)
State FEs	X	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X	X	X	X
Observations	20,562	20,562	20,562	20,562	20,562	20,562	20,562	20,562	20,562

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 VI – EFFECTS OF EBT ON SNAP PARTICIPATION

	SNAP Participant	SNAP Participation Rate	SNAP Take-up Rate
	Dependent variable:		
	(1)	(2)	(3)
Percent EBT	0.003	-0.0005	0.028
	(0.004)	(0.002)	(0.015)
Year and State FEs	X	X	X
Household Characteristics	X		
Income	X		
State Characteristics	X		
Observations	406,129	735	735

Table VI: 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.