The Impact of SNAP on Obesity in the Presence of Endogenous Misreporting

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Abstract

This paper estimates the causal effect of the Supplemental Nutrition Assistance Program (SNAP) on adult obesity, addressing self-selection and endogenous misreporting of participation. I exploit an exclusion restriction for program participation and survey characteristics as predictors of misreporting for identification. The estimated misreporting model confirms some prior findings in the literature regarding the correlates of misreporting: respondents who have an adult present during the interview and are more cooperative are more likely to provide accurate responses. However, contrary to most previous studies, I do not find any evidence of a statistically significant effect of SNAP participation on adult obesity.

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1. Introduction

This paper estimates the causal effect of the Supplemental Nutrition Assistance Program (SNAP) on obesity when participation is endogenously misreported.[1] SNAP is the largest nutrition assistance program in the U.S., serving millions of low-income individuals and households to reduce food insecurity and support a healthy population. High adult obesity rates, coupled with a higher prevalence among low-income families targeted by SNAP, motivates a thorough understanding of the relationship between SNAP and obesity.[2] Since obesity remains a public health concern, policymakers are interested in whether SNAP has any unintended consequences for the weight of recipients. For instance, if SNAP adversely affects recipients' weight, then the size of the negative externalities associated with obesity would need to incorporate those effects (Bhattacharya & Sood 2006, Bailey 2013). Understanding such unintended consequences can inform debates regarding proposals to restructure SNAP.

A common assertion is that SNAP participation reduces food insecurity, lifts millions from poverty, and provides a fiscal boost to the economy during downturns without any significant adverse impact on the health of participants (U.S. Department of Agriculture 2012). However, existing research on the relationship between SNAP participation and obesity is mixed, inconclusive, and deserves closer examination. Obesity is one of the leading health problems in the U.S., with an adult, age-adjusted prevalence rate of 37.7% (35% for

[1] SNAP was formerly called the Food Stamp Program (FSP).

[2] Descriptive empirical evidence suggests that lower incomes are associated with higher probabilities of obesity and severe obesity, and this gradient is more pronounced for women. For instance, using the 2001-2010 National Health and Nutrition Examination Survey (NHANES) data, Gundersen (2015) finds that obesity rates (BMI$\geq$30) decline from 36.3% to 31.3% moving from below the federal poverty level to above 400% of the federal poverty level, while severe obesity rates (BMI$\geq$35) declines from 19.1% to 13.0%. Also, using NHANES data from 2007-2010, Condon et al. (2015) reports that adult SNAP participants were more likely to be obese compared to income-eligible nonparticipants (43.6% vs. 33.3%) and higher-income nonparticipants (43.6% vs. 31.9%).

men and 40.4% for women) as of 2014 (Flegal et al. 2016). Obesity heightens a person's risk of many debilitating diseases and health problems such as diabetes, cardiovascular risk factors, lower quality of life, and many other chronic conditions (Colditz et al. 1995, McGee et al. 2005, Kim & Kawachi 2008). Also, there are significant health care costs of obesity (Finkelstein et al. 2009, Cawley & Meyerhoefer 2012) and adverse effects of obesity on labor market outcomes (Bhattacharya & Bundorf 2009). The fact that the low-income households targeted by SNAP are also relatively more vulnerable to obesity risk factors reinforces the need to provide credible estimates of SNAP's impacts on weight outcomes.

This paper aims to examine the relationship between SNAP participation and adult obesity when we account for misreporting in survey data. The literature evaluating SNAP's causal impacts has always grappled with the twin problems of the endogeneity of program participation and misreporting of participation. First, participants may differ in systematic ways from income-eligible non-participants, making it difficult to obtain unbiased estimates of SNAP's effect on obesity. SNAP participants are likely negatively selected into the program given that participation is often associated with adverse nutrition-related health outcomes such as worse diets and nutrition intake, obesity, or overweight compared to non-recipients (Currie 2003, Kreider et al. 2012, Hoynes & Schanzenbach 2016).

Second, measurement error arising from the misreporting of SNAP status in national surveys poses a considerable threat to causal identification. Misreporting occurs when participants report receiving no benefits when they did or vice versa. Meyer et al. (2015) provide evidence of extensive under-reporting of program benefits of ten transfer programs in five nationally representative surveys and reports that at least one-third of SNAP benefits are not reported in survey data. Validation studies confirm severe misreporting of program participation, sometimes up to almost 50%, with the measurement error being possibly correlated with covariates (Meyer et al. 2020). False negatives tend to be more frequent than false positives, particularly with means-tested government programs. Generally, misreporting of SNAP participation (or any binary treatment indicator) creates biases whose magnitude and direction are not known without further assumptions (Bound et al. 2001, Kreider et al. 2012, Meyer & Mittag 2017, Nguimkeu et al. 2019).

To overcome the above challenges, I utilize Nguimkeu et al. (2019)'s misreporting model to estimate the causal impacts of SNAP on obesity. The advantage of this approach is that it provides a unifying framework to identify SNAP's impacts when we account for both self-selection and misreporting of participation. The basic idea of the model is that self-reported participation is a product of true participation and a misclassification indicator, both unobserved to the researcher. True participation is only observed if the individual participates in the program and correctly reports her participation status. Estimation proceeds in two stages. In the first step, we obtain probabilities of true participation from a partial observability model, which models the self-reported participation as a function of the unobserved participation and the misreporting indicator. In the second step, we regress the outcome of interest on the predicted probabilities of participation (which are now free of selection bias and measurement error) to obtain consistent estimates of SNAP participation's causal effect.

Two sets of variables are needed to implement Nguimkeu et al. (2019)'s proposed estimator: instruments of participation and predictors of misreporting. I follow existing literature that relies on leveraging state-level SNAP administrative policies in terms of the instrument for participation. Although SNAP is a federal program, the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996 ushered in an era where states can adopt policies to influence several aspects of the program's administration such as eligibility, transaction costs of enrollment, and outreach efforts. A number of these state-level SNAP policies have been shown to influence program participation in prior work (Kabbani & Wilde 2003, Hanratty 2006, Meyerhoefer & Pylypchuk 2008, Almada et al. 2016).

I exploit the identifying variation in the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards to instrument for SNAP participation. Moffitt (1983) studied the theoretical connection between stigma and program participation in his seminal work, which modeled stigma as a cost of program participation. There is evidence that the transition from benefit disbursement using coupons to EBT cards following the 1996 welfare reform likely reduced the costs of program participation and induced SNAP take-up (Wright et al. 2017).

To predict misreporting, I leverage the unique features of the 1979 National Longitudinal Survey of Youth (NLSY79) survey in terms of interview mode and information based on interactions between interviewers and respondents for identification. The NLSY79 collects post-interview information from interviewers, including demographic characteristics and other perceptions regarding the interview process, such as the respondent's general attitude and the presence of third parties during the interview. Both the instrument for participation and the predictors of misreporting permit us to obtain predictions of each person's true

[2] Another strand of the literature exploits the early introduction of the Food Stamp Program to estimate the impacts of childhood exposure to the program on long term health outcomes, including adult obesity; see, for instance, Hoyne et al. (2016).

participation propensities, which are then used in place of the self-reported participation to obtain the causal impacts of program participation.

