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WHO BENEFITS MOST FROM SNAP? A STUDY OF FOOD SECURITY AND FOOD SPENDING

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ABSTRACT

We study the effects of SNAP participation on food insecurity and food spending using finite mixture models that allow for a priori unspecified heterogeneous effects. We identify a low food security subgroup comprising a third of the population for whom SNAP participation increases the probability of high food security by 20-30 percentage points. There is no affect of SNAP on the remaining two-thirds of the population. SNAP increases food spending in the previous week by $50-$65 for a low modal spending subgroup comprising two-thirds of the population, with no effect for the remaining third of the population.

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1 Introduction

Empirical methods for estimating the treatment effects of the Supplemental Nutrition Assistance Program (SNAP) routinely focus on the average treatment effect of the program. This statistic is satisfactory and useful for many policy makers, although researchers understand that it is unlikely that program effects are constant across the treatment population. Obviously, differences in treatment across observed household, individual or geographic characteristics could lead to heterogeneous outcomes. And there are good reasons to think that effects of treatment will vary across *unobserved* factors in household: food preferences, subjective poverty thresholds, discount rates, and financial acumen all could affect the distribution of outcomes not captured in the mean treatment effect.

This issue is not confined to studies of SNAP or food assistance, though it is particularly salient in this case. An important stylized fact to emerge from recent research concerned with the effects of the SNAP on food insecurity is that, on average, participation results in a decrease in the likelihood of food insecurity (Yen et al., 2008; Ratcliffe et al., 2011; DePolt et al., 2009; Mabli et al., 2013; Kreider et al., 2012). However, it seems likely that there would be many households for which SNAP has a very large effect–namely, those for whom disruption of eating patterns is a real possiblity–and others for which the effect is smaller. With respect to food spending, several decades of mixed results in this literature suggest–differences in methods, data, and focus notwithstanding–that we could be missing distributional effects that appear as statistical zeros.

In this study, we allow for the possibility of heterogeneous effects in a general, a priori unspecified way. We identify subgroups of the population for whom improvements in outcomes are large and subgroups for whom SNAP may have little or no effect. We pay particular attention to two outcomes: food security and food spending. We chose these outcomes precisely because they are important for judging the program's effectiveness. Reduction in food insecurity is a primary goal of the program articulated in its enabling legislation; food spending is important not only because it is the assumed mechanism by which SNAP affects food security, but also because questions about SNAP's effectiveness in increasing *food*–as opposed to total–household spending have always been somewhat contentious. Moreover, food spending is particularly salient for households near the lower end of the food security spectrum–that is, for those whom household resources might prompt disruptions in food intake. Nevertheless, we also examine total spending when at least some food is purchased to understand the extent to which income and substitution effects might lead to increases in non-food consumption.

For this study, we estimate finite mixture models to explore the possibility of treatment effect heterogeneity, to estimate heterogeneous effects and to characterize the sources of such unobserved heterogeneity (Lindsay, 1995; McLachlan and Peel, 2004; Deb and Trivedi, 1997). Econometric applications of finite mixture models include the seminal work of Heckman and Singer (1984) to labor economics, Wedel (1993) to marketing data, El-Gamal and Grether (1995) to data from experiments in decision-making under uncertainty, and Deb and Trivedi (1997) to the economics of health care. More recent applications include Ayyagari et al. (2013) and Deb et al. (2011) in studies of BMI and alcohol consumption, Bruhin et al. (2010) to experimental data and Caudill et al. (2009) and Günther and Launov (2012) to issues in economic development. Despite this growing use of FMM, they have not been brought to bear in food assistance program research.

We find that finite mixtures of two components are preferred for all three of our measures: food security, food spending and total spending. We find that, for these outcomes, SNAP improves outcomes significantly. It increases the probability of high food security by between 20 and 30 percentage points for about one-third of the sample. It increases food spending by between \$50 and \$65 in the previous week for about two-thirds of the sample, with no effect for the remaining third of the sample. These results suggest not only the importance of this program for low-income households, but also the importance of accounting for heterogeneity in outcomes–since they can tell us more about for whom food assistance does and does not "work," and suggest how to improve their performance.

2 Related Research

There are large literatures devoted to the estimation of the treatment effects of SNAP participation. Food insecurity and food spending are among the more important outcomes for measuring success of SNAP, the former because it is the stated goal of the program's enabling legislation, and the latter because it presumed to be the mechanism by which reductions in the former might happen. Both of these questions have literatures that extend back decades: for a comprehensive review of the literature before 2004, see Fox et al. (2004). A general overview of a theoretical framework for estimating treatment effects, as well as the recent history of empirical literatures can be found in Meyerhoefer and Yang (2011).

A recent history of the literature concerned with SNAP and food expenditures is outlined in Beatty and Tuttle (2015) and many of its insights might be brought to bear on food security as well. In brief, the authors suggest that questions about how SNAP effects spending have to contend with changes to the program itself and to econometric practice since the program's rollout. Perhaps the most important of changes to the program has been its modernization since Welfare Reform. Since that time, states have been given considerable leeway to relax eligibility rules set out by the federal government, which has precipitated enormous changes to the SNAP-recipient population. (See, on these questions, studies by Ganong and Liebman (2013); Ziliak (2016).) Additionally, the administration of the program by electronic benefit card since the late 1990's has mostly eliminated the secondary market in food stamps.

Meanwhile, econometric practice has undergone a "credibility revolution," which frequently looks to establish treatment effects that do not rely on the functional form of specifications or simple comparisons of treated and untreated households to establish effects. The variation in identification strategies is particularly evident in the spending literature, which has estimated marginal propensity to consume out of SNAP benefits using a range of comparison strategies (Fox et al., 2004; Wilde et al., 2009; Fraker, 1990), by cash-out experiments (Moffitt, 1989; Levedahl, 1995), and by looking at the phased roll-out of the SNAP program in the late 1960's (Hoynes and Schanzenbach, 2009). Beatty and Tuttle (2015) employ a difference-in-difference method and coarsened matching to get at the effect of changes in food stamp benefits due to the American Recovery and Reconstruction Act (ARRA) on food spending.

