Essays On Misreporting, The Supplemental Nutrition Assistance Program, and Adult Health

Augustine Denteh

Follow this and additional works at: https://scholarworks.gsu.edu/econ_diss

Recommended Citation
https://scholarworks.gsu.edu/econ_diss/143

This Dissertation is brought to you for free and open access by ScholarWorks @ Georgia State University. It has been accepted for inclusion in Economics Dissertations by an authorized administrator of ScholarWorks @ Georgia State University. For more information, please contact scholarworks@gsu.edu.
ABSTRACT

ESSAYS ON MISREPORTING, THE SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM, AND ADULT HEALTH

BY

AUGUSTINE DENTEH

MAY, 2018

Committee Chair: Dr. Rusty Tchernis
Major Department: Economics

This dissertation examines the causal impacts of the Supplemental Nutrition Assistance Program (SNAP) on adult weight and nutrition outcomes using novel approaches to address nonrandom selection into the program and survey data quality issues. The overarching objective of this study is to provide credible estimates of the effects of SNAP to help policy makers and administrators engage in meaningful debates and prescribe nutrition assistance policies that promote the overall well-being of low-income Americans as well as mitigate any unintended consequences of the largest nutrition assistance program in the United States.

The first chapter proposes a model to estimate treatment effects when program participation is endogenously misreported. This chapter shows that failure to account for endogenous misreporting can result in the estimate of the treatment effect having an opposite sign from the true effect. Expressions for the asymptotic bias of the ordinary least squares (OLS) and instrumental variable (IV) estimators are provided and the conditions under which sign reversal may occur are discussed. This chapter then develops a method for eliminating this bias when researchers have access to information related to both participation and misreporting. The root-n consistency and asymptotic normality of the proposed estimator are established.
after which Monte Carlo simulations are used to demonstrate the remarkable performance of the estimator in small samples.

The second chapter estimates the effect of SNAP on adult obesity addressing self-selection and endogenous misreporting of participation. Using the methodology developed in the first chapter of this dissertation, the second chapter estimates the causal impact of SNAP on obesity using data from the National Longitudinal Survey of Youth – 1979 cohort. From a simple partial observability model of participation and misreporting, I predict probabilities of participation which are used to consistently estimate the average effect of SNAP on body mass index (BMI). The estimated misreporting model confirms some prior findings in the literature regarding the correlates of reporting error. However, contrary to most previous studies, I do not find any evidence of a statistically significant effect of SNAP on BMI.

The third chapter studies the potential problems with administrative records and their implications for econometric estimates using the National Household Food Acquisition and Purchase Survey (FoodAPS) data set, which contains two different administrative measures of SNAP participation as well as a survey-based measure. This chapter first documents substantial ambiguity in the two administrative participation variables and show that they disagree with each other almost as often as they disagree with self-reported participation. Estimated participation and misreporting rates can be meaningfully sensitive to choices made to resolve this ambiguity and disagreement. Finally, this chapter documents similar sensitivity in regression estimates of the associations between SNAP and food insecurity, obesity, and the Healthy Eating Index. These results serve as a cautionary tale about uncritically relying on linked administrative records when conducting program evaluation research.
ESSAYS ON MISREPORTING, THE SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM, AND ADULT HEALTH

BY

AUGUSTINE DENTEH

A Dissertation Submitted in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy in the Andrew Young School of Policy Studies of Georgia State University

GEORGIA STATE UNIVERSITY
2018
ACCEPTANCE

This dissertation was prepared under the direction of the candidate’s Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

Dissertation Chair: Dr. Rusty Tchernis

Committee: Dr. Charles Courtemanche
Dr. Ian McCarthy
Dr. Nguedia Pierre Nguimkeu

Electronic Version Approved:

Sally Wallace, Dean
Andrew Young School of Policy Studies
Georgia State University
May, 2018
DEDICATION

This dissertation is dedicated to Mary Akosua Kumavie and Victor Agbeko Denteh.
ACKNOWLEDGMENTS

I have been privileged to be inspired and guided by many great people in writing this dissertation. I have benefited from the unsearchable love of my family and close friends, the lessons of excellent professors, the wise counsel and admonishment of mentors, and generous financial support through several fellowships and grants.

I am eternally indebted to my dissertation committee for their tireless and concerted efforts to support me throughout my graduate studies. To Dr. Ian McCarthy, thank you for being a patient and dependable mentor. To Dr. Charles Courtemanche, thank you for always providing insightful feedback and being available to help in whatever ways you can. To Dr. Nguedia Pierre Nguimkeu, I cannot thank you enough for your patience and perseverance throughout the writing of my dissertation. Thank you for extensive and thought-provoking discussions at any hour, and for deepening my interest in econometrics. To my advisor, Dr. Rusty Tchernis, thank you for believing in me against all the odds. Words will fail me to adequately express my gratitude for giving me the opportunity to learn and think like an economist. Your perseverance and willingness to help are unparalleled, and I am inspired every day to be a better researcher. Above all, thank you for being a kind human being.

I am grateful to my parents to whom I dedicate this work. Your sacrifices are without limit, and you remain an example to us. Thank you to all my siblings – Lucy, Alex, Joyce, Bright, Mawuli, and David – for your encouragement and prayers. To my many friends and colleagues, I appreciate you. To Delali Agbenyegah, Victoria Azakudo, Wendyam Koanda, Delali Kumavie, and Makayla Palmer, you will forever have a special place in my heart.
Table of Contents

Acknowledgments ......................................................... v
List of Tables ......................................................... viii
List of Figures ......................................................... ix
1 Introduction ......................................................... 1

2 On the Estimation of Treatment Effects with Endogenous Misreporting ........................................ 6
  2.1 Introduction ..................................................... 6
  2.2 Framework ....................................................... 10
    2.2.1 Model with Endogenous Misreporting .................. 10
    2.2.2 Bias due to Endogenous Misreporting ................. 13
    2.2.3 IV Estimator under Endogenous Misreporting ........ 16
  2.3 The Proposed Estimator ....................................... 17
    2.3.1 First Step Estimation ................................... 18
    2.3.2 Second Step Estimation ................................ 19
  2.4 Monte Carlo Simulations ...................................... 22
    2.4.1 Simulation setup ........................................ 22
    2.4.2 Simulation Results ...................................... 24
  2.5 Conclusion ..................................................... 28
  2.6 Figures and Tables ............................................ 29

3 The Effect of SNAP on Obesity in the Presence of Endogenous Misreporting .................................. 33
  3.1 Introduction ..................................................... 33
  3.2 Background and Conceptual Framework ....................... 37
    3.2.1 Brief Overview of SNAP ................................ 37
    3.2.2 Can SNAP Participation Influence BMI? ............... 39
  3.3 Related Literature and Misreporting of SNAP .............. 42
  3.4 Data ............................................................ 45
  3.5 Methodology .................................................... 47
    3.5.1 Estimation Procedure ................................... 52
  3.6 Results and Discussion ....................................... 53
    3.6.1 First Stage Estimation ................................ 54
    3.6.2 Second Stage Estimation ................................ 57
  3.7 Conclusion ..................................................... 60
  3.8 Figures and Tables ............................................ 62
## List of Tables

<table>
<thead>
<tr>
<th>Table</th>
<th>Description</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.1</td>
<td>Monte Carlo Simulations</td>
<td>30</td>
</tr>
<tr>
<td>2.2</td>
<td>Sensitivity of the Proposed Estimator to Misspecification</td>
<td>31</td>
</tr>
<tr>
<td>2.3</td>
<td>Sign-switching intervals of $\alpha$ for the OLS</td>
<td>32</td>
</tr>
<tr>
<td>3.1</td>
<td>Summary Statistics by SNAP Participation Status</td>
<td>64</td>
</tr>
<tr>
<td>3.2</td>
<td>Partial Observability Probit Model of Observed Participation</td>
<td>65</td>
</tr>
<tr>
<td>3.3</td>
<td>Effects of SNAP participation on BMI</td>
<td>66</td>
</tr>
<tr>
<td>3.4</td>
<td>First Stage IV Estimates</td>
<td>67</td>
</tr>
<tr>
<td>3.5</td>
<td>Effects of SNAP on Adjusted BMI</td>
<td>68</td>
</tr>
<tr>
<td>3.6</td>
<td>Effects of SNAP on BMI with Alternative Eligibility</td>
<td>69</td>
</tr>
<tr>
<td>4.1</td>
<td>Summary Statistics</td>
<td>97</td>
</tr>
<tr>
<td>4.2</td>
<td>Possible Classifications for Administrative Participation Measure from Caseload Data (ADMIN)</td>
<td>98</td>
</tr>
<tr>
<td>4.3</td>
<td>Possible Classifications for Administrative Participation Measure from EBT Transactions (ALERT)</td>
<td>99</td>
</tr>
<tr>
<td>4.4</td>
<td>Estimated Participation and Misreporting Rates under Different Approaches to Using ADMIN and ALERT Separately</td>
<td>100</td>
</tr>
<tr>
<td>4.5</td>
<td>Estimated Participation and Misreporting Rates under Different Approaches to Combining ADMIN and ALERT</td>
<td>100</td>
</tr>
<tr>
<td>4.6</td>
<td>Extent of Disagreement among SNAP Participation Variables</td>
<td>101</td>
</tr>
<tr>
<td>4.7</td>
<td>Regression Results using Each Participation Measure Separately</td>
<td>102</td>
</tr>
<tr>
<td>4.8</td>
<td>Regression Results using Each Participation Measure Separately</td>
<td>103</td>
</tr>
<tr>
<td>4.9</td>
<td>Regression Results Combining Participation Measures through Various Rules</td>
<td>104</td>
</tr>
<tr>
<td>4.10</td>
<td>10-Question Food Security Question in FoodAPS</td>
<td>105</td>
</tr>
</tbody>
</table>
List of Figures

2.1 Illustration of the OLS bias ........................................... 29
3.1 Neoclassical framework for analyzing impact of SNAP on consumption 62
3.2 First Stage (Partial Observability Model) .............................. 62
3.3 The distribution of the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards (by year) ....................... 63
CHAPTER 1

Introduction

Obesity is one of the leading health problems in the U.S., with an adult, age-adjusted prevalence rate of 37.7% (35% for men and 40.4% for women) as of 2014 (Flegal, Kruszon-Moran, Carroll, Fryar & Ogden 2016). The debilitating negative consequences of obesity are well known. For instance, 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, Willett, Rotnitzky & Manson 1995, McGee, Collaboration et al. 2005, Kim & Kawachi 2008). Also, there are significant health care costs of obesity (Finkelstein, Trogdon, Cohen & Dietz 2009, Cawley & Meyerhoefer 2012) as well as adverse effects of obesity on labor market outcomes (Bhattacharya & Bundorf 2009).

The Supplementary Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program, is the largest nutrition assistance program in the U.S. that influences the diets of millions of low-income individuals and households annually with the goal of reducing food insecurity and supporting a healthy population. Although SNAP has no specific objective to influence obesity directly, obtaining accurate estimates of its effects on health outcomes in general and obesity, in particular, is critical in the broader ongoing policy debates surrounding its existence and role in the lives of the millions of Americans who benefit from it. For instance, understanding the causal link between SNAP and obesity can help us understand and evaluate the merits of recent proposals aimed at influencing the nutritional choice and well-being of participants. It is often asserted 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, the high adult obesity rates coupled with a higher prevalence among low-income households targeted by SNAP motivates a thorough understanding of the relationship between SNAP and obesity.

Evaluating the impacts of SNAP is challenging, however. Non-random selection of people into the program complicates the estimation of the causal impact of SNAP on obesity. SNAP participation is a choice and hence endogenous. Participants may differ in systematic ways from income-eligible non-participants, making it difficult to obtain credible estimates of SNAP’s effect on obesity. Such factors as current or expected future health, human capital characteristics, financial stability, time and risk preferences, preferences for food and other health inputs, and attitudes toward work are simultaneously related to SNAP participation and health outcomes (Currie 2003, Kreider, Pepper, Gundersen & Jolliffe 2012).

Several papers examine the impact of SNAP on a host of outcomes, including poverty, food insecurity, food consumption, and weight outcomes.1 In terms of SNAP’s relationship with obesity, a common finding is that SNAP participation is positively correlated with the probability of being obese or overweight (Townsend, Peerson, Love, Achterberg & Murphy 2001, Gibson 2003, Chen, Yen & Eastwood 2005, Baum 2011, Meyerhoefer & Pylypchuk 2008). However, even when one accounts for the endogeneity of participation, the estimated treatment effects may not be credible due to the rampant measurement error in self-reported or survey-based participation measure.

Just like many other social programs, SNAP is substantially misreported in survey data, sometimes with misclassification levels close to 50% (Meyer, Mok & Sullivan 2009). When a binary regressor is misreported (or misclassified), the

---

1For reviews, see Currie (2003), Bartfeld, Gundersen, Smeeding & Ziliak (2015), and Hoynes & Schanzenbach (2016).
measurement error is necessarily negatively correlated with the true underlying value of the regressor, thus making the classical measurement error assumptions implausible. While earlier researchers show that exogenous misreporting leads to attenuation bias, the consequences of endogenous misreporting can be much more severe. This dissertation examines the causal impacts of SNAP on adult weight and nutrition outcomes using novel approaches to address nonrandom selection into the program and survey data quality issues.

The first essay proposes a solution to the problem of the estimation of treatment effects of a binary regressor in the presence of endogenous misreporting and possibly endogenous participation. Previous studies on misclassified binary regressors are mostly concerned with exogenous or random misreporting (Aigner 1973, Brachet 2008, Lewbel 2007, Mahajan 2006, Frazis & Loewenstein 2003), where it is commonly assumed that misclassification probabilities depend only on the true treatment status and are thus, independent of measurement errors and other regressors.

Our proposed two-step estimator relaxes this arguably strong assumption and shows that, when the researcher has access to information related to why individuals misreport, the treatment effect can be consistently estimated. We derive and prove the consistency and asymptotic normality of our proposed two-step estimator and show that OLS and IV estimators are inconsistent and may yield wrong (opposite) signs from the true effect. We also provide Monte Carlo simulations to this effect. To our knowledge, this paper is the first attempt to provide point estimates of treatment effect in the context of endogenous misreporting of a binary treatment variable.

The second chapter estimates the effect of SNAP on adult obesity addressing self-selection and endogenous misreporting of participation using the methodology developed in the first chapter of this dissertation. This paper examines whether
SNAP participation is linked to weight gain in adults when we account for the typical case of false negative reporting errors. This paper makes two contributions. First, using a novel approach developed in Nguimkeu, Denteh & Tchernis (2017), 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, the results highlight the consequences of misreporting on estimated treatment effects in empirical work by comparing our approach to standard estimators. I do not find evidence that SNAP participation significantly increases weight for the full sample or separately by gender. This finding departs from most previous studies suggesting positive impacts of SNAP on adult weight outcomes, especially for females. Even when SNAP participation is positively associated with BMI such as for males and the full sample, the magnitude of the effects is not large enough to cause people of normal weight to become obese.

The third essay explores several issues that may arise from using administrative data to overcome measurement error in program participation. A growing literature documents the problems with relying on survey measures of program participation, which suffer from significant reporting error when conducting impact evaluations (Meyer, Mok & Sullivan 2015). Administrative data are ordinarily assumed to be the “gold standard” to overcoming these econometric challenges, but relatively little evidence exists on the potential problems with administrative records or econometric strategies to address them.

This essay utilizes the FoodAPS data, which combines a panel of household purchases with a survey and linked administrative data on Supplemental Nutrition Assistance Program (SNAP) participation from both state enrollment records and Electronic Benefits Transfer (EBT) card expenditures to investigate these issues. FoodAPS provides a unique opportunity to evaluate the reliability of administrative sources of participation measures together with self-reported participation.
First, we document substantial ambiguity in both of the administrative measures and show that they are only slightly more strongly correlated with each other than with self-reported participation. Estimated SNAP participation and misreporting rates vary with the coding rules used to resolve this ambiguity and disagreement. We then examine the relationships between SNAP and food insecurity, obesity, and the Healthy Eating Index. While the signs of regression estimates are not sensitive to different coding rules, their magnitudes and levels of statistical significance exhibit meaningful variability. In sum, these results serve as a cautionary tale about uncritically relying on linked administrative records when conducting program evaluation research.
CHAPTER 2

On the Estimation of Treatment Effects with Endogenous Misreporting

2.1 Introduction

This paper proposes a solution to the problem of identification and estimation of treatment effects in parametric regressions when participation is endogenously misreported. In particular, we provide a two-step estimation procedure that consistently estimates the conditional average treatment effect. Participation in social programs is substantially misreported in survey data, sometimes with misclassification levels close to 50% (Meyer et al. 2009). When a binary regressor is misreported (or misclassified), the measurement error is necessarily negatively correlated with the underlying true value of the regressor, thus making the classical measurement error assumptions implausible. While earlier papers (Aigner 1973, Lewbel 2007) show that exogenous misreporting leads to attenuation bias, we demonstrate that the effects of endogenous misreporting are much more severe. To our knowledge, this paper is the first attempt to provide point estimates of treatment effects in the context of endogenous misreporting of a binary treatment variable.

Misreporting occurs when program participants report not receiving treatment when they actually did (“false negatives”) or vice versa (“false positives”). One-sided misreporting (i.e., the occurrence of either false negatives or false positives) is pervasive in practice and in many empirical studies. For example, Lynch, Marioni & Tavaré (2007) and Meyer & Goerge (2011) report that validation studies typically find high rates of false negatives in the Supplemental Nutrition Assistance Program.
SNAP) ranging from 20% to 50%, depending on the survey and time period.\textsuperscript{2} False positives are typically low with only less than 1.5\% of non-recipients reporting SNAP receipt (Meyer & Goerge 2011).

One-sided misreporting is not confined to government programs. For example, according to Bound (1991), there are a number of reasons to be suspicious of any survey response to questions concerning self-evaluated health, not only because respondents are being asked for subjective judgments, but also because responses may be endogenous to the outcomes we may wish to use them to explain. Brachet (2008) argues that in health-related surveys, self-reported smoking status is significantly misreported, with false negatives ranging from 3.4\% to 73\%. Other instances of one-sided misreporting can be found in the development literature where a firm’s formality status is often misreported, with informal firms more likely to falsely report their status (see Gandelman & Rasteletti 2013), or the education literature where misclassification error is more likely to arise from over-reporting of qualifications (Battistin & Sianesi 2011).

Recognizing the documented evidence of misclassification errors, a related literature considers the consequences of measurement errors in a binary regressor in Monte Carlo studies. For instance, in studying the worst-case bounds of regression coefficients under arbitrary misclassification of a binary regressor, Kreider (2010) finds that even with misclassification error rates of less than 2\%, the confidence intervals from the contaminated data that the researcher observes and the true, error-free data do not overlap. Similarly, Millimet (2011) studies the performance of several estimators employed in the causal inference literature while introducing measurement error in the treatment (binary) regressor, and cautions researchers to be conscious of the consequences of not addressing measurement error.

\textsuperscript{2}Misreporting has also been documented for other government programs; see, e.g., Marquis & Moore (1990) for an earlier validation study discussing measurement error in the reports of participation in eight government transfer programs in the 1984 Survey of Income and Program Participation (SIPP).
The existing literature has focused on accounting for random (exogenous) misreporting when participation is exogenous. For instance, Aigner (1973) considers misclassification in exogenous binary regressors, shows that OLS estimates are biased downwards, and proposes a technique based on knowledge of the misclassification probabilities to consistently estimate the parameters of interest. More recently, Lewbel (2007) examines the identification and estimation of the treatment effect of a misclassified binary regressor in nonparametric and semiparametric regressions. Lewbel reaches the same attenuation-bias result that Aigner (1973) finds and introduces assumptions that identify the conditional average treatment effect of the misclassified binary regressor. Related works by Bollinger (1996), Black, Berger & Scott (2000) and van Hasselt & Bollinger (2012) provide partial identification bounds in the linear regression model, while Chen, Hu & Lewbel (2008a,b), provide identification in the nonparametric regression model.

Some attempts have been made to address exogenous misreporting when treatment selection (participation) is endogenous. In the education literature, Kane, Rouse & Staiger (1999) address misreporting when estimating returns to schooling by proposing a generalized method of moments (GMM) estimator that relies on the existence of two categorical reports of educational attainment. In estimating the effects of maternal smoking on infant health, Brachet (2008) proposes a two-step GMM estimator, that essentially follows Hausman, Abrevaya & Scott-Morton (1998) and Kane et al. (1999). An admitted weakness of Brachet’s approach is the assumption that misreporting probabilities are independent of covariates, conditional on treatment status. Frazis & Loewenstein (2003) and Mahajan (2006) study identification with the usual IV assumptions under homogenous and heterogenous treatment effects due to observables, respectively. More recently, DiTraglia & García-Jimeno (2017) derive a sharp identified set under standard first-moment assumptions and propose a Bonferroni-based procedure for
identification robust inference. Also, Bollinger & van Hasselt (2017) use a Bayesian approach while Ura (2017) allows for heterogeneous treatment effects due to unobservables in these models.

Much less is known about the case in which the regressor and the measurement error are both endogenous. Hu, Shiu & Woutersen (2015a, 2016) provide identification results in a class of nonseparable index models with measurement error and endogeneity. Kreider et al. (2012) is the most closely related to our work, in the sense that they allow for both treatment endogeneity and endogenous measurement error in the case of binary treatment as we do in this paper. In estimating the effect of SNAP on health outcomes, they use auxiliary administrative data on the size of SNAP caseloads to address misreporting by bounding the average treatment effect under increasingly stronger assumptions. While this partial identification approach identifies treatment effects with their tightest bounds, it does not yield point estimates. As such its relevance for policy making may not be widespread.

This paper has three salient contributions. First, we propose a parametric model of endogenous misreporting and endogenous participation. We only analyze the case of one-sided misreporting at this stage, which is the predominant case of misreporting described in Meyer et al. (2009). Second, we show that when misreporting is endogenous, OLS and IV estimators are inconsistent and OLS estimates can be of opposite signs from the true effects (sign reversal), whether participation is endogenous or not. We provide theoretical expressions for these biases under the normality assumption as well as Monte Carlo simulation evidence. Third, we propose an estimator that is root-n consistent and asymptotically normal and show that it performs remarkably well in small samples.

Identification in our framework relies on the existence of both an additional random variable that is correlated with the unobserved true underlying treatment, but unrelated to the outcome and the misclassification error (e.g., Frazis &
Loewenstein 2003, Mahajan 2006), as well as a random variable that is correlated with the misclassification error and needs not be excluded from the outcome. Also, we assume that the observed treatment probability is a joint (known) function of the treatment and misclassification probabilities. This allows us to pin down the marginal distribution of true participation and thus point-identify the treatment effect.

The rest of the paper is organized as follows. Section 2 presents the model of endogenous misreporting and shows the inconsistency of OLS and IV estimators. Section 3 develops the proposed estimator. Section 4 provides Monte Carlo simulations, and Section 5 concludes.

2.2 Framework

This section describes the proposed model and associated framework, and presents our estimation strategy.

2.2.1 Model with Endogenous Misreporting

Consider the following specification of the usual treatment effects model. The outcome variable, $y_i$, is related to the $k$–vector of correctly measured exogenous covariates, $x_i$, and the (true) participation indicator, $\delta_i^*$, by

$$y_i = x_i'\beta + \delta_i^*\alpha + \epsilon_i,$$

(2.1)

and we model participation as

$$\delta_i^* = 1 (z_i'\theta + v_i \geq 0),$$

(2.2)

where $\alpha$ is a scalar capturing the treatment effect of interest, $\beta$ and $\theta$ are parameter vectors of sizes $k \times 1$ and $q \times 1$ respectively, $z_i$ is a $q$-vector of exogenous variables
that includes $x_i$ as well as additional instruments that are unrelated to $\epsilon_i$. In this model, the correlation between the error terms $\epsilon_i$ and $v_i$ captures the (possible) endogeneity of participation.

However, the researcher does not observe the true participation indicator $\delta_i^*$ but only a possibly misclassified surrogate, $\delta_i$, contaminated by a misreporting unobserved dummy variable, $d_i$, such that $\delta_i = \delta_i^* d_i$. In other words, an individual correctly reports her treatment status only if $d_i = 1$ (conditional on true participation) and reports not receiving treatment otherwise. We assume that misreporting, $d_i$, is related to a $p$-vector of observable covariates $w_i$ such that

$$d_i = 1 (w_i' \gamma + u_i \geq 0),$$

where $\gamma$ is a parameter vector of size $p \times 1$ and $u_i$ is the error term. Hence, the observed participation, $\delta_i$, can be modeled by

$$\delta_i = \delta_i^* d_i = 1 (z_i' \theta + v_i \geq 0, w_i' \gamma + u_i \geq 0).$$

Our modeling of misreported participation is generally in the spirit of a wider class of methods that have been developed for incomplete data scenarios and specifically similar to partial observability models studied in Poirier (1980). Partial observability models such as Poirier’s have been widely applied in many fields of study, including Feinstein’s examination of the problem of incomplete detection of violations of laws and regulations (Feinstein 1990).