Overall, we do not find evidence that SNAP participation significantly increases body mass index or the probability of being obese. This finding departs from most previous studies suggesting positive impacts of SNAP on adult weight outcomes, especially for females. Also, the estimated model of misreporting yields key insights about the predictors of misreporting and the potential to use survey data to overcome the challenges posed by measurement error, especially when administrative data are unavailable or may be imperfect.

This paper makes three salient contributions. First, using Nguimkeu et al. (2019)'s novel approach, this paper informs the longstanding policy discussions and debates regarding the impacts of SNAP on recipient weight by addressing endogenous participation and misreporting of benefit receipt. Second, we demonstrate that leveraging survey characteristics is a promising way to overcome the biases due to measurement error in treatment effect estimation. Thirdly, our results highlight the consequences of misreporting on estimated treatment effects in empirical work by comparing our approach to standard estimators. The rest of the paper is organized as follows. Section 2. presents background information on SNAP and discusses the related literature. Section 3. presents the data. Section 4. presents the methodology. Section 5. discusses the results and Section 6. concludes.

2. Background and Related Literature

Brief Overview of SNAP

The Food Stamp Program has undergone numerous legislative changes from its establishment under the Food Stamp Act of 1964 to the Food, Conservation and Energy Act of 2008, which changed the name of the Food Stamp Act of 1977 to the Food and Nutrition Act of 2008 and renamed the program as the Supplemental Nutrition Assistance Program.[3] SNAP is administered by the USDA with the objective of increasing food security, reducing hunger, and improving health and well-being of low-income individuals and households by expanding access to food, nutritious diets, and nutrition education.

The Food and Nutrition Act of 2008 contains national eligibility standards (categorical, financial, and non-financial) as well as exceptions to the eligibility criteria.[4] Households are categorically eligible for SNAP if all members of the household are receiving Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), or General Assistance (GA) in certain cases. Households that are not categorically eligible must meet two basic income eligibility standards – a gross income test and a net income test.

The gross income tests requires households to have no more than 130 percent of the federal poverty level while they must have net income (gross income less allowable deductions) no more than the poverty level to pass the net income test. Under current federal rules, the allowable deductions include such items as an earned income deduction (currently set at 20 percent of earned income), a standard deduction (based on household size), a dependent

[3]The change of name presumably was an attempt to reduce the associated stigma with program participation. Also, see Institute of Medicine and National Research Council (2013) for a more detailed discussion of SNAP's historical milestones.

[4]See U.S. Department of Agriculture (2017) for further information on SNAP eligibility criteria.

care deduction, qualified medical expenses, child support deduction, and some excess shelter costs. Households must also meet resource limits such as $2,250 in countable resources (e.g., cash). Households with an elderly or disabled member only need to meet the net income limit and can have up to $3,500 in countable resources.[5] A household's monthly SNAP allotment is determined as the maximum allotment (based on household size) less 30 percent of monthly net income.

Between 2000 and 2014, the number of Americans receiving SNAP benefits has almost tripled from about 17 million to 46 million while total spending on SNAP has more than quadrupled from about $17 billion to almost $75 billion.[6] This translates to about one in seven Americans (or roughly 14% of the total U.S. population) and monthly average benefits of $257 per household, or $125 per person, or $4.11 per person per day in 2014.[7]

Related Literature and Misreporting of SNAP

The impact of SNAP on obesity is theoretically ambiguous. Based on neoclassical economic theory, SNAP participation may affect obesity through its effect on consumption. Does SNAP lead to greater food consumption that could increase the probability of becoming obese? Following the standard Southworth model (Southworth 1945, Bartfeld et al. 2015), individuals allocate total income (cash income plus SNAP benefits) between food and a composite non-food good. Since relative prices are unchanged, SNAP benefits induce a

[5]Households must also meet general work requirements such as not quitting or reducing hours of work and must be U.S. citizens or lawfully present non-citizens.

[6]SNAP statistics can be found at http://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap. Part of this dramatic SNAP expansion is presumably due to the Great Recession and this is a testament to the importance of SNAP in the social safety net in the U.S.

[7]See Hoynes & Schanzenbach (2016) for a review of SNAP and other nutrition assistance programs in the U.S.

pure income effect with a predicted increase in consumption of all *normal* goods. In this standard framework, the receipt of SNAP benefits merely loosens the budget constraint of participants, which leads to greater consumption of food and non-food goods.

There is some evidence that SNAP participants with excess allowances tend to purchase more food than they otherwise would due to the in-kind nature of SNAP benefits (Fox et al. 2004, Devaney & Moffitt 1991, Fraker et al. 1995). However, Hoynes & Schanzenbach (2009) provide recent evidence that the marginal propensity (MPC) to consume food out of SNAP benefits is similar to the MPC of cash income, albeit with dated data from the initial introduction of the program between 1961 and 1975 across about 3000 U.S. counties.

Depending on households' preferences, the increased spending on food can positively or negatively impact obesity depending on the mix of "healthy" and "unhealthy" food purchased and consumed. Suppose SNAP participants are selected from a population with stronger preferences for "unhealthy" food. In that case, one will expect participants to have relatively greater consumption of "unhealthy" foods at all levels of income, leading to weight gain. The converse also holds. Beyond the underlying preferences for different types of food, the loosening of the budget constraint could lead to spending on goods that increase (decrease) sedentary activities resulting in weight gain (loss). Depending on the proportion of recipient households that are inframarginal versus extramarginal and the types of food purchased, SNAP participation may or may not empirically have anything to do with obesity.

Several papers have examined SNAP's impact on many outcomes, including poverty, food insecurity, food consumption, and weight outcomes.[8] In terms of SNAP's relationship with obesity, a common finding is that SNAP participation is positively correlated with

[8]For reviews, see Currie (2003), Bartfeld et al. (2015), and Hoynes & Schanzenbach (2016).

the probability of being obese or overweight (Townsend et al. 2001, Gibson 2003, Chen et al. 2005, Baum 2011, Meyerhoefer & Pylypchuk 2008). For instance, Gibson (2003) uses individual fixed effects estimation and concludes that SNAP participation increases obesity among women but finds no significant effects for men. Meyerhoefer & Pylypchuk (2008) adopts discrete factor random effects and IV estimation approaches and comes to the same conclusion as Gibson (2003).

A few studies have found no statistically significant relationship between SNAP participation and obesity (Fan 2010, Almada & Tchernis 2015). Using propensity score matching methods, Fan (2010) finds no significant effect of SNAP on obesity, overweight, or BMI. Nonetheless, the consensus among policymakers is that while SNAP participation does not increase or decrease the probability of being obese for children and non-elderly men, it tends to increase the likelihood of being obese or overweight for non-elderly adult women (U.S. Department of Agriculture 2012).

The consequences of misreported participation have recently received increased attention in the literature examining SNAP's impacts. Based on an administrative record linkage, Meyer et al. (2020) estimates false negatives to be around $20-30\%$ in the 2001 and 2005 panels of the Survey of Income and Program Participation (SIPP), 35% in the 2001 American Community Survey (ACS) and up to 50% in the 2002-2005 Annual Social and Economic Supplement (March CPS). However, the corresponding false positives are typically less than 1.5%. Also, Almada et al. (2016) estimates $23-45\%$ false negative rates in the National Longitudinal Survey of Youth (NLSY) - 1979 cohort based on a model of misreporting.

