Journal of the American Statistical Association

Identifying the Effects of SNAP (Food Stamps) on Child Health Outcomes When Participation Is Endogenous and Misreported

Brent Kreider [a] , John V. Pepper [b] , Craig Gundersen [c] & Dean Jolliffe [d]

[a] Department of Economics, Iowa State University, Ames, IA, 50011-1070

[b] Department of Economics, University of Virginia, Charlottesville, VA, 22904-4182

[c] Department of Agricultural and Consumer Economics, University of Illinois, Urbana, IL, 61801-3605

[d] The World Bank Group, Washington, DC, 20433

PLEASE SCROLL DOWN FOR ARTICLE

Identifying the Effects of SNAP (Food Stamps) on Child Health Outcomes When Participation Is Endogenous and Misreported

Brent KREIDER, John V. PEPPER, Craig GUNDERSEN, and Dean JOLLIFFE

The literature assessing the efficacy of the Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program, has long puzzled over positive associations between SNAP receipt and various undesirable health outcomes such as food insecurity. Assessing the causal impacts of SNAP, however, is hampered by two key identification problems: endogenous selection into participation and extensive systematic underreporting of participation status. Using data from the National Health and Nutrition Examination Survey (NHANES), we extend partial identification bounding methods to account for these two identification problems in a single unifying framework. Specifically, we derive informative bounds on the average treatment effect (ATE) of SNAP on child food insecurity, poor general health, obesity, and anemia across a range of different assumptions used to address the selection and classification error problems. In particular, to address the selection problem, we apply relatively weak nonparametric assumptions on the latent outcomes, selected treatments, and observed covariates. To address the classification error problem, we formalize a new approach that uses auxiliary administrative data on the size of the SNAP caseload to restrict the magnitudes and patterns of SNAP reporting errors. Layering successively stronger assumptions, an objective of our analysis is to make transparent how the strength of the conclusions varies with the strength of the identifying assumptions. Under the weakest restrictions, there is substantial ambiguity; we cannot rule out the possibility that SNAP increases or decreases poor health. Under stronger but plausible assumptions used to address the selection and classification error problems, we find that commonly cited relationships between SNAP and poor health outcomes provide a misleading picture about the true impacts of the program. Our tightest bounds identify favorable impacts of SNAP on child health.

KEY WORDS: Food insecurity; Food Stamp Program; Health outcomes; Nonclassical measurement error; Nonparametric bounds; Partial identification; Supplemental Nutrition Assistance Program; Treatment effect.

1. INTRODUCTION

The Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program, is by far the largest food assistance program in the United States and, as such, constitutes a crucial component of the social safety net in the United States. In any given month during 2009, SNAP provided assistance to more than 15 million children (Leftin, Gothro, and Eslami 2010), and it is estimated that nearly one in two American children will receive assistance during their childhood (Rank and Hirschl 2009). As a consequence, policymakers expect this program to have major beneficial impacts on numerous health and nutrition challenges facing the nation, particularly for low-income children who constitute half of the recipients. Paradoxically, however, the vast empirical literature examining the impact of SNAP on health reveals little support-

ing evidence regarding the efficacy of the program in promoting food security and alleviating health problems. Children residing in households receiving food stamps are substantially more likely to suffer from an array of health-related problems than observationally similar nonparticipating children (e.g., Currie 2003; Coleman-Jensen et al. 2011).

While SNAP is associated with adverse health- and nutrition-related outcomes, drawing inferences on the efficacy of the program is complicated by two fundamental identification problems. A *selection problem* arises because the decision to participate in SNAP is unlikely to be exogenous. To the contrary, unobserved factors such as expected future health status, parents' human capital characteristics, financial stability, and attitudes toward work and family are all thought to be jointly related to participation in the program and health outcomes (Currie 2003). Families may decide to participate precisely because they expect to be food insecure or in poor health.

A nonrandom *measurement error problem* arises because large fractions of food stamp recipients fail to report their program participation in household surveys. Using administrative data matched with data from the Survey of Income and Program Participation (SIPP), for example, Bollinger and David (1997) find that errors in self-reported receipt of food stamps exceed 12% and are related to respondents' characteristics including their true participation status, health outcomes, and demographic attributes. Meyer, Mok, and Sullivan (2009) provide evidence of extensive underreporting in the SIPP, the Current

Population Survey (CPS), and the Panel Study of Income Dynamics (PSID).

While these identification problems have long been known to confound inferences on the impact of SNAP, credible solutions remain elusive. In reviewing this literature, Currie (2003, p. 240) asserts that "many studies have [...] simply 'punted' on the issue of identification." Most studies treat selection as exogenous and receipt as accurate. A few recent exceptions address the selection problem using instrumental variables within a linear response model (e.g., Gundersen and Oliveira 2001; DePolt, Moffitt, and Ribar 2009; Hoynes and Schanzenbach 2009). Gundersen and Kreider (2008) formally allow for the possibility of misclassified program participation, but they focus on identifying descriptive statistics rather than causal parameters.

In this article, we consider what can be inferred about impacts of SNAP when formally accounting for the ambiguity created by the selection and measurement error problems. This study is the first to simultaneously address both of these treatment effect identification problems within a single methodological framework. To do so, we extend recently developed partial identification methods that allow one to consider weaker assumptions than required under conventional parametric approaches (e.g., Manski 1995; Pepper 2000; Kreider and Pepper 2007, 2008; Gundersen and Kreider 2008; Molinari 2008, 2010; Kreider and Hill 2009). Introducing a nonparametric regression discontinuity design, Gundersen, Kreider, and Pepper (2012) apply some of the methods developed in this article to study effects of the National School Lunch Program (NSLP). Recent research in Nicoletti, Peracchi, and Foliano (2011) study identification of marginal distributions (for poverty rates) using a framework that formally accounts for both classification errors and missing data.