For the estimation, no further restrictions are imposed on $x_i$. However, we require the covariates $z_i$ and $w_i$ to be different but possibly overlapping and to have sufficient variation (e.g., at least one covariate in $z$ and in $w$ is continuous) to avoid the local identification problems discussed in Poirier (1980). We also make the following basic assumptions, some of which are standard in the literature.
Assumption 1. The error term $\epsilon_i$ is independent of the exogenous variables $x_i, z_i$, with variance $\sigma^2$; and the error terms $(u_i, v_i)$ are independent of all covariates $x_i, z_i, w_i$, and have unit variances. The correlations for the pairs $(\epsilon_i, u_i), (\epsilon_i, v_i)$ and $(u_i, v_i)$ are denoted $\varphi_u, \varphi_v$ and $\rho$, respectively.

Assumption 2. The $k \times k$ matrix, $E(x_i x_i')$, is nonsingular (and hence finite).

Assumption 3. The joint CDF of $(-u_i, -v_i)$ is known, and is defined by

$$F_{u,v}(u, v, \rho) = \Pr[-u_i \leq u, -v_i \leq v], \quad \text{for any } -\infty < u, v < +\infty.$$ 

In particular, we assume that conditional on $z_i$ and $w_i$, $(-u_i, -v_i)$ follows a bivariate normal distribution.

Assumption 4. The error terms, $(\epsilon_i, u_i, v_i)$, follow a trivariate normal distribution, conditional on all covariates $x_i, z_i, w_i$. That is,

$$(\epsilon_i, u_i, v_i)'|x_i, z_i, w_i \sim N(0, \Sigma), \quad \text{with} \quad \Sigma = \begin{pmatrix} \sigma^2 & \varphi_u \sigma & \varphi_v \sigma \\ \varphi_u \sigma & 1 & \rho \\ \varphi_v \sigma & \rho & 1 \end{pmatrix}, \quad (2.5)$$

Assumptions 1 and 2 are standard. However, it is important to notice that unlike $x_i$ and $z_i$, the exogeneity requirement does not apply to $w_i$, the additional predictors of misreporting in equation (3.4). This could be of substantial interest in practice where exogenous covariates are often difficult to find. Assumption 3 is critical to parametrically identify the probability of true (unobserved) participation. While we assume joint normality of the disturbance terms in the observed participation equation for simplicity (as in Poirier 1980), normality is not needed and the following discussion would hold for other absolutely continuous distributions (e.g., the bivariate logistic distributions discussed in Gumbel 1961). Assumption 4 is
only needed to derive closed-form formulas for the OLS bias (see Section 2.2.2) and for extensions to binary choice models and full information maximum likelihood (see Appendix B); but it is not essential for the rest of our main discussions.

Our estimation strategy relies on observing $z$ and $w$. We recognize that exclusion restrictions for participation as well as relevant predictors for misreporting may be difficult to obtain in practice and our suggestion is to rely on different data sources. For instance, exclusion restrictions for participation may come from qualification laws (eligibility requirements) for program participation. Relevant predictors of misreporting, $w_i$, could include peculiar features of the survey in question and its administration such as survey date, length of survey, interview mode, etc., and the proportion of questions to which the individual refused to respond.

2.2.2 Bias due to Endogenous Misreporting

We first show that a naive OLS estimator of the treatment effect is biased and may assume a sign opposite to the true effect. Since the true participation status $\delta^*_i$ is unobserved and only $\delta_i$ is observed, the model with reported participation status estimated by the researcher is given by

$$y_i = x'_i \beta + \delta_i \alpha + \varepsilon_i. \quad (2.6)$$

Given the true outcome equation defined by equation (2.1), equation (2.6) implies that

$$\varepsilon_i = \varepsilon_i + (\delta^*_i - \delta_i) \alpha. \quad (2.7)$$

For a random sample of size $n$, equation (2.6) can be re-written in the matrix form as follows:

$$y = X \beta + \delta \alpha + \varepsilon, \quad (2.8)$$
where \( y = [y_1, \ldots, y_n]' \), \( X = [x_1, \ldots, x_n]' \), \( \delta = [\delta_1, \ldots, \delta_n]' \), and \( \varepsilon = [\varepsilon_1, \ldots, \varepsilon_n]' \).

Denoting by \( \hat{\alpha}_{LS} \) the OLS estimator obtained by naively estimating equation (2.6) using reported participation \( \delta_i \), we have the following result.

**Theorem 1.** Under Assumptions 1 - 4, the ordinary least squares estimator, \( \hat{\alpha}_{LS} \), is biased and inconsistent, and the asymptotic bias is given by

\[
\text{plim}(\hat{\alpha}_{LS} - \alpha) = \frac{A - \alpha B}{C}, \tag{2.9}
\]

with

\[
A = \mathbb{E}\left[ \sigma \varphi_v \varphi(-z_i' \theta) \Phi\left( \frac{w_i' \gamma - \rho z_i' \theta}{\sqrt{1 - \rho^2}} \right) + \sigma \varphi_u \varphi(-w_i' \gamma) \Phi\left( \frac{z_i' \theta - \rho w_i' \gamma}{\sqrt{1 - \rho^2}} \right) \right],
\]

\[
B = \mathbb{E}(\delta_i x_i') \mathbb{E}(x_i x_i')^{-1} \mathbb{E}(\delta_i - \delta_i) x_i \quad \text{and} \quad C = \mathbb{E}(\delta_i) - \mathbb{E}(\delta_i x_i') \mathbb{E}(x_i x_i')^{-1} \mathbb{E}(\delta_i x_i),
\]

where \( \varphi(\cdot) \) and \( \Phi(\cdot) \) are respectively the pdf and cdf of the standard normal.

**Proof.** See Appendix A. \(\square\)

Since the denominator in (2.9), \( C \), is always positive by the Cauchy-Schwarz Inequality (see, e.g., Tripathi 1999) the sign of the asymptotic bias only depends on the numerator of the expression. For example, if \( B > 0 \), then \( \text{plim}(\hat{\alpha}_{LS}) < \alpha \) for all \( \alpha > A/B \) (i.e., there is an attenuation bias) and \( \text{plim}(\hat{\alpha}_{LS}) > \alpha \) for all \( \alpha < A/B \) (i.e., there is an expansion bias). Also there are many instances in which \( \text{plim}(\hat{\alpha}_{LS}) \) and \( \alpha \) have opposite signs. For example, if \( B - C < 0 \), then \( \text{plim}(\hat{\alpha}_{LS}) \) and \( \alpha \) have opposite signs whenever \( \alpha \) lays between \( A/(B - C) \) and 0 (Figure 2.1 depicts the regions where bias and sign switching occur in this case).

Note that sign-switching can occur even when participation is exogenous.

Without loss of generality, consider the case of exogenous participation (i.e \( \varphi_v = 0 \)).

---

\(^3\)Re-writing the model in matrix notation is not necessary but makes the exposition (especially the proofs) less cumbersome. The matrix form also gives alternative (simpler) expressions for the various estimators considered.
The sign-switching result depicted in Figure 2.1 follows if (i) $A > 0$, (ii) $B - C < 0$, and (iii) $A/(B - C) < \alpha < 0$. Condition (i) holds if misreporting is endogenous and the disturbance terms in equations (2.1) and (3.4) are positively correlated ($\varphi_u > 0$). Thus, the size of the sign-switching region depends on how large the ratio $A/(B - C)$ is in general. In particular, in our example, the size of the sign-switching region increases with the rate of false negatives and the variance of the error term in the outcome equation, and decreases with the rate of true participation, ceteris paribus. We provide evidence on the sign-switching region and these relationships in our Monte Carlo study in Section 2.4.

The above discussion shows that the consequence of misreporting is not merely an attenuation bias as found in many other studies (e.g., Aigner 1973, Black et al. 2000, Lewbel 2007). Under endogenous misreporting, the estimated treatment effect can possibly assume an opposite sign, yielding misleading policy prescriptions. This sign reversal phenomenon would generally occur when misreporting is significant and the direction of its correlation with the outcome is opposite to the direction of the treatment effect. For example, in the SNAP participation and obesity relationship, much empirical work have relied on self-reported SNAP participation and have found a positive or no effect on obesity. But, if people who are overweight are also more likely to correctly report SNAP participation (i.e., $A$ positive) and since, as mentioned above, misreporting in SNAP is very severe in the data (i.e., $B - C$ is negative with a small magnitude) then we could observe a positive relationship between SNAP participation and obesity (i.e. plim$\hat{\alpha}_{LS} > 0$) even if the true effect is negative (i.e. $\alpha < 0$).

In the next section, we provide an estimation strategy that allows consistent estimation of the treatment effect, $\alpha$. But first, we examine how well an IV estimation strategy would perform in our framework.
2.2.3 IV Estimator under Endogenous Misreporting

The misreporting mechanism described above shows that in equation (2.6), the regressor $\delta_i$ is correlated with the error term $\varepsilon_i$ as implied by equation (2.7). Thus, equation (2.1) can be seen as a regression with an endogenous binary regressor, even if true participation is exogenous and only misreporting is endogenous. So it may be tempting to suppose that if an instrument is present, then a standard IV estimator will address the issue raised in our framework. Here, we show that this is not the case.

Suppose we have access to a valid instrumental variable, $z_i$, such that $E[z_i \varepsilon_i] = 0$ and $\text{Cov}(z_i, \delta_i) \neq 0$, and assume, for simplicity, that $z_i$ is a scalar so that $\alpha$ is just identified. Then the (simple) instrumental variable estimator is given by

$$\hat{\alpha}_{IV} = (z'M\delta)^{-1}z'My,$$

where $M = I - X'(X'X)^{-1}X'$ is the orthogonal projection matrix onto the null space of $X$.

We can show using the same reasoning as above that,

$$\text{plim}(\hat{\alpha}_{IV}) = \frac{E(z_i \delta_i^*) - E(z_i x_i')E(x_i x_i')^{-1}E(x_i \delta_i^*)}{E(z_i \delta_i) - E(z_i x_i')E(x_i x_i')^{-1}E[x_i \delta_i]} \alpha.$$

(2.10)

Thus, the IV estimator of $\alpha$ is inconsistent, and we cannot sign the bias in general. However, in the special case where misreporting is uncorrelated with true participation and the other covariates, it can be shown that,

$$\text{plim}(\hat{\alpha}_{IV}) = \frac{\alpha}{E[d_i]} = \frac{\alpha}{\Pr[d_i = 1]}, \quad \text{so that } |\text{plim}(\hat{\alpha}_{IV})| > |\alpha|.$$

Hence, in this specific scenario, the IV estimator is upwardly biased. This result is similar to those obtained by Black et al. (2000), (see also Frazis & Loewenstein...
The finding that the IV estimator is inconsistent is not new, given the results of the above authors and others. However, Black et al. (2000) showed that the IV estimator yields an expansion bias, which corresponds to the special case of exogenous measurement errors. By contrast, as suggested by equation (2.10), the sign of the IV bias is not obvious when misreporting is endogenous, and our simulations show that the ensuing bias can take either direction (i.e., expansion or attenuation).

We now present an estimation procedure that delivers consistent and asymptotically normal estimates for the treatment effect, $\alpha$.

### 2.3 The Proposed Estimator

Recall that our objective is to estimate $\alpha$ in the outcome equation (2.1), where true (and possibly endogenous) participation status, $\delta^*_i$, is unobserved, but only a possibly misreported (and possibly endogenous) participation status, $\delta_i$, is observed. The proposed estimation strategy proceeds in the following two steps.

1. With the joint distribution of $u_i$ and $v_i$ given by $F_{u,v}(u, v, \rho)$, use the partial observability probit model given by equation (2.4) to estimate the parameter vectors $\theta$ and $\gamma$. Then, compute the predicted probability for person $i$'s true participation status as $\hat{\delta}^*_i = \Phi(z_i'\hat{\theta})$.

2. Estimate equation (2.1) by substituting $\hat{\delta}^*_i$ for $\delta^*_i$. Assuming correct model specification and distribution of the error terms, the resulting two-step estimator of $\alpha$ is consistent. Moreover, with standard regularity assumptions, this estimator is asymptotically normal.
2.3.1 First Step Estimation

Following Poirier (1980), the parameters $\gamma$, $\theta$ and $\rho$ can be jointly estimated from the joint distribution of the error terms using the binary choice model defined by

$$\Pr[\delta_i = 1|w_i, z_i] = \Pr[-u_i \leq w_i'\gamma, -v_i \leq z_i'\theta] = F_{u,v}(w_i'\gamma, z_i'\theta, \rho) = P_i(\gamma, \theta, \rho).$$

The log-likelihood function of this model is given by

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

The maximum likelihood estimator of the vector of parameters $(\gamma, \theta, \rho)$ is consistent and asymptotically normal, and the covariance matrix consistently estimated with the inverse of the information matrix. In particular, for the parameter $\theta$, the MLE $\hat{\theta}$ is consistent and asymptotically normal, i.e.,

$$\hat{\theta} \overset{p}{\rightarrow} \theta \quad \text{and} \quad \sqrt{n}(\hat{\theta} - \theta) \overset{d}{\rightarrow} N(0, V_{\theta}),$$

where the asymptotic variance of $\hat{\theta}$ is obtained from the information matrix equality as

$$V_{\theta} = \left\{ \mathbb{E} \left[ \frac{1}{P_i(1 - P_i)} \frac{\partial P_i}{\partial \theta} \frac{\partial P_i}{\partial \theta'} \right] \right\}^{-1}. \tag{2.11}$$

From this expression, a consistent estimator for the variance matrix can be obtained as

$$\hat{V}_{\theta} = \left[ \frac{1}{n} \sum_{i=1}^{n} \frac{1}{P_i(1 - P_i)} \frac{\partial \hat{P}_i}{\partial \theta} \frac{\partial \hat{P}_i}{\partial \theta'} \right]^{-1}, \tag{2.12}$$

where $\hat{P}_i = P_i(\hat{\gamma}, \hat{\theta}, \hat{\rho}) = F_{u,v}(w_i'\hat{\gamma}, z_i'\hat{\theta}, \hat{\rho})$. For the normal case, the gradient takes a fairly simple form

$$\frac{\partial \hat{P}_i}{\partial \theta} = \Phi(z_i'\hat{\theta}) \phi(w_i'\hat{\gamma} - \hat{\rho}z_i'\hat{\theta}) \frac{z_i}{\sqrt{1 - \hat{\rho}^2}} z_i.$$
Since this first-step is a maximum likelihood, parametric identification of \((\theta, \gamma, \rho)\) can be discussed in terms of non-singularity of the corresponding information matrix (Rothenberg 1971). This means perfect multicollinearity needs to be ruled out, implying that both \(w_i\) and \(z_i\) should satisfy the standard rank conditions as a basic requirement. In addition, as we explained earlier, a single exclusion restriction between \(w_i\) and \(z_i\) (i.e., at least one covariate in \(z_i\) should not be relevant in \(w_i\), or vice-versa) is sufficient to identify all the first step parameters locally (Poirier 1980). Also, notice that only the (correct) specification of the marginal distribution of \(v\) is necessary for the parametric identification and estimation of the model in the second step. If the distribution of \(u\) or the joint distribution of \((u, v)\) are unknown, one may still obtain a consistent estimator of \(\theta\) in the first step by using a semiparametric approach such as the series expansion of the joint PDF of \((u, v)\) proposed by ? or the single equation multiple index model described in Ichimura & Lee (1991).

### 2.3.2 Second Step Estimation

In the second step, we compute the predicted values of true unobserved participation \(\delta^*_i\), given by \(\hat{\delta}^*_i = \Phi(z'_i\hat{\theta})\), which are used in lieu of \(\delta^*_i\) to estimate the parameters of the new model given by

\[
y_i = x'_i\beta + \hat{\delta}^*_i\alpha + \eta_i.
\] (2.13)

Using the same approach as above, the second step estimator is obtained as

\[
\hat{\alpha}_{2S} = (\hat{\delta}'M\hat{\delta})^{-1}\hat{\delta}'My
\]

\[
= \frac{\sum_{i=1}^n \Phi(z'_i\hat{\theta})y_i - \sum_{i=1}^n \Phi(z'_i\hat{\theta})x'_i[\sum_{i=1}^n x_i x'_i]^{-1} \sum_{i=1}^n x_i y_i}{\sum_{i=1}^n \Phi(z'_i\hat{\theta})^2 - \sum_{i=1}^n \Phi(z'_i\theta)x'_i[\sum_{i=1}^n x_i x'_i]^{-1} \sum_{i=1}^n x_i \Phi(z'_i\theta)}
\] (2.14)

\(^4\)Essentially, identification implies much stronger conditions than the standard rank condition for linear IV, since it requires that participation and hence the (nonlinear) relationship between true treatment and instruments be fully parameterized and correctly specified.
We have the following consistency result.

**Theorem 2.** Under Assumptions 1-3, the two-step estimator is consistent for $\alpha$, that is, $\hat{\alpha}_{2S} \xrightarrow{p} \alpha$.

**Proof.** See Appendix A.

Notice that only the component $\hat{\theta}$ of the parameter vector is used at this second stage to predict the true unobserved participation status. The other components, $\hat{\gamma}$ and $\hat{\rho}$ are only used in the computation of the asymptotic variance estimator, as described below. In this second step, exclusion restriction is not strictly needed for identification as long as nonlinearity in the marginal distribution of $v_i$ is assumed.

We have the following asymptotic normality result.

**Theorem 3.** Under the model assumptions the two-step estimator is asymptotically normal, i.e.,

$$\sqrt{n}(\hat{\alpha}_{2S} - \alpha) \xrightarrow{d} N(0, \sigma^2_{\alpha})$$

with

$$\sigma^2_{\alpha} = \alpha^2 \frac{\mathbb{E}[\Lambda_i(\theta)\phi(z_i'\theta)z_i']V(\hat{\theta})\mathbb{E}[z_i\phi(z_i'\theta)\Lambda_i(\theta)]}{\mathbb{E}[\Lambda_i^2(\theta)]^2} + \frac{\alpha^2\mathbb{E}[\Lambda_i^2(\theta)\Phi(z_i'\theta)(1 - \Phi(z_i'\theta))]}{\mathbb{E}[\Lambda_i^2(\theta)]^2} + \frac{\sigma^2}{\mathbb{E}[\Lambda_i^2(\theta)]}$$

where

$$\Lambda_i(\theta) = \Phi(z_i'\theta) - \mathbb{E}[\Phi(z_i'\theta)x_i']\mathbb{E}[x_i; x_i]^{-1}x_i$$

**Proof.** See Appendix A.

This result is an application of the central limit theorem in the context of two-step estimators, and is useful for our procedure to be readily usable for parametric inference. An expression for the variance estimator $\hat{\sigma}^2_{\alpha}$ of $\sigma^2_{\alpha}$ is given in the Appendix. However, this variance is quite involved and can be difficult to
estimate. In practice, a simpler approach to evaluate the precision of $\hat{\alpha}_{2S}$ and make inference about the treatment effect $\alpha$ is to use a bootstrap.

Summarizing, the outcome equation requires true participation status, $\delta^*$, which is unobserved to the econometrician. Given the observed participation, $\delta$, the first step in our estimation procedure amounts to a partial observability probit analysis on the indicator variable $\delta$ using both $z$ and $w$, which are respectively the instrumental variables driving true participation and the covariates related to misreporting. The result of this analysis is an estimator, $\hat{\theta}$, of $\theta$, the coefficient of $z$, which allows constructing a proxy $\hat{\delta}^*$ for truly being a participant. By construction, this proxy is purged from both endogeneity and misreporting, and is then used in lieu of $\delta^*$ in the outcome equation of interest to derive a consistent treatment effect estimator. The estimate $\hat{\theta}$ obtained from the first step can then be used along with the other model estimates to compute a consistent variance estimator for the treatment effect estimator.

A natural alternative to our two-step procedure is to estimate our model equations jointly via maximum likelihood (ML). Under appropriate assumptions, the ML procedures yield more efficient estimators and asymptotically correct estimates of standard errors. Unfortunately, in many situations, due to sample size and other considerations, the ML estimation can be both computationally complex and costly to implement, which may limit its use. For example, the correlations between the outcome equation error and the participation and reporting equations errors, $\varphi_u$ and $\varphi_v$, might not be strongly identified, resulting in a likelihood function with ridges or multiple local maxima. In addition, in some applications, the researcher may be reluctant to hypothesize a specific joint distribution between the random errors of the observed participation and the outcome as is required by maximum likelihood.\footnote{In the Appendix, we briefly discuss the ML estimation of this model under the assumption of joint (trivariate) normality of the errors. In the same vein, we also briefly discuss how our method can be extended to the case of binary outcomes.}
While this framework focuses on one-sided misreporting (i.e., either only false negatives or false positives) which may be more appealing in certain contexts (e.g., when studying scenarios of participation in risky behavior or activities associated with stigma), a more general framework should account for misreporting in both directions (i.e., both false negatives and false positives). In the following section, we provide Monte Carlo simulations results on the performance of our estimator for both the one-sided case and the case where there is a small amount of misclassification in the other direction.\(^6\)

### 2.4 Monte Carlo Simulations

This section presents the results of Monte Carlo simulations comparing the proposed two-step estimator (2S) with OLS and IV estimators. Our goal is to consistently estimate \(\alpha\), the (conditional) average treatment effect of participation, \(\delta^*\), on an outcome, \(y\), given by equation (2.1). However, since true participation is unobserved, our task reduces to use the proposed method to estimate \(\alpha\) from equation (2.6) under the assumption that observed (misclassified) participation, \(\delta\), arises according to the process described by equation (2.4). In the simulations, we also consider a slight departure from equation (2.4) and allow for small amounts of false positives as described below.

#### 2.4.1 Simulation setup

The baseline data generating process is simulated as follows. The true treatment indicator, \(\delta_i^*\), is given by

\[
\delta_i^* = 1 (\theta_0 + \theta_1 z_i + v_i \geq 0), \quad \text{where } z_i \sim N(0,1), \quad \theta_0 = 0.1, \quad \theta_1 = 1.
\]

\(^6\)Extending this framework to the two-sided endogenous misreporting case is not straightforward. It would require at least two sets of excluded covariates, that is, \(w_1\) and \(w_2\), each associated with one of the misreporting directions, and possibly other additional functional form/distributional assumptions for identification.
The outcome equation \( y_i \) is given by

\[
y_i = \beta_0 + x_i \beta_1 + \delta_i^* \alpha + \epsilon_i \quad \text{where} \quad x_i \sim N(0, 1) \quad \beta_0 = \beta_1 = 1, \quad \alpha = -0.2.
\]

Note that \( \alpha = -0.2 \) is the true population treatment effect we seek to estimate.

The econometrician only observes an error-ridden treatment indicator, \( \delta_i \), defined by

\[
\delta_i = \delta_i^* 1(\gamma_0 + \gamma_1 w_i + u_i \geq c) + (1 - \delta_i^*) 1(\zeta_i < b),
\]

where \( w_i \sim N(0, 1), \gamma_0 = 0.01, \gamma_1 = 2, \) and \( b \in [0, 1) \).

The parameter \( c \) is the threshold that determines the proportion of false negatives in the sample.\(^7\) The disturbance term, \( \zeta_i \), is drawn from a uniform \((0, 1)\) distribution independently from \( z_i \) and \( v_i \) so that the parameter \( b \) corresponds to the rate of false positives. For example, when \( b = 0 \) (baseline case), the observed treatment indicator is given by \( \delta_i = \delta_i^* 1(\gamma_0 + \gamma_1 w_i + u_i \geq c) \) which only allows for false negatives as given by equation (2.4). However, when \( b > 0 \), the observed treatment indicator allows for both false negatives and a \((100 \times b)\)% rate of false positives.

The disturbances \( \epsilon_i, u_i \) and \( v_i \) are drawn from a trivariate distribution given by

\[
(\epsilon_i, u_i, v_i) \sim \text{IID}(0, \Sigma), \quad \text{where} \quad \Sigma = \begin{pmatrix}
\sigma^2 & \varphi_{u\epsilon} & \varphi_{v\epsilon} \\
\varphi_{u\epsilon} & 1 & \rho \\
\varphi_{v\epsilon} & \rho & 1
\end{pmatrix}, \quad \sigma = 1.
\]

The baseline results assume joint normality although we consider non-normal distributions as well. The values of the parameters \( \varphi_{v\epsilon} \) and \( \varphi_{u\epsilon} \), which are the correlations of the outcome equation error term with participation and misreporting equation disturbance terms, respectively, are varied in the simulations to examine

\(^7\)By appropriately choosing the value of \( c \), one can simulate varying rates of misreporting.
how various degrees of the endogeneity of participation and misreporting with respect to the outcome impact the results. We also allow $\rho$, the correlation between participation and misreporting to vary. We estimate the treatment effect $\alpha$ and the associated bias using the naive OLS approach, $\hat{\alpha}_{LS}$ and the proposed two-step approach, $\hat{\alpha}_{2S}$. We also estimate the instrumental variable estimators $\hat{\alpha}_{IV1}$ and $\hat{\alpha}_{IV2}$ using $z$ and $[z, w]$ as instruments, respectively.

2.4.2 Simulation Results

We report simulation results averaged over 1000 replications each with sample size 5000 for different levels of false negatives (0%, 5%, 10%, 20%, 40%), $\rho \in \{0, 0.3\}$, $\varphi_u \in \{0, 0.2, 0.8\}$ and $\varphi_v \in \{-0.3, 0, 0.3\}$. These results are first presented for $b = 0$ (i.e., 0% false positives) and, subsequently, for $b \in \{0.01, 0.05, 0.1\}$ (i.e., 1%, 5% and 10% false positives). Thus, no misreporting corresponds to the case of 0% false negatives and false positives. The cases of exogenous participation and exogenous misreporting correspond to $\varphi_u = \varphi_v = 0$. Table 2.1 presents the results of the Monte Carlo simulations for OLS, IVs, and the proposed two-step (2S) estimators when the errors are jointly normal, the false positive rate is 0%, and $\rho = 0.3$. We report both the OLS estimates using the true treatment indicator, $\delta_i^*$ (OLS-T) and the observed treatment indicator $\delta_i$ (OLS-O). Although $\delta_i^*$ is unobserved to the econometrician, the OLS-T estimates provide a theoretical benchmark for the estimates obtained using the misclassified $\delta_i$. We also report both the IV estimates using $z$ as an instrument (IV-1) and those using $[z, w]$ as instruments (IV-2). The proposed estimator is denoted (2S) in the tables.