As mentioned above, the recent history of studies in SNAP on food insecurity appears to have overcome some of the problems of selection that plagued researchers for decades. Many recent studies have found that SNAP reduces food insecurity. Studies using control functions, in which functions of the unobservables are included in the model rather than differenced away, have been particularly prominent in this listerature. Examples of this approach can be found in Yen et al. (2008), who found that SNAP significantly reduced severity of food insecurity; Mykerezi and Mills (2010), who showed that SNAP participation lowers household food insecurity by 18 percent; and Ratcliffe et al. (2011), who found that SNAP reduced the probability of food insecurity by 30 percent and the probability of very low food insecurity by 20 percent. Other methods that have found that SNAP reduces food insecurity include non-parametric bounding techniques (Kreider et al., 2012) and structural models (DePolt et al., 2009). Gregory et al. (2015) review and replicate studies using most of these methods. Mabli et al. (2013) used new data collection to examine the effect of SNAP on participants and found that it reduced the prevalence of food insecurity.

In all of the studies mentioned above, interest centers in some parameter or function of parameters that expresses a mean treatment effect. That is most often the average treatment effect but sometimes the average effect of treatment on the treated. While these are indispensable quantities of interest for both researchers and policy makers, it is also important to understand how effects might vary across subgroups of observations. In particular, we would like to know whether there are parts of the population of interest who benefit more, and others less, from SNAP. Finite mixture models, which we describe in more detail below, are one way to do that.

3 Data

The data for this application come from 2006-2012 December CPS Food Security Supplement (CPS-FSS). For each of these years of the CPS-FSS, our main specifications include households with annual incomes at or below 185 percent of the federal poverty line (FPL). We chose this income level for two reasons: first, it is the income cut-off that the CPS uses to determine the households asked about participation in SNAP. Second, although the gross income cut-off for SNAP eligibility is 130 percent of the FPL, the relaxation of categorical eligibility rules in many states has meant that a non-trivial fraction of household who enroll in SNAP have incomes above this threshold. We additionally restrict our analysis sample to households that responded to the FSS, that provided sufficient information to determine their food security status, and that provided information for other FSS measures that we use as explanatory variables.

In analyses that serve as checks of our main specification, we also consider the sample of individuals whose incomes fall below 130% of the FPL, the sample of data from 2009 onwards, for the sample of females only and the sample of primary families (within households) only.

In terms of outcome variables, tor food insecurity, we consider a count of the affirmatives in the adult FSM rather than an ordinal variable indicating the level of food insecurity so that households with and without children will comparable in the analysis. Food spending is constructed from a series of questions in the FSM that ask about expenditures at grocery and non-grocery stores on food and non-food expenditures in the previous week. Our principal explanatory variables are indicators for the receipt of any SNAP benefits in the previous year and the amount of SNAP benefits received the last time they were received. For models that examine spending we count as SNAP participants any households whose respondents affirm that they participated in SNAP in November or December of the year in question. Summary statistics for the outcomes across the different samples are reported in table 2.

We include a number of variables that adjust for additional demographic, labor market, and economic well-being of the household: these variables include the household heads gender, age, race, ethnicity, nativity, marital status, education, and employment status ; the number elderly members in the household; the number of disabled members of the household; the number of children in the household; residence in urban area; household income; homeownership; subjective food needs; and state and year fixed effects. We include these variables because of their theoretical or empirically established relation to food security or spending (Barrett, 2002). We summarize all of these variables, save state and year fixed effects, as well as the instruments that we used in our specifications in table 1.

4 Methods

We estimate an ordered probit model for the item response raw scale of food insecurity. We estimate gamma regressions for food and total spending. Gamma regressions are sufficiently flexible to accommodate the severe skewness of the distributions of spending. We use finite mixture models of ordered probits and of gamma regressions to elicit the existence and nature of possible heterogeneity in the effects of SNAP on food insecurity and food and total spending. These models are described in greater detail below.

As with most nonlinear models, the parameter estimates themselves are often not particularly informative. Therefore, we report the marginal effects of SNAP on each of the outcomes instead. We calculate average marginal effects, i.e, we report the average, over all observations, of marginal effects calculated for each observation in the sample.

For each outcome, we consider specifications that adjust for the endogeneity of SNAP receipt. In the case of the ordered probit, a linear approximation is not feasible so that linear instrumental variables models cannot be applied. Finite mixture models also have no linear instrumental variables analog. In the case of spending, we find considerable efficiency gains from the use of nonlinear models that account for skewness. Therefore, we use a control function method to take the endogeneity of enrollment in SNAP into account in all the models. The approach is described in greater detail below.

4.1 Finite mixture model

As mentioned above, most empirical models for estimating treatment effects assume that the effect is constant across the population. Yet there are many reasons for expecting that treatment effects are not constant. In most large experiments, quasi-experimental designs or observational studies, there are many opportunities for the intensity of treatment to be heterogeneous across individual characteristics, household characteristics, sites or geographies and for compliance to and consequences of treatment to vary by individual or group characteristics. Heterogeneity in each of these dimensions lead to heterogeneity of treatment effects.

Heterogeneity of treatment effects is typically explored via the use of interaction terms in regression analyses or by stratifying the sample by indicators of the source of heterogeneity. For example, stratified analyses by race or gender are commonplace. However, there are data and statistical limits to the amount of stratification that can be done given a sample, and such analyses increase the risk of false findings. Furthermore, often heterogeneity exists along the distribution of the outcome itself, by complex configurations of observed characteristics, or on unobserved characteristics. Quantile regressions are an appealing technique to explore heterogeneity along the outcome distribution but cannot be applied to ordinal outcomes. In addition, for continuous outcomes, quantile regression does not provide insight into the other dimensions of heterogeneity. Finite mixture models can be formulated to do just that – identify heterogeneous treatment effects, if they exist, and characterize that heterogeneity along dimensions of the outcome distribution, observed characteristics and unobserved characteristics.