There is a growing literature suggesting that the estimated effect of a misclassified binary explanatory variable may be substantially biased (Aigner 1973, Bollinger & David 1997, Hausman et al. 1998, Black et al. 2000, Frazis & Loewenstein 2003, Brachet 2008, Kreider 2010, Kreider et al. 2012, Nguimkeu et al. 2019). When a binary explanatory variable is misclassified, the measurement error is necessarily nonclassical. Without additional assumptions about the nature of the measurement error, Gundersen & Kreider (2008) find wide bounds on the resulting bias. This resulting bias persists even when misclassification is entirely random or exogenous.

Evidence from validation studies suggests that misreporting may be correlated with individual and household characteristics. Moreover, in their extensive review of measurement error in survey data, Bound et al. (2001) discuss the possibility that the measurement error can be *differential*, where measurement error depends on the outcomes of interest. For instance, in this paper's context, misreporting may be endogenous to the outcome equation if individuals with higher body weight are more or less likely to report program receipt.

Developing methods for consistently estimating the treatment effects of an endogenous and misreported binary regressor remains an active area of research. The OLS estimator is inconsistent for the average treatment effect of program participation and may even assume a "wrong sign" in special cases (Hu et al. 2015, Nguimkeu et al. 2019). Traditional IV methods have also been shown to be inconsistent (Black et al. 2000, Frazis & Loewenstein 2003). Most existing methods for addressing misreporting in a right-hand side binary variable have focused on the case of exogenous or random misreporting (Mahajan 2006, Lewbel 2007).

Few studies have attempted to address both the endogeneity and the misclassification of SNAP participation. Using partial identification methods to bound the treatment effect of SNAP participation on child health outcomes, Kreider et al. (2012) find that commonly cited relationships are misleading, concluding that "under the weakest restrictions, there is substantial ambiguity; we cannot rule out the possibility that SNAP increases or decreases poor health." In the context of adult weight, Almada et al. (2016) pursue various parametric and nonparametric approaches to identify the effects of SNAP on the probability of being obese or overweight. In addition to not finding any statistically significant results for SNAP's impact on the probability of being obese, Almada et al. (2016) caution that traditional IV estimates are overstated and exceed nonparametric upper bounds by over 200%. To my knowledge, this paper is the first study to examine the effect of SNAP on body weight outcomes using a framework that yields point estimates when participation and misreporting are allowed to endogenous.

3. Data

This paper uses data from the National Longitudinal Survey of Youth - 1979 Cohort (NLSY79), a nationally representative sample of 12,686 men and women surveyed annually from 1979 and biennially after 1994. The NLSY79 comprises three sub-samples: a cross-sectional sample of 6,111 respondents representing the non-institutionalized population, a supplemental sample of 5,295 civilian Hispanic or Latino, black, and economically disadvantaged non-black/non-Hispanic population, and a sample of 1,280 military youth. The analysis sample is limited to 1996, 1998, 2000, 2002, and 2004 waves due to the availability of state-level policy variables from the SNAP Policy Database, which are used to instrument SNAP participation.[9] The respondents were between 14 and 22 years old in 1979. The dependent vari-

[9]The Economic Research Service (ERS) of the USDA maintains the SNAP Policy Database, which contains state-level SNAP policy choices for all 50 states and the District of Columbia from 1996 to 2011 (as of October 2016).

ables are two bodyweight measures constructed from the self-reports of weight and height, namely body mass index and the indicator of being obese (BMI$\geq$30). I restrict the sample to observations with non-missing values of weight and height biennially from 1996 to 2004.[10]

We define the treatment indicator as a dummy variable equal to 1 for SNAP participation in at least one month of the past calendar year and zero otherwise. In the final analysis sample, about 16% of the survey respondents reported SNAP participation in at least one month of the previous calendar year. Out of those who reported participation, about 72% received SNAP during every month of the year. It is a more complicated process to determine which respondents are eligible in the NLSY79 and almost any other nationally representative survey because of the inadequacy of the income and asset data required for such an exercise. As previously mentioned, individuals must meet gross income, net income, and asset tests. Although these criteria are determined at the federal level, many exceptions exist, and states can make exemptions in some instances.

Most studies resort to checking whether a household's income (after adjusting for household size) meets a particular multiple of the federal poverty level. While some studies use the gross income cutoff of 130% of the federal poverty level to determine SNAP eligibility, other studies have used higher thresholds of up to 250%. Using just the gross income test to determine eligibility can result in comparisons with individuals that are not truly eligible for SNAP. Using a more restrictive threshold might miss those who become eligible for only portions of the year since eligibility is based on *monthly* gross and net income. I restrict the primary analysis sample to respondents at or less than 250% of the federal poverty level

[10] Weight is reported in pounds in the survey years 1981, 1982, 1985, 1986, 1988, 1989, 1990, 1992, 1993, 1994, 1996, 1998, 2000, 2002, 2004, 2006, 2008, 2010, 2012 and 2014. However, height in inches is only reported in 1981, 1982, and 1985. I use height in 1985 to calculate each respondent's BMI as weight in kilograms divided by height in meters squared.

who are observed in at least two waves from 1996 to 2004, capturing about 96% of reported participation.

The NLSY79 permits the construction of demographic variables such as race, gender, and marital status. It also contains information on household characteristics such as household members' age, household size, family income, information on labor market activities, and educational attainment of respondents and their mothers. Dummy variables for completing high school or more are used to measure respondents and their mothers' educational attainments. Labor market activity is captured by weekly hours worked in the past calendar year and current employment status.

An essential feature of the NLSY79 that makes it suited for our research design is that it collects post-interview information from interviewers, including demographic characteristics and other remarks about the interview process, such as the respondent's general attitude and the presence of third parties during the interview. Section 4. discusses how I exploit these additional interview and interviewer characteristics in the estimation strategy.

The final data set consists of 9,925 person-year observations. Table 1 reports the means and standard deviations of the variables used in the regressions for the full sample and by participation status. The average BMI for SNAP participants is 29.4, while it is 27.6 for nonparticipants. Obesity rates for participants and nonparticipants are 38.1% and 28.1%, respectively. The summary statistics indicate that SNAP participants are negatively selected on a variety of observable dimensions. On average, SNAP recipients belong to households with lower family incomes ($14,838 vs. $24,030), work for fewer weekly hours (20.6 vs. 34.7), have slightly larger household sizes (3.6 vs. 3.3) with more children (2 vs. 1.6), are less likely to be married (0.2 vs. 0.5), are less likely to have a high school diploma or higher (0.7 vs.

0.8), are more likely to have mothers have graduated from high school (0.4 vs. 0.5), and are more likely to participate in WIC (0.2 vs. 0.1) relative to nonparticipants.

4. Methodology

This study aims to estimate the causal effect of SNAP participation on obesity, accounting for selection bias and endogenous misreporting of participation. As previously discussed, standard linear IV estimators may address the self-selection problem but are inconsistent for the treatment effect in light of the nonclassical nature of misreported participation (Black et al. 2000, Frazis & Loewenstein 2003). In the remainder of this section, I present the econometric framework of Nguimkeu et al. (2019), which addresses these problems by simultaneously modeling participation and misreporting decisions in relation to the evolution of body weight.