The naive OLS estimates using $\delta_i$ (OLS-O) show that, not only is the OLS estimator inconsistent as asserted in Theorem 1, but also yields the wrong (i.e., positive) sign, whether participation is exogenous or endogenous. Sign switching is observed at all nonzero false negative rates i.e. 5%, 10%, 20% and 40% and is more
pronounced at higher values of $\phi_u$. These results persist even under the special case of exogenous misreporting ($\phi_u = 0$). The IV estimates (IV-1) and (IV-2) show that the classic IV estimator is also inconsistent and sometimes worse than the OLS, albeit keeping the correct (negative) sign.\(^8\) Interestingly, IV-1 estimates exhibit expansion biases while IV-2 estimates are attenuated. This confirms, as we explained in Section 2.2.3, that we cannot generally sign the bias in the IV estimator when misreporting is endogenous. In contrast, the proposed two-step estimator (2S), presented in the last column of Table 2.1, yields consistent estimates of the true treatment effect and by comparison, is superior to both the OLS and IV estimators under both endogenous and exogenous misreporting or participation. In addition, the proposed estimator remains accurate and performs remarkably well, even when the rate of false negatives is substantially high in the data. Moreover, there is no cost in doing our procedure since the proposed estimator remains as good as the OLS and the IV when there is 0% false negatives and participation is exogenous ($\phi_v = 0$). This performance is not sensitive to the choice of parameters such as the variance of the outcome equation error or the correlation between the error terms in the participation and misreporting equations (see, e.g., the results for $\rho = 0$ in the ‘Baseline’ column of Table 2.2).

To further assess the robustness of our proposed estimator, we investigate its sensitivity to misspecification in a number of directions. First, we allow the reported participation to include both false negatives (as before) and a small amount of false positives. We consider false positive rates of 1%, 5%, and 10%. Second, we allow for the error terms to be non-normal. We consider both the trivariate Gamma distribution and the trivariate Chi-squared distribution as alternatives to allow

\(^8\)This is actually a better set of simulations for IV-2 because the covariate $w_i$ can be used as an additional instrument to improve the IV. Additional simulations with $w_i$ being endogenous yielded worse results for this IV while the proposed estimator (2S) remained consistent.
more skewness and kurtosis in the distributions of error terms. Third, we allow for the misreporting equation to be misspecified by considering the case where the predictor is unavailable to the researcher (i.e., only $x$ is included) or by introducing a quadratic term in $w$ in the data generating process but turns out to be omitted by the researcher in the estimation. Fourth, we introduce correlation between the predictors of misreporting $w$ and the error terms in both participation and outcome equations.

Table 2.2 summarizes the results where $\rho$ and $\varphi_v$ are fixed to zero (exogenous participation) and the focus is on the sensitivity to different degrees of endogeneity of misreporting $\varphi_u$ and various rates of false negatives. These results show that at any false positive rate the bias increases with false negative rates. Interestingly, for small amounts of false positives, the proposed estimator still performs quite well, although it gets worse with higher rates of false positives. Specifically, when false positive rates range from 1% to 5%, the median value of the proposed estimator (2S) ranges between $-0.2042$ and $-0.2180$ for all ranges of false negatives in our setting. The proposed estimator is robust to non-normality of the error terms and remains consistent when the true error distributions are Gamma or Chi-Squared in this setting. When the misreporting equation is misspecified by including only the covariates from the outcome equation (i.e., only $x$) or when this equation includes a quadratic term in $w$ that is omitted by the researcher in the estimation, the 2S estimator still performs well. Finally, the 2S estimator is robust to introducing correlations between the predictor $w$ and both the outcome and misreporting equation errors. When $w$ is correlated with the outcome equation or the misreporting equation errors, the 2S estimator remains consistent. However, the 2S estimator performs poorly, exhibiting an attenuation bias just like the IV-1, when $w$ and the participation equation error are correlated. Our recommendation is to

---

9These multivariate distributions can be simulated using the Copulas method or the inverse transformation method as described in ?
access $w$ and $z$ from different data sources to minimize the chances of having $w$ endogenous to true participation in practice.

There are a few additional facts that are worth mentioning. On the one hand, it is not surprising that the OLS estimator only works well when there are 0% false negatives and participation is exogenous ($\varphi_v = 0$). On the other hand, the IV estimator tends to work well for low levels of false negatives (0% - 5%) but gets worse for higher false negative rates (10% and higher). As explained earlier, the sign-reversal regions for the OLS depends on the quantity $A/(B - C)$ (given in Theorem 1), which varies with $\varphi_v$, $\varphi_u$, $\sigma$, and the extent of misclassification. Even when participation is exogenous (i.e. $\varphi_v = 0$), various degrees of endogeneity of misreporting (e.g., $\varphi_u \in \{-0.8, -0.2, 0, 0.2, 0.8\}$), various sizes of the error variance (e.g., $\sigma \in \{1, 4\}$), and various rates of false negatives (e.g., 5%, 10%, 40%) yield different sign-switching regions for the OLS, as shown in Table 2.3.

Table 2.3 shows the ranges of the true treatment effects $\alpha$ for which the OLS estimator $\hat{\alpha}_{LS}$ would yield the wrong (opposite) signs in our simulation design. For example, negative correlations between misreporting and outcome errors yield positive intervals of the treatment effect for which the OLS takes the wrong (negative) sign, while positive correlations between misreporting and outcome errors yield negative intervals of the true treatment effect for which the OLS takes the wrong (positive) sign. In all cases, higher levels of endogeneity of misreporting, higher rates of false positives or greater error variance in the outcome equation yield wider sign-reversal intervals. It is only when misreporting is also exogenous ($\varphi_u = 0$) that the OLS keeps the same sign as the true treatment effect (albeit still biased), so that the sign-switching set is empty (see Table 2.3).
2.5 Conclusion

This study examines the identification and estimation of the conditional average treatment effect of a binary regressor in the presence of endogenous misreporting and possibly endogenous participation. We derive and prove the consistency and asymptotic normality of our proposed two-step estimator and show that OLS and IV estimators are inconsistent and may yield wrong (opposite) signs from the true effect. We also provide Monte Carlo simulations to this effect. Previous studies on misclassified binary regressors are mostly concerned with exogenous or random misreporting (Aigner 1973, Brachet 2008, Lewbel 2007, Mahajan 2006, Frazis & Loewenstein 2003), where it is commonly assumed that misclassification probabilities depend only on the true treatment status and are thus, independent of measurement errors and other regressors. Our two-step estimator relaxes this arguably strong assumption and shows that, when the researcher has access to information related to why individuals misreport, the treatment effect can be consistently estimated.

To our knowledge, this paper is the first attempt to provide point estimates of treatment effect in the context of endogenous misreporting of a binary treatment variable. This is important because of the prevalence of misreporting in public programs and survey data (Meyer et al. 2009, Bollinger 1996, Kane & Rouse 1995, Kane et al. 1999, Brachet 2008). While this paper focused on one-sided endogenous misreporting when participation is possibly endogenous, future work should allow for bidirectional misreporting (i.e., false negatives and false positives). It would also be useful to show the level of dependence of our approach on distributional and functional form assumptions by considering parametric or semi-parametric estimation approaches.
2.6 Figures and Tables

Figure 2.1: Illustration of the OLS bias
### Table 2.1: Monte Carlo Simulations

<table>
<thead>
<tr>
<th>False Negatives</th>
<th>$\varphi_v$</th>
<th>$\varphi_u$</th>
<th>OLS-T</th>
<th>OLS-O</th>
<th>IV-1 $[x,z]$</th>
<th>IV-2 $[x,z,\omega]$</th>
<th>2S</th>
</tr>
</thead>
<tbody>
<tr>
<td>0%</td>
<td>0.0</td>
<td>-0.3</td>
<td>-0.523</td>
<td>-0.523</td>
<td>-0.2006</td>
<td>-0.2003</td>
<td>-0.1998</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.3</td>
<td>-0.5524</td>
<td>-0.5524</td>
<td>-0.2036</td>
<td>-0.2033</td>
<td>-0.2030</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.3</td>
<td>-0.557</td>
<td>-0.557</td>
<td>-0.1991</td>
<td>-0.1988</td>
<td>-0.1980</td>
</tr>
<tr>
<td>5%</td>
<td>0.0</td>
<td>-0.3</td>
<td>-0.5499</td>
<td>-0.5066</td>
<td>-0.2041</td>
<td>-0.2152</td>
<td>-0.1997</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.3</td>
<td>-0.5513</td>
<td>-0.4827</td>
<td>-0.2064</td>
<td>-0.2166</td>
<td>-0.2001</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.3</td>
<td>-0.5506</td>
<td>-0.4072</td>
<td>-0.2044</td>
<td>-0.2158</td>
<td>-0.2013</td>
</tr>
<tr>
<td>10%</td>
<td>0.0</td>
<td>-0.3</td>
<td>-0.5515</td>
<td>-0.4649</td>
<td>-0.1949</td>
<td>-0.2345</td>
<td>-0.2000</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.3</td>
<td>-0.5513</td>
<td>-0.4203</td>
<td>-0.1960</td>
<td>-0.2350</td>
<td>-0.2008</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.3</td>
<td>-0.5513</td>
<td>-0.2916</td>
<td>-0.1953</td>
<td>-0.2340</td>
<td>-0.1998</td>
</tr>
<tr>
<td>20%</td>
<td>0.0</td>
<td>-0.3</td>
<td>-0.5508</td>
<td>-0.4065</td>
<td>-0.1674</td>
<td>-0.2668</td>
<td>-0.1984</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.3</td>
<td>-0.5518</td>
<td>-0.3421</td>
<td>-0.1743</td>
<td>-0.2728</td>
<td>-0.2036</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.5</td>
<td>-0.5511</td>
<td>-0.1443</td>
<td>-0.1690</td>
<td>-0.2679</td>
<td>-0.1988</td>
</tr>
<tr>
<td>40%</td>
<td>0.0</td>
<td>-0.3</td>
<td>-0.5502</td>
<td>-0.3161</td>
<td>-0.1215</td>
<td>-0.3730</td>
<td>-0.1981</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.3</td>
<td>-0.5501</td>
<td>-0.2184</td>
<td>-0.1160</td>
<td>-0.3687</td>
<td>-0.1963</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.3</td>
<td>-0.5509</td>
<td>0.0675</td>
<td>-0.1207</td>
<td>-0.3757</td>
<td>-0.1993</td>
</tr>
</tbody>
</table>

The true treatment effect is $\alpha = -0.2$. Each calibration in the Monte Carlo Design involved 1000 replications each of size 5000. We report results for five false negative rates (0%, 5%, 10%, 20%, and 40%) – the proportion of true participants who misreport their status. $\varphi_v$ and $\varphi_u$ are correlations that indicate the extents of endogeneity of participation and misreporting, respectively. The correlation between participation and misreporting is $\rho = 0.3$. Also, the error terms are jointly normally distributed and the false positive rate is 0%.
Table 2.2: Sensitivity of the Proposed Estimator to Misspecification

<table>
<thead>
<tr>
<th>False Negatives</th>
<th>( \varphi_u )</th>
<th>Baseline</th>
<th>Types of Misspecification of Baseline</th>
<th>Distribution of errors</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>False Positives</td>
<td>1%</td>
</tr>
<tr>
<td>0%</td>
<td>0</td>
<td>-0.2000</td>
<td>-0.2021</td>
<td>-0.2141</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.1970</td>
<td>-0.2003</td>
<td>-0.2126</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.1981</td>
<td>-0.2008</td>
<td>-0.2129</td>
</tr>
<tr>
<td>5%</td>
<td>0</td>
<td>-0.2041</td>
<td>-0.2057</td>
<td>-0.2191</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.1974</td>
<td>-0.2074</td>
<td>-0.2208</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.1989</td>
<td>-0.2035</td>
<td>-0.2169</td>
</tr>
<tr>
<td>10%</td>
<td>0</td>
<td>-0.1987</td>
<td>-0.2029</td>
<td>-0.2169</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.2001</td>
<td>-0.2060</td>
<td>-0.2207</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.1983</td>
<td>-0.2027</td>
<td>-0.2171</td>
</tr>
<tr>
<td>20%</td>
<td>0</td>
<td>-0.2018</td>
<td>-0.2063</td>
<td>-0.2234</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.1989</td>
<td>-0.2056</td>
<td>-0.2229</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.2003</td>
<td>-0.2036</td>
<td>-0.2210</td>
</tr>
<tr>
<td>40%</td>
<td>0</td>
<td>-0.1961</td>
<td>-0.2066</td>
<td>-0.2314</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>-0.2000</td>
<td>-0.2085</td>
<td>-0.2333</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>-0.2009</td>
<td>-0.2087</td>
<td>-0.2363</td>
</tr>
</tbody>
</table>

*The true treatment effect is \( \alpha = -0.2 \). We fix \( \rho = \varphi_u = 0 \). Each calibration in the Monte Carlo Design involved 1000 replications each of size 5000. We report results for five false negatives rates (0%, 5%, 10%, 20%, and 40%) and false positive rates of (0%, 1%, 5%, and 10%). The correlation \( \varphi_u \) indicates the extent of endogeneity of misreporting. Here, the true misreporting equation includes both \( w \) and \( w^2 \), but the estimation omits \( w^2 \).*
Table 2.3: Sign-switching intervals of $\alpha$ for the OLS

<table>
<thead>
<tr>
<th>False Negatives</th>
<th>$\varphi_u$</th>
<th>Sign-switching region $\sigma = 1$</th>
<th>$\sigma = 4$</th>
</tr>
</thead>
<tbody>
<tr>
<td>5%</td>
<td>-0.8</td>
<td>[0, 0.2307]</td>
<td>[0, 0.9227]</td>
</tr>
<tr>
<td></td>
<td>-0.2</td>
<td>[0, 0.0577]</td>
<td>[0, 0.2309]</td>
</tr>
<tr>
<td></td>
<td>0</td>
<td>$\emptyset$</td>
<td>$\emptyset$</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>[-0.0577, 0]</td>
<td>[-0.2309, 0]</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>[-0.2307, 0]</td>
<td>[-0.9227, 0]</td>
</tr>
<tr>
<td>10%</td>
<td>-0.8</td>
<td>[0, 0.4054]</td>
<td>[0, 1.6216]</td>
</tr>
<tr>
<td></td>
<td>-0.2</td>
<td>[0, 0.1033]</td>
<td>[0, 0.8368]</td>
</tr>
<tr>
<td></td>
<td>0</td>
<td>$\emptyset$</td>
<td>$\emptyset$</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>[-0.1033, 0]</td>
<td>[-0.8368, 0]</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>[-0.4054, 0]</td>
<td>[-1.6216, 0]</td>
</tr>
<tr>
<td>40%</td>
<td>-0.8</td>
<td>[0, 1.1347]</td>
<td>[0, 5.3786]</td>
</tr>
<tr>
<td></td>
<td>-0.2</td>
<td>[0, 0.3399]</td>
<td>[0, 1.3597]</td>
</tr>
<tr>
<td></td>
<td>0</td>
<td>$\emptyset$</td>
<td>$\emptyset$</td>
</tr>
<tr>
<td></td>
<td>0.2</td>
<td>[-0.3399, 0]</td>
<td>[-1.3597, 0]</td>
</tr>
<tr>
<td></td>
<td>0.8</td>
<td>[-1.1347, 0]</td>
<td>[-5.3786, 0]</td>
</tr>
</tbody>
</table>

Results are reported for three false negatives rates (5%, 10%, and 40%). The correlation $\varphi_u$ indicates the extent of endogeneity of misreporting; $\varphi_v$ and $\rho$ are fixed to 0.
CHAPTER 3

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

3.1 Introduction

This paper estimates the casual effect of the Supplemental Nutrition Assistance Program (SNAP) on adult Body Mass Index (BMI) 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 in an effort to reduce food insecurity and support a healthy population. High adult obesity rates coupled with a higher prevalence among low-income households targeted by SNAP motivates a thorough understanding of the relationship between SNAP and obesity.\(^2\)

Since obesity remains a public health or policy concern, it is of interest to policymakers to know whether SNAP and any other government program have any unintended consequences for the weight of recipients. For instance, if SNAP affects the weight of recipients, then the size of the negative externalities associated with obesity would need to incorporate these effects (Bhattacharya & Sood 2006, Bailey 2013). Also, knowing whether there are any such effects can inform debates regarding proposals to restructure SNAP.

It is often asserted that SNAP participation reduces food insecurity, lifts millions

---

\(^1\)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, Gunder- sen (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, Drilea, Jowers, Lichtenstein, Mabli, Madden & Niland (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%).
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 especially because the low-income households targeted by SNAP are also relatively more vulnerable with respect to obesity risk factors and other negative health conditions (Bitler 2015, Gundersen 2015). Obesity is one of the leading health problems in the U.S., with an adult, age-adjusted prevalence rate of 37.7% (35% for 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). In addition, there are significant health care costs of obesity (Finkelstein et al. 2009, Cawley & Meyerhoefer 2012) as well as negative effects of obesity on labor market outcomes (Bhattacharya & Bundorf 2009). It is against this backdrop that providing credible estimates of SNAP’s relationship with recipient body weight remains critical for policymaking bodies and administrators.

Although SNAP has no specific objective to influence obesity directly, obtaining accurate estimates of its effects on health outcomes in general and obesity in particular is critical in the broader ongoing policy debates surrounding its existence and role in the lives of the millions of Americans who benefit from it. For instance, understanding the causal link between SNAP and obesity can help us understand and evaluate the merits of recent proposals aimed at influencing the nutritional choice and well-being of participants. Gundersen (2015) discusses state- and national-level proposals aimed at restricting the food choices of participants and prohibiting the purchase of foods deemed as “unhealthy” or “junk.” For instance, a much publicized proposal is the State of New York’s 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 10 calories per 8-ounce serving (Gundersen 2015, Kansagra, Kennelly, Nonas, Curtis, Van Wye, Goodman & Farley 2015). The State of New York’s proposal which would have banned sports drinks, soda, vegetable drinks and iced tea while allowing others such as milk and 100% fruit juices was ultimately denied by the USDA.

Similar state-level proposals have been made by Minnesota, Maine, Wisconsin, and South Carolina although none has been granted by the USDA. 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). Without a causal SNAP-obesity link, it is unclear whether any of these proposals restricting consumption choices of SNAP participants will reduce the probability of being obese among low-income households and may in effect lead to unintended consequences such as increased stigma associated with participation and higher transaction and program administration costs.

Non-random selection of people into the program complicates the estimation of the causal impact of SNAP on obesity. Participants may differ in systematic ways from income-eligible non-participants, making it difficult to obtain unbiased estimates of SNAP’s effect on obesity. Such factors as current or expected future health, human capital characteristics, time and risk preferences, preferences for food and other health inputs, and attitudes toward work are simultaneously related to SNAP participation and health outcomes (Currie 2003, Kreider et al. 2012).

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). A recent NPR story discusses the Trump administration’s budget proposal for fiscal year 2019 which aims to disburse SNAP benefits partly in the form of the so-called “USDA Foods package.” (Hunzinger, Charles, Godoy & Aubrey 2018).
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, Hoynes & Schanzenbach 2016). Descriptive evidence suggests that SNAP participants are less likely to consume appropriate amounts of vitamins and minerals and are more likely to derive energy from solid fats, alcoholic beverages, and added sugars relative to SNAP-eligible households who do not participate in SNAP (Cole & Fox 2008). SNAP households also have lower scores on the Healthy Eating index (HEI) 2005 than income-eligible nonparticipants and income-ineligible nonparticipants (Cole & Fox 2008). Attempts have been made in the literature to surmount this selection bias using instrumental variable (IV) and panel data methods such as fixed effects and propensity score matching approaches.

Beyond addressing the endogeneity of participation, (non-classical) measurement error arising due to the potential misreporting of SNAP status in national surveys poses a considerable threat to causal identification. Misreporting is pervasive in survey data and occurs when SNAP participants report receiving no benefits when they actually did (false negatives) or vice versa (false positives). Meyer et al. (2009) 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, Goerge & Mittag 2015). Also, false negative reporting errors tend to be more frequent than false positives, particularly with government programs.

Even so, models that allow one to quantify and sign the resulting bias from a misclassified binary variable are scarce while the few studies that take on the issue of misclassification seriously usually assume that misreporting occurs randomly with fixed or constant probability (Lewbel 2007).

This paper examines whether SNAP participation is linked to weight gain in adults when we account for the typical case of false negative reporting errors. This paper makes two contributions. First, using a novel approach developed in Nguimkeu et al. (2017), 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, the results highlight the consequences of misreporting on estimated treatment effects in empirical work by comparing our approach to standard estimators. I do not find evidence that SNAP participation significantly increases weight for the full sample or separately by gender. This finding departs from most previous studies suggesting positive impacts of SNAP on adult weight outcomes, especially for females.

The rest of the paper is organized as follows. Section 3.2 presents background information on SNAP. Section 3.3 discusses the related literature. Section 3.4 presents the data. Section 3.5 presents the methodology. Section 3.6 discusses the results and Section 3.7 concludes.

### 3.2 Background and Conceptual Framework

#### 3.2.1 Brief Overview of SNAP

The Food Stamp Program has undergone numerous legislative changes from its establishment under the Food Stamp Act of 1964, through the Food Stamp Act of 1977 (which eliminated the purchase requirement), to the Food, Conservation and Energy Act of 2008 that changed the name of the Food Stamp Act of 1977 to the Food and Nutrition Act of 2008 and renamed the Federal program the Supplemental
Nutrition Assistance Program. 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 (Mabli, Ohls, Dragoset, Castner & Santos 2013). The Food Stamp Act of 2008 contains national eligibility standards (categorical, financial and non-financial) as well as exceptions to the eligibility criteria. 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 (U.S. Department of Agriculture 2017). 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 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. A household’s monthly SNAP allotment is determined as the maximum allotment (based on household size) less 30 percent of monthly net income.

4The 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 more detailed discussion of SNAP’s historical milestones.

5Households 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.
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.\(^7\)

3.2.2 Can SNAP Participation Influence BMI?

Theoretically, the impact of SNAP on obesity is ambiguous. This section explores two theoretical links between SNAP participation and obesity: neoclassical economic theory and the “Food Stamp Cycle” hypothesis.

Neoclassical 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 nonfood good. Since relative prices are unchanged, SNAP benefits can be thought of as a 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 and affords greater consumption of food and nonfood goods as would any arbitrary increase in disposable income or cash transfer.

However, due to the in-kind nature of SNAP, the Southworth model presented in Figure 2.1 distinguishes between inframarginal or unconstrained participants and

---

\(^6\)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 current review of SNAP and other nutrition assistance programs in the U.S.
extramarginal or constrained participants. Before the receipt of SNAP, the individual chooses the mix of food and nonfood goods such that her utility is maximized and her budget exhausted – point $I_1$ in Figure 2.1. Upon receiving SNAP benefits, the individual’s budget constraint shifts outward from $AD$ to the kinked budget constraint represented by $BE_2D$.\(^8\)

Households are unconstrained or inframarginal if they receive less in SNAP benefits than they would otherwise spend on food if their total income were solely cash. From Figure 2.1, inframarginal households would choose the new consumption bundle represented by $I_2$, consuming more of both food and nonfood goods.\(^9\) In this scenario, we can draw on existing research on how changes in income affects obesity to predict the impact of SNAP on obesity. Even so, existing research on the relationship between income and weight (or obesity) is mixed and inconclusive (see, for e.g., Cawley, Moran & Simon (2010), Schmeiser (2009), Lindahl (2005)).

Households are classified as extramarginal or constrained if they receive more in SNAP benefits than they would otherwise have allocated for food if all their income were cash. These consumers may have stronger preferences for relatively lower food consumption and their consumption bundle before SNAP participation is represented by point $E_1$ in Figure 2.1. After receiving SNAP, the extramarginal consumer spends only her SNAP benefits on food expenditures and chooses the bundle denoted by point $E_2$ (kink) in Figure 2.1. Such an individual is predicted to spend proportionately more on food with SNAP than an equivalent cash transfer. There is some evidence, possibly plagued by selection bias, that SNAP participants with excess allowances tend to purchase more food than they otherwise would (Fox, Hamilton & Lin 2004, Devaney & Moffitt 1991, Fraker, Martini & Ohls 1995). Hoynes & Schanzenbach (2009) provides recent evidence that estimates of the

---

\(^8\)The triangle marked $DE_2C$ represents consumption bundles that are attainable since SNAP benefits are targeted or in-kind.

\(^9\)Consumption of food goes up by less than the full amount of SNAP benefits.
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.

Whether SNAP participants are inframarginal or extramarginal, the increased spending on food can positively or negatively impact obesity depending on the mix of “healthy" and “unhealthy" food purchased and consumed, which ultimately depends on the preferences of households. If SNAP participants are selected from a population with stronger preferences for “unhealthy" food, then 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. Even given the same preferences, the loosening of the budget constraint could lead to spending on goods that increase (decrease) sedentary activities resulting in weight loss (gain).