Let $f_c(y|\mathbf{x};\theta_c)$ denote the probability of observing a particular value of an ordered multinomial outcome and the density of a continuous outcome for class (subpopulation) c where $c = 1, 2, ..., C$. Let $\mathbf{x}$ denote the vector of observed characteristics and θ_c denote the parameters of the distribution $f_c(.)$. Let π_c denote the probabilities of membership in each class such that $0 < \pi_c < 1$, and $\sum_{c=1}^{C} \pi_c = 1$. Then, the density function for a C-component finite mixture (Lindsay, 1995; Deb and Trivedi, 1997; McLachlan and Peel, 2004), is

$$f(y|\mathbf{x};\theta_1,\theta_2,...,\theta_C;\pi_1,\pi_2,...,\pi_C) = \sum_{c=1}^{C} \pi_c f_c(y|\mathbf{x};\theta_c). \tag{1}$$

We describe specific details of $f_c(.)$ for the two cases below. We first describe a specification for the finite mixture of ordered probit regressions and then describe the finite mixture of gamma regressions. We estimate the parameters of this model using maximum likelihood. Inference is based on standard errors adjusted for clustering at the state level.

4.2 Finite mixture of ordered probit regressions

As described above, our measure of food insecurity is ordinal, taking values $y = 0, 1, 2, ..., J$. Thus an ordered probit (or logit) would be an appropriate starting point for statistical analysis of the determinants of food insecurity. We extend the ordered probit model to allow for differences in determinants across a priori unobserved subpopulations in the data using a finite mixture of ordered probit regressions. Generally, for individuals in class c, the ordered probit distribution function for outcomes

$j = 0, 1, 2, ..., J$ can be written as

$$f_c(y|\mathbf{x};\theta_c) = \begin{cases} \Phi(-\mathbf{x}_i'\beta_c), & \text{if } y = 0 \\ \Phi(\mu_{1,c} - \mathbf{x}_i'\beta_c) - \Phi(-\mathbf{x}_i'\beta_c), & \text{if } y = 1 \\ \Phi(\mu_{2,c} - \mathbf{x}_i'\beta_c) - \Phi(\mu_{1,c} - \mathbf{x}_i'\beta_c), & \text{if } y = 2 \\ ..., & ... \\ 1 - \Phi(\mu_{J-1,c} - \mathbf{x}_i'\beta_c), & \text{if } y = J \end{cases} \tag{2}$$

where β_c are "regression" coefficients and $0 < \mu_{1,c} < \mu_{2,c} < ... < \mu_{J-1,c}$ are "threshold" coefficients for observations in class c. Without additional restrictions on $\{\mu_{j,c}\}$ and β_c, which are generally specified to vary across each latent class, a finite mixture model of such ordered probit probabilities is not uniquely identified (Teicher, 1963; Grün and Leisch, 2008). So we parameterize the component distribution for an identified finite mixture of ordered probit regressions as follows:

$$f_c(y|\mathbf{x};\theta_c) = \begin{cases} \Phi(-\mathbf{x}_i'\beta_c), & \text{if } y = 0 \\ \Phi(\tau_c\mu_1 - \mathbf{x}_i'\beta_c) - \Phi(-\mathbf{x}_i'\beta_c), & \text{if } y = 1 \\ \Phi(\tau_c\mu_2 - \mathbf{x}_i'\beta_c) - \Phi(\tau_c\mu_1 - \mathbf{x}_i'\beta_c), & \text{if } y = 2 \\ ..., & ... \\ 1 - \Phi(\tau_c\mu_{J-1} - \mathbf{x}_i'\beta_c), & \text{if } y = J \end{cases} \tag{3}$$

with $\tau_1 = 1$. This specification allows the β_c coefficients to vary freely across latent classes but restricts the threshold parameters to be *proportional* across latent classes while being anchored as being equal across classes for the first threshold τ_1. We should note that we have experimented with other sets of restrictions and have found this to be the most computationally stable and to generically deliver the most intuitive results in this study and in small simulation trials we used to validate the model and its coding.

4.3 Finite mixture of gamma regressions

Food and total spending are continuous random variables measured on $\mathbb{R}^+$. They also have considerably right skewed distributions. The gamma distribution (Johnson et al., 1994) describes such data exceedingly well. Therefore, we estimate gamma regressions for spending and extend standard gamma regressions to a finite mixture of gamma regressions to accommodate heterogeneity across latent classes. Specifically, for individuals in class c, the gamma density function for an outcome y can be written as

$$f_c(y|\mathbf{x};\theta_c) = \frac{1}{\Gamma(\alpha_c)e^{(\mathbf{x}_i'\beta_c)\alpha_c}} y^{\alpha_c - 1} \exp(-y/e^{\mathbf{x}_i'\beta_c}) \tag{4}$$

where $\alpha_c > 0$ is the shape parameter of a typical parameterization of the gamma density. Note that, because α_c is allowed to vary across classes, the distributions of spending for different subpopulations accommodate varying degrees of skewness. In the results below, we often use the predicted modes of spending to characterize the gamma distributions. Because the distributions are differentially skewed across classes, the mode summarizes the central tendency and the skewness of the distributions better than the predicted mean.

4.4 Posterior classification of observations

In each of the finite mixture models described above the *prior* probabilities of class membership are assumed to be constants. Although it is technically possible to parameterize the prior probabilities to allow them to vary by characteristics, the tradition in the literature, for intuitive and computational reasons, is to assume they are constant (McLachlan and Peel, 2004). Following the literature, however, in a post-estimation step, we calculate the posterior probability that observation y_i belongs to component c:

$$\Pr[i \in \text{class } c|\mathbf{x}_i, y_i;\theta] = \frac{\pi_c f_c(y_i|\mathbf{x}_i, \theta_c)}{\sum_{k=1}^{C} \pi_k f_k(y_i|\mathbf{x}_i, \theta_k)}, \qquad c = 1, 2, ..C. \tag{5}$$

These posterior probabilities vary across individuals and provide a mechanism for assigning individuals to latent classes. We estimate OLS regressions of the predicted latent class to explore the relationships between observed covariates and class membership.