We are interested in the causal effect of participating in SNAP on a measure of body weight in the linear treatment effects model

$$y_{it} = \boldsymbol{x}'_{\boldsymbol{it}}\boldsymbol{\beta} + T^*_{it}\alpha + \varepsilon_{it}, \tag{1}$$

where y_{it} is the outcome of interest for individual i at time t, T^*_{it} is individual i's true, unobserved (to the researcher) participation status in year t, $\boldsymbol{x}_{\boldsymbol{it}}$ is a vector of observed characteristics, $\boldsymbol{\beta}$ is a k-parameter vector, and ε_{it} is the idiosyncratic disturbance term. Our interest lies in estimating the treatment effect denoted by α.

In the empirical analyses, $\boldsymbol{x}_{\boldsymbol{it}}$ includes demographic characteristics such as respondent's age, race, gender, marital status. It also includes family characteristics such as household size, number of children, the logarithm of income, and human capital characteristics such as educational attainment and mother's education. Other variables included in $\boldsymbol{x_{it}}$ are labor market activity measured by average weekly hours worked in past calendar year and current employment status as well as indicators for living in an urban area, receiving WIC benefits, AFDC/TANF receipt, and SSI receipt.

To address self-selection into the program, an individual's true participation decision is modeled following the latent utility formulation as

$$T^*_{it} = \mathbf{1}\left(\boldsymbol{z'_{it}\theta} + v_{it} \geq 0\right), \tag{2}$$

where $\boldsymbol{z_{it}}$ is a vector of observed covariates related to the decision to participate in SNAP, $\boldsymbol{\theta}$ is a q-parameter vector, and v_{it} is the error term. The endogeneity of true participation arises due to the selection mechanism in equation (2) and the correlation of the error terms in equations (1) and (2).

In equation (2), $\boldsymbol{z_{it}}$ includes $\boldsymbol{x_{it}}$, the control variables in Table 1 and an exclusion restriction, namely, the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards. Theoretically, this state-level exclusion restriction should affect the true probability of take-up but should not directly influence the bodyweight outcomes in equation (1) or the propensity to misreport in equation (3). The Personal Responsibility and Work Opportunity Reconciliation Act of August 22, 1996 (PRWORA) mandated all states to implement EBT systems by the year 2002, allowing recipients to authorize their benefits to be electronically transferred into their EBT accounts monthly.[11] The number of states implementing EBT systems grew from 15 in 1996, 37 in 1998, 42 in 2000, 49 in 2002, to all states by 2004.[12] Figure 1 presents the distribution of the percentage of benefits issued via EBT card for the sample period across the U.S., depicting variation across both state and time.

We argue that the percentage of benefits issued via EBT card can influence participation without directly affecting body weight, thus, satisfying the exclusion restriction. In terms of relevance, states that disburse funds through EBT cards instead of direct mail decrease the transaction costs associated with participating. Receiving benefits via an EBT card is less burdensome and can make it easier for the marginal individual to take up the program. Through the "stigma hypothesis," households living in states that issue a higher percentage of benefits via EBT cards instead of coupons may be more likely to take up the program because of the lower participation costs resulting from reduced stigma from using benefits (Moffitt 1983, Currie 2003, Wright et al. 2017).

Since true participation, T^*_{it}, is unobserved, the researcher observes a surrogate, T_{it}, that is generated as

$$T_{it} = T^*_{it} \times R_{it}, \tag{3}$$

[11] Other major changes that came along with PRWORA included removing eligibility for most legal immigrants, limiting benefit receipt to 3 out of 36 months for individuals classified as able-bodied adults without dependents (ABAWDs), and setting the maximum allotments at 100 percent of the change in the Thrifty Food Plan (TFP). A complete description of the changes due to PRWORA can be found at https://www.fns.usda.gov/snap/short-history-snap.

[12] Also, I initially used other state-level policies such as whether the state requires biometric identification, whether the state operates a call center and the proportion of SNAP units with and without earnings with 1-3 month re-certification periods. None of these policies significantly predicted participation in this sample.

where R_{it} is a reporting dummy variable characterized by the reporting equation

$$R_{it} = \mathbf{1}\left(\boldsymbol{w}_{\boldsymbol{it}}'\gamma + u_{it} \geq 0\right), \tag{4}$$

where $\boldsymbol{w}_{\boldsymbol{it}}$ is a vector of observed covariates related to the decision to correctly (or incorrectly) report program participation, $\boldsymbol{\gamma}$ is a p-parameter vector, and u_i is the error term. Again, the endogeneity of misreporting arises because of the mechanism described by equation (4) and the fact that the error terms in equations (1) and (4) are allowed to be correlated.

Equations (2) and (4) together form a complete model of SNAP participation and reporting. It is obvious from the observation mechanism in equation (3) that misreporting captures only false negatives since an individual *correctly* reports participation only if $R_i = 1$ (conditional on true participation) and reports non-participation otherwise.[13] Thus, $R_i = 0$ denotes a "zero-reporter" who might either be a true non-participant or a false negative, both of which are indistinguishable to the researcher.

In equation (4), $\boldsymbol{w}_{\boldsymbol{it}}$ includes $\boldsymbol{x}_{\boldsymbol{it}}$ and additional predictors that are hypothesized to be associated with one's probability of accurately reporting participation. These additional predictors will be excluded from equation (2) but need not be excluded from the outcome equation. The exclusion restrictions in equations (2) and (4) come from different data sources. I use a set of interview and interviewer characteristics available in the NLSY79 as additional predictors of the misreporting mechanism.[14]

The NLSY79 interviewers participate in a survey after the interview process where infor-

[13] Abstracting away from false positives is not a major concern in this context since false negatives are the predominant measurement errors in most means-tested programs such as SNAP.

[14] Although the covariates z_i and w_i may overlap, it is required that they be different in general, at least to avoid the local identification problems discussed in Poirier (1980).

mation is collected on their perceptions regarding the interview process and their interaction with respondents. Some of the post-interview information is the respondent's general attitude during the interview and whether a third party was present with the primary respondent during the interview. I use indicators for the interview mode, indicators of the respondent's attitude during the interview based on the interviewer's remarks, and the gender and race of the interviewer as the excluded predictors of misreporting in equation (4). Collectively, these variables are in the spirit of the "cooperator hypothesis" in Bollinger & David (2001) who find favorable evidence for the hypothesis that respondents with a high propensity to cooperate with the survey are more likely to report their participation truthfully. For example, respondents who are impatient, restless, or hostile during the interview are less cooperative with the survey and are more likely to respond incorrectly. I expect these characteristics to be strongly associated with the probability of misreporting but should not affect one's participation decision or body weight.