Depending on the proportion of recipient households that are inframarginal versus extramarginal and the types of food purchased, SNAP participation may or may not have anything to do with obesity.

*The Food Stamp Cycle Hypothesis*

The Food Stamp Cycle describes the phenomenon where SNAP participants unevenly use SNAP benefits and consume calories during the course of the month. Wilde & Ranney (2000) and Shapiro (2005) present evidence that SNAP participants’ food spending and food energy intake (calories) peaks sharply in the first few days upon receipt of SNAP benefits and declines significantly with the passage of time, suggesting that the timing of SNAP receipt may induce a preference for immediate consumption. The Food Stamp Cycle thus leads to periods of over-consumption (surpluses) and under-consumption (shortages) that is linked to weight gain in both adults and children (Blackburn, Wilson, Kanders, Stein, Lavin, Adler & Brownell 1989, Polivy, Zeitlin, Herman & Beal 1994, Dietz 1995).
3.3 Related Literature and Misreporting of SNAP

Several papers examine the impact of SNAP on a host of outcomes, including poverty, food insecurity, food consumption, and weight outcomes. In terms of SNAP's relationship with obesity, a common finding is that SNAP participation is positively correlated with 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 an individual fixed effects estimator 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 researchers 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 policy makers is that while SNAP participation does not increase or decrease probability of being obese for children and non-elderly men, it tends to increase the probability of being obese or overweight for non-elderly adult women (U.S. Department of Agriculture 2012).

Apart from the well-known selection bias in evaluating SNAP, the consequence of the high and rising rates of misreported participation has not received sufficient attention in this literature. Misreporting of SNAP participation in national surveys has been well-documented with false negatives being more prevalent than false positives. For instance, false negatives for SNAP are estimated 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

\footnote{For reviews, see Currie (2003), Bartfeld et al. (2015), and Hoynes & Schanzenbach (2016).}
to 50% in the 2002-2005 Annual Social and Economic Supplement (March CPS) (Meyer, Goerge & Mittag 2015). Also, Mittag (2013) finds 26% false negatives in the 2008-2010 ACS while Almada, McCarthy & Tchernis (2016) estimate 23 – 45% false negative rates in the National Longitudinal Survey of Youth (NLSY) - 1979 cohort.\footnote{As in my paper, Almada et al. (2016) do not have access to administrative data as do the previous two papers.} However, the corresponding false positive error rates are less than 1.5% (Meyer, Goerge & Mittag 2015).

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. 2017). When a binary explanatory variable is misclassified, the measurement error is necessarily nonclassical and 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 completely random or exogenous. Examining the consequences of infrequent arbitrary errors in a binary explanatory variable, Kreider (2010) finds that even with misclassification error rates of 2%, the confidence intervals from the contaminated data that the researcher observes and the true error-free data might not overlap.

Evidence from several papers including 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 the context of this paper, misreporting may be endogenous to the outcome if individuals with higher body weight are more or less likely to report program receipt.

Methods for estimating the treatment effects of an endogenous and possibly
misreported binary regressor remains an active area of research. Obviously, the OLS estimator inconsistent for the average treatment effect of SNAP participation and may even assume a “wrong sign” in special cases (see for e.g., Hu, Shiu & Woutersen (2015b) and Nguimkeu et al. (2017) for sign-reversal results). 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. For instance, Frazis & Loewenstein (2003) provide a Generalized Method of Moments (GMM) estimator when instruments are available and provide bounds when the misclassified variable is endogenous. Following Mahajan (2006), Lewbel (2007) also considers the estimation of the treatment effect of a misclassified binary regressor in nonparametric and semiparametric regression and achieves identification using an “instrument-like” variable.

Only a handful of papers 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 significant effects for SNAP’s effects on the probability of being obese, Almada et al. (2016) caution that instrument-based estimators are overstated and exceed nonparametric upper bounds (by over 200% in 12

For instance, Black et al. (2000) show that, under appropriate assumptions, the parameter estimate of a mismeasured independent variable may be asymptotically bounded between the OLS and IV estimators and provide a method-of-moments estimator for the case of binary or discrete variables.

13See Nguimkeu et al. (2017) for some evidence on the performance of Lewbel’s estimator.
certain cases) under reasonable assumptions of the treatment selection and misreporting probabilities. When both participation and response error are allowed to endogenous, this study employs the framework developed in Nguimkeu et al. (2017) to consistently estimate the average treatment effect of SNAP on body weight.

3.4 Data

This paper uses data from the National Longitudinal Survey of Youth - 1979 Cohort (NLSY79), which is a nationally representative sample of 12,686 men and women surveyed annually from 1979 and biennially after 1994. The NLSY79 is comprised of 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 the 1996, 1998, 2000, 2002, and 2004 waves in part due to the availability of state-level policy variables from the SNAP Policy Database, which are used to instrument SNAP participation (U.S. Department of Agriculture (USDA) 2016).

The respondents were between 14 and 22 years old in 1979, thus, the ages of the respondents in the analysis sample range from 31 to 39 years.

The dependent variable of interest is respondents’ body weight as measured by BMI, which is constructed from the self-reports of weight and height. I restrict the sample to observations with non-missing values of weight and height biennially from 1996 to 2004.

---

14 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.

The treatment indicator of interest is a dummy that equals 1 for SNAP participation in at least one month of the past calendar year and zero otherwise. In the final analysis sample, 16.26% of the survey respondents reported SNAP participation in at least one month of the previous calendar year. Out of this reported participation, about 72.36% participated 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 individual states can make exemptions in certain cases.

As a result, the majority of studies have resorted to checking whether a household’s income (after adjusting for household size) meets a particular multiple of the federal poverty line. While some studies use the gross income cutoff of 130% of the federal poverty line to determine SNAP eligibility, other studies have used higher thresholds of up to 250% of the federal poverty line. Using just the gross income test to determine eligibility can result in comparisons with individuals that are not truly eligible for SNAP. Also, since eligibility is based on monthly gross and net income, using a more restrictive threshold might miss those who become eligible for only certain portions of the year (Mykerezi & Mills 2010, Almada et al. 2016). Thus, I restrict the final analysis sample to respondents at or less than 250% of the federal poverty level who are observed in at least two waves from 1996 to 2004. Doing so captures about 96% of reported SNAP 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 the age of household members, household size, family income, information on labor market activities, and educational attainment of respondents and their
mothers. Educational attainments for respondents and their mothers are measured by dummies for completing of high school or more. Labor market activity is captured by weekly hours worked in the past calendar year as well as current employment status. Also, the NLSY79 collects post-interview information from interviewers include demographic characteristics and other remarks about the interview process such as the respondents’ general attitude and the presence of third parties during the interview. Section 3.5 discusses how I exploit these additional interview and interviewer characteristics in the estimation strategy.

The final data set consists of 2,798 persons and 8,502 person-year observations. Table 3.1 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 29.39 while it is 27.94 for nonparticipants. The summary statistics indicate that SNAP participants are negatively selected on a variety of observable dimensions. For instance, SNAP recipients belong to households with lower family incomes ($15,147.11 vs. $24,522.59), work for fewer average weekly hours (20.67 vs. 34.38), have slightly larger household sizes (3.70 vs. 3.43) with more children (2.07 vs. 1.69), are less likely to be married (0.25 vs. 0.47), are less likely to have a high school diploma or higher (0.72 vs. 0.84), are more likely to have mothers have graduated from high school (0.41 vs. 0.52), and are more likely to participate in WIC (0.21 vs. 0.06) relative to nonparticipants.

3.5 Methodology

The objective of this study is to estimate the average treatment effect of SNAP participation on BMI, accounting for selection bias and possible (endogenous) misreporting of participation. As previously discussed, self-selection into SNAP along unobservable dimensions as well as the possible misclassification of participation status renders a naive regression of weight status on the binary
participation indicator an inconsistent estimator for the average treatment effect of interest. In fact, although standard linear IV estimators may address the self-selection problem, they are inconsistent for the treatment effect in light of the nonclassical nature of misreported participation.

In the remainder of this section, I present the econometric framework, developed in Nguimkeu et al. (2017), which addresses these problems by simultaneously modeling SNAP participation and misreporting decisions in relation to the evolution of BMI. For concreteness, we are interested in the causal effect of participating in SNAP on BMI in the linear treatment effects model

$$y_{it} = x_{it}' \beta + T^*_i \alpha + \epsilon_{it},$$

(3.1)

where $y_{it}$ is BMI for individual $i$ at time $t$, $T^*_i$ is individual $i$’s true unobserved (to the researcher) SNAP participation status in year $t$, $x_{it}$ is a vector of observed characteristics, $\beta$ is a k-parameter vector, and $\epsilon_{it}$ is the error term. Our interest lies in estimating the treatment effect denoted by $\alpha$.

In the empirical analyses, $x_{it}$ includes demographic characteristics such as respondent’s age, race, gender, marital status. It also includes family characteristics such as household size, number of children, logarithm of income and human capital characteristics such as educational attainment and mother’s education. Other variables included in $x_{it}$ 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, SSI receipt, and indicators for having an infant ($\leq 5$ years) and an elderly person ($\geq 65$ years) living in the home.

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

\[ T_{it}^* = 1 \left( z_{it}' \theta + v_{it} \geq 0 \right), \quad (3.2) \]

where \( z_{it} \) is a vector of observed covariates related to the decision to participate in SNAP, \( \theta \) is a \( q \)-parameter vector, and \( v_{it} \) is the error term. The endogeneity of true participation arises due to the self-selection mechanism in equation (3.2) and the correlation of the error terms in equations (3.1) and (3.2).

In equation (3.2), \( z_{it} \) includes \( x_{it} \) in addition 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 probability of take-up but should not directly influence BMI in equation (3.1) or the propensity to misreport in equation (3.3) below. The Personal Responsibility and Work Opportunity Reconciliation Act of August 22, 1996 (PRWORA) mandated all states to implement EBT systems by the year 2002 which allows recipients to authorize their SNAP benefits to be electronically transferred unto their EBT accounts monthly.\(^{16}\) 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.\(^{17}\) Figure 3.3 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.

There are at least two ways in which the percentage of benefits issued via EBT card can influence participation without directly influencing BMI. First, states that mail benefits by direct mail (as opposed to using EBT cards), increase the costs.

\(^{16}\)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 effected by PRWORA can be found at https://www.fns.usda.gov/snap/short-history-snap.

\(^{17}\)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.
associated with participation. Supposedly, receiving benefits via an EBT card is less burdensome and can make it easier for the marginal individual to take up SNAP. Second, households in states that issue a higher percentage of benefits via EBT cards may be more likely to take up SNAP because of the (perceived) lower costs of participation resulting from reduced stigma associated with using benefits via EBT cards (which function just like regular debit cards) as opposed to coupons (Currie 2003, Wright, Tekin, Topalli, McClellan, Dickinson & Rosenfeld 2017).

Since true SNAP participation, \( T_{it}^* \), is unobserved (due to possible misreporting), the researcher observes a surrogate, \( T_{it} \) that is generated as

\[
T_{it} = T_{it}^* \times R_{it},
\]

where \( R_{it} \) is a reporting dummy variable characterized by

\[
R_{it} = 1 \left( w_{it}' \gamma + u_{it} \geq 0 \right),
\]

where \( w_{it} \) is a vector of observed covariates related to the decision to correctly (or incorrectly) report program participation, \( \gamma \) 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 (3.4) and the fact that the error terms in equations (3.1) and (3.4) are allowed to be correlated.

Equations (3.2) and (3.4) together form a complete model of SNAP participation and reporting although \( T_{it} \) and \( R_{it} \) are unobserved. It is obvious from the observation mechanism in equation (3.3) 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.

Figure 3.2 illustrates the partial observability of participation inherent in equation (3.3). If we suppose that we have a random sample of eligible participants,
then Figure 3.2 shows two levels of data observability. In Level 1, the data is split into two distinct classes, namely true participants and true nonparticipants. Level 1 is not observable to the researcher but only the individual respondent. Level 2 describes the observed data which are split into two cases, A and B. Case A consists of two observationally indistinguishable cases, namely, true reports of nonparticipation and false reports of nonparticipation (false negatives). These two scenarios under Case A are observationally equivalent because the researcher only observed $T_{it} = 0$ in both instances. In the second case denoted by B in Figure 3.2, true participants (correctly) report participation. These distinct groups of observations (A and B) motivate the maximum likelihood estimation of the partial observability model described in equations (3.3) and (3.4).

In equation (3.4), $w_{it}$ includes $x_{it}$ and additional regressors that are hypothesized to be associated with one’s probability of accurately reporting participation. These extra covariates will be excluded from equation (3.2) but need not be excluded from the outcome equation. The exclusion restrictions in equations (3.2) and (3.4) 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.\textsuperscript{18}

As previously mentioned, the NLSY79 interviewers participate in a survey after the interview process where information is collected on their perceptions regarding the interview process and their interaction with interviewees such as 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 in the post-interview survey as well as the gender and race of the interviewer as the excluded predictors of misreporting in equation (3.4).

\textsuperscript{18}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).
Collectively, these variables are in the spirit of the “cooperativeness hypothesis” in Bollinger & David (2001) who find favorable evidence for the hypothesis that respondents with high propensity to cooperate with the survey are more likely to truthfully report their participation. 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 to misreport participation but should not affect one’s participation decision or the evolution of body weight.

3.5.1 Estimation Procedure

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 (3.3) and (3.4), we can write the double-index model for observed participation, \( T_{it} \), as

\[
T_{it} = T_{it}^* \times R_{it} = 1(z_i'\theta + v_{it} \geq 0, \ w_i'\gamma + u_{it} \geq 0).
\]

(3.5)

If we denote the joint cumulative density function (CDF) of \((-u, -v)\) by

\[
F(u, v, \rho) = \Pr[-u_i \leq u, \ -v_i \leq v], \ \text{for any} \ -\infty < u, \ v < +\infty,
\]

(3.6)

then the parameters \(\theta\) (equation (3.2)), \(\gamma\) (equation (3.4)), and \(\rho\) (the correlation between \(u\) and \(v\)) may be consistently estimated in the first stage. I estimate the following binary choice model by maximum likelihood:

\[
\Pr[T_{it} = 1|w_{it}, z_{it}] = \Pr[-u_{it} \leq w_{it}'\gamma, \ -v_{it} \leq z_{it}'\theta] = F(w_{it}'\gamma, z_{it}'\theta, \rho) = P_i(\gamma, \theta, \rho),
\]

(3.7)
where the log-likelihood function of the model is given by

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

In the second step, each person’s predicted probability of true participation, \(\hat{T}_{it}^*\), is obtained as \(\hat{T}_{it}^* = \Phi(z'_{it} \hat{\theta})\) using the estimates of \(\theta\) 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 in the new model given by

$$y_{it} = x'_{it} \beta + \hat{T}_{it}^* \alpha_{2S} + \eta_{it},$$

where \(\alpha_{2S}\) denotes the average treatment effect of SNAP on BMI and \(\eta_i\) is the associated disturbance term. It can be shown that the above two-step procedure is a consistent and asymptotically normal estimator of the treatment effect of interest (Nguimkeu et al. 2017).

### 3.6 Results and Discussion

I present estimates from the first step estimation of the partial observability model in equation (3.5) followed by the second step results from equation (3.8). Before turning to the regression results, one may be interested in the estimated false negative rate using the first stage estimates from equation (3.7). Given the one-sided nature of the the econometric framework I adopt in this paper, it can be shown that the false negative rate for each person is given by

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

(3.9)
where $F(.,.,.)$ and $\Phi(.)$ respectively denote the joint bivariate and univariate normal CDFs, and the hats denote parameter estimates from the first stage estimation of the binary choice model in equation (3.7). Thus, averaging the quantity in equation (3.9) yields a consistent estimate of the population false negative rate. The estimated false negative rates are 20.72 percent for the full sample, 8.67 percent for females, and 54.18 percent for males. These estimated false negative rates are similar to those obtained in Almada et al. (2016).

### 3.6.1 First Stage Estimation

Table 3.2 reports the maximum likelihood estimates of the parameters of the excluded regressors in the true participation and reporting equations. Panels A and B in Table 3.2 correspond with equations (3.2) and (3.4), respectively.

As discussed in Section 3.5, two sets of covariates need to be distinguished: (a) instruments for true participation ($z_{it}$), and (b) predictors of misreporting ($w_{it}$). Although these sets of covariates may overlap, they must be different for identification purposes. In other words, at least one excluded variable (exclusion restriction) in either the participation or reporting equation suffices for identification. All regressions also include the additional covariates from the outcome equation.

As previously mentioned, the state-level policy variable that is used to instrument for true participation is the percentage of SNAP benefits issued by the state via EBT cards. Panel A in Table 3.2 shows that the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards is positive and statistically significantly correlated with the true participation probability for the full sample and by gender. The wald test of excluded instrument also suggests that EBT card benefit issuance is a strong predictor of participation.

Panel B in Table 3.2 presents coefficient estimates of the excluded predictors of
misreporting in the reporting equation (3.4). I rely on the NLSY79 interview and interviewer characteristics as excluded covariates driving the reporting process. It is noteworthy that these covariates 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 race of the interviewer. I expect these covariates to influence one’s probability of misreporting participation without affecting the probability of participation.

The interview mode is a categorical variable with three levels describing features of the interview process. The three levels are: 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). Also, the NLSY79 interviewers were surveyed after each interview 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 a dummy variable for whether the interviewee and the interviewer are of the same gender, whether the interviewee and the interviewer are of the same race, and an interaction of these two dummy variables.

Due to a lack of a general theory of misreporting, I do not have strong \textit{a priori} expectations about the directions of the effects of these interview and interviewer characteristics. Nonetheless, one can draw on a 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, Müller & Riphahn (2015), O’Muircheartaigh & Campanelli (1998), Schober & Conrad (1997), Suchman & Jordan (1990)).
The estimates in Panel B of Table 3.2 suggest that the interview, interviewee, and interviewer characteristics are correlated with the probability of true participation in equation (3.4). A large body of literature has considered measurement error in survey responses to sensitive questions, especially when the answers may be stigmatized or not viewed as socially desirable. For instance, Tourangeau & Yan (2007) report substantial error in responses to sensitive questions and also notes that such inaccurate responses vary significantly with the mode of administering the survey. I find that having an adult present during the interview is positively associated with the probability of truthfully reporting participation status, relative to being interviewed alone in person, although not statistically significant for females (see Table 3.2). 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.

Bollinger & David (1997, 2001) discuss the so-called “cooperator hypothesis” where survey respondents may or may not cooperate with the survey in terms of giving accurate responses. They provide evidence in support of the hypothesis that cooperators have a tendency to give more accurate responses (Bollinger & David 2001, 2005). I find evidence that the respondent’s attitude during the interview is associated with the probability of truthfully reporting participation. The results suggest that interviewees who are impatient, restless or hostile during the interview are less likely to truthfully report participation and this association is statistically significant for the full and male samples. For females, respondents who are not interested are less likely to report participation truthfully.

Finally, a related literature studies how interviewers (for e.g., interviewer demographic characteristics) affects the accuracy of survey responses. One might
expect interviewers’ gender and race to affect survey responses when respondents know or can perceive the demography of the interviewer.\textsuperscript{19} 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 an interaction of these two dummy variables. From Table 3.2, I find that, for females, being interviewed by someone of the same sex is negatively correlated with the propensity of truthfully reporting participation and this effect varies statistically significantly by race. I do not find statistically significant effects of such gender and race combinations for the full and male samples.

Overall, the results from the first stage of the two-step estimator used in this paper suggest that the instruments for true participation and predictors of participation are strongly correlated with the observed, reported SNAP participation.

3.6.2 Second Stage Estimation

Table 3.3 reports the estimated average treatment effect of SNAP participation on BMI using the two-step estimator in equation (3.8). I do not find a statistically significant effect of SNAP participation on BMI for the full sample or by gender. The estimated treatment effect of SNAP participation on BMI for females of -1.973 implies a weight loss of approximately 12 pounds, albeit not statistically significant.\textsuperscript{20} For the full and male samples, the estimated coefficients are also statistically insignificant and imply a weight gain of about 3 pounds and 7.7 pounds, respectively.

\textsuperscript{19}See Weisberg (2009) for a more detailed review of the literature on interviewer effects in surveys.

\textsuperscript{20}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.
For the purposes of comparison with the two-step estimator, Table 3.3 also presents estimates of SNAP’s effect on BMI using ordinary least squares (OLS) and standard instrumental variable (IV) estimators. The OLS estimates of SNAP’s effects are positive and statistically significant, with participation being associated with an increase in weight of about 5.1 pounds and 4.8 pounds for the full sample and females, respectively. The coefficient estimate is slightly smaller in magnitude for males but is not statistically significant. Obviously, the OLS estimator is biased and inconsistent due to both self-selection and possible misreporting of participation and the direction of bias aligns with SNAP participants being negatively selected.

The IV estimator uses the same instrument for participation (i.e., the percentage of SNAP benefits issued by the state via electronic benefit cards) as the two-step estimator adopted in this paper. The first stage results for the IV estimator are summarized in Table 3.4, showing high and statistically significant F-statistics. From Table 3.3, the IV estimates are negative and statistically insignificant but with magnitudes implying weight reductions of about 7.2 pounds for females and notably implausible weight reductions of almost 100 pounds for males (see Table 3.3). The IV results for males partly highlights the sensitivity of IV methods that do not account for misreporting process directly.

Robustness Checks

Since the self-reported height and weight in the NLSY can be misreported as well, I re-estimate the models adjusting for BMI using predicted height and weight using data from the Third National Health and Nutrition Examination Survey (NHANES III) following Courtemanche, Pinkston & Stewart (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 as well as a polynomial in age by race and gender. Thereafter, 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 measure as usual. The results using the adjusted BMI measure are reported in Table 3.5 and are very similar to using the reported NLSY values.

As previously mentioned, qualifying for SNAP is based on financial, non-financial and categorical eligibility rules. I initially restricted the analysis sample to respondents below 250% of the federal poverty line since the literature recognizes that the federal gross income eligibility threshold of 130% is too restrictive (Mykerezi & Mills 2010, Almada et al. 2016). However, I re-estimated the model with the sample restricted to 185% and 130% of the federal poverty level for the full sample and females. Table 3.6 presents the results for these alternative eligibility criteria (as well as the initial 250% FPL threshold for comparison) and shows that the pattern of results is unchanged in terms of statistical significance. For the full sample, the magnitudes of the estimated impacts of SNAP on BMI suggests (statistically insignificant) weight gains of 7.9 pounds and 6.6 pounds for the 185%-FPL and 130%-FPL samples, respectively. The estimated effect of SNAP on BMI remains negative and statistically insignificant for females, with magnitudes indicating smaller weight reductions of about 8.3 pounds and 2.6 pounds for 185% and 130% of the federal poverty level, respectively.

My results suggest no statistically significant effect of SNAP on BMI for the full sample and also by gender when we account for both the endogeneity and possible misreporting of participation in one unifying framework. The two-step estimates also suggest that the estimated average treatment effect of SNAP is not bounded between the OLS and IV estimates as has been suggested elsewhere when misreporting is exogenous.

Moreover, the findings of this paper depart from previous studies suggesting

\[21\text{Due to non-convergence, results are not reported for the male sample using these alternative eligibility criteria.}\]
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). For instance, Chen et al. (2005) find that SNAP participation is associated with an increase of 3.61 BMI units, implying a weight increase of almost 22 pounds. In particular, although not statistically significant, I find a reduction in BMI for females that is linked to SNAP participation. Thus, the estimated coefficients in this study do not support the hypothesis that SNAP participation associated with obesity.

3.7 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 which mostly finds a positive impact of SNAP on the probability of being obese, especially for females, has inadequately addressed the problem of high misreporting rates in reported participation in national surveys. Not only is SNAP participation subject to severe misreporting but such measurement error may be endogenous. Although the prevalence of misreporting is not new, few researchers have examined its consequences for estimating the impacts of SNAP. This paper estimates the casual effect of SNAP on adult body weight in the presence of endogenous misreporting using a novel identification strategy that explicitly addresses both the endogeneity of participation and the systematic nature of misreporting (Nguimkeu et al. 2017). In contrast to most previous studies, I find that SNAP participation is associated with reductions of approximately 2 BMI units (about 7% on average) for females but these changes are not statistically different from zero.

The econometric framework models the evolution of body weight, true participation, and the misreporting mechanism jointly. The first equation is the
usual treatment effects model relating BMI to an unobserved SNAP participation indicator and other contributing factors. To address selection bias, the second equation models true SNAP participation as a function of observed covariates such as those in the outcome equation and exclusion restrictions. Finally, the third equation describes an individual’s probability to misreport her true SNAP participation status as a function of observed covariates including demographic and survey/interviewer characteristics.

I use predicted probabilities of true participation from a first stage estimation which are free from self-selection and measurement error to estimate the causal effect of SNAP participation on BMI. In addition to functional form assumptions, I employ exclusion restrictions for participation and predictors of misreporting such as survey and interviewer characteristics to strengthen identification. I do not find evidence of a statistically significant effect of SNAP on weight status for females with the estimated effect suggesting reductions in BMI. Even when SNAP participation is positively associated with BMI such as for males and the full sample, the magnitude of the effects is not large enough to cause people of normal weight to become obese.