4.5 Control function methods

As mentioned above, identification of the effects of SNAP has long had to contend with the selection problem of selection into SNAP. That is, households that participate in SNAP are likely systematically different from those who do not in ways that are not observed by the researcher. The recent literature on both spending and food insecurity has been keen to address this problem. In order to identify SNAP participation in our models, we use a control function approach developed by and extended and generalized by and Newey et al. (1999). These researchers showed that there exists a function of first stage residuals in a simultaneous equations system that performs as a control function in the second stage regression in the sense that inclusion of this function of residuals eliminates endogeneity bias. Blundell and Powell (2004) and Lee (2007) show how control function methods can be used in semiparametric and quantile regression settings. Recently, a specific form of this method in which the residual itself is included in the second stage (often referred to as 2-stage residual inclusion) has been used in the context of health econometrics (Terza et al., 2008; Lindrooth and Weisbrod, 2007; Petrin and Train, 2010) and to examine the effect of participation in the National School Lunch Program and child food insecurity (Ishdorj and Higgins, 2015).

We first estimate a logit regression of a binary indicator for SNAP participation on an excluded instrument and all of our control variables. We then estimate residuals from that regression and include those as an additional covariate in our outcome regressions for insecurity and spending, including in the finite mixture models. Note that the power of identification does not come from the functional form specified for the residuals. The intuition behind this method is that the residual controls for everything that is unexplained about participation by the observables and the instrument in the second stage specification. In preliminary work, we experimented with polynomials of residuals but settled on including only the residuals upon noticing no substantive qualitative and quantitative changes in the effect of SNAP on the outomes.

As in most two-step estimators, the standard errors obtained from the second stage regression are not correct. They typically underestimate the true standard errors. Nonparametric bootstrap replicates of the model estimation produce correct standard errors. In our work, we have conducted a nonparametric bootstrap analysis by estimating the first and second stages 100 times and reporting empirical standard deviations of the distributions of the marginal effects of SNAP in the ordered probit, gamma, and finite mixture regressions.

5 Results

5.1 Descriptive Measures

Table 1 shows descriptive statistics for our primary sample described above. SNAP households are disadvantaged in several important ways, relative to low-income non-SNAP households. They generally have less income, as is well established in the literature on food security (Coleman-Jensen et al., 2015), perhaps as a result of having less education. On the other hand, the reduced income could reflect the fact that SNAP households have more disabled members and are more likely to have an adult not in the labor force because of a disability–which are both increase the likelihood of food insecurity (Coleman-Jensen and Nord, 2013). This is also reflected the lower level of full-time employment by the main wage earner in the SNAP household. SNAP households have higher subjective food thresholds – defined here as the gap between current spending and what the household would need to meet its food needs. This could be because SNAP households, even though they spend more for food, generally have larger households. Survey respondents in SNAP households are more likely to be unmarried, black, and Hispanic than their non-SNAP counterparts. It is also well established in the literature that elderly persons have lower SNAP take-up rates than younger households (Ziliak, 2016; Wu, 2009). In addition, all regression models include indicators for the nine Census divisions and years.

Table 2 shows means and sample sizes for the primary sample, as well as for each of the subsamples we consider: these samples are stratified by income (income-to-poverty ratio$\leq$ 1.85 – our main sample – and $\leq$ 1.30), date (post Great-Recession), family structure, and whether additional covariates are available for the sample. These samples were chosen to demonstrate the robustness and consistency of our regression results with intuition. In each case, we see that SNAP households are more food insecure than non-SNAP households even though they spend a bit more on food than their non-SNAP comparators. Similarly, for all of the samples except single female respondents, the differences in unconditional food spending are less than 10 percent between non-SNAP and SNAP households. It is somewhat larger for female respondents, but still less than 10 percent.

5.2 Preliminary Regressions

Tables 3 shows marginal effects of simplified reporting requirements (the excluded instrument in the outcome regressions) on the probability of SNAP participation for each of the samples. In states that have simplified reporting rules in place, households with earnings have reduced requirements for reporting changes in household circumstances, including income and employment. Because the samples of data for the food spending regressions are smaller than those for food insecurity, we show the effects for the "first stage" logit regressions for both outcomes. The results show that simple reporting requirements are a highly significant and substantial predictor of enrollment in SNAP. In each case, the marginal effect is in the order of a 20 percentage point increase. We note that the control variables have the expected signs (not shown) : education, income, being married, home ownership, elderly persons in the household, and metropolitan residence reduce the probability of SNAP participation, while household size, disability, and number of children in the household increase the probability of SNAP participation.

Food insecurity is modeled using ordered probit regressions. Table 4 reports the marginal effects of SNAP on the probabilities of observing no food insecurity ($y = 0$) and for observing high food insecurity ($y \geq 3$) from ordered probit regression models that assume SNAP is exogenous and from models in which the endogeneity of SNAP accounted for using a control function approach that includes the residual from the first stage logit regressions for SNAP. The results are incredibly consistent across samples when SNAP is assumed exogenous. SNAP is significantly associated with lower probabilities of no insecurity and higher probabilities of high insecurity. These results are completely reversed in sign when the endogeneity of SNAP is taken into account. Now, SNAP increases the probability of having no insecurity by 7-26 percentage points depending on the sample used for estimation and is statistically significant in most cases. Households with SNAP have lower probabilities of being highly food insecure; marginal effects range from 6-20 percentage points. Again these effects are generally, but not always, statistically significant. In these regressions more education, more income, more elderly in the household, owning one's home, and being employed full time are strongly related to a lower number of affirmatives. The residuals have the opposite sign of the SNAP participation variable in these specifications and are statistically significant.

For the main sample, in square brackets, we report bootstrap standard errors. As expected, these are larger than those obtained directly from regression output, but they do not change any qualitative conclusions. Because the bootstrap resampling approach is computationally time-consuming, especially in the finite mixture model cases, we do not repeat the bootstrap analysis for the alternate samples. Judging from the relative change between "naive" and bootstrap standard errors in the main sample, we do not expect any other qualitative conclusions to change.

Turning to the food spending regressions reported in table 5, we note that, in the model which assumes that SNAP is exogenous, that SNAP participation is predicted to increase food spending by about $2 and total spending by just about the same amount. Once the endogeneity of SNAP is taken into account, participation is predicted to increase spending by $15-20, which is in line with intuition and expectation. Once again, we also report bootstrap standard errors in square brackets for the main sample. While these are larger than the "naive" estimates, qualitative conclusions do not change for the main sample. In addition, we do not expect qualitative conclusions to change for any of the alternate samples. Being married, having more education, more income, a larger household, being in a metro area, having a higher subjective food threshold, owning your own home, having more elderly in the household, and being employed full time all increase food spending. The SNAP residual is negatively correlated with food and total spending.