The estimation of the model presented above proceeds in two steps. The first stage is estimated as a partial observability model following Poirier (1980), which is followed by ordinary least squares regression in the second stage (regression with a proxy variable). Notice that, from equations (3) and (4), we can write the double-index model for observed participation, T_{it}, as

$$T_{it} = T_{it}^{*} \times R_{it} = \mathbf{1}\left(\boldsymbol{z_{it}'\theta} + v_{it} \geq 0,\ \boldsymbol{w_{it}'\gamma} + u_{it} \geq 0\right). \tag{5}$$

Then the parameters θ (equation (2)), γ (equation (4)), and ρ can be consistently estimated using a partial observability Probit model by maximum likelihood:

$$\Pr[T_{it} = 1|\boldsymbol{w_{it}}, \boldsymbol{z_{it}}] = \Pr\left[-u_{it} \leq \boldsymbol{w'_{it}}\gamma, \quad -v_{it} \leq \boldsymbol{z'_{it}}\theta\right] = F\left(\boldsymbol{w'_{it}}\gamma, \boldsymbol{z'_{it}}\theta, \rho\right) = P_i(\gamma, \theta, \rho), \quad (6)$$

with $F(.,.,.)$ being the joint bivariate Normal cumulative distribution function (CDF) and the log-likelihood function given by

$$L_n(\gamma, \theta, \rho) = \sum_{i=1}^{n} T_i \ln P_i(\gamma, \theta, \rho) + (1 - T_i) \ln\left(1 - P_i(\gamma, \theta, \rho)\right).$$

In the second step, each person's predicted probability of true participation, $\hat{T}^*_{it}$, is obtained as $\hat{T}^*_{it} = \Phi(\boldsymbol{z'_{it}}\hat{\theta})$ using the estimates of θ from the first stage. The predicted values, $\hat{T}^*_{it}$, which are free from self-selection and non-classical measurement error are substituted for T^*_{it} in the outcome equation to obtain

$$y_{it} = \boldsymbol{x'_{it}}\beta + \hat{T}^*_{it}\alpha_{2-STEP} + \eta_{it}, \quad (7)$$

where α_{2-STEP} denotes the average treatment effect of SNAP on the outcome of interest and η_i is the associated disturbance term. Consistency and asymptotic normality results for the two-step estimator are discussed in Nguimkeu et al. (2019).

5. Results and Discussion

This section presents the estimates from the first stage estimation of the partial observability model in equation (5) followed by the second stage results from equation (7). Before turning to the regression results, we report the estimated false negative rate using the first stage estimates from equation (6). Given the misreporting model adopted in this paper, the rate of false negatives for each person is given by

$$P(T_i = 0 \mid T_i^* = 1) = 1 - \frac{P(R_i = 1, T_i^* = 1)}{P(T_i^* = 1)} = 1 - \frac{F\left(\boldsymbol{w}_{it}'\hat{\gamma}, \boldsymbol{z}_{it}'\hat{\theta}, \hat{\rho}\right)}{\Phi(\boldsymbol{z}_{it}'\hat{\theta})}, \tag{8}$$

where $F(.,.,.)$ and $\Phi(.)$ respectively denote the bivariate and univariate normal CDFs, and the hats denote parameter estimates from the first stage estimation of the binary choice model in equation (6). Thus, averaging the quantity in equation (8) across the sample yields a consistent estimate of the population false negative rate. The estimated average false negative rates are 76.2 percent for the full sample, 8.5 percent for females, and 73.4 percent for males. Notably, the estimated false negative rate is largely driven by males and the pattern of results is similar to those obtained in Almada et al. (2016). While not based on validation data, these estimated misreporting rates reinforce the documented evidence of high misreporting rates in survey data.

First Stage Results: True Participation and Reporting Equations

Table 2 presents the maximum likelihood estimates and average marginal effects of the instrument for participation in the true participation equation and the excluded predictors of misreporting in the reporting equation. Panels A and B in Table 2 correspond with equations (2) and (4), respectively. Recall that two sets of variables need to be distinguished for Nguimkeu et al. (2019)'s two-step estimator: (a) instruments for true participation ($\boldsymbol{z}_{it}$), and (b) predictors of misreporting ($\boldsymbol{w}_{it}$). Although these sets of covariates may overlap, they must be different for identification purposes. There must be at least one excluded variable (exclusion restriction) in either the participation or reporting equation that is not included in the other equation. All regressions also include the additional covariates from the outcome equation (1).

As previously mentioned, we instrument true participation using the percentage of SNAP benefits issued by the state via EBT cards. Panel A in Table 2 shows that the percentage of SNAP benefits issued by the state via EBT cards is positive and statistically significantly correlated with the true participation probability for the full sample and by gender. The Wald test of the excluded instrument also suggests that EBT card benefit issuance is a strong predictor of true participation.[15]

Panel B in Table 2 presents the results for the excluded predictors of misreporting in the reporting equation (4). I utilize the NLSY79 interview and interviewer characteristics as predictors of misreporting. These predictors only need to be excluded from the true participation equation but not the outcome equation. The set of excluded predictors describing the reporting mechanism are interview mode, descriptors for respondents' attitude during the interview, gender of the interviewer, and interviewer's race. I expect these covariates to affect the probability of misreporting but not the likelihood of true participation.

The interview mode is a categorical variable with three levels describing features of the interview process. The three levels are as follows: 1=in-person and alone, 2=in-person with third party present, and 3=phone interview. The excluded category in the regressions is level 1 (in-person and alone). After each interview, the NLSY79 interviewers were surveyed

[15]See Table 5 for the corresponding first stage results for the linear IV estimator. Note that the linear IV estimator is not consistent in the presence of non-classical measurement error.

and asked to indicate their perception of the respondent's attitude during the interview. Responses were grouped on a three-point scale: 1=Friendly and interested, 2=Cooperative but not particularly interested, and 3=Impatient, restless, or hostile. This attitude variable is included as a set of dummy variables in the regressions, with the excluded category being level 1 (Friendly and interested). I include dummy variables for whether the interviewee and the interviewer are of the same gender, whether the interviewee and the interviewer are of the same race, and the interaction of these two dummy variables.

Due to a lack of a general theory of misreporting, I do not have strong a priori expectations about the directions of the effects of these interview and interviewer characteristics. Nonetheless, I draw on related literature studying the relationship between the probability of misreporting in surveys and both interview and interviewer characteristics for insights in discussing the results (e.g., Bruckmeier et al. (2015), O'Muircheartaigh & Campanelli (1998), Schober & Conrad (1997), Suchman & Jordan (1990)).

The results in Panel B of Table 2 suggest that the interview, interviewee, and interviewer characteristics are correlated with the probability of truthfully reporting participation in equation (4). For the interview mode indicator, the results show that having an adult present during the interview is positively and statistically significantly associated with truthfully reporting participation, relative to being interviewed alone in person for the full and male samples. This finding is similar to Bruckmeier et al. (2015) who find that survey respondents are more likely to give truthful answers on welfare receipt when a third person is present. However, I do not find statistically significant association between being in a phone interview relative to being interviewed alone in person. Our results are consistent with the literature that finds that measurement error in responses to sensitive questions varies significantly with the mode of administering the survey, especially when the answers may be stigmatized or viewed as socially undesirable (e.g., Tourangeau & Yan 2007).

Turning to the respondents' attitude characteristics, Bollinger & David (1997, 2001, 2005) discuss the so-called "cooperator hypothesis" which associates respondent's cooperativeness with their willingness to provide accurate responses. They provide evidence supporting the hypothesis that cooperators tend to give more accurate responses. I find evidence that the respondent's attitude during the interview is associated with the probability of truthfully reporting participation. The results suggest that impatient, restless, or hostile interviewees are less likely to report participation truthfully during the interview. This association is statistically significant for the full sample and men. For women, respondents who are not interested but cooperative are less likely to report participation truthfully.

Finally, a related literature studies how interviewers (e.g., interviewer demographic characteristics) may influence the accuracy of survey responses. One might expect interviewers' gender and race to affect survey responses when respondents know or can perceive the interviewer's demography.[16] I include three variables controlling for interviewer effects: a same-gender indicator variable that takes on 1 if both respondent and interviewer are of the same sex and 0 otherwise, a same-race indicator variable that assumes 1 if both respondent and interviewer are of the same race (i.e., either black, Hispanic, or non-black/non-Hispanic) and 0 otherwise, and the interaction of these two dummy variables. For women, the results show that being interviewed by someone of the same sex is reduces the propensity to report participation truthfully.