This study has a few limitations. 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).
Figure 3.1: Neoclassical framework for analyzing impact of SNAP on consumption

Figure 3.2: First Stage (Partial Observability Model)
Figure 3.3: The distribution of the percentage of SNAP benefits issued by the state via electronic benefit (EBT) cards (by year)
Table 3.1: Summary Statistics by SNAP Participation Status

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Nonparticipants</th>
<th>Participants</th>
</tr>
</thead>
<tbody>
<tr>
<td>Body Mass Index</td>
<td>28.13</td>
<td>27.94</td>
<td>29.39</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.16)</td>
<td>(0.42)</td>
</tr>
<tr>
<td>Age</td>
<td>38.71</td>
<td>38.79</td>
<td>38.21</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.19)</td>
</tr>
<tr>
<td>Female</td>
<td>0.58</td>
<td>0.55</td>
<td>0.75</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.09</td>
<td>0.08</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Black</td>
<td>0.23</td>
<td>0.21</td>
<td>0.35</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Household Size</td>
<td>3.46</td>
<td>3.43</td>
<td>3.70</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Married</td>
<td>0.44</td>
<td>0.47</td>
<td>0.25</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Mother’s education (High school graduate or higher)</td>
<td>0.51</td>
<td>0.52</td>
<td>0.41</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>High school graduate or higher</td>
<td>0.82</td>
<td>0.84</td>
<td>0.72</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Number of children</td>
<td>1.74</td>
<td>1.69</td>
<td>2.07</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Household with child (&lt; 5 years)</td>
<td>0.17</td>
<td>0.17</td>
<td>0.22</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Lives in Urban Area</td>
<td>0.66</td>
<td>0.66</td>
<td>0.70</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>WIC</td>
<td>0.08</td>
<td>0.06</td>
<td>0.21</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>SSI</td>
<td>0.09</td>
<td>0.06</td>
<td>0.27</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>AFDC/TANF</td>
<td>0.07</td>
<td>0.01</td>
<td>0.40</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.00)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Household in poverty</td>
<td>0.31</td>
<td>0.26</td>
<td>0.65</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Average Weekly Hours worked (Past Calendar Year)</td>
<td>32.55</td>
<td>34.38</td>
<td>20.67</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.51)</td>
<td>(0.92)</td>
</tr>
<tr>
<td>Household with elderly (&gt; 65 years)</td>
<td>0.07</td>
<td>0.07</td>
<td>0.05</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Employed</td>
<td>0.83</td>
<td>0.86</td>
<td>0.65</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Total Net Family Income (2004 dollars)</td>
<td>23,267.58</td>
<td>24,522.59</td>
<td>15,147.11</td>
</tr>
<tr>
<td></td>
<td>(484.88)</td>
<td>(518.98)</td>
<td>(446.10)</td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>7120</td>
<td>1382</td>
</tr>
</tbody>
</table>

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 line.
Table 3.2: Partial Observability Probit Model of Observed Participation

<table>
<thead>
<tr>
<th>Panel A: True Participation Equation</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage of Benefits issued via EBT Card</td>
<td>0.452***</td>
<td>0.467***</td>
<td>0.451***</td>
</tr>
<tr>
<td>(0.088)</td>
<td>(0.108)</td>
<td>(0.173)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>5036</td>
<td>3466</td>
</tr>
<tr>
<td>Wald Test of Excluded Instruments p-value</td>
<td>0.000</td>
<td>0.000</td>
<td>0.009</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: True Reporting Equation</th>
</tr>
</thead>
</table>

*Interview Mode Dummies*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Any Adult Present During Interview</td>
<td>0.166</td>
<td>0.073</td>
<td>0.597**</td>
</tr>
<tr>
<td>(0.176)</td>
<td>(0.140)</td>
<td>(0.255)</td>
<td></td>
</tr>
<tr>
<td>Phone Interview</td>
<td>-0.152</td>
<td>-0.203</td>
<td>-0.0437</td>
</tr>
<tr>
<td>(0.166)</td>
<td>(0.137)</td>
<td>(0.186)</td>
<td></td>
</tr>
</tbody>
</table>

*Respondent’s Attitude Dummies*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Not Interested But Cooperative</td>
<td>-0.330</td>
<td>-0.345***</td>
<td>-0.188</td>
</tr>
<tr>
<td>(0.213)</td>
<td>(0.129)</td>
<td>(0.234)</td>
<td></td>
</tr>
<tr>
<td>Impatient/Restless/Hostile</td>
<td>-0.485**</td>
<td>-0.128</td>
<td>-1.053**</td>
</tr>
<tr>
<td>(0.216)</td>
<td>(0.238)</td>
<td>(0.433)</td>
<td></td>
</tr>
</tbody>
</table>

*Interviewer Characteristics*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Same Gender Dummy (Interviewer &amp; Interviewee)</td>
<td>-0.093</td>
<td>-1.128**</td>
<td>0.419</td>
</tr>
<tr>
<td>(0.280)</td>
<td>(0.497)</td>
<td>(0.358)</td>
<td></td>
</tr>
<tr>
<td>Same Race Dummy (Interviewer &amp; Interviewee)</td>
<td>-0.233</td>
<td>-1.065*</td>
<td>-0.281</td>
</tr>
<tr>
<td>(0.257)</td>
<td>(0.579)</td>
<td>(0.231)</td>
<td></td>
</tr>
<tr>
<td>Interaction of Same-gender &amp; Same-race Dummy (Interviewer &amp; Interviewee)</td>
<td>-0.007</td>
<td>1.049*</td>
<td>-0.413</td>
</tr>
<tr>
<td>(0.240)</td>
<td>(0.582)</td>
<td>(0.503)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>5036</td>
<td>3466</td>
</tr>
<tr>
<td>Wald Test of Excluded Instruments p-value</td>
<td>0.056</td>
<td>0.0287</td>
<td>0.110</td>
</tr>
</tbody>
</table>

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 line. 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, receiving SSI benefits, and indicators for having an infant (≤ 5 years) and an elderly person (≥ 65 years) living in the home.

*p < 0.10, **p < 0.05, ***p < 0.010*
Table 3.3: Effects of SNAP participation on BMI

<table>
<thead>
<tr>
<th>Estimator</th>
<th>OLS</th>
<th>IV</th>
<th>2S</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent Variable: BMI</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full Sample</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.835***</td>
<td>-6.040</td>
<td>0.490</td>
</tr>
<tr>
<td></td>
<td>(0.334)</td>
<td>(5.856)</td>
<td>(1.498)</td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>8502</td>
<td>8502</td>
</tr>
<tr>
<td>Female Sample</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.794*</td>
<td>-1.180</td>
<td>-1.973</td>
</tr>
<tr>
<td></td>
<td>(0.428)</td>
<td>(7.782)</td>
<td>(1.632)</td>
</tr>
<tr>
<td>Observations</td>
<td>5036</td>
<td>5036</td>
<td>5036</td>
</tr>
<tr>
<td>Male Sample</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.649</td>
<td>-16.261</td>
<td>1.265</td>
</tr>
<tr>
<td></td>
<td>(0.510)</td>
<td>(9.831)</td>
<td>(1.531)</td>
</tr>
<tr>
<td>Observations</td>
<td>3466</td>
<td>3466</td>
<td>3466</td>
</tr>
</tbody>
</table>

Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2S) 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 line. 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 (≤5 years) and an elderly person (≥ 65 years) living in the home.

*p < 0.10, **p < 0.05, ***p < 0.010
Table 3.4: First Stage IV Estimates

<table>
<thead>
<tr>
<th>Dependent Variable: SNAP Participation</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage of Benefits issued via EBT Card</td>
<td>0.046***</td>
<td>0.048***</td>
<td>0.0425***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.014)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>F- statistics</td>
<td>22.54***</td>
<td>11.93***</td>
<td>10.73***</td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>5036</td>
<td>3466</td>
</tr>
</tbody>
</table>

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 line. 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, receiving SSI benefits, and indicators for having an infant (≤5 years) and an elderly person (≥ 65 years) living in the home.

*p < 0.10, **p < 0.05, ***p < 0.010
### Table 3.5: Effects of SNAP on Adjusted BMI

**Dependent Variable: Adjusted BMI**

<table>
<thead>
<tr>
<th>Estimator</th>
<th>OLS</th>
<th>IV</th>
<th>2S</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Full Sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.804***</td>
<td>-5.499</td>
<td>0.460</td>
</tr>
<tr>
<td>(0.307)</td>
<td>(5.168)</td>
<td>(1.387)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>8502</td>
<td>8502</td>
</tr>
<tr>
<td><strong>Female Sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.694*</td>
<td>-0.845</td>
<td>-1.818</td>
</tr>
<tr>
<td>(0.384)</td>
<td>(6.643)</td>
<td>(1.697)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>5036</td>
<td>5036</td>
<td>5036</td>
</tr>
<tr>
<td><strong>Male Sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.648</td>
<td>-16.261</td>
<td>1.264</td>
</tr>
<tr>
<td>(0.510)</td>
<td>(9.831)</td>
<td>(1.575)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>3466</td>
<td>3466</td>
<td>3466</td>
</tr>
</tbody>
</table>

Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2S) 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 line. Additionally, BMI is calculated from predicted height and weight as described in the text following Courtemanche et al. (2015). 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 (≤5 years) and an elderly person (≥65 years) living in the home.

*p < 0.10, **p < 0.05, ***p < 0.010*
Table 3.6: Effects of SNAP on BMI with Alternative Eligibility

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Females</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>IV</td>
</tr>
<tr>
<td><strong>250% FPL</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.835***</td>
<td>-6.040</td>
</tr>
<tr>
<td></td>
<td>(0.334)</td>
<td>(5.856)</td>
</tr>
<tr>
<td>Observations</td>
<td>8502</td>
<td>8502</td>
</tr>
<tr>
<td><strong>185% FPL</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.878**</td>
<td>-4.272</td>
</tr>
<tr>
<td></td>
<td>(0.358)</td>
<td>(5.597)</td>
</tr>
<tr>
<td>Observations</td>
<td>5758</td>
<td>5758</td>
</tr>
<tr>
<td><strong>130% FPL</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SNAP Participation</td>
<td>0.972**</td>
<td>1.366</td>
</tr>
<tr>
<td></td>
<td>(0.419)</td>
<td>(5.707)</td>
</tr>
<tr>
<td>Observations</td>
<td>3607</td>
<td>3607</td>
</tr>
</tbody>
</table>

Standard errors in parentheses and are bootstrapped (200 replications) for the two-step (2S) 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 line. 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 (≤5 years) and an elderly person (≥65 years) living in the home.

*p < 0.10, **p < 0.05, ***p < 0.010
CHAPTER 4

Estimating the Associations between SNAP and Food Insecurity, Obesity, and Food Purchases with Imperfect Administrative Measures of Participation

4.1 Introduction

A growing literature documents the problems with relying on survey measures of program participation, which suffer from significant reporting error, when conducting impact evaluations (Meyer, Mok & Sullivan 2015, Nguimkeu et al. 2017). Administrative data are ordinarily assumed to be the “gold standard” to overcoming these econometric challenges, but relatively little evidence exists on the potential problems with administrative records or econometric strategies to address them. We investigate these issues using data from the FoodAPS, which combines a panel of household purchases with a survey and linked administrative data on Supplemental Nutrition Assistance Program (SNAP) participation from both state enrollment records and Electronic Benefit Transfer (EBT) card expenditures. The data, therefore, provide the unique opportunity to evaluate the reliability of administrative records by comparing the two different administrative measures to each other as well as to self-reported participation. Moreover, the data also allow us to examine the sensitivity of participation and misreporting rates and estimated associations between SNAP and food insecurity, obesity, and diet healthfulness to different approaches to cleaning and combining the administrative participation variables.

SNAP is the largest means-tested nutrition assistance program in the U.S., serving millions of low-income individuals and households. It is administered by the
U.S. Department of Agriculture (USDA) with the objectives 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 (Mabli et al. 2013). The number of Americans receiving SNAP benefits tripled from about 17 million to 46 million between 2000 and 2014, while total spending on SNAP has more than quadrupled from about $174 billion to almost $75 billion.\(^1\)

Proponents assert that SNAP participation reduces food insecurity, lifts millions from poverty, and provides a fiscal boost to the economy during downturns (U.S. Department of Agriculture 2012). However, the empirical literature on the impacts of SNAP has produced mixed results. Several studies have documented the expected negative relationship between SNAP and food insecurity (Van Hook & Balistreri 2006, Nord & Prell 2011, Schmidt, Shore-Sheppard & Watson 2016), but others have found statistically insignificant or even positive associations (Gundersen & Oliveira 2001, Hofferth 2004, Huffman, Jensen et al. 2003, Wilde & Nord 2005, Hoynes & Schanzenbach 2016). SNAP is also often found to be positively correlated with obesity, but some studies find insignificant or negative effects (Meyerhoefer & Pylypchuk 2008, Bartfeld et al. 2015, Almada et al. 2016, Almada & Tchernis 2016, Nguimkeu et al. 2017).

These mixed results reflect two main methodological challenges in evaluating the causal effects of SNAP. The first is non-random selection. SNAP participation is endogenous, so there is a strong likelihood that specific unobservable characteristics are correlated with both SNAP participation and nutrition-related outcomes. Such factors might include current or expected future health, human capital, financial stability, and attitudes toward work (Currie 2003, Kreider et al. 2012).

The second identification problem, and the focus of our paper, is measurement

\(^1\)Statistics are from http://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap.
error in SNAP participation, which occurs when SNAP participants are coded as receiving no benefits when they truly did (false negatives) or vice versa (false positives). Misreporting of SNAP participation in national surveys has been documented with false negatives being much more prevalent than false positives.\(^2\) For instance, the estimated false negative rates for SNAP in various surveys range from 20% to almost 50% (Mittag 2013, Meyer & Goerge 2011). There is a growing literature suggesting that the estimated effect of a misclassified binary explanatory variable (such as SNAP participation) may be substantially biased and may even yield “wrong signs” (Kreider 2010, Kreider et al. 2012, Nguimkeu et al. 2017).

Within a one-sided model of endogenous misreporting, (Nguimkeu et al. 2017) provide sign-switching results for the ordinary least squares (OLS) estimator even when participation is exogenous. In this case, they show that the OLS estimator yields the wrong sign if misreporting is endogenous, with the size of the sign-switching region increasing with the rate of false negatives and decreasing with the true participation rate. Similar severe consequences of reporting errors also occur within an instrumental variables framework (Almada et al. 2016). Most researchers using survey data to study SNAP do not account for the possibility of non-classical measurement error and the few that do so make assumptions akin to random misreporting.

A fundamental difficulty in dealing with misreporting is that true participation status is unobserved in almost all surveys, and validation datasets that link survey responses to administrative records are scarce. Ultimately, the usefulness of linked administrative records depends crucially on the quality of the linkage. While administrative data are usually considered the “gold standard,” they can still be missing, incorrectly entered, or outdated. Some measurement error may therefore remain. By linking survey responses to administrative data on SNAP participation

\(^2\)See Bound et al. (2001) for a comprehensive review of measurement error in survey data.
from two different sources, FoodAPS provides a unique opportunity to investigate
issues related to measurement error in both self-reported and administrative
measures.

Specifically, we use data from the FoodAPS to offer some novel insights related
to the reliability of linked administrative SNAP measures. First, we document
substantial ambiguity in both of the administrative measures and show that they
are only slightly more strongly correlated with each other than with self-reported
participation. Estimated SNAP participation and misreporting rates vary with the
coding rules used to resolve this ambiguity and disagreement. We then examine the
relationships between SNAP and food insecurity, obesity, and the Healthy Eating
Index. While the signs of regression estimates are not sensitive to different coding
rules, their magnitudes and levels of statistical significance exhibit meaningful
variability. In sum, these results serve as a cautionary tale about uncritically relying
on linked administrative records when conducting program evaluation research.

4.2 Data

The FoodAPS survey is the first nationally representative survey of U.S. households
to collect comprehensive data about household food purchases as well as health and
nutrition outcomes. FoodAPS is sponsored by the Economic Research Service
(ERS) and the Food and Nutrition Service (FNS) of the USDA to support critical
research that informs policymaking on health and obesity, food insecurity, and
nutrition assistance policy.

The FoodAPS surveyed 4,826 households through a multistage sampling design
with a target population roughly equally divided into SNAP households,
non-participating low income households with income less than the poverty
guideline, non-participating households with income between 100 percent and 185
percent of the poverty guideline, and non-participating households with income at
least equal to 185 percent of the poverty guideline. Survey questions relate to demographic characteristics, income, program participation, food insecurity, health, weight, and height. Also, FoodAPS contains detailed information about individual food purchases and acquisitions (merged with nutrition information), along with variables related to local food availability and prices. A unique feature of FoodAPS that makes it well-suited for our study is the linked administrative records on SNAP participation for consenting respondents. This presents an opportunity to study SNAP misreporting more thoroughly than past research.

Participants were interviewed before they were given a survey to record their food purchases for one week. Self-reported SNAP participation comes from the Initial Interview before the survey week. The primary respondent (PR) was asked about SNAP receipt, including information on the date of last receipt and the amount of benefits received. The PR was the designated “main food shopper” for the household. The specific question asking about SNAP participation states, “(Do you/Does anyone in your household) receive benefits from the SNAP program? This program used to be called food stamps. It puts money on a SNAP EBT card that you can use to buy food.” This question (named SNAPNOWREPORT on the FoodAPS data files) does not specify a reference period, and only respondents who answered “yes” were further asked to provide dates of the last receipt as well as benefit amounts received. Respondents who answered “no” were then asked, “Have (you/anyone in your household) ever received benefits from the SNAP program?” Households who responded in the affirmative to this follow-up question were further asked, “Did (you/anyone in your household) receive SNAP benefits in the last 12 months?” Respondents who answered “yes” to both follow-up questions were also

---

3The FoodAPS field operations were conducted from April 2012 through January 2013, during which each participating household provided information on all acquisitions of all household members during a 7-day interview period.
asked to provide a date of the last receipt. For our indicator of reported SNAP participation (hereinafter “REPORT”), we consider all respondents who answered “yes” to be self-reported participators (including those who answered “no” to the first participation question but “yes” to both follow-up questions), and consider the time-frame to reflect either current or recent participation. In our view, a flexible time-frame is reasonable, as our outcomes (particularly BMI, which is a capital stock) may not respond immediately to changes in benefit receipt, while people who have recently become non-participants may still spend down previously accrued benefits during the reference period.

The FoodAPS contains two distinct administrative measures of SNAP participation. The first is from state caseload files covering March 2012 to November 2012 (“ADMIN”). The second is from the electronic benefit transfer (EBT) ALERT database (“ALERT”). The ALERT transaction data contain one recorded swipe of an EBT Card per user from April through December 2012. FoodAPS is the only nationally representative survey that links reported SNAP participation to two administrative sources, thus making it particularly suitable for our purposes.

While such administrative records sound appealing, they have several limitations that likely lead to measurement error. Both ADMIN and ALERT variables do not always agree either with each other or with the self-reported participation; they also contain various levels of missing data and could be mismeasured. The quality and

---

466 out of the 1461 people who answered “yes” to the first participation question subsequently reported date of the last receipt outside of the previous 31 days. Also, 8 out of 171 people who answered “no” to the first participation question but “yes” to both follow-up participation questions reported date of the last receipt within the previous 31 days. These reported dates of last receipt reflect the ambiguity about whether the initial participation question indicated current or recent receipt of SNAP. Our conclusions remain similar if we code these individuals as non-participants.

5We thank John Kirlin for suggesting this modification to the original SNAPNOWREPORT via email correspondence.

6The EBT ALERT database is Anti-Fraud Locator EBT Retailer Transactions (ALERT) system of the Food and Nutrition Service (FNS) of the USDA designed to help detect signs of abuse, fraud, and waste in the SNAP program. Each record of the EBT ALERT data represents one swipe of the EBT card and includes such variables as information on the state, store ID, EBT account number, date/time of the event, and purchase amount.
availability of the administrative data vary considerably across states. Households can fall into one of four (4) state groups: (a) Group 1: one-to-one match was possible between ADMIN and ALERT data because they both contain the same case identifiers (13 states); (b) Group 2: either the CASEIDs in the ALERT data were scrambled or they are different in the ALERT and caseload data (8 states); (c) Group 3: CASEIDs are different in the caseload and ALERT data, and the former does not include benefit disbursement dates (2 states); and (d) Group 4: the state did not provide SNAP enrollment data (5 states).

Another source of measurement error is that matching from the FoodAPS to administrative SNAP records was probabilistic. All the matches to ADMIN data were based on first name, last name, phone number, and house address (including apartment number) and links were considered “certain matches” if the associated matching score exceeded a pre-determined threshold.7 The linkage to the ALERT data was similarly probabilistic, except in the state Group 1 described above. In state Group 1, if a household first matched probabilistically to caseload data, then a one-to-one match was possible to the ALERT data using CASEIDs. Thus, it is reasonable to presume that the quality of the administrative linkage would be highest in the 13 states in state Group 1. Nonetheless, the quirks of probabilistic matching would suggest unknown degrees of error in the administrative measures of participation in all states. In other words, one can imagine those true SNAP households whose matching score was not high enough to be sufficiently definitive would have to be classified as non-matches (non-participants), and vice versa.

Additionally, the ADMIN and ALERT data may contradict each other because of discrepancies in timing. In the ADMIN data, participation is in most cases defined based on current enrollment status during the interview week. However, in

7The probabilistic matching was implemented using LinkageWIZ record linkage software and resulted in a Cartesian join of each surveyed household with all SNAP enrolment record (or EBT ALERT). The contractors determined a pre-specified score above which to classify a match as “certain.” FoodAPS does not contain the raw matching scores.
the two states in Group 3 mentioned above, exact dates are not available; thus, their current participation status was conditional on the results of the EBT ALERT linkage. For instance, in a few cases, an individual is considered a current participant if they matched at any point during the nine-month data availability window and also matched to the EBT ALERT, with the date of the last receipt (per ALERT) within 36 days of the end of the survey week. Some former and future participants will therefore incorrectly be coded as current participants. The same logic applies to the EBT ALERT data. In the ALERT data, an individual is coded as a participant if she had an EBT card transaction during the survey week and matched to the EBT ALERT data. SNAP participants who did not use the EBT card that week - for instance, because they stocked up on groceries the previous week, or because their monthly benefits already ran out (food stamp cycling) - were coded as non-participants if they were also current non-participants per ADMIN.

Another source of discrepancy regarding timing is that, while the ADMIN variable considers matches to represent current participation if the date of the last receipt is within 32 days of the end of the survey week, the ALERT variable uses 36 days of the end of the survey week. This may be related to the fact that the ALERT data do not have variables indicating the exact timing of deposits into the SNAP accounts. The ALERT issuance dates are approximate because issuance dates are determined by noting increments in the last SNAP balance between swipes. Thus, households classified as current recipients per ALERT may show up as current nonrecipients per the ADMIN variable due to the shorter window used by the latter.

Finally, another issue with the ALERT data is that no match is attempted (and

---

8FoodAPS’s measure of current SNAP participation based on the two administrative linkages is summarized in the SNAPNOWADMIN variable, which combines the results of the two administrative matches into a single variable and also imputes missing data using the self-report.

9In the remainder (majority) of cases in the two states whose current SNAP participation cannot be determined based on EBT ALERT matching (conditional on ADMIN) or ADMIN (conditional on ALERT) due to missing information or non-matches, their current SNAP participation is coded as “no match” in SNAPNOWADMIN.
therefore the variable is missing) if the household does not either report SNAP participation or a transaction during the survey week using an EBT card. While the majority of such individuals are likely true non-participants, some could be true participants whom both denied participation in the program and also did not disclose that certain purchases during the survey week were made with an EBT card. Given the high prevalence of false negatives reported in the literature, the fraction of such individuals could be non-trivial.

These issues create substantial ambiguity about the “correct” ways to code the administrative variables that we will explore in more detail in the following section. For now, we define the baseline versions of these two measures as follows. We set ADMIN=1 if there is a successful match to caseload records, even if the date of the match is outside of the previous month or missing. The rationale for the flexible timing mirrors that discussed above for the self-reported measure. We set ADMIN=0 for individuals who did not match to the caseload records, and leave the variable missing for those in states that did not provide caseload records. For ALERT, we assign a value of 1 if there is a confirmed match (again, regardless of whether the match occurs during the survey month) and 0 if a match was attempted but unsuccessful. If no match was attempted because either there were no EBT-type payments or no SNAP-authorized store acquisitions for households not reporting EBT-type payments, we set ALERT to missing.

Our first two dependent variables relate to food insecurity. These come from the ten-question household food security questionnaire included in FoodAPS based on USDA’s 30-day Food Security Scale. The specific outcomes are a dummy for whether the household has low food security (defined as having affirmative responses to three to five questions) and a dummy for whether the household has very low food security (six or more affirmative responses).

Please see Table 4.10 for the list of question on the ten-question household food security question.
The next several dependent variables relate to body weight. The FoodAPS contains self-reported height and weight for the household responder. We use this information to create three outcomes: body mass index (BMI) and indicators for obese (BMI ≥ 30) and severely obese (BMI ≥ 35).\footnote{Body mass index is defined as weight in kilograms divided by height in square meters.} Dichotomous variables are often used in addition to continuous BMI in the obesity literature since health is not monotonically decreasing in weight. Weight gain generally improves health at low levels of BMI, and the large increase in mortality risk from excess weight does not begin until around the severe obesity threshold Courtemanche, Pinkston, Ruhm & Wehby (2016). The health implications of any impacts of SNAP would depend on the portion of the BMI distribution in which the effects are strongest (i.e., the health implications of SNAP’s effects would potentially be more substantial if they are stronger on severe obesity).