5.3 Finite Mixture Model Results

We now report on the analyses that allow for the possibility that different subpopulations in our sample have different effects of SNAP participation. First, we report on the effects of SNAP on food insecurity which are estimated using finite mixtures of order probit regressions. For each specification, we account for the endogeneity of SNAP by including the residual from the first-stage SNAP regression. Statistical model selection criteria (AIC and BIC) show that two-class mixtures of ordered probit regressions fit the data adequately. Therefore, in Table 6 we show the marginal effects of SNAP on no food insecurity and high food insecurity from 2-class finite mixture models. Figure 1 shows the empirical distribution of the food insecurity scale alongside the predicted distributions for each class. The first thing we note is that in our primary sample, there is a latent class of about 65% of the sample for which SNAP has no discernible effect. This probability varies somewhat across specifications, decreasing to as low as 42% when the sample of households with income-poverty ratios of less than 1.3 are considered. Individuals in the first latent class are also quite likely to have any affirmative responses to the FSS on average. In contrast, for individuals in the other latent class (33% of the primary sample) who are considerably less food secure on average – have more affirmative responses to the FSS – participation in SNAP increases the likelihood of having high food security (zero affirmative responses) by 20-30 percentage points across samples. For individuals in the second latent class, who have relatively high probabilities of having more affirmatives, SNAP participation has substantial and statistically significant marginal effects. The qualitative conclusions remain the same if bootstrap standard errors, reported in square brackets, are used for inference.

Among the alternative specifications, it is especially instructive to examine the sample from 2009 onwards, which is the post-ARRA sample. Because ARRA increased SNAP benefits substantially, the sample of participants is likely healthier and wealthier than SNAP households pre-ARRA: in that case, we should expect to see that SNAP has a smaller effect on food security, which is what columns two and four of this table show. While the marginal effect on high food security was to increase it by 27 percentage points in the full sample, when just post-ARRA households are taken into account, that estimate is 21 percent. However, more households (51% as compared to 33%) saw an improvement in their food security status from participation in SNAP. We have not calculated bootstrap standard errors for these samples, but, judging from the difference between the "naive" and bootstrap standard errors for the main sample, we do not expect any of these conclusions to change.

Table 7 reports results from finite mixture models for food and total spending. As described above, we use a mixture of gamma distributions for this model; models with two classes were found to describe the data adequately. The empirical and class-specific predicted distributions of food and total spending are shown in Figure 1. The two latent classes can be be characterized as high and low modal expenditure classes. In the primary sample, individuals with typically high expenditures, who constitute about 35% of individuals, have a modal spending of about \$64 on food and \$75 in total, while individuals with typically low expenditures, who constitute about 65% of individuals, have modal spending of about \$51 on food and \$62 in total. These patterns and estimates are consistent across each of the other samples. In each case, SNAP participation has small and statistically insignificant effects on spending for individuals in the high modal spending class (class 1). For individuals in the low modal spending class (class 2), however, SNAP participation increases food spending substantially – $58 in the primary sample and somewhat higher or lower across the other samples, but always substantially large and statistically significant. For the main sample, inferential conclusions are robust to the use of bootstrap standard errors, reported in square brackets. For the other samples, although we have not conducted bootstrap analyses, we expect inference to remain unchanged.

The results also show that SNAP participation increases total spending for individuals in the low modal total spending class (class 2) by substantial amounts. These estimates, e.g., $58 in the primary sample, are very close to those obtained for the marginal effect of SNAP on food spending among class 2 individuals. This evidence strongly suggests that SNAP participants are using their benefits to increase spending on food, rather than as a mechanism to also spend more on non-food items. Note that finite mixture models for food and total spending were estimated independently, so the consistency of results – marginal effects and class probabilities – are especially notable.

Once again, the sample of observations post-2008 are substantively important given the changes in SNAP due to the American Recovery and Reinvestment Act (ARRA) beginning in April of 2009. As is well known, SNAP benefits were increased by as much as (13%) due to the ARRA. Hence, both selection into SNAP and the marginal effects of the program may have changed. Our effort here is not to estimate the effect of ARRA on food spending and food insecurity–that has been done elsewhere (Beatty and Tuttle, 2015; Nord and Prell, 2011; Nord, 2013). Rather, we want to know if the general intuition these outcomes remains robust to changes in the program. While the basic characteristics of the finite mixture models and patterns of marginal effects remain the same, we see that the estimates of spending for individuals in class 2 (about $65) are somewhat larger than those obtained using the primary sample. This increase is consistent with expectations.

5.4 Determinants of class membership

Using the estimates from our main sample, we compute posterior probabilities of class membership for food insecurity, and for food and total spending. To be precise, we calculate the posterior probability of being in class 2, which is the class for which participation in SNAP has significant effects for all three outcomes; the class with higher food insecurity and lower modal food and total spending. We estimate OLS regressions of the posterior probabilities on the covariates used in our analysis and a subjective food threshold variable to explore the relationships between observed characteristics of individuals and the likelihood of being in class 2.[1] To facilitate interpretation of coefficients, we standardize each of the continuous variables in the regressions. Thus the interpretations of the coefficients on age, income, total, elderly, children and disabled members of the household and subjective food threshold are in standard deviation units. The indicator variables are left in their natural units.

The results show that gender, race, ethnicity and education are not significant determinants of class membership. Neither are home ownership and residence in a metropolitan area. Marital status, income and number of children affect class membership for spending but not for food insecurity. Married individuals, those with higher incomes and those with more children are significantly less likely to be in class 2, the class of observations which are modified by SNAP participation. Older individuals are more likely to be in the food insecure class and less likely to be in the high spending class. The strongest associations between observables and class membership, however, are with the subjective food needs measure. The positive coefficient on this variable suggests that families with higher subjective food needs are both more likely to be food insecure and to be helped more by the program.

[1]Subjective food threshold is calculated by using the series of questions in the FSS that ask respondents whether they need to spend more or less to meet household food needs.