The overall first stage results of the two-step estimator suggest that the instruments for

[16]See Weisberg (2009) for a more detailed review of the literature on interviewer effects in surveys.

participation and predictors of misreporting are strongly correlated with both true participation and truthfully reporting participation.

Second Stage Results: The Impacts of SNAP on obesity

We now present the estimated causal effect of SNAP participation on BMI and the probability of being obese using Nguimkeu et al. (2019)'s two-step estimator in Table 3. Generally, we do not find any evidence of statistically significant effects of SNAP participation on BMI or the probability of being obese. From Panel A of Table 3, the estimated treatment effect of SNAP participation on BMI for women is a decrease of 1.6 units, which implies a reduction in weight of approximately 10 pounds, albeit not statistically significant.[17] For the full and men, the estimated treatment effects imply a weight decrease (but not statistically different from zero) of about 0.6 pounds and 7.3 pounds, respectively.

We also do not find any statistically significant effects of SNAP participation on the probability of being obese for the primary sample in Panel A of Table 3. The estimated coefficients suggest that SNAP participation decreases the probability of being obese by 4.4 percentage points. The coefficient estimates are larger (and insignificant) when we separate by gender and are 9.3 and 8.1 percentage points for men and women, respectively.

Robustness and Comparison with other Methods

First, I investigate sensitivity to using reported measures of height and weight, which are also subject to measurement error, in the computation of BMI and the indicator of being

[17] The mean height in the final data set is 65.75 inches, suggesting that relative to the average height, a one BMI unit change translates into a weight change of 6.12 pounds.

obese. I re-estimate the models after re-constructing BMI using predicted height and weight based on Third National Health and Nutrition Examination Survey (NHANES III) following Courtemanche et al. (2015). Using the NHANES III as a validation data set, I regress actual weight and height on the cubic basis splines of the percentile rank of the respective reported measures and a polynomial in age by race and gender. After that, I predict weight and height in my NLSY sample using the estimated relationship between actual and reported measures in the NHANES III data, which are then used to calculate an adjusted BMI and the probability of being obese.

The results using the adjusted weight outcome measure are reported in Panel B of Table 3. The estimates of SNAP's impacts on BMI and the probability of being obese using the adjusted outcomes are very similar to using the original NLSY height and weight values. The signs of the estimated coefficients are unchanged, although the magnitudes are larger for BMI and smaller for the probability of being obese; however, all the estimates remain indistinguishable from zero.

Second, as previously mentioned, SNAP eligibility is based on financial, non-financial, and categorical eligibility rules. I initially restricted the analysis sample to respondents below 250% of the federal poverty level since the literature recognizes that the federal gross income eligibility threshold of 130% is too restrictive. However, I re-estimated the model with the sample restricted to 185% and 130% of the federal poverty level for the full sample and women.[18] Table 6 presents the results for these alternative eligibility criteria and shows that the pattern of results is unchanged in terms of statistical significance. For the full sample, the

[18] Due to non-convergence partly resulting from the reduced sample size, results are not reported for men using these alternative eligibility criteria.

signs of treatment effects are positive but continue to be negative for women across the lower eligibility thresholds. For women, SNAP's estimated impact on BMI remains negative and statistically insignificant, with magnitudes indicating weight reductions of about 7.8 pounds and 9.8 pounds for 185% and 130% of the federal poverty level, respectively. Similarly, the estimated (insignificant) effects on the probability of being obese are 4.4 and 5.2 percentage point reductions for 185% and 130% of the federal poverty level, respectively.

Finally, while our preferred estimates are those from the two-step estimator, Table 4 presents estimates of SNAP's impacts using the ordinary least squares (OLS) and linear instrumental variable (IV) estimators merely for comparison. The OLS estimates of SNAP's effects are positive and statistically significant, with participation being associated with an increase in weight of about 5.6 pounds and 5.2 pounds for the full sample and women, respectively. The coefficient estimate is slightly smaller in magnitude for men but is not statistically significant. The OLS estimates also yield a positive and statistically significant effect of SNAP participation on the probability of being obese. The OLS estimator is biased and inconsistent due to both self-selection and misreporting, with the direction of bias being consistent with adverse selection into the program.

The IV estimator does address the endogeneity of participation but is inconsistent for the treatment effect in the presence of misreporting. The IV estimates in Table 4 use the same instrument for participation (i.e., the percentage of SNAP benefits issued by the state via electronic benefit cards) as does the two-step estimator, but does nothing to address misreporting. The first stage results for the IV estimator are summarized in Table 5, showing high and statistically significant F-statistics. The IV estimates maintain the same sign as the two-step estimator but are notably larger in magnitude and also more imprecise. From

Table 4, the IV estimates of SNAP's impact on BMI and the probability of being obese are negative and statistically insignificant but with magnitudes implying implausibly large weight reductions for the full sample and men. This pattern of results for the IV estimator in the presence of misreporting is not new and Almada et al. (2016) conclude that, "[i]n the presence of misreporting, our comparison between parametric estimates and nonparametric bounds further suggests that instrument-based corrections, even when the proposed instruments pass standard IV tests, may perform worse than applying no correction at all."

The two-step estimator's overall results suggest no statistically significant effects of SNAP on BMI and the probability of being obese when we account for both the endogeneity and possible misreporting of participation in one unifying framework. Moreover, the findings in this paper depart from previous studies suggesting positive associations between SNAP participation and obesity, especially for females (Townsend et al. 2001, Gibson 2003, Chen et al. 2005, Kaushal 2007, Meyerhoefer & Pylypchuk 2008, Baum 2011). Although not statistically significant, the sign of SNAP's estimated impacts suggests plausible reductions in BMI and the probability of being obese for females that are linked to SNAP participation.

6. Conclusion

The Supplemental Nutrition Assistance Program remains the largest nutrition assistance program in the United States and currently influences the diets of about 1 in 7 Americans. The existing literature mostly finds a positive impact of SNAP on the probability of being obese, especially for females. Few studies examining SNAP's effects on adult obesity have addressed the well-known problem of high misreporting rates in national surveys. Not only is SNAP participation subject to severe misreporting, but such measurement error may be endogenous. This paper estimates the causal effect of SNAP on adult obesity in the presence of endogenous misreporting using Nguimkeu et al. (2019)'s a novel identification strategy that explicitly addresses both the endogeneity of participation and the systematic nature of misreporting.

In contrast to most previous studies, we do not find evidence that SNAP participation is associated with obesity, even for women, when we address endogeneity and measurement error in a unifying framework. Our results also highlight the problems with using standard instrumental variable techniques in estimating treatment effects when the treatment variable is misclassified. We demonstrate how available information on the characteristics of the survey and interview process can be exploited to strengthen identification to address measurement error. This approach is important because administrative data may have better quality measures of participation but are not easily accessible in studying many important questions of policy interest such as SNAP's impacts. Even when such administrative data are available, they may be inadequate or imperfect, suggesting an important role for methods that provide credible answers using survey data (Courtemanche et al. 2019).