The final dependent variable relates to food purchases. Following prior studies such as Volpe, Okrent & Leibtag (2013), we use a summary measure of the healthfulness of food purchases called the Healthy Eating Index (HEI). The HEI-2010, designed by the USDA, aims to capture the degree of adherence to dietary guidelines. We use the total HEI-2010 scores for all items for all the entire survey week for each household.\footnote{Further information on HEI scores can be found at http://epi.grants.cancer.gov/hei.} The HEI score is made up of 12 components which sum up to a maximum score of 100. This HEI variable is computed by FoodAPS staff and available as a linkable auxiliary dataset.

The FoodAPS also contains a number of variables that we use as controls. These include dummy variables for gender, educational attainment (dummy variables for having less than high school diploma, high school diploma but no college education, and some college education, with college degree or higher being the omitted base category), race/ethnicity (non-Hispanic black and non-Hispanic white, with other being the base category), marital status (married and formerly married, with never...
married as the base category), whether any individuals under 5 years old or at least 65 years old are present in the household, whether the respondent worked last week, whether the household lives in rural census tract, and whether the household’s primary food store is SNAP-authorized. Continuous controls include respondent’s age, household size, number of children, household monthly gross total income, and straight-line distance from household’s residence to its primary food store (in miles).

Our final sample is subject to four restrictions. First, we include only households in which the primary respondent is at least 18 years old. Second, we drop households with missing values of any variables. Next, we follow Mykerezi & Mills (2010) and Almada et al. (2016) and drop those with incomes over 250% FPL. The final step is to exclude 122 households who did not provide consent for administrative verification. The resulting sample contains 2,108 households. The sample sizes in some of the sensitivity checks will vary, though, as we will experiment with different ways to handle ambiguous cases in the administrative SNAP variables.

Table 1 presents means and standard deviations for our main sample. The SNAP participation rate is 32 percent using the self-report compared to 29 percent with ADMIN and 30 percent with ALERT. The correlations between the three measures are 0.782 for REPORT and ADMIN, 0.792 for REPORT and ALERT, and 0.847 for ADMIN and ALERT. In other words, the two administrative measures exhibit almost as much disagreement with each other as either of them do with the self-reported measure. FoodAPS’s primary respondents have an average BMI of 28.81, while 38 percent are obese and 16 percent are severely obese. About 20 percent of FoodAPS households are food insecure (low food security) while 13 percent experience very low food security. In terms of compliance with the U.S. Dietary Guidelines for Americans, FoodAPS households have an average HEI score of 50.56 out of a maximum score of 100; higher HEI scores indicate greater conformity with recommended dietary guidelines.
The primary respondents are on average, about 49 years old with a household size of about 2.56. Also, almost 71 percent of the primary respondents are female, 31 percent are married, 16 percent are black, 71 percent are white, and about 38 percent report having worked last week. FoodAPS primary respondents have a gross monthly income of about $1,860 and live in households with 26 percent holding college degrees or higher, 21 percent with some college education, 34 percent with a high school diploma, and 19 percent with less than high school diploma. Finally, 33 percent of FoodAPS household live in a rural census tract, 61 percent have children at most five years of age, and 28 percent have elderly at least 65 years present.

4.3 Sensitivity of Participation and Misreporting Rates

This section examines the sensitivity of SNAP participation and misreporting rates along two dimensions. The first type of sensitivity concerns different classification choices for the potentially ambiguous cases when continuing to use ADMIN and ALERT separately. The second is with respect to different approaches to combining ADMIN and ALERT into a single, “true” participation measure.

Different Classification Choices for ADMIN and ALERT Separately

The discussion of the SNAP variables in Section II revealed several challenges when coding ADMIN and ALERT. Tables 2 and 3 categorize the potential values of these variables to elucidate the specific sources of ambiguity. The tables also report the number of households in each category, how they are classified in the “baseline” classification used in Section II, and other reasonable ways in which they could be classified. The latter is given in the column names “Alternate 1” through “Alternate 3,” wherein each column the specific categorization that differs from the baseline choice is in bold.

Focusing first on the ADMIN variable in Table 2, we see that there are five
different broad categories a household can fall into. First is the straightforward case where the state did not make caseload records available, and therefore the ADMIN variable is clearly missing. The second category contains the most definitive non-participants: households who did not match to caseload records. Conceivably, someone could be a true participant but not match due to, for instance, name misspellings, or changes in household identifying information such as addresses and phone numbers. We expect such cases to be infrequent enough that exploring an alternate classification is not warranted, especially considering that some matches to the caseload data not deemed to be automatically “certain” were manually reviewed to address such concerns. Category 3 consists of the clearest participants: those who matched to caseload records within the 32 days before and including the survey week.

The final two categories are the most ambiguous. Category 4 contains households that matched to caseload records but with a date outside the 32-day window. As discussed in Section II, the intention of our baseline classifications is to measure either current or recent participation, in which case the most natural classification of these households is as participators. Moreover, a sizeable number of households show matches in both the months immediately before and after the survey month, but not in the survey month. In these cases, the lack of a match in the survey month is likely an error, and a determination of “current participant” seems reasonable. Nonetheless, the lack of an exact match on timing creates sufficient ambiguity to warrant sensitivity analyses. Alternate Classification 1, therefore, considers households in Category 4 to be non-participants, while Alternate Classification 2 treats them as missing. Category 5 consists of those who matched to caseload records, but the dates of SNAP receipt are not available. Again, since our goal with the baseline classifications is to capture current or recent participation, the lack of an exact date is not especially problematic, so we consider these households
to be participators. However, if the objective was to measure current participation, strictly speaking, the lack of an exact date would prevent any determination from being made, so Alternate Classification 3 treats these households as missing.

Turning to the ALERT variable in Table 3, households can fall into four different categories. Category 1 contains those for whom no match was attempted. This could happen for three reasons. First, no match was attempted if the respondent did not self-report SNAP receipt or any EBT-type payments. While most such individuals are likely true non-participants, this might not be the case for all of them. We already know that some people falsely report not receiving SNAP, and it seems plausible that some of these same people would also not voluntarily disclose using an EBT card for any of their purchases. A match to EBT records was also not attempted if the individual reported SNAP participation but did not make a purchase at a SNAP-eligible store during the survey week. While some of these individuals may be genuine false positives, others might have simply not gone to the grocery store that week. Category 1 households could also be those who did not match to the ADMIN data to provide a CASEID that would permit a deterministic match to the EBT ALERT database. Given the substantial ambiguity surrounding Category 1 households, we code them as missing in our baseline classification but treat them as non-participants in Alternative Classification 3.

ALERT Categories 2 and 3 parallel those same numbered categories from the ADMIN data. Category 2 contains those for whom a match was attempted but not successful, indicating non-participation. Category 3 indicates a match to the EBT ALERT database was successful with date of last receipt within the 36-day window. Since the ALERT matches were probabilistic based on STOREID, amount, and date, it is conceivable that some of the Category 2 households may have failed to match due to reasons such as mistakes in the reported amounts and dates. Analogously, some of the Category 3 households may have been determined
manually when a single FoodAPS transaction matched to multiple ALERT transactions. Thus, the final account number assigned to the FoodAPS transaction may result in an erroneous Category 3 determination. However, in our judgment such misclassification is unlikely to happen in more than a few cases, so we do not consider alternative classifications of ALERT Categories 2 and 3.

ALERT Category 4 households are similar to ADMIN Category 4 above; they matched to the EBT ALERT database, but the associated date of the last receipt is outside of the 36-day window. For similar reasons as mentioned above, we initially consider these households to be true participants, but Alternate Classification 1 considers them to be non-participants while Alternate Classification 2 treats them as missing.

Table 4 presents estimated participation and error rates for the various ADMIN and ALERT classification choices discussed above. Panel A uses the baseline classifications and the main sample that drops observations with missing values of either ADMIN or ALERT (under their baseline classifications) or any of the control variables. This enables an “apples-to-apples” comparison of the differences caused by ADMIN versus ALERT within the same sample. Panel B allows the sample size to vary depending on the treatment of missing data. The row labeled “ADMIN Baseline” in Panel B adds back in the observations with a valid value of that variable but missing “ALERT baseline,” and vice versa. The rows for the alternate classifications can either contain more or fewer observations depending on the relative stringency of the criteria for handling ambiguous cases. For instance, the sample is much larger for “ALERT Alternate 3” than “ALERT Baseline” because the former treats the large number of households for whom no match was attempted (Category 1) as non-participants, whereas the latter considers them missing.

Panel A shows that the participation and misreporting rates in the main sample are broadly similar using the baseline constructions of ADMIN and ALERT. The
estimated SNAP participation rate is 29% using ADMIN compared to 30% using ALERT. The false negative rates are 11.65 percent using ADMIN and 11.46 percent using ALERT, while the false positive rates are 8.39 percent using ADMIN and 7.83 percent using ALERT. Interestingly, for both participation measures, the prevalence of false negatives is substantially lower than previously reported by studies using more traditional survey datasets (Mittag 2013, Meyer & Goerge 2011). One possible explanation is that FoodAPS households were asked to consent to having their responses verified. Even though all but 122 households gave consent, it is reasonable to presume that merely informing respondents about data verification and asking for consent may elicit more truthful responses and partly account for the lower estimated false negatives. Additionally, estimated false positives in the FoodAPS are much higher than typically found. Conceivably, individuals who were unsure whether or not their household received SNAP might have been more inclined to report affirmatively because of the looming verification.

Panel B documents considerable variation in participation and misreporting rates depending on the particular classification decisions for ADMIN and ALERT. The estimated participation rates for ADMIN vary from 23.75 percent (Alternate 1) to 28.59 percent for our baseline ADMIN classification choice, for a spread of 4.84 percentage points, or 20 percent of the lower end of the range. The ALERT classification choices lead to even more variability, ranging from about 24.14 percent (Alternate 3) to 33.51 percent for our baseline ALERT classification, for a spread of 9.37 percentage points, or 39 percent. The sensitivity in false negative rates is even more striking. For ADMIN, the estimated false negative rates vary from 6.83 percent (Alternate 2) to 13.23 percent (Alternate 3), meaning that judgment calls about classifications could potentially cause this rate to vary by up to 94%. The false negative rates using ALERT range from 6.55 percent (Alternate 2) to 10.89 percent (Alternate 1), for a spread of 66%. The false positive rate for ADMIN is less
sensitive, ranging from a low of 6.75 percent (Alternative 1) to 8.08 percent in each of the other three cases, for a difference of 20 percent. For ALERT, the estimated false positives range from 7.04 percent (Alternative 1) to 12.17 percent (Alternative 3), for a more substantial spread of 73%. Despite the considerable variation in these estimates, the finding that the false negative rate is notably lower in the FoodAPS than other surveys while the false positive rate is higher is nonetheless robust to all classifications.

Different Classification Choices for Combining ADMIN and ALERT

This section introduces several approaches or ad hoc rules to consolidate the two administrative participation measures into a single “true” participation variable and then evaluates how these rules influence the estimated rates of SNAP participation and misreporting. For the rest of this section, ADMIN and ALERT refer to the baseline classification choices as described in Tables 2 and 3, respectively. We develop five decision rules to combine the administrative participation variables as follows:

1. Always use ADMIN unless missing. For households missing ADMIN data, their participation status is set to ALERT.

2. Always use ALERT unless missing. For households missing ALERT data, their participation status is set to ADMIN.

3. Drop if Disagreement: This rule sets the “true” participation variable to equal to both ADMIN and ALERT, only if they agree (i.e., if $\text{ADMIN} = \text{ALERT} = i$, $i=0, 1$). When they disagree or if either of them is missing, the “true” variable is set to missing. This conservative approach will minimize errant classification but at a substantial cost to sample size.

4. More weight to matches: This rule is similar to (3) as it uses both ADMIN
and ALERT if they agree. However, when they disagree, we set the “true”
status to participation (“1”), unless either is missing in which case the “true”
status is set to the value of the non-missing variable. In other words, this rule
treats households as “true” participants if at least ADMIN or ALERT confirms
participation. Otherwise, the household is considered a non-participant unless
both are missing.

5. More weight to non-matches: This rule is the reverse of (4). When ADMIN
and ALERT disagree, we set the “true” status to non-participation (“0”),
unless either is missing in which case the “true” status is set to the value of the
non-missing variable. In other words, this rule treats households as “true”
non-participants if at least ADMIN or ALERT confirms non-participation.
Otherwise, the household is considered a participant unless both are missing.

Table 5 presents estimates of participation, false negative, and false positive
rates under each of the above decision rules. The estimated participation rates
range from 28.25 percent (Rule 3) to 34.81 percent (Rule 5). This is a spread of
6.56, which represents 23% of the low end of the range. The estimated false negative
rates range from 10.71 percent (Rule 5) to 12.28 percent (Rule 1), for a spread of
1.57 percentage points, or 15%. The false positive rates vary quite substantially,
from 4.53 (Rule 3) to 11.41 percent (Rule 5), for a spread of over 150%. While our
decision rules are by no means exhaustive, this exercise illustrates that how one
chooses to resolve the ambiguity in the administrative variables has nontrivial
consequences for estimated participation and reporting errors.

Some patterns also emerge. First, as expected, giving the benefit of the doubt to
matches (Rule 4) leads to a relatively high estimated participation rate, and keeps
the rate of false positives low but at the expense of a high rate of false negatives.
The reverse is true when we give the benefit of the doubt to non-matches (Rule 5).
Perhaps more surprising is that dropping cases where there is any ambiguity
(ADMIN and ALERT disagree or either are missing; Rule 3) results in the lowest estimated participation rate, lowest rate of false positives, and second-lowest rate of false negatives. In other words, once we restrict the sample to households for whom the administrative measures are likely quite accurate, we see less disagreement with self-reported participation. There is a particularly large reduction in the number of cases in which the respondent reports participation but the administrative data disagree. This implies that some of the estimated misreporting observed under other decision rules is not actually misreporting at all, but instead reflective of flaws in the administrative variables. It is also noteworthy that the sample shrinks so much - 2,446 to 1,898, or 29% - under Rule 3, underscoring that the amount of ambiguity, and therefore scope for error, in the administrative measures is substantial.

**Preferred Approach to combining REPORT, ADMIN, and ALERT**

Given the ambiguity and sensitivity documented above, it is reasonable to ask whether linked administrative data can still be used to obtain insights beyond what could be done with self-reported information alone. The conservative Rule 3 should lead to a very accurate participation measure but at the cost of discarding nearly a third of the sample, which creates concerns about endogenous sample selection and external validity. The other decision rules avoid such a large reduction in same size but at the expense of accuracy. The goal of this section is to implement a more detailed strategy for combining ADMIN and ALERT that utilizes self-reports to help resolve ambiguous cases, with the goal of leveraging insights from all three measures to produce reliable estimates while preserving sample size.

To motivate this approach, Table 6 presents information about the extent of disagreement among the three measures as well as the extent of missing data in each variable. Also, the last column reports how we classify disagreements into various categories for the purpose of developing our SNAP new participation variable, which
we refer to as our “preferred” measure. There is about 63 percent agreement among all three measures (i.e., all three variables either indicate participation or non-participation), which we label as Category A. The rest of the households with non-missing self-reported data have different types of disagreement. In Category B, making up about 10 percent of households, any two of the three measures agree while the third is missing. Category C respondents, which account for 5 percent, have both administrative measures agreeing but in conflict with the self-report. Households with only the self-reported participation variable who are missing both administrative measures (Category D) make up 12 percent, while the remaining 10 percent of respondents are lumped into miscellaneous types of disagreement in Category E.

The new, “preferred” measure of SNAP participation combines information from Categories A, B, and C and sets to missing observations in Categories D and E. For Category A, all three variables are in agreement, so we are comfortable setting the “true” participation variable equal to the associated value. For Category B, we are also comfortable making a determination since, although one variable is missing, the other two agree. For Category C, we consider the self-reported participation value to be erroneous since it opposes both administrative measures, and there is no particular reason to expect errors in the administrative variables to be correlated with each other. This maintains the preference for administrative records if the information from those records appears to be reliable. Next, those in Category D have non-missing self-reported participation but are missing both administrative measures. We code their participation as missing given the established concerns in the literature about relying only on self-reports. Finally, we also set the participation status of respondents in Category E to missing. There are three types of Category E households: ADMIN and ALERT are non-missing but disagree, ADMIN and REPORT disagree while ALERT is missing, and ALERT and
REPORT disagree with ADMIN missing. In such cases of explicit disagreement, a
determination cannot be reached without establishing a rank ordering among the
measures.

Ultimately, our preferred measure is non-missing for the entire main sample of
2,108 respondents. Relative to the sample sizes using the various decision rules in
Table 5, this is less than the 2,446 observations obtained using decision rules that
force an outcome even in ambiguous cases, but significantly larger than the 1,898
observations obtained under the conservative Rule 3. The estimated participation
rate using the preferred measure is 30.92 percent, which is slightly higher than those
obtained using ADMIN and ALERT separately (Panel A in Table 4) but well within
the ranges established by the various sensitivity checks in Tables 4 and 5.\textsuperscript{13}

4.4 Econometric Analyses and Results

We next turn to our regression estimates of the associations of SNAP with food
insecurity, weight outcomes, and dietary healthfulness. This section’s goal is to
illustrate the sensitivity of these estimates to the assumptions, introduced in the
previous section, about how to code ADMIN and ALERT separately as well as how
to assign “true” participation in cases of disagreement between them. We do not
attempt to address the endogeneity of participation because doing so with a single
cross-section of data such as the FoodAPS is daunting, and our focus here is to
examine measurement issues rather than identify causal effects. Negative selection
into SNAP is well-documented in the literature, so our OLS estimates will likely be
biased toward unfavorable outcomes (greater food insecurity, higher BMI and
obesity rates, and less healthy diets), even aside from measurement issues.

Our regressions take the form

\textsuperscript{13}The preferred SNAP participation measure leads to relatively low estimated rates of false neg-
atives (8.53 percent) and false positives (3.99 percent), but this is by construction since the self-
reported value is factored into the coding process.
\[ y_{is} = \beta_0 + \beta_1 SNAP_{is} + \beta_2 X_{is} + \varepsilon_{is}, \]  

(4.1)

where \( y_{is} \) is the outcome variable for individual/household \( i \) (separate regressions for each of the outcomes discussed in Section 4.2), \( SNAP_{is} \) is an indicator of SNAP participation (separate regressions for each decision rule from Section 4.3), is a vector of the control variables from Section 4.2, and \( \varepsilon_{is} \) is the error term.

Measurement error in a binary variable is necessarily non-classical, so one cannot simply assume to be biased toward zero (Kreider 2010, Kreider et al. 2012, Nguimkeu et al. 2017). Measurement error in SNAP participation could potentially even lead the OLS estimator to be wrongly signed. It might be reasonable to suspect that some of the inconsistencies among the administrative measures, such as the inability to match names with sufficient certainty, are as good as random. However, other inconsistencies, such as appearing in the caseload records but not using an EBT card in the past 30 days, arise from personal choices and may, therefore, be correlated with the error term, hence leading to endogenous misclassification.

We begin our presentation of the regression results with Table 7, which uses the main sample and compares OLS estimates (linear probability model if the outcome is binary) using REPORT, the baseline version of ADMIN (as described in Table 2), and the baseline version of ALERT (as described in Table 3). Similar to Panel A of Table 4, the purpose here is to use a common sample to provide an apples-to-apples comparison of the results across the three measures. The first key result is that the results are qualitatively similar regardless of the SNAP participation measure used. As expected, SNAP participation is consistently associated with worse values of all six outcomes. Estimates for food insecurity and body mass index are significant at the 1% level for all three SNAP measures, while those for very low food security are never significant. Mild discrepancies are observed for HEI and obesity, as two of the estimates are significant at the 1% level while the third (using ALERT for HEI,
ADMIN for obesity) is significant at the 5% level. For severe obesity, the estimates for REPORT and ADMIN are significant at the 5% level and 1% level, respectively, while the estimate for ALERT is insignificant.

The magnitudes, however, are more sensitive to the choice of SNAP measure. The associations between SNAP and food insecurity range from 6 to 7 percentage points, for a 16.67 percent spread. The estimates for very low food security vary between 2 and 2.7 percentage points, or 35 percent. SNAP reduces HEI by between 1.3 and 2 units, for a sizeable 54 percent difference. The results for BMI are less sensitive, as they only vary from 1.05 to 1.17 units, or 11%. Greater sensitivity is observed for the dichotomous weight outcomes. The estimates for Pr(Obese) and Pr(Severely Obese) range from 5.7 to 7.9 and 2.1 to 3.9 percentage points, respectively, for spreads of 39% and 86%. Note also that the pattern of results is inconsistent with simple attenuation bias, in which case we would expect the magnitudes to be larger using the administrative SNAP measures than the self-report. For three of the outcomes the magnitudes are actually largest using self-reported participation, and in only one case is the magnitude using self-reported participation the smallest. This is consistent with the reporting error being non-classical (which can yield an expansion bias), but is also consistent with the administrative measures not being any more reliable than the self-report (i.e. there is some attenuation bias regardless of the measure used).

Table 8 presents similar OLS results using the self-reported participation and the eight classification choices described in Tables 2 and 3. The first row reports the estimates using the self-reported participation variable. The next eight rows use the different classification rules for coding ADMIN and ALERT separately. As in Table 7, for all outcomes the signs are robust across SNAP measures. However, there are some noteworthy differences in terms of significance levels and magnitudes. For instance, the association between self-reported SNAP participation and very low
food security is a sizeable and statistically significant 4 percentage points. In contrast, the same association is never significant using any classification scheme for the administrative measures, and the magnitudes are much smaller: 0.2 to 2.3 percentage points. Recall from Table 7 that using the self-report also led to an insignificant result for very low food security for the common sample, meaning that much of the sensitivity observed here is actually from the difference in sample (i.e. adding back in 27 to 680 observations with non-missing self-reports but a missing value of one or both administrative measures depending on the administrative classification). This underscores the external validity concerns raised by the large amounts of missing data for the administrative variables.

The results for HEI and severe obesity are also quite sensitive. For HEI, the estimates using REPORT and ADMIN are large (-1.42 to -1.66 units) and significant, but they shrink considerably (-0.72 to -1.2) using ALERT and are only significant in two of the four cases. Accordingly, the spread between the smallest and largest magnitude for HEI is over 130%. For severe obesity, the estimates range from 4.5 to 5.2 percentage points using REPORT and ADMIN, but shrink to 1.2 to 2.3 percentage points using ALERT and are always insignificant. The spread for severe obesity is therefore an enormous 333%.

Finally, Table 9 presents regression results using the five decision rules as well as our preferred consolidation rule discussed in Section 4.3. Additionally, we consider a version of our preferred measure that imputes the missing values from Categories D and E. We perform multiple imputations under the assumption that missing data is correlated with observables but conditionally independent of unobservables, usually referred to as a “Missing at Random (MAR)” assumption. We implement the multiple imputation procedures using Stata’s mi impute and mi estimate commands, with 50 multiply imputed samples.

The first five rows show the results using the ad hoc decision rules, while the last
two rows use our preferred measure both with and without imputation. Again, the signs are robust to the different SNAP measures, but there are important differences in significance levels and magnitudes. For instance, the association between SNAP and very low food security is significant and large (3.4 percentage points) using Rule 3 but insignificant in the other cases with a magnitude as small as 0.9. The difference between the largest and smallest estimates is therefore 280%. The estimate for HEI is usually significant and reaches as large as -1.69 units, but it is an insignificant -0.9 units under Rule 1, for a spread of 88%. For severe obesity, significance levels are again mixed, with the estimates ranging from 2.1 to 4.7 percentage points (spread of 124%).

Using the preferred measure, the results are very similar both with and without imputation. SNAP is predicted to increase the probabilities of being food insecure, having very low food security, being obese, and being severely obese by 6.7, 2.7, 7.2, and 4.5 percentage points, respectively. SNAP also increases BMI by 1.45 units and reduces HEI by 1.4 units. SNAP is significant at the 5% level or better for all outcomes except very low food security.

Summarizing, we find that the classification choices one makes with FoodAPS’s three participation measures (REPORT, ADMIN, and ALERT) have important consequences not only for estimated participation and reporting error rates but also for the estimated associations between SNAP and food security, diet healthfulness, and weight outcomes. However, the different classification choices do not seem to matter for the signs of the estimated associations since they line up with our expectations if we suppose that participants are negatively selected.

4.5 Conclusion

This paper leverages the availability of self-reported and two different administrative measures of SNAP participation in the FoodAPS to investigate
several issues related to SNAP and measurement error. We first present evidence that the two administrative SNAP variables suffer from considerable ambiguity and disagree with each other almost as much as they disagree with self-reported participation. We then demonstrate that different methods of coding the two administrative variables separately as well as various approaches to combining their resulting preferred versions into a single “true” participation measure can lead to meaningfully different estimated participation and misreporting rates. Next, we document similar sensitivity to assumptions about the administrative variables across ordinary least squares estimates of the associations of SNAP with food insecurity, body weight, and healthfulness of food purchases.