6 Conclusion

This paper has addressed heterogeneity in responses to SNAP participation in food spending and food insecurity. Although there is good reason to think that SNAP would improve these outcomes on average, it is also clear that households participate in the program because have *a priori* knowledge of their *ex post* outcomes: this suggests not only that there is non-random selection into the program, but that households' outcomes likely differ, perhaps considerably, from one another. This paper tackles both of these issues, but focuses in particular on understanding which households benefit most from SNAP.

We find that, for both food spending and food insecurity, there is a sizable proportion of the sample for whom SNAP has little measurable effect. With respect to food spending, this implies that SNAP increases spending on food and total spending by between $50 and $75 for the week before the survey. Although these estimates seem large, it is worth remembering that the CPS-FSS is administered in the week of December that contains the 12th of the month: because SNAP benefits are distributed between the first and the twelfth of the month for many if not most recipients, this means that many respondents are reflecting spending in the first week in which benefits are received. Spending in this week is known to be greater than for other parts of the SNAP monthly cycle. With respect to food insecurity, we find, similarly, that there are some households for whom SNAP can be said to have little effect. However, for those households for which SNAP does have an effect, the results are large and significant: SNAP reduces the probability of food security between 20 and 30 percentage points, or about 50 percent of the prevalence for our sample. Our results for both outcomes take into account the endogeneity of SNAP participation to both of these outcomes.

The results with respect to subjective food needs are very suggestive, but it is unclear precisely how to interpret them. They might just signal that SNAP benefits are inadequate and that more benefits could be a way to help the most in-need families. On the other hand, the strong association of the subjective food needs measure and the more food insecure latent class indicates that their may be cognitive framing issues in responses to the food security questions. This would be consistent with the work of Kapteyn et al. (1988). Finally, policy makers might be gratified that those with the greatest subjective food needs were the most helped by SNAP. Further research into all of these possibilities is warranted to understand how the program works and how it might best benefit its recipients.

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Table 1: Summary statistics of covariates

	Not in SNAP	In SNAP
Age (in 10 years)	5.639	5.457
Female	0.532	0.623
Black race	0.157	0.270
Hispanic ethnicity	0.144	0.158
Married	0.276	0.156
High school diploma	0.626	0.566
Bachelors degree	0.088	0.043
Graduate degree	0.030	0.012
Foreign born	0.151	0.116
Income ($10K)	1.626	1.286
Income squared	3.439	2.462
Own Home	0.533	0.316
Household size	1.911	2.097
Metro area	0.778	0.756
Number of Elderly People in Home	2.724	2.640
Number of Children in the Home	0.066	0.166
Number of Disabled People in Home	0.353	1.025
N	45,776	9,364

Summary statistics for census division and year indicators not shown.

Table 2: Summary statistics of outcomes

Sample		Food insecurity		Food spending		Total spending	
		Not SNAP	SNAP	Not SNAP	SNAP	Not SNAP	SNAP
Income-poverty ratio ≤ 1.85	$\overline{Y}$	1.30	3.19	90.39	94.91	106.15	109.22
	N	43,893	9,114	38,666	7,921	38,666	7,921
Income-poverty ratio ≤ 1.3	$\overline{Y}$	1.44	3.28	86.76	92.40	101.73	106.10
	N	28,145	8,048	24,388	6,963	24,388	6,963
2009 onwards	$\overline{Y}$	1.32	3.22	93.64	98.07	110.15	113.21
	N	27,124	6,573	22,646	5,544	22,646	5,544
Primary families only	$\overline{Y}$	1.21	3.04	110.57	120.66	130.65	139.92
	N	18,377	3,636	16,733	3,265	16,733	3,265
Female respondents only	$\overline{Y}$	1.28	3.21	84.68	94.71	100.10	109.08
	N	23,785	5,674	20,671	4,918	20,671	4,918

Table 3: First stage logit regressions of SNAP

Sample	Average Marginal Effect	
	Food insecurity	Food spending
Income-poverty ratio ≤ 1.85	0.185***	0.178***
	(0.027)	(0.027)
Income-poverty ratio ≤ 1.3	0.230***	0.222***
	(0.032)	(0.033)
2009 onwards	0.219***	0.212***
	(0.014)	(0.014)
Primary families only	0.138***	0.135***
	(0.023)	(0.024)
Female respondents only	0.202***	0.195***
	(0.032)	(0.031)
Additional covariate	0.186***	0.179***
	(0.027)	(0.027)

Significance levels denoted by * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Cluster-robust (state level) standard errors in parentheses.
Income-poverty ratio ≤ 1.85 if not specified otherwise.
Models control for age, income and income squared, number of household members, number of older members, number of children, number of disabled members, and indicators for gender, black race, hispanic ethnicity, high school diploma, bachelors degree, graduate degree, foreign born, owns a home, lives in a metro area, Census divisions and year.
Additional covariate includes an indicator for whether the primary earner in the household works full time.

Table 4: Effects of SNAP on Food Insecurity

Sample	AME (Exogenous)		AME (Endogenous)	
	$\Pr(y=0)$	$\Pr(y \geq 3)$	$\Pr(y=0)$	$\Pr(y \geq 3)$
Income-poverty ratio ≤ 1.85	-0.211***	0.172***	0.129***	-0.106***
	(0.007)	(0.005)	(0.042)	(0.034)
			[0.054]	[0.044]
Income-poverty ratio ≤ 1.3	-0.221***	0.188***	0.075	-0.064
	(0.008)	(0.007)	(0.052)	(0.044)
2009 onwards	-0.222***	0.184***	0.098*	-0.081*
	(0.008)	(0.007)	(0.059)	(0.049)
Primary families only	-0.195***	0.154***	0.260***	-0.206***
	(0.011)	(0.008)	(0.080)	(0.064)
Female respondents only	-0.219***	0.179***	0.101	-0.083
	(0.009)	(0.008)	(0.062)	(0.051)
Additional covariate	-0.213***	0.173***	0.106***	-0.087***
	(0.007)	(0.005)	(0.037)	(0.030)

Significance levels denoted by $*p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.
Cluster-robust standard errors from second-stage regressions in parentheses; bootstrap standard errors in square brackets.
Income-poverty ratio ≤ 1.85 if not specified otherwise.
Models control for age, income and income squared, number of household members, number of older members, number of children, number of disabled members, and indicators for gender, black race, hispanic ethnicity, high school diploma, bachelors degree, graduate degree, foreign born, owns a home, lives in a metro area, Census divisions and year.
Additional covariate includes an indicator for whether the primary earner in the household works full time.