SNAP has no specific objective to influence obesity directly, but understanding the causal link between SNAP and obesity can help us evaluate the merits of ongoing proposals aimed at influencing the nutritional choice of the millions of Americans who benefit from it.Gundersen (2015) discusses state- and national-level proposals aimed at restricting participants' food choices and prohibiting the purchase of foods deemed as "unhealthy" or "junk." For instance, the State of New York's much-publicized waiver request to the U.S. Department of Agriculture (USDA) in 2010 to permit a two-year demonstration project that will ban the use of SNAP benefits to purchase any beverage with more than ten calories per 8-ounce serving (Gundersen 2015, Kansagra et al. 2015). The USDA denied the State of New York's proposal, which would have banned sports drinks, soda, vegetable drinks and iced tea, while permiting milk and fruit juices.[19] Without a causal SNAP-obesity link, it is unclear whether proposals to restrict SNAP participants' consumption choices will reduce the prevalence of obesity among low-income households. Moreover, such proposals also come with potential unintended consequences, including stigmatization of program participation and higher transaction and program administration costs.

In terms of policy recommendations, our results caution against making policy prescriptions regarding the obesity pandemic especially in terms of SNAP program changes to make it more "anti-obese." A few caveats are noteworthy. This study focused on false negative reporting errors which are the more prevalent case of reporting errors. Future work should address bidirectional reporting error. As pointed out by other researchers, there are more accurate measures of fatness besides BMI used in this study (Burkhauser & Cawley 2008).

[19] Similar state-level proposals have been made by Minnesota, Maine, Wisconsin, and South Carolina although the USDA has granted none. Typical discussions about restructuring SNAP relates to the food and nutritional choices of recipients. For instance, the Washington Post recently reported that the USDA has rejected for the second time (after doing so in 2015) the state of Maine's request to ban the purchase of sugar-sweetened beverages (soft drinks) and candy with SNAP benefits, at least to make the program anti-obese (Dewey 2018). At the national level, an amendment sponsored by Senator Tom Coburn in 2013 to prohibit the use of SNAP benefits to purchase junk food was not passed (Gundersen 2015). A recent NPR story discusses the Trump administration's budget proposal for the fiscal year 2019, which aims to disburse SNAP benefits partly in the form of the so-called "USDA Foods package." (Hunzinger et al. 2018).

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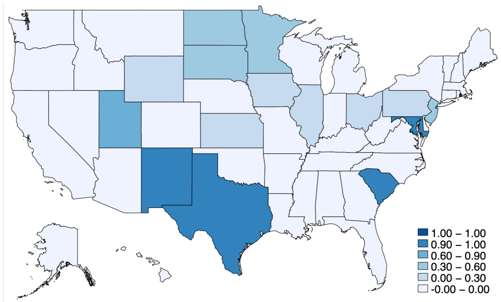
7. Figures and Tables



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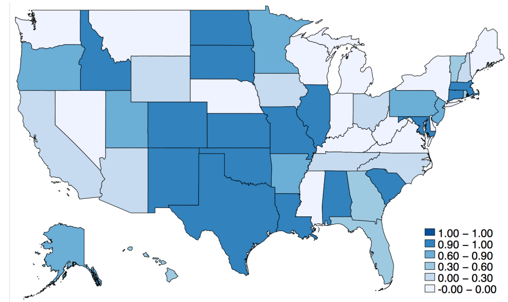

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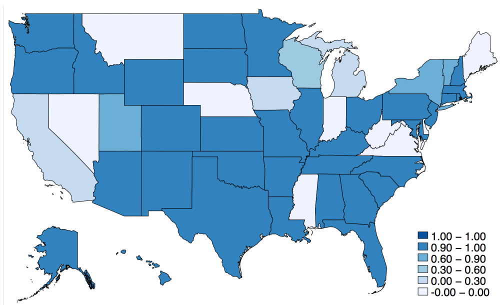

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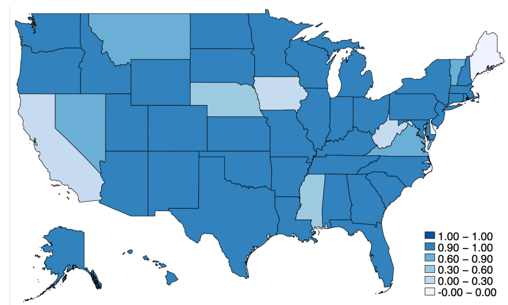
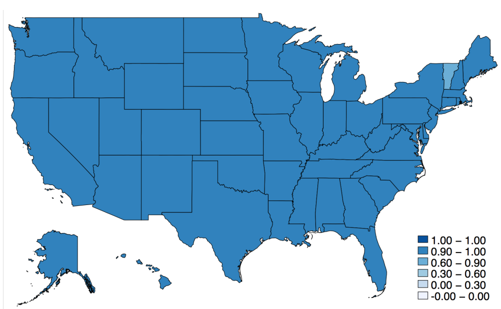


(d) 2002



(e) 2004

Figure 1: The distribution of the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards (by year)

Table 1: Summary Statistics by SNAP Participation Status

	Nonparticipants	**Participants**	**Full Sample**
Dependent variables:			
Body Mass Index	27.60	29.35	27.81
	(6.114)	(7.433)	(6.307)
Obese (=1)	0.281	0.381	0.293
	(0.449)	(0.486)	(0.455)
Control variables:			
Age	38.71	38.30	38.67
	(3.672)	(3.772)	(3.686)
Female	0.529	0.729	0.553
	(0.499)	(0.445)	(0.497)
Hispanic	0.0786	0.101	0.0812
	(0.269)	(0.301)	(0.273)
Black	0.193	0.331	0.209
	(0.395)	(0.471)	(0.407)
Household Size	3.299	3.578	3.331
	(1.725)	(1.871)	(1.745)
Married	0.471	0.248	0.445
	(0.499)	(0.432)	(0.497)
Mother's education (High school graduate or higher)	0.548	0.427	0.534
	(0.498)	(0.495)	(0.499)
High school graduate or higher	0.848	0.739	0.835
	(0.359)	(0.439)	(0.371)
Number of children	1.559	1.985	1.609
	(1.424)	(1.560)	(1.447)
Lives in Urban Area	0.673	0.708	0.677
	(0.469)	(0.455)	(0.468)
WIC	0.0500	0.199	0.0673
	(0.218)	(0.399)	(0.251)
SSI	0.0547	0.267	0.0795
	(0.227)	(0.442)	(0.271)
AFDC/TANF	0.0128	0.391	0.0570
	(0.112)	(0.488)	(0.232)
Average Weekly Hours worked (Past Calendar Year)	34.72	20.62	33.07
	(21.13)	(22.13)	(21.72)
Total Net Family Income (2004 dollars)	24030.2	14838.2	22956.2
	(14930.1)	(9869.3)	(14729.4)
Observations	8,473	1,452	9,925

Standard errors in parentheses are adjusted for the complex design survey design of the NLSY79. Based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income lower than 250% of the federal poverty level.