Our work serves as a cautionary tale for using administrative records uncritically under the assumption that they represent the “gold standard” with regard to measurement. While some of the difficulties we observed with the linked administrative variables may be unique to FoodAPS, others likely generalize to other settings. For instance, challenges with obtaining data from all states and differences in data quality across states are hardly unique to SNAP caseload files, as many programs (such as Medicaid and public schools) are operated at the state or local levels and standards for data collection may differ across different geographic areas. Additionally, probabilistic matching between survey respondents and verified program participants would be necessary for other contexts as well since it is unlikely that both sources include universal identifiers such as social security numbers. The fact that matches to EBT transaction data were not attempted for individuals who (perhaps erroneously) reported not participating in SNAP points to the broader tradeoff between rigor and budgetary/practical constraints during data collection. When faced with a choice between nationwide surveys and administrative records that are only available for certain areas and potentially flawed for others, it is not obvious that the administrative data are preferable.
With all that said, we do not stop at pointing out the flaws in administrative data. Instead, we propose a strategy to construct a single, “true” participation variable based on all available information from both administrative and self-reported measures. This allows us to obtain “preferred” results, both for participation rates and regression estimates. Similar strategies could potentially be utilized in other contexts as well.

Nonetheless, our study suffers from several limitations that should be addressed in future work. For instance, while we propose a method that intuitively should minimize measurement error, there is no way to directly test whether it indeed accomplishes that objective or whether other strategies could be superior. Additionally, we purposefully do not address endogenous SNAP participation because of inherent difficulties in pursuing standard IV methods with a single cross-section data with a relatively small sample size. Much is therefore left to be learned about both the impacts of SNAP and best practices for measurement when multiple flawed indicators of program participation are available.
4.6 Figures and Tables

Table 4.1: Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean (Standard Error)</th>
</tr>
</thead>
<tbody>
<tr>
<td>SNAP Participation</td>
<td></td>
</tr>
<tr>
<td>Self-Reported (REPORT)</td>
<td>0.32 (0.02)</td>
</tr>
<tr>
<td>Administrative from Caseload Data (ADMIN)</td>
<td>0.29 (0.02)</td>
</tr>
<tr>
<td>Administrative from EBT Transactions (ALERT)</td>
<td>0.30 (0.02)</td>
</tr>
<tr>
<td>Dependent Variables</td>
<td></td>
</tr>
<tr>
<td>Low Food Security</td>
<td>0.20 (0.02)</td>
</tr>
<tr>
<td>Very Low Food Security</td>
<td>0.13 (0.01)</td>
</tr>
<tr>
<td>Total 2010 HEI Score</td>
<td>50.56 (0.58)</td>
</tr>
<tr>
<td>Body Mass Index</td>
<td>28.81 (0.25)</td>
</tr>
<tr>
<td>Obese</td>
<td>0.38 (0.02)</td>
</tr>
<tr>
<td>Severely Obese</td>
<td>0.16 (0.01)</td>
</tr>
<tr>
<td>Control Variables</td>
<td></td>
</tr>
<tr>
<td>Age (years)</td>
<td>49.62 (0.98)</td>
</tr>
<tr>
<td>Female</td>
<td>0.71 (0.02)</td>
</tr>
<tr>
<td>Black</td>
<td>0.16 (0.03)</td>
</tr>
<tr>
<td>White</td>
<td>0.71 (0.04)</td>
</tr>
<tr>
<td>Other race (non-black, non-white)</td>
<td>0.13 (0.02)</td>
</tr>
<tr>
<td>Married</td>
<td>0.31 (0.02)</td>
</tr>
<tr>
<td>Formerly Married</td>
<td>0.43 (0.02)</td>
</tr>
<tr>
<td>Household Size</td>
<td>2.56 (0.10)</td>
</tr>
<tr>
<td>Number of children</td>
<td>0.93 (0.07)</td>
</tr>
<tr>
<td>Rural Tract</td>
<td>0.33 (0.06)</td>
</tr>
<tr>
<td>Less than High School Education</td>
<td>0.19 (0.02)</td>
</tr>
<tr>
<td>High School Graduate</td>
<td>0.34 (0.02)</td>
</tr>
<tr>
<td>Some College Education</td>
<td>0.21 (0.01)</td>
</tr>
<tr>
<td>College Degree or Higher</td>
<td>0.26 (0.02)</td>
</tr>
<tr>
<td>Worked Last Week</td>
<td>0.38 (0.03)</td>
</tr>
<tr>
<td>Gross Monthly Family Income (Thousand Dollars)</td>
<td>1.86 (0.06)</td>
</tr>
<tr>
<td>Child Less than 5 years present in HH</td>
<td>0.61 (0.02)</td>
</tr>
<tr>
<td>Elderly at least 65 years present in HH</td>
<td>0.28 (0.03)</td>
</tr>
<tr>
<td>Never Married</td>
<td>0.26 (0.02)</td>
</tr>
<tr>
<td>Straight-line Distance from residence to primary food store (miles)</td>
<td>3.15 (0.34)</td>
</tr>
<tr>
<td>Primary food store is SNAP-authorized</td>
<td>0.98 (0.00)</td>
</tr>
</tbody>
</table>

Statistics are from main analysis sample of 2108 observations. Observations are weighted to account for the complex sampling design of FoodAPS.
Table 4.2: Possible Classifications for Administrative Participation Measure from Caseload Data (ADMIN)

<table>
<thead>
<tr>
<th>Category</th>
<th>Description</th>
<th>N</th>
<th>Baseline Classification</th>
<th>Alternate 1</th>
<th>Alternate 2</th>
<th>Alternate 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>ADMIN data not available from state</td>
<td>448</td>
<td>Missing</td>
<td>Missing</td>
<td>Missing</td>
<td>Missing</td>
</tr>
<tr>
<td>2</td>
<td>Match to caseload data attempted but did not meet threshold for certainty</td>
<td>1268</td>
<td>Non-participant</td>
<td>Non-participant</td>
<td>Non-participant</td>
<td>Non-participant</td>
</tr>
<tr>
<td>3</td>
<td>Match confirms participation within 32 days of the survey week</td>
<td>763</td>
<td>Participant</td>
<td>Participant</td>
<td>Participant</td>
<td>Participant</td>
</tr>
<tr>
<td>4</td>
<td>Match confirms participation more than 32 days before the survey week or after the survey week</td>
<td>134</td>
<td>Participant</td>
<td>Participant</td>
<td>Non-participant</td>
<td>Missing</td>
</tr>
<tr>
<td>5</td>
<td>Match confirms participation but dates not available</td>
<td>175</td>
<td>Participant</td>
<td>Participant</td>
<td>Participant</td>
<td>Missing</td>
</tr>
</tbody>
</table>

Based on the main sample augmented with observations with missing ADMIN or ALERT but not any other variable.
<table>
<thead>
<tr>
<th>Category</th>
<th>Description</th>
<th>N</th>
<th>Baseline Classification</th>
<th>Alternate 1</th>
<th>Alternate 2</th>
<th>Alternate 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>No acquisitions available for matching and no match to ADMIN to provide CASEID</td>
<td>574</td>
<td>Missing</td>
<td>Missing</td>
<td>Missing</td>
<td>Non-participant</td>
</tr>
<tr>
<td>2</td>
<td>Match to ALERT data attempted but did not meet threshold for certainty</td>
<td>1174</td>
<td>Non-participant</td>
<td>Non-participant</td>
<td>Non-participant</td>
<td>Non-participant</td>
</tr>
<tr>
<td>3</td>
<td>Match confirms participation within 36 days of the survey week</td>
<td>961</td>
<td>Participant</td>
<td>Participant</td>
<td>Participant</td>
<td>Participant</td>
</tr>
<tr>
<td>4</td>
<td>Match confirms participation more than 32 days before the survey week or after the survey week</td>
<td>79</td>
<td>Participant</td>
<td>Non-participant</td>
<td>Missing</td>
<td>Participant</td>
</tr>
</tbody>
</table>

Based on the main sample augmented with observations with missing ADMIN or ALERT but not any other variable.
Table 4.4: Estimated Participation and Misreporting Rates under Different Approaches to Using ADMIN and ALERT Separately

<table>
<thead>
<tr>
<th>Determination of final ADMIN and ALERT status</th>
<th>Sample Size</th>
<th>Participation Rate (%)</th>
<th>False Negative Rate (%)</th>
<th>False Positive Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Main Sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ADMIN Baseline</td>
<td>2108</td>
<td>29.00</td>
<td>11.65</td>
<td>8.39</td>
</tr>
<tr>
<td>ALERT Baseline</td>
<td>2108</td>
<td>30.00</td>
<td>11.46</td>
<td>7.83</td>
</tr>
<tr>
<td>Panel B: Varying Samples</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ADMIN Baseline</td>
<td>2340</td>
<td>28.59</td>
<td>12.28</td>
<td>8.08</td>
</tr>
<tr>
<td>ADMIN Alternate 1</td>
<td>2340</td>
<td>23.75</td>
<td>11.70</td>
<td>6.75</td>
</tr>
<tr>
<td>ADMIN Alternate 2</td>
<td>2206</td>
<td>24.96</td>
<td>6.83</td>
<td>8.08</td>
</tr>
<tr>
<td>ADMIN Alternate 3</td>
<td>2165</td>
<td>25.55</td>
<td>13.23</td>
<td>8.08</td>
</tr>
<tr>
<td>ALERT Baseline</td>
<td>2214</td>
<td>33.51</td>
<td>10.49</td>
<td>8.64</td>
</tr>
<tr>
<td>ALERT Alternate 1</td>
<td>2214</td>
<td>29.73</td>
<td>10.89</td>
<td>7.04</td>
</tr>
<tr>
<td>ALERT Alternate 2</td>
<td>2135</td>
<td>30.89</td>
<td>6.55</td>
<td>8.64</td>
</tr>
<tr>
<td>ALERT Alternate 3</td>
<td>2788</td>
<td>24.14</td>
<td>10.49</td>
<td>12.17</td>
</tr>
</tbody>
</table>

Observations are weighted to account for the complex sampling design of FoodAPS.

Table 4.5: Estimated Participation and Misreporting Rates under Different Approaches to Combining ADMIN and ALERT

<table>
<thead>
<tr>
<th>Decision Rule when ADMIN and ALERT Differ</th>
<th>Sample Size</th>
<th>Participation Rate (%)</th>
<th>False Negative Rate (%)</th>
<th>False Positive Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rule 1: Always use ADMIN unless missing</td>
<td>2446</td>
<td>31.95</td>
<td>11.25</td>
<td>8.80</td>
</tr>
<tr>
<td>Rule 2: Always use ALERT unless missing</td>
<td>2446</td>
<td>32.30</td>
<td>11.10</td>
<td>8.31</td>
</tr>
<tr>
<td>Rule 3: Drop if disagreement</td>
<td>1898</td>
<td>28.25</td>
<td>10.98</td>
<td>4.53</td>
</tr>
<tr>
<td>Rule 4: More weight to matches</td>
<td>2446</td>
<td>34.81</td>
<td>11.57</td>
<td>5.46</td>
</tr>
<tr>
<td>Rule 5: More weight to non-matches</td>
<td>2446</td>
<td>29.44</td>
<td>10.71</td>
<td>11.41</td>
</tr>
</tbody>
</table>

Observations are weighted to account for the complex sampling design of FoodAPS.
### Table 4.6: Extent of Disagreement among SNAP Participation Variables

<table>
<thead>
<tr>
<th>REPORT</th>
<th>ADMIN</th>
<th>ALERT</th>
<th>Observations</th>
<th>Category</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>0</td>
<td>0</td>
<td>952</td>
<td>A</td>
</tr>
<tr>
<td>0</td>
<td>0</td>
<td>1</td>
<td>11</td>
<td>E</td>
</tr>
<tr>
<td>0</td>
<td>0</td>
<td>.</td>
<td>144</td>
<td>B</td>
</tr>
<tr>
<td>0</td>
<td>1</td>
<td>0</td>
<td>21</td>
<td>E</td>
</tr>
<tr>
<td>0</td>
<td>1</td>
<td>1</td>
<td>77</td>
<td>C</td>
</tr>
<tr>
<td>0</td>
<td>1</td>
<td>.</td>
<td>12</td>
<td>E</td>
</tr>
<tr>
<td>0</td>
<td>.</td>
<td>0</td>
<td>1</td>
<td>B</td>
</tr>
<tr>
<td>0</td>
<td>.</td>
<td>1</td>
<td>9</td>
<td>E</td>
</tr>
<tr>
<td>0</td>
<td>.</td>
<td>.</td>
<td>261</td>
<td>D</td>
</tr>
<tr>
<td>1</td>
<td>0</td>
<td>0</td>
<td>74</td>
<td>C</td>
</tr>
<tr>
<td>1</td>
<td>0</td>
<td>1</td>
<td>69</td>
<td>E</td>
</tr>
<tr>
<td>1</td>
<td>0</td>
<td>.</td>
<td>18</td>
<td>E</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>0</td>
<td>109</td>
<td>E</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>1</td>
<td>795</td>
<td>A</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>.</td>
<td>58</td>
<td>B</td>
</tr>
<tr>
<td>1</td>
<td>.</td>
<td>0</td>
<td>17</td>
<td>E</td>
</tr>
<tr>
<td>1</td>
<td>.</td>
<td>1</td>
<td>79</td>
<td>B</td>
</tr>
<tr>
<td>1</td>
<td>.</td>
<td>.</td>
<td>81</td>
<td>D</td>
</tr>
</tbody>
</table>

Total: 2,788

Frequencies are based on the main sample augmented with observations with missing ADMIN or ALERT but not any other variable.
Table 4.7: Regression Results using Each Participation Measure Separately

<table>
<thead>
<tr>
<th>Participation Variable</th>
<th>Food Insecurity</th>
<th>Very Low Food Security</th>
<th>Healthy Eating Index</th>
<th>Body Mass Index</th>
<th>Obese</th>
<th>Severely Obese</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self-Reported</td>
<td>0.066***</td>
<td>0.027 (0.018)</td>
<td>-1.680***</td>
<td>1.045***</td>
<td>0.079***</td>
<td>0.039**</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.343)</td>
<td>(0.024)</td>
<td>(0.018)</td>
<td></td>
</tr>
<tr>
<td>ADMIN Preferred</td>
<td>0.060***</td>
<td>0.022 (0.018)</td>
<td>-2.071***</td>
<td>1.166***</td>
<td>0.057**</td>
<td>0.035*</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.601)</td>
<td>(0.344)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>ALERT Preferred</td>
<td>0.070***</td>
<td>0.020 (0.018)</td>
<td>-1.292**</td>
<td>1.114***</td>
<td>0.061***</td>
<td>0.021 (0.019)</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.605)</td>
<td>(0.340)</td>
<td>(0.023)</td>
<td></td>
</tr>
</tbody>
</table>

Statistics are from main sample of 2108 observations. Heteroscedasticity-robust standard errors are in parentheses. *** indicates statistically significant at the 1% level, ** 5%, * 10%.
<table>
<thead>
<tr>
<th>Participation Variable</th>
<th>Sample Size</th>
<th>Food Insecurity</th>
<th>Very Low Food Security</th>
<th>Healthy Eating Index</th>
<th>Body Mass Index</th>
<th>Obese</th>
<th>Severely Obese</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self-Reported</td>
<td>2788</td>
<td>0.059***</td>
<td>0.040**</td>
<td>-1.500***</td>
<td>1.272***</td>
<td>0.094***</td>
<td>0.043***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.018)</td>
<td>(0.016)</td>
<td>(0.541)</td>
<td>(0.302)</td>
<td>(0.021)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>ADMIN Preferred</td>
<td>2340</td>
<td>0.055***</td>
<td>0.019</td>
<td>-1.663***</td>
<td>1.328***</td>
<td>0.065***</td>
<td>0.048***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.017)</td>
<td>(0.569)</td>
<td>(0.325)</td>
<td>(0.022)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>ADMIN Alternate 1</td>
<td>2340</td>
<td>0.056***</td>
<td>0.002</td>
<td>-1.484**</td>
<td>1.202***</td>
<td>0.057***</td>
<td>0.045**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.577)</td>
<td>(0.333)</td>
<td>(0.022)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>ADMIN Alternate 2</td>
<td>2206</td>
<td>0.060***</td>
<td>0.012</td>
<td>-1.640***</td>
<td>1.377***</td>
<td>0.066***</td>
<td>0.051***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.597)</td>
<td>(0.344)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>ADMIN Alternate 3</td>
<td>2165</td>
<td>0.053***</td>
<td>0.023</td>
<td>-1.417**</td>
<td>1.249***</td>
<td>0.059**</td>
<td>0.052***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.599)</td>
<td>(0.341)</td>
<td>(0.023)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>ALERT Preferred</td>
<td>2214</td>
<td>0.064***</td>
<td>0.023</td>
<td>-1.201**</td>
<td>1.154***</td>
<td>0.064***</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.018)</td>
<td>(0.591)</td>
<td>(0.333)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>ALERT Alternate 1</td>
<td>2214</td>
<td>0.062***</td>
<td>0.009</td>
<td>-0.907</td>
<td>1.159***</td>
<td>0.065***</td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.596)</td>
<td>(0.335)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>ALERT Alternate 2</td>
<td>2135</td>
<td>0.067***</td>
<td>0.016</td>
<td>-0.66272</td>
<td>1.218***</td>
<td>0.068***</td>
<td>0.023</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.542)</td>
<td>(0.342)</td>
<td>(0.023)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>ALERT Alternate 3</td>
<td>2788</td>
<td>0.052***</td>
<td>0.016</td>
<td>-0.717</td>
<td>1.133***</td>
<td>0.059***</td>
<td>0.012</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.018)</td>
<td>(0.016)</td>
<td>(0.529)</td>
<td>(0.302)</td>
<td>(0.021)</td>
<td>(0.017)</td>
</tr>
</tbody>
</table>

Statistics based on the main sample augmented with observations with missing ADMIN or ALERT but not any other variable. Heteroscedasticity-robust standard errors are in parentheses. *** indicates statistically significant at the 1% level, ** 5%, * 10%.
<table>
<thead>
<tr>
<th></th>
<th>Sample Size</th>
<th>Food Insecurity</th>
<th>Very Low Food Security</th>
<th>Healthy Eating Index</th>
<th>Body Mass Index</th>
<th>Obese</th>
<th>Severely Obese</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rule 1: Always use ADMIN unless missing</td>
<td>2446</td>
<td>0.050***</td>
<td>0.022</td>
<td>-1.538***</td>
<td>1.352***</td>
<td>0.068***</td>
<td>0.047***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.017)</td>
<td>(0.558)</td>
<td>(0.320)</td>
<td>(0.021)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Rule 2: Always use ALERT unless missing</td>
<td>2446</td>
<td>0.060***</td>
<td>0.02</td>
<td>-0.901</td>
<td>1.308***</td>
<td>0.070***</td>
<td>0.035**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.017)</td>
<td>(0.561)</td>
<td>(0.316)</td>
<td>(0.022)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Rule 3: Drop if disagreement</td>
<td>1898</td>
<td>0.075***</td>
<td>0.028</td>
<td>-1.689***</td>
<td>1.297***</td>
<td>0.063**</td>
<td>0.032</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.021)</td>
<td>(0.019)</td>
<td>(0.652)</td>
<td>(0.363)</td>
<td>(0.025)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Rule 4: More weight to matches</td>
<td>2446</td>
<td>0.061***</td>
<td>0.034**</td>
<td>-1.482***</td>
<td>1.410***</td>
<td>0.071***</td>
<td>0.045***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.017)</td>
<td>(0.567)</td>
<td>(0.316)</td>
<td>(0.022)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Rule 5: More weight to non-matches</td>
<td>2446</td>
<td>0.050***</td>
<td>0.009</td>
<td>-0.551862</td>
<td>1.276***</td>
<td>0.069***</td>
<td>0.038**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.019)</td>
<td>(0.017)</td>
<td>(0.321)</td>
<td>(0.021)</td>
<td>(0.017)</td>
<td></td>
</tr>
<tr>
<td>Combined (SNAP-ABC)</td>
<td>2108</td>
<td>0.069***</td>
<td>0.026</td>
<td>-1.298**</td>
<td>1.475***</td>
<td>0.073***</td>
<td>0.043**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.018)</td>
<td>(0.607)</td>
<td>(0.337)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Combined (SNAP-ABC) with imputation</td>
<td>2788</td>
<td>0.067***</td>
<td>0.027</td>
<td>-1.401**</td>
<td>1.447***</td>
<td>0.072***</td>
<td>0.045**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.017)</td>
<td>(0.626)</td>
<td>(0.328)</td>
<td>(0.023)</td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

Statistics based on the main sample augmented with observations with missing ADMIN or ALERT but not any other variable. Heteroscedasticity-robust standard errors are in parentheses. *** indicates statistically significant at the 1% level, ** 5%, * 10%.
Table 4.10: 10-Question Food Security Question in FoodAPS

<table>
<thead>
<tr>
<th>Question</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>E2</td>
<td>In last 30 days, worried food would run out before we got more money</td>
</tr>
<tr>
<td>E3</td>
<td>Food ran out and had no money to buy more, in last 30 days</td>
</tr>
<tr>
<td>E4</td>
<td>Couldn’t afford to eat balanced meals, in last 30 days</td>
</tr>
<tr>
<td>E5</td>
<td>Adults skipped or cut size of meals b/c not enough money, in last 30 days (Y/N) Universe: Answered “Sometimes not enough to eat” or “Often not enough to eat” description of food sufficiency question within last 30 days, OR answered “Often true” or “Sometimes true” to E2, E3 or E4.</td>
</tr>
<tr>
<td>E5a</td>
<td>Number of days adults skipped/cut meal size b/c not enough money, last 30 days Universe: Answered “Yes” to E5</td>
</tr>
<tr>
<td>E6</td>
<td>Eat less than felt you should b/c not enough money, in last 30 days (Y/N) Universe: Answered “Sometimes not enough to eat” or “Often not enough to eat” description of food sufficiency question within last 30 days, OR answered “Often true” or “Sometimes true” to E2, E3 or E4.</td>
</tr>
<tr>
<td>E7</td>
<td>Ever hungry but didn’t eat b/c not enough money, in last 30 days (Y/N) Universe: Answered “Sometimes not enough to eat” or “Often not enough to eat” description of food sufficiency question within last 30 days, OR answered “Often true” or “Sometimes true” to E2, E3 or E4.</td>
</tr>
<tr>
<td>E8</td>
<td>Lose weight b/c not enough money for food, in last 30 days (Y/N) Universe: Answered “Sometimes not enough to eat” or “Often not enough to eat” description of food sufficiency question within last 30 days, OR answered “Often true” or “Sometimes true” to E2, E3 or E4.</td>
</tr>
<tr>
<td>E9</td>
<td>Skip food all day b/c not enough money for food, in last 30 days (Y/N) Universe: Answered “Yes” to E5, E5a, E6, E7, or E8.</td>
</tr>
<tr>
<td>E9a</td>
<td>“How often adults skipped food all day b/c not enough money, in last 30 days Universe: Answered “Yes” to E9”</td>
</tr>
</tbody>
</table>
References


Huffman, S. K., Jensen, H. H. et al. (2003), *Do food assistance programs improve household food security?: Recent evidence from the United States*, Citeseer.

Hunzinger, E., Charles, D., Godoy, M. & Aubrey, A. (2018), ‘Trump administration wants to decide what food snap recipients will get’, *National Public*


Schmeiser, M. D. (2009), ‘Expanding wallets and waistlines: the impact of family
income on the bmi of women and men eligible for the earned income tax credit’, 
*Health economics* 18(11), 1277–1294.


APPENDIX A

Proof of Theorem 1

Proof.

Biasedness: By the Frisch-Waugh-Lovell Theorem, see, e.g. Davidson & MacKinnon (2004, page 68), the regression

\[ My = M\delta\alpha + v \]

yields the same least squares estimate of \( \alpha \) as the regression equation of interest (2.8). It follows that,

\[ \hat{\alpha}_{LS} = (\delta'M\delta)^{-1}\delta'My. \]  \hspace{1cm} (A.1)

This implies that \( \hat{\alpha}_{LS} - \alpha = (\delta'M\delta)^{-1}\delta'M\varepsilon. \)

Hence, \( \mathbb{E}[\hat{\alpha}_{LS} - \alpha|X,\delta] = (\delta'M\delta)^{-1}\delta'M\mathbb{E}[\varepsilon|X,\delta] \neq 0 \), since \( \mathbb{E}[\varepsilon|\delta,X] \neq 0 \) by the correlation of \( \varepsilon \) and \( \delta \) through \( u \) and \( v \).