Table 5: Effects of SNAP on Food and Total Spending

Sample	Food spending		Total spending	
	AME (Exog)	AME (Endo)	AME (Exog)	AME (Endo)
Income-poverty ratio ≤ 1.85	2.013***	17.781***	1.444***	17.805***
	(0.476)	(4.143)	(0.546)	(4.778)
		[4.962]		[5.854]
Income-poverty ratio ≤ 1.3	1.982***	15.352***	1.387**	15.276**
	(0.582)	(5.232)	(0.636)	(6.230)
2009 onwards	2.493***	20.613***	2.036***	21.470***
	(0.585)	(4.588)	(0.678)	(5.023)
Primary families only	2.314***	21.911***	1.746***	22.527***
	(0.598)	(3.659)	(0.658)	(4.422)
Female respondents only	2.797***	16.372***	2.281***	16.051***
	(0.532)	(4.552)	(0.616)	(5.068)
Additional covariate	2.159***	17.230***	1.635***	17.521***
	(0.479)	(3.984)	(0.551)	(4.648)

Significance levels denoted by *$p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Cluster-robust-standard errors from second-stage regressions in parentheses; bootstrap standard errors in square brackets.
Income-poverty ratio ≤ 1.85 if not specified otherwise.
Models control for age, income and income squared, number of household members, number of older members, number of children, number of disabled members, and indicators for gender, black race, hispanic ethnicity, high school diploma, bachelors degree, graduate degree, foreign born, owns a home, lives in a metro area, Census divisions and year.
Additional covariate includes an indicator for whether the primary earner in the household works full time.

Table 6: Effects of SNAP on Food Insecurity by Latent Class

Sample		$\Pr(y = 0)$		$\Pr(y \geq 3)$	
		Class 1	Class 2	Class 1	Class 2
Income-poverty ratio ≤ 1.85	AME	0.026	0.265***	-0.017	-0.285***
		(0.069)	(0.068)	(0.045)	(0.074)
		[0.094]	[0.097]	[0.060]	[0.104]
	π	0.672	0.328	0.672	0.328
		(0.031)	(0.031)	(0.031)	(0.031)
	Pr(y)	0.710	0.367	0.143	0.474
Income-poverty ratio ≤ 1.3	AME	-0.197	0.195***	0.105	-0.188***
		(0.131)	(0.063)	(0.069)	(0.061)
	π	0.418	0.582	0.418	0.582
		(0.041)	(0.041)	(0.041)	(0.041)
	Pr(y)	0.633	0.498	0.126	0.400
2009 onwards	AME	-0.133	0.213***	0.075	-0.205***
		(0.107)	(0.077)	(0.062)	(0.075)
	π	0.487	0.513	0.487	0.513
		(0.058)	(0.058)	(0.058)	(0.058)
	Pr(y)	0.694	0.492	0.124	0.392
Primary families only	AME	0.151	0.313***	-0.089	-0.303***
		(0.133)	(0.083)	(0.085)	(0.080)
	π	0.627	0.373	0.627	0.373
		(0.134)	(0.134)	(0.134)	(0.134)
	Pr(y)	0.659	0.488	0.139	0.407
Female respondents only	AME	-0.088	0.311***	0.054	-0.322***
		(0.078)	(0.098)	(0.048)	(0.108)
	π	0.674	0.326	0.674	0.326
		(0.087)	(0.087)	(0.087)	(0.087)
	Pr(y)	0.685	0.382	0.139	0.498
Additional covariate	AME	0.009	0.230***	-0.006	-0.244***
		(0.067)	(0.071)	(0.043)	(0.076)
	π	0.661	0.339	0.661	0.339
		(0.033)	(0.033)	(0.033)	(0.033)
	Pr(y)	0.711	0.376	0.142	0.465

Significance levels denoted by $*p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$.
Cluster-robust standard errors from second-stage regressions in parentheses; bootstrap standard errors in square brackets.
Income-poverty ratio ≤ 1.85 if not specified otherwise.
Models control for age, income and income squared, number of household members, number of older members, number of children, number of disabled members, and indicators for gender, black race, hispanic ethnicity, high school diploma, bachelors degree, graduate degree, foreign born, owns a home, lives in a metro area, Census divisions and year.
Additional covariate includes an indicator for whether the primary earner in the household works full time.

Table 7: Effects of SNAP on Food and Total Spending by Latent Class

Sample		Food spending		Total spending	
		Class 1	Class 2	Class 1	Class 2
Income-poverty ratio ≤ 1.85	AME	6.551	57.836***	10.566	57.987***
		(10.562)	(11.180)	(12.332)	(13.748)
		[13.511]	[13.167]	[15.390]	[16.807]
	π	0.346	0.654	0.348	0.652
		(0.025)	(0.025)	(0.020)	(0.020)
	Mode(y)	63.814	51.188	74.651	61.937
Income-poverty ratio ≤ 1.3	AME	-7.070	54.692***	1.939	51.019***
		(12.470)	(14.286)	(13.842)	(17.623)
	π	0.336	0.664	0.329	0.671
		(0.025)	(0.025)	(0.028)	(0.028)
	Mode(y)	60.367	48.142	70.194	58.774
2009 onwards	AME	7.819	65.456***	17.863	67.234***
		(15.434)	(12.550)	(16.285)	(15.230)
	π	0.320	0.680	0.326	0.674
		(0.025)	(0.025)	(0.022)	(0.022)
	Mode(y)	65.381	53.162	76.711	64.870
Primary families only	AME	20.064	69.902***	18.246	75.781***
		(26.874)	(13.792)	(28.567)	(16.936)
	π	0.293	0.707	0.307	0.693
		(0.042)	(0.042)	(0.028)	(0.028)
	Mode(y)	82.754	67.433	96.825	82.273
Female respondents only	AME	10.373	48.323***	9.746	49.693***
		(12.673)	(12.598)	(14.622)	(14.573)
	π	0.336	0.664	0.347	0.653
		(0.037)	(0.037)	(0.031)	(0.031)
	Mode(y)	61.532	47.011	72.285	57.427
Additional covariate	AME	1.751	58.461***	5.739	59.430***
		(9.448)	(11.262)	(11.423)	(13.939)
	π	0.352	0.648	0.354	0.646
		(0.025)	(0.025)	(0.020)	(0.020)
	Mode(y)	63.802	51.127	74.725	61.841