Table 2: Partial Observability Probit Model of SNAP Participation

	Full sample		Female sample		Male sample	
Panel A: True Participation Equation	Coefficients	Marginal Effects on P(T*=1)	Coefficients	Marginal Effects on P(T*=1)	Coefficients	Marginal Effects on P(T*=1)
Percentage of Benefits issued via EBT Card	0.376***	0.117***	0.395***	0.0574***	0.340***	0.0898**
	(0.103)	(0.0327)	(0.0961)	(0.0139)	(0.118)	(0.0435)
Wald Test of Excluded Instruments p-value	0.0003		0.0000		0.0040	
Panel B: True Reporting Equation	Coefficients	Marginal Effects on P(R=1)		Marginal Effects on P(R=1)		Marginal Effects on P(R=1)
Interview Mode Dummies						
Any Adult Present During Interview	0.190**	0.0450***	0.0737	0.0208	0.343***	0.0987***
	(0.0753)	(0.0173)	(0.152)	(0.0430)	(0.126)	(0.0363)
Phone Interview	-0.0129	-0.00295	-0.193	-0.0576	0.0574	0.0170
	(0.0593)	(0.0136)	(0.146)	(0.0430)	(0.0933)	(0.0276)
Respondent's Attitude Dummies						
Not Interested But Cooperative	-0.0487	-0.0111	-0.346***	-0.106**	-0.00913	-0.00269
	(0.0613)	(0.0140)	(0.134)	(0.0422)	(0.0949)	(0.0279)
Impatient/Restless/Hostile	-0.421***	-0.0887***	-0.111	-0.0325	-0.876***	-0.256***
	(0.155)	(0.0332)	(0.255)	(0.0762)	(0.306)	(0.0891)
Interviewer Characteristics						
Same Gender Dummy (Interviewer & Interviewee)	0.160*	0.0156	-1.163**	-0.142*	0.259	0.0447
	(0.0936)	(0.0181)	(0.479)	(0.0750)	(0.178)	(0.0350)
Same Race Dummy (Interviewer & Interviewee)	0.0676	-0.00844	-0.989	-0.0202	0.0137	-0.00285
	(0.0860)	(0.0136)	(0.609)	(0.0386)	(0.102)	(0.0290)
Interaction of Same-gender & Same-race Dummy (Interviewer & Interviewee)	-0.178*		0.969		-0.204	
	(0.0986)		(0.610)		(0.241)	
Wald Test of Excluded Instruments p-value	0.0403		0.0288		0.0957	
Observations	9,925		5710		4,215	

Standard errors in parentheses. Results are based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income below 250% of the federal poverty level. Results are based on maximum likelihood estimation of the partial observability model in equation (5). Panel A reports the true participation equation parameter estimates and marginal effects from equation (2). Panel B reports the reporting equation parameter estimates and marginal effects from equation (4). In Panel B, the excluded category for the interview mode dummies is "In person (alone)" and that for the respondent attitude dummies is "Friendly and Interested." Regressors not reported in here include respondent's age, race, household size, number of children, weekly hours worked in the past calendar year, current employment status, educational attainment, mother's education, marital status, log of income, time fixed effects, and indicators for living in an urban area, receiving WIC benefits, receiving AFDC/TANF, and receiving SSI benefits.

$*p < 0.10, **p < 0.05, ***p < 0.010$

Table 3: Effects of SNAP Participation on Obesity (Two-step Estimator)

	Panel A: Main sample[a]		Panel B: Adjusted sample[b]	
	BMI	Obese	BMI	Obese
Full sample (N=9,925)				
SNAP Participation	-0.107	-0.0444	-0.320	-0.0182
	(1.157)	(0.0792)	(1.096)	(0.0744)
Female sample (N=5,710)				
SNAP Participation	-1.602	-0.0928	-1.468	-0.0691
	(1.781)	(0.110)	(1.607)	(0.105)
Male sample (N=4,215)				
SNAP Participation	-1.194	-0.0816	-1.009	-0.0804
	(1.580)	(0.122)	(1.548)	(0.115)

[a] Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2-STEP) estimation. Panel A results are based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income lower than 250% of the federal poverty level. Regressors not reported in here include respondent's age, race, gender, household size, number of children, weekly hours worked in the past calendar year, current employment status, educational attainment, mother's education, marital status, log of income, time fixed effects, and indicators for living in an urban area, receiving WIC benefits, receiving AFDC/TANF, and receiving SSI benefits.

[b] Additionally, Panel B results are based on the main sample except BMI is calculated from predicted height and weight as described in the text following Courtemanche et al. (2015).

$*p < 0.10, **p < 0.05, ***p < 0.010$

Table 4: **Effects of SNAP Participation on Obesity (Comparison of Estimators)**

	BMI			Obese		
	OLS	IV	2-STEP	OLS	IV	2-STEP
Full sample (N=9,925)						
SNAP Participation	0.911***	-4.070	-0.107	0.0448**	-0.283	-0.0444
	(0.321)	(5.573)	(1.157)	(0.0208)	(0.410)	(0.0792)
Female sample (5,710)						
SNAP Participation	0.842**	-1.785	-1.602	0.0224	-0.158	-0.0928
	(0.410)	(7.407)	(1.781)	(0.0253)	(0.505)	(0.110)
Male sample (4,215)						
SNAP Participation	0.749	-8.921	-1.194	0.0829**	-0.509	-0.0816
	(0.491)	(8.426)	(1.580)	(0.0365)	(0.692)	(0.122)

Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2-STEP) estimation. Results are based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income lower than 250% of the federal poverty level. Regressors not reported in here include respondent's age, race, gender, household size, number of children, weekly hours worked in the past calendar year, current employment status, educational attainment, mother's education, marital status, log of income, time fixed effects, and indicators for living in an urban area, receiving WIC benefits, receiving AFDC/TANF, receiving SSI benefits, and indicators for having an infant ($\leq$5 years) and an elderly person ($\geq$ 65 years) living in the home.
$*p < 0.10, **p < 0.05, ***p < 0.010$

Table 5: First Stage IV Estimates

Dependent Variable: SNAP Participation			
	Full Sample	Female	Male
Percentage of Benefits issued via EBT Card	0.0428***	0.0459***	0.0384***
	(0.00867)	(0.0127)	(0.0109)
F- statistics	24.36***	13.05***	12.48***
Observations	9,925	5,710	4,215

Standard errors in parentheses. Results are based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income lower than 250% of the federal poverty level. Regressors not reported in here include respondent's age, race, gender, household size, number of children, weekly hours worked in the past calendar year, current employment status, educational attainment, mother's education, marital status, log of income, time fixed effects, and indicators for living in an urban area, receiving WIC benefits (female-only regressions), receiving AFDC/TANF, and receiving SSI benefits.
$*p < 0.10, **p < 0.05, ***p < 0.010$

Table 6: Effects of SNAP Participation on Obesity (Alternative Eligibility Samples)

	Panel A: Full sample		Panel B: Female sample	
	BMI	Obese	BMI	Obese
185% FPL				
SNAP Participation	1.020	0.0393	-1.274	-0.0444
	(1.313)	(0.0934)	(1.675)	(0.104)
Observations	7,001	7,001	4,190	4,190
130% FPL				
SNAP Participation	1.248	0.0684	-1.594	-0.0520
	(1.551)	(0.102)	(1.904)	(0.112)
Observations	4,741	4,741	2,923	2,923

Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2-STEP) estimation. Results are based on the 1996-2004 biennial waves of the NLSY79, restricted to individuals or households with income lower than 250%, 185%, and 130% of the federal poverty level. Regressors not reported in here include respondent's age, race, gender, household size, number of children, weekly hours worked in the past calendar year, current employment status, educational attainment, mother's education, marital status, log of income, time fixed effects, and indicators for living in an urban area, receiving WIC benefits, receiving AFDC/TANF, and receiving SSI benefits

$*p < 0.10, **p < 0.05, ***p < 0.010$