Inconsistency: We can write

\[ \hat{\alpha}_{LS} - \alpha = (\delta'M\delta)^{-1}\delta'M\varepsilon = \left(\frac{\delta'M\delta}{n}\right)^{-1}\frac{\delta'M\varepsilon}{n} \]

\[ = \left(\frac{\delta'M\delta}{n}\right)^{-1}\left(\frac{\delta'M\varepsilon}{n} + \frac{\delta'M(\delta^* - \delta)\alpha}{n}\right) \]  \hspace{1cm} by Equation (2.7) (A.2)

Notice that,

\[ \frac{\delta'M\delta}{n} = \frac{\delta'[I - X(X'X)^{-1}X']\delta}{n} = \frac{\delta'\delta}{n} - \frac{\delta'X}{n} \left(\frac{X'X}{n}\right)^{-1}X'\delta \]
Hence, by the Weak Law of Large Numbers and the Slutsky’s lemma, we have

\[
\frac{\delta'M\delta}{n} \xrightarrow{p} E(\delta_i^2) - E(\delta_i x'_i)E(x_i x'_i)^{-1}E(\delta_i x_i)
\]

By a matrix extension of the Cauchy-Schwarz inequality (see Tripathi 1999), we know that \(E(\delta_i^2) - E(\delta_i x'_i)E(x_i x'_i)^{-1}E(\delta_i x_i) > 0\). The Continuous Mapping Theorem then implies that

\[
\left(\frac{\delta'M\delta}{n}\right)^{-1} \xrightarrow{p} \left[E(\delta_i^2) - E(\delta_i x'_i)E(x_i x'_i)^{-1}E(\delta_i x_i)\right]^{-1}.
\]  \hspace{1cm} (A.3)

Likewise, the term \(\frac{\delta'M\epsilon}{n}\) can also be decomposed as

\[
\frac{\delta'M\epsilon}{n} = \frac{\delta'[I - X(X'X)^{-1}X']\epsilon}{n} = \frac{\delta'\epsilon}{n} - \frac{\delta'X}{n} \left(\frac{X'X}{n}\right)^{-1}X'\epsilon.
\]

Then, using the same arguments as above we have

\[
\frac{\delta'M\epsilon}{n} \xrightarrow{p} E(\delta_i \epsilon_i) - E(\delta_i x'_i)E(x_i x'_i)^{-1}E(x_i \epsilon_i) = E(\delta_i \epsilon_i),
\]

where the last equality follows from Assumption 1.

Using the expression of \(\delta_i\) given by Equation (2.4) and the trivariate normality of \((\epsilon_i, u_i, v_i)\), it can be shown by integration that

\[
E[\delta_i \epsilon_i] = E[\epsilon_i 1(z'_i \theta + v_i \geq 0, \ w'_i \gamma + u_i \geq 0)]
\]

\[
= E[\text{Pr}[u_i \geq -w'_i \gamma, \ v_i \geq -z'_i \theta, \rho]E[\epsilon_i | u_i \geq -w'_i \gamma, \ v_i \geq -z'_i \theta]]
\]

\[
= E\left[\sigma_\nu \phi(-z'_i \theta) \Phi\left(\frac{w'_i \gamma - \rho z'_i \theta}{\sqrt{1 - \rho^2}}\right) + \sigma_\nu \phi(-w'_i \gamma) \Phi\left(\frac{z'_i \theta - \rho w'_i \gamma}{\sqrt{1 - \rho^2}}\right)\right],
\]

121
where $\Phi(\cdot)$ and $\phi(\cdot)$ are the CDF and PDF of the standard normal. It follows that

\[ \frac{\delta'M\epsilon}{n} \xrightarrow{p} \mathbb{E}\left[ \sigma \varphi \phi (-z_i'\theta) \Phi \left( \frac{w_i'\gamma - \rho z_i'\theta}{\sqrt{1 - \rho^2}} \right) + \sigma \varphi \phi (-w_i'\gamma) \Phi \left( \frac{z_i'\theta - \rho w_i'\gamma}{\sqrt{1 - \rho^2}} \right) \right]. \quad (A.4) \]

Finally, using the same reasoning as above for the term $\frac{\delta'M(\delta^*-\delta)\alpha}{n}$, we have

\[ \frac{\delta'M(\delta^*-\delta)\alpha}{n} \xrightarrow{p} -\alpha \mathbb{E}(\delta_i x_i')\mathbb{E}(x_i x_i')^{-1}\mathbb{E}[(\delta^*_i - \delta_i) x_i]. \quad (A.5) \]

The desired result follows by taking (A.5), (A.4) and (A.3) to Equation (A.2).

**Proof of Theorem 2**

**Proof.** We can write

\[ \hat{\alpha}_{2S} = (\hat{\delta}'M\hat{\delta}^*)^{-1}\hat{\delta}'M\delta^*\alpha + (\hat{\delta}'M\hat{\delta}^*)^{-1}\hat{\delta}'M\epsilon \quad (A.6) \]

By the exogeneity of $X$ and $Z$ given by Assumption 1, the consistency of $\hat{\theta}$, the continuity of $\Phi(\cdot)$ and the law of large numbers, we have

\[ \frac{\hat{\delta}'M\epsilon}{n} \xrightarrow{p} \mathbb{E}[\Phi(z_i'\theta)\epsilon_i] = \mathbb{E}[\Phi(z_i'\theta)\mathbb{E}[\epsilon_i|z_i]] = 0, \]

so that the second term on the RHS of Equation (A.6) goes to zero. We also have, by Assumption 2, the consistency of $\hat{\theta}$, the continuity of $\Phi(\cdot)$ and the law of large numbers,

\[ \frac{\hat{\delta}'M\hat{\delta}^*}{n} \xleftarrow{p} \mathbb{E}[\Phi(z_i'\theta)^2] - \mathbb{E}[\Phi(z_i'\theta)x_i']\mathbb{E}[x_i x_i']^{-1}\mathbb{E}[x_i\Phi(z_i'\theta)] \]
and

\[
\frac{\hat{\delta}' M \delta^*}{n} \xrightarrow{p} E \left[ \Phi(z_i' \delta_i^*) \right] - E \left[ \Phi(z_i' \theta) x_i' \right] E \left[ x_i, x_i' \right]^{-1} E \left[ x_i \delta_i^* \right] \\
= E \left[ \Phi(z_i' \theta) E[\delta_i^* | z_i] \right] - E \left[ \Phi(z_i' \theta) x_i' \right] E \left[ x_i, x_i' \right]^{-1} E \left[ x_i E[\delta_i^* | z_i] \right] \\
= E \left[ \Phi(z_i' \theta)^2 \right] - E \left[ \Phi(z_i' \theta) x_i' \right] E \left[ x_i, x_i' \right]^{-1} E \left[ x_i \Phi(z_i') \right]
\]

where the last display follows from the fact that \( E[\delta_i^* | z_i] = \Phi(z_i' \theta) \), as implied by Equation (3.2). Hence,

\[
(\hat{\delta}' M \hat{\delta}^*)^{-1} \hat{\delta}' M \delta^* = \left( \frac{\hat{\delta}' M \hat{\delta}^*}{n} \right)^{-1} \frac{\hat{\delta}' M \delta^*}{n} \xrightarrow{p} 1
\]

so that

\[
\hat{\alpha}_{2s} \xrightarrow{p} \alpha
\]

\textbf{Proof of Theorem 3}

\textbf{Proof.} We can write

\[
\sqrt{n}(\hat{\alpha}_{2s} - \alpha) = \left( \frac{\hat{\delta}' M \hat{\delta}^*}{n} \right)^{-1} \left( \frac{\hat{\delta}' M (\delta^* - \hat{\delta}^*)}{\sqrt{n}} \right) \alpha + \left( \frac{\hat{\delta}' M \hat{\delta}^*}{n} \right)^{-1} \frac{\hat{\delta}' M \epsilon}{\sqrt{n}} \\
= \left( \frac{\hat{\delta}' M \hat{\delta}^*}{n} \right)^{-1} \left( \frac{\hat{\delta}' M (\Psi^* - \hat{\delta}^*)}{\sqrt{n}} \right) \alpha + \left( \frac{\hat{\delta}' M \hat{\delta}^*}{n} \right)^{-1} \frac{\hat{\delta}' M (\alpha(\delta^* - \Psi^*) + \epsilon)}{\sqrt{n}} \\
= q_n^{-1} \left[ \sqrt{n} V_{1n} \alpha + \sqrt{n} V_{2n} \right]
\]

where

\[
q_n = \frac{\hat{\delta}' M \hat{\delta}^*}{n}, \quad V_{1n} = \frac{\hat{\delta}' M (\Psi^* - \hat{\delta}^*)}{n}, \quad \text{and} \quad V_{2n} = \frac{\hat{\delta}' M (\alpha(\delta^* - \Psi^*) + \epsilon)}{n},
\]

123
with \(\Psi^* = \Psi^*(\theta) = [\Phi(z_1' \theta), \ldots, \Phi(z_n' \theta)]'\).

Denote \(\hat{\Lambda}_i = \hat{\delta}_i^* - \left(\frac{1}{n} \sum_{i=1}^{n} \hat{\delta}_i x_i^t\right) \left(\frac{1}{n} \sum_{i=1}^{n} x_i x_i^t\right)^{-1} x_i\) and by

\[\Lambda_i = \Phi(z_i' \theta) - \mathbb{E}[\Phi(z_i' \theta) x_i^t] \mathbb{E}[x_i x_i^t]^{-1} x_i\] its probability limit. Notice that

\[q_n = \frac{1}{n} \sum_{i=1}^{n} \hat{\Lambda}_i^2.\]

We know, from the consistency results above that

\[q_n \xrightarrow{p} q = \mathbb{E}[\Phi(z_i' \theta)^2] - \mathbb{E}[\Phi(z_i' \theta) x_i^t] \mathbb{E}[x_i x_i^t]^{-1} \mathbb{E}[\Phi(z_i' \theta) x_i] = \mathbb{E}[\Lambda_i^2]. \quad (A.8)\]

Since \(\hat{\delta}^* = \Psi^*(\hat{\theta})\), then expanding \(\Psi^*(\theta)\) in a Taylor series about \(\hat{\theta}\), we have

\[\Psi^* - \hat{\delta}^* \overset{a}{=} \psi^* (\hat{\theta})(\theta - \hat{\theta})\]

where \(\overset{a}{=}\) denotes asymptotic equivalence in probability, and \(\psi^*(\theta)\) is the vector of partial derivatives \(\partial \Psi^*(\theta)/\partial \theta'\) is given by

\[\psi^*(\theta) = \frac{\partial \Psi^*(\theta)}{\partial \theta'} = [\phi(z_1' \theta) z_1, \ldots, \phi(z_n' \theta) z_n]'\]

Therefore

\[\sqrt{n} V_{1n} \overset{d}{=} \frac{\hat{\delta}^* M \psi^*(\hat{\theta})}{n} \sqrt{n} (\theta - \hat{\theta}) = \frac{1}{n} \sum_{i=1}^{n} \hat{\Lambda}_i \phi(z_i' \hat{\theta}) z_i \sqrt{n} (\theta - \hat{\theta}).\]

A direct application of the central limit theorem then gives,

\[\sqrt{n} V_{1n} \alpha \xrightarrow{d} N(0, \alpha^2 \nu_1^2), \quad \text{where}\]

\[\nu_1^2 = \mathbb{E}[\Lambda_i \phi(z_i' \theta) z_i^t] V(\hat{\theta}) \mathbb{E}[z_i \phi(z_i' \theta) \Lambda_i]. \quad (A.9)\]

Likewise,

\[\sqrt{n} V_{2n} \overset{d}{=} \frac{\hat{\delta}^* M (\alpha (\delta^* - \Psi^*) + \epsilon)}{\sqrt{n}} = \frac{1}{\sqrt{n}} \sum_{i=1}^{n} \hat{\Lambda}_i \zeta_i\]
where $\zeta_i = \alpha (\delta^*_i - \Phi(z'_i\theta)) + \epsilon_i$, with $\mathbb{E}[\zeta_i|z_i] = 0$ and $\text{Var}[\zeta_i|z_i] = \alpha^2 \Phi(z'_i\theta)(1 - \Phi(z'_i\theta)) + \sigma^2$.

Hence, by the central limit theorem,

$$\sqrt{n} V_{2n} \xrightarrow{d} N(0, \sigma^2_2), \quad \text{where}$$

$$\sigma^2_2 = E [\Lambda_i (\alpha^2 \Phi(z'_i\theta)(1 - \Phi(z'_i\theta)) + \sigma^2) \Lambda_i] \quad (A.10)$$

$$= \alpha^2 E [\Lambda_i^2 \Phi(z'_i\theta)(1 - \Phi(z'_i\theta))] + \sigma^2 E [\Lambda_i^2]$$

Finally, the asymptotic covariance term between the elements of $\sqrt{n} V_{1n} \alpha$ and $\sqrt{n} V_{2n}$ is

$$\sigma_{12} = E [\Lambda_i \phi(z'_i\theta)z'_i]\mathbb{E}[(\theta - \hat{\theta}) (\alpha (\delta^*_i - \Phi(z'_i\theta)) + \epsilon_i)] E[\Lambda_i] \alpha \quad (A.11)$$

$$= -E [\Lambda_i \phi(z'_i\theta)z'_i] E[\hat{\theta} \epsilon_i] E[\Lambda_i] \alpha$$

It then follows from Slutsky’s Lemma, (A.7), (A.8), (A.9), (A.10) and (A.11) that

$$\sqrt{n} (\hat{\alpha}_{2S} - \alpha) \xrightarrow{d} N(0, \sigma^2_\alpha), \quad \text{where}$$

$$\sigma^2_\alpha = \frac{\alpha^2 \nu^2_i}{q^2} + 2 \frac{\sigma_{12}}{q^2} + \frac{\sigma^2_2}{q^2}$$

$$= \frac{\alpha^2 E [\Lambda_i \phi(z'_i\theta)z'_i] V(\hat{\theta}) E[z_i \phi(z'_i\theta)\Lambda_i]}{E[\Lambda_i^2]^2} - 2 \frac{\alpha E [\Lambda_i \phi(z'_i\theta)z'_i] E[\hat{\theta} \epsilon_i] E[\Lambda_i]}{E[\Lambda_i^2]^2}$$

$$+ \frac{\alpha^2 E [\Lambda_i^2 \Phi(z'_i\theta)(1 - \Phi(z'_i\theta))] - \sigma^2}{E[\Lambda_i^2]^2}.$$

With $\hat{\theta}$ and $\epsilon_i$ uncorrelated conditionally on $z_i$ and $w_i$ the covariance term is zero,
and the variance reduces to
\[
\sigma^2_\alpha = \frac{\alpha^2 \mathbb{E}[\Lambda_i \phi(z_i' \theta)z_i'] V(\hat{\theta}) \mathbb{E}[\Lambda_i \phi(z_i' \theta) \Lambda_i]}{\mathbb{E}[\Lambda_i^2]} + \frac{\alpha^2 \mathbb{E}[\Lambda_i^2 \Phi(z_i' \theta)(1 - \Phi(z_i' \theta))]}{\mathbb{E}[\Lambda_i^2]} + \frac{\sigma^2}{\mathbb{E}[\Lambda_i^2]}
\]

A consistent estimator for this asymptotic variance can be defined by
\[
\hat{\sigma}^2_\alpha = \frac{\hat{\alpha}_{2S}^2 \hat{\nu}^2_1}{\hat{q}^2} + \frac{\hat{\alpha}_{2S}^2 \hat{\nu}^2_2}{\hat{q}^2} + \hat{\sigma}^2
\]

where
\[
\hat{\nu}^2_1 = \left( \frac{1}{n} \sum_{i=1}^{n} \hat{\Lambda}_i \phi(z_i' \hat{\theta}) z_i' \right) \hat{V}(\hat{\theta}) \left( \frac{1}{n} \sum_{i=1}^{n} z_i \phi(z_i' \hat{\theta}) \hat{\Lambda}_i \right),
\]
\[
\hat{\nu}^2_2 = \frac{1}{n} \sum_{i=1}^{n} \hat{\Lambda}_i^2 \Phi(z_i' \hat{\theta}) \left( 1 - \Phi(z_i' \hat{\theta}) \right),
\]
\[
\hat{\sigma}^2 = \frac{1}{n} \sum_{i=1}^{n} \left[ \left( y_i - x_i' \hat{\beta} - \hat{\alpha}_{2S} \Phi(z_i' \hat{\theta}) \right)^2 - \hat{\alpha}_{2S}^2 \Phi(z_i' \hat{\theta}) \left( 1 - \Phi(z_i' \hat{\theta}) \right) \right],
\]
\[
\hat{q} = \frac{1}{n} \sum_{i=1}^{n} \hat{\Lambda}_i^2 = \frac{1}{n} \sum_{i=1}^{n} \Phi(z_i' \hat{\theta})^2 - \left( \frac{1}{n} \sum_{i=1}^{n} \Phi(z_i' \hat{\theta}) x_i' \right) \left( \frac{1}{n} \sum_{i=1}^{n} x_i x_i' \right)^{-1} \left( \frac{1}{n} \sum_{i=1}^{n} x_i \Phi(z_i' \hat{\theta}) \right),
\]
and \(\hat{\alpha}_{2S}\) is our proposed estimator of \(\alpha\). □
Maximum Likelihood Estimation

Let Assumptions 1-4 hold and assume that the covariance matrix of the joint distribution of errors $\Sigma$ defined in (2.5) is positive definite. Then our model can be estimated jointly using full information maximum likelihood. The log-likelihood function is built from the joint density of $y_i$, $\delta_i^*$ and $d_i$, which we write as the product of the conditional and the marginal densities

$$f(\delta_i^*, d_i, y_i) = f(\delta_i^*, d_i|y_i)f(y_i).$$

To derive the conditional distributions, we use results for the trivariate normal, and write

$$v_i = \rho_1 \epsilon_i / \sigma + \rho_2 u_i + \eta_i, \quad \text{with} \quad \eta_i|\epsilon_i, u_i \sim N(0, \kappa^2)$$

where $\rho_1$, $\rho_2$ and $\kappa^2$ are defined in terms of the original parameters $\varphi_v$, $\varphi_u$ and $\rho$ by:

$$\rho_1 = \frac{\varphi_v - \rho \varphi_u}{1 - \varphi_u^2}, \quad \rho_2 = \frac{\rho - \varphi_v \varphi_u}{1 - \varphi_u^2}, \quad \kappa^2 = 1 - \rho_1^2 - \rho_2^2 - 2\rho_1 \rho_2 \varphi_u$$

Denote $\Theta = (\theta', \gamma', \rho, \varphi_u, \varphi_v, \beta', \alpha, \sigma^2)'$ the vector of all the parameters of the model. Then,

---

1 The fact that the covariance matrix of the joint distribution of errors is constrained to be positive definite guarantees that these new parameters are well-defined, namely, $0 < \rho_1 < 1$, $0 < \rho_2 < 1$, and $\kappa^2 > 0$.  

---
\[ \Upsilon_{11i}(\Theta) = f(\delta_i^* = 1, d_i = 1 | y_i) = \Phi_2 \left( \frac{z_i' \theta + \rho_1 (y_i - \alpha - x_i' \beta) / \sigma}{\sqrt{\rho_2^2 + \kappa^2}}, w_i' \gamma, \frac{\rho_2}{\sqrt{\rho_2^2 + \kappa^2}} \right) \]

\[ \Upsilon_{10i}(\Theta) = f(\delta_i^* = 1, d_i = 0 | y_i) = \Phi \left( \frac{z_i' \theta + \rho_1 (y_i - \alpha - x_i' \beta) / \sigma}{\sqrt{\rho_2^2 + \kappa^2}} \right) - \Upsilon_{11i}(\Theta) \]

\[ \Upsilon_{0i}(\Theta) = f(\delta_i^* = 0 | y_i) = 1 - \Phi \left( \frac{z_i' \theta + \rho_1 (y_i - x_i' \beta) / \sigma}{\sqrt{\rho_2^2 + \kappa^2}} \right) = 1 - \Upsilon_{11i}(\Theta) - \Upsilon_{10i}(\Theta) \]

where \( \Phi_2(\cdot, \cdot, \cdot) \) is the CDF of the standard bivariate normal distribution.

The full information log-likelihood function of the model is then defined by

\[ l(\Theta) = \sum_{i=1}^{n} l_i(\Theta), \]

with

\[ l_i(\Theta) = \delta_i \ln \left[ \Upsilon_{11i}(\Theta) \frac{1}{\sigma} \phi \left( \frac{y_i - \alpha - x_i' \beta}{\sigma} \right) \right] + \]

\[ + (1 - \delta_i) \ln \left[ \Upsilon_{10i}(\Theta) \frac{1}{\sigma} \phi \left( \frac{y_i - \alpha - x_i' \beta}{\sigma} \right) + \Upsilon_{0i}(\Theta) \frac{1}{\sigma} \phi \left( \frac{y_i - x_i' \beta}{\sigma} \right) \right] \] (B.1)

Maximizing this function with respect to \( \Theta \) yields a consistent and asymptotically efficient estimator of the model parameters.
Extension to Binary Outcomes

The method discussed in this paper can be extended to the case of binary outcomes. However, in this case we can not just do the plug-in method described earlier, because a linear probability model would exhibit serious problems, especially the fact that it could produce a wrong sign for the treatment effect, even if the treatment status is correctly classified (see, e.g., discussion provided by Lewbel, Dong & Yang 2012). A more reliable alternative in this case would be the maximum likelihood estimation. We assume the binary outcome $y_i$ is related to the exogenous covariates $x_i$ and to the true treatment indicator $\delta_i^*$ by

$$y_i = 1 \left[ x_i' \beta + \delta_i^* \alpha + \epsilon_i > 0 \right]. \tag{C.1}$$

True participation $\delta_i^*$ and misreporting $d_i$ are defined, as before, by equations (3.2) and (3.4), respectively. We maintain Assumptions 1-4 above, except that the conditional variance of the error $\epsilon_i$ is now normalized to 1 (as is usually the case for identification in Probit models). Given the observed participation $\delta_i = \delta_i^* d_i$, and the outcome $y_i$, the log-likelihood function of the binary choice model (BCM) is built up from the joint probabilities $\Pr[y_i, \delta_i^*, d_i]$ of these dichotomous variables as follows.

$$l_{BCM}(\Theta) = \sum_{i=1}^{n} \delta_i \ln \Pr[y_i, \delta_i^* = 1, d_i = 1] +$$

$$+ (1 - \delta_i) \ln (\Pr[y_i, \delta_i^* = 1, d_i = 0] + \Pr[y_i, \delta_i^* = 0]) \tag{C.2}$$
where

\[
\begin{align*}
\Pr[y_i, \delta^*_i = 1, d_i = 1] = \Pr[y_i = 1, \delta^*_i = 1, d_i = 1] & \Pr[y_i = 0, \delta^*_i = 1, d_i = 1]^{1-y_i}, \\
\Pr[y_i, \delta^*_i = 1, d_i = 0] = \Pr[y_i = 1, \delta^*_i = 1, d_i = 0] & \Pr[y_i = 0, \delta^*_i = 1, d_i = 0]^{1-y_i}, \\
\text{and} \quad \Pr[y_i, \delta^*_i = 0] = \Pr[y_i = 1, \delta^*_i = 0] & \Pr[y_i = 0, \delta^*_i = 0]^{1-y_i}
\end{align*}
\]

The probabilities in these equations can be obtained in terms of model parameters:

\[
\begin{align*}
\Pr[y_i = 1, \delta^*_i = 1, d_i = 1] &= \Phi_3 (x'_i \beta + \alpha, z'_i \theta, w'_i \gamma; \varphi_v, \varphi_u, \rho) \\
\Pr[y_i = 0, \delta^*_i = 1, d_i = 1] &= \Phi_2 (z'_i \theta, w'_i \gamma; \rho) - \Pr[y_i = 1, \delta^*_i = 1, d_i = 1] \\
\Pr[y_i = 1, \delta^*_i = 1, d_i = 0] &= \Phi_2 (x'_i \beta + \alpha, w'_i \gamma; \varphi_v) - \Pr[y_i = 1, \delta^*_i = 1, d_i = 1] \\
\Pr[y_i = 0, \delta^*_i = 1, d_i = 0] &= \Phi (z'_i \theta) - \Phi_2 (z'_i \theta, w'_i \gamma; \rho) - \Pr[y_i = 1, \delta^*_i = 1, d_i = 0] \\
\Pr[y_i = 1, \delta^*_i = 0] &= \Phi (x'_i \beta) - \Phi_2 (x'_i \beta, z'_i \theta; \varphi_v) \\
\Pr[y_i = 0, \delta^*_i = 0] &= 1 - \Phi (z'_i \theta) - \Pr[y_i = 1, \delta^*_i = 0]
\end{align*}
\]

where \(\Phi_3 (\cdot, \cdot, \cdot)\) is the CDF of the standard trivariate normal distribution.
VITA

Augustine Denteh was born in Anfoega in the Volta Region of Ghana. He studied at the University of Ghana where he received a BA in Economics and Statistics in May, 2010. Augustine served as a national service person in the Department of Economics at the University of Ghana before pursuing graduate studies in the United States. Before enrolling in the Ph.D. program in Economics at the Andrew Young School of Policy Studies at Georgia State University (GSU), he earned an MA degree in Economics from the University of Akron. His primary research interests are in applied econometrics, health economics, and public economics.

During his graduate studies in the Andrew Young School of Policy Studies, Augustine worked as a Graduate Research Assistant for Dr. Jorge Martinez-Vazquez and Dr. Rusty Tchernis. He was also an instructor for undergraduate econometrics at GSU. As part of a Federal Reserve Bank of Atlanta Fellowship, Augustine worked at the Atlanta Fed during the 2015/2016 academic year as a Research Assistant for Dr. Julie Hotchkiss.

Augustine received several awards throughout his graduate studies at Georgia State University including the Carolyn McClain Young Leadership Fund Award, the Center for the Economic Analysis of Risk Fellowship, the Joseph K. Heyman Scholarship, the AYSPS Excellence in Teaching Economics Award, and the Jack Blicksilver Scholarship.

Augustine was awarded his Ph.D. in Economics in May, 2018. He has accepted a Postdoctoral Research Fellow position at the Harvard Medical School’s Department of Health Care Policy, Boston, MA, and a faculty position at the Department of Economics at Tulane University, New Orleans, LA.