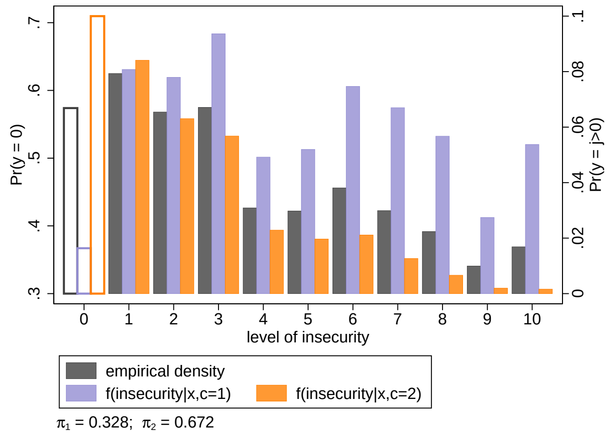
Significance levels denoted by $*p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Cluster-robust standard errors from second-stage regressions in parentheses; bootstrap standard errors in square brackets.
Income-poverty ratio ≤ 1.85 if not specified otherwise.
Models control for age, income and income squared, number of household members, number of older members, number of children, number of disabled members, and indicators for gender, black race, hispanic ethnicity, high school diploma, bachelors degree, graduate degree, foreign born, owns a home, lives in a metro area, Census divisions and year.
Additional covariate includes an indicator for whether the primary earner in the household works full time.

Table 8: Correlates of Posterior Probabilities of Class Membership

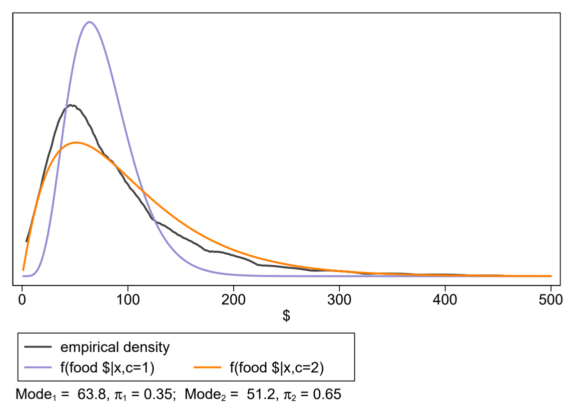
	Insecurity	Food spending	Total spending
Female	0.002	0.004*	0.001
	(0.002)	(0.002)	(0.002)
Black race	-0.003	0.003	0.002
	(0.004)	(0.004)	(0.004)
Hispanic ethnicity	-0.002	-0.007	-0.009*
	(0.004)	(0.005)	(0.005)
Married	0.004	-0.009***	-0.007***
	(0.003)	(0.003)	(0.003)
High school diploma	0.000	-0.001	0.001
	(0.004)	(0.002)	(0.002)
Bachelors degree	-0.009	-0.001	0.000
	(0.006)	(0.002)	(0.003)
Graduate degree	0.003	0.003	0.005
	(0.009)	(0.007)	(0.006)
Foreign born	-0.001	-0.003	-0.004
	(0.003)	(0.004)	(0.004)
Own Home	-0.001	0.000	-0.001
	(0.003)	(0.003)	(0.003)
Metro area	-0.002	-0.003	-0.004
	(0.004)	(0.003)	(0.003)
Age (in 10 years)	0.006***	-0.002**	-0.003**
	(0.002)	(0.001)	(0.001)
Income ($10K)	0.001	-0.009***	-0.010***
	(0.002)	(0.001)	(0.001)
Household size	-0.008***	-0.007***	-0.009***
	(0.002)	(0.002)	(0.002)
Number of Elderly People in Home	-0.006***	0.002*	0.002
	(0.002)	(0.001)	(0.001)
Number of Children in the Home	0.003***	-0.001	-0.000
	(0.001)	(0.001)	(0.001)
Number of Disabled Members	0.002	0.000	0.001
	(0.002)	(0.001)	(0.001)
HH Subjective Food Needs Measure	0.019***	0.053***	0.055***
	(0.001)	(0.003)	(0.002)
Missing or 0 for Sub. Food Needs Measure	0.033***	0.029***	0.031***
	(0.002)	(0.002)	(0.002)
Log SNAP Benefits	-0.003**	0.007***	0.005***
	(0.001)	(0.001)	(0.001)
Constant	0.405***	0.631***	0.638***
	(0.005)	(0.003)	(0.003)

Significance levels denoted by $*p < 0.1$; $** \; p < 0.05$; $*** \; p < 0.01$.
Cluster-robust standard errors in parentheses.
Income-poverty ratio ≤ 1.85.
Coefficients on census divisions and year not shown.

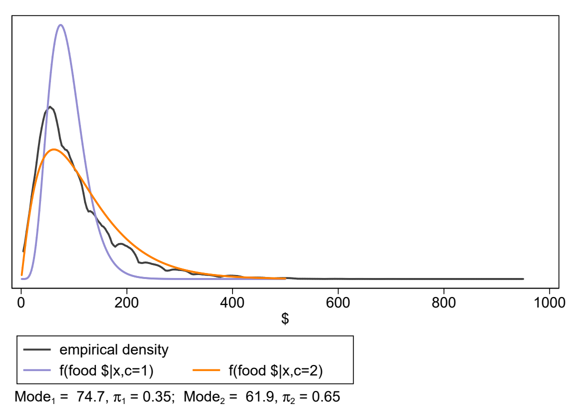
Figure 1: Distributions of Food Insecurity and Spending Conditional on Latent Class



(a) Food insecurity



(b) Food spending



(c) Total spending