This partial identification approach is especially well-suited for studying the impact of SNAP where classical methodological prescriptions are often untenable. The literature evaluating the impact of means-tested assistance programs typically relies on linear response models coupled with an assumption that some observed instrumental variable (IV), often based on cross-state and time variation in program rules and regulations, affects program participation but otherwise has no effect on the potential outcomes. Yet, SNAP is mostly defined at the federal level and has not substantively changed since the early 1980s, so many of the key program rules and regulations are not as useful as instrumental variables. Moreover, as is now widely recognized, the classical linear response model assumption is difficult to justify when considering programs that are thought to have heterogeneous effects (Moffitt 2005). Finally, the implicit assumption of accurate classification of participation status is known to be violated, yet addressing the problem of classification errors in a binary regressor is difficult. The assumption of non-mean-reverting errors cannot apply with binary variables, and the systematic underreporting of SNAP participation violates the classical assumption that measurement error arises independently of the true value of the underlying variable (e.g., Bollinger 1996).

The methods applied in this article do not require the linear response model, the classical measurement error model, or an instrumental variable assumption. Instead, we focus on weaker models that are straightforward to motivate in practice and result in informative bounds on the health consequences of SNAP. In light of the methodological challenges in addressing these identification problems, deriving informative bounds under assumptions that may share some consensus seems like an important step.

Using data from the National Health and Nutrition Examination Survey (NHANES), we assess the impact of SNAP on the health of children, an important subpopulation that comprises half of all recipients and whose well-being is followed closely by policymakers and program administrators. A primary strength of the NHANES is the wealth of health-related information provided in the survey. We exploit these data by assessing the impact of SNAP on food insecurity, obesity, poor general health, and anemia. In what follows, we use the terms "SNAP" and "food stamps" interchangeably since benefits were called food stamps during the years the data were collected for our analysis.

After describing the data in Section 2, we formally define the empirical questions and the identification problems in Section 3. Our analysis is complicated by two distinct identification problems: (a) the selection problem that arises because the data cannot reveal unknown counterfactuals (e.g., the outcomes of a nonparticipant in an alternate state of the world in which SNAP benefits are received), and (b) the measurement error problem that arises because the data cannot reveal respondents with misclassified participation status.

In Section 4, we focus on the selection problem, abstracting away from classification errors. Following Manski (1995) and Pepper (2000), we begin by examining what can be learned without imposing any assumptions on the selection process, and then consider the identifying power of several alternative assumptions. We first consider the *Monotone Treatment Selection* (MTS) restriction (Manski and Pepper 2000) that formalizes the common assumption that the decision to participate in SNAP is monotonically related to poor latent health outcomes. We then consider the *Monotone Instrumental Variable* (MIV) assumption that the latent probability of a poor health outcome is nonincreasing in household income (adjusted for family composition). Requiring no a priori exclusion restriction, the MIV assumption can be plausible in many applications where the standard independence assumption is a matter of considerable controversy. Finally, in parts of the analysis, we consider a *Monotone Treatment Response* (MTR) assumption that participation in SNAP does not worsen health status. While recipients appear to be worse off on average than eligible nonrecipients, many have argued that participating in SNAP would not cause health or food security to deteriorate (e.g., Currie 2003). Section 4 concludes with a brief analysis of data from the 2003 CPS to assess whether the results are consistent across the surveys and to consider standard instrumental variables made available in the rich CPS covariate data.

In Section 5, we introduce classification errors in the model. In doing so, we make two notable contributions to the literature. First, departing from the usual treatment effects literature that formally acknowledges ambiguity associated with counterfactuals but not ambiguity associated with misreporting, our methods simultaneously account for both problems. Second, we develop new methods that use administrative information on the

size of the SNAP caseload to derive informative constraints on the classification error problem.

We draw conclusions in Section 6 and emphasize three findings. First, the ambiguity associated with the selection and classification error problems can be substantially mitigated by applying some basic restrictions including MTS and no-false positive reports of participation (as discussed below, the empirical literature on SNAP suggests that errors of commission are negligible). Second, under the joint MIV–MTS assumption, we find that SNAP reduces food insecurity rates. This result holds even for modest degrees of misclassification error. Finally, under the joint MIV–MTS–MTR assumption, we find that SNAP leads to a decline in food insecurity rates and other poor health outcomes even when allowing for high rates of classification error.

2. DATA

To study the impact of SNAP on child nutritional health, we use data from the 2001–2006 NHANES. The NHANES, conducted by the National Center for Health Statistics, Centers for Disease Control (NCHS/CDC), is a program of surveys designed to collect information about the health and nutritional status of adults and children in the United States through interviews and direct physical examinations. The survey currently includes a national sample of about 5,000 persons each year, about half of whom are children. Vulnerable groups, including Hispanics and African-Americans, are oversampled. Given the wealth of health-related information, NHANES has been widely used in previous research on health- and nutrition-related child outcomes (recent work includes, e.g., Gundersen et al. 2008).

We focus our analysis on households with children eligible to receive SNAP. To be eligible for assistance during the time period of our study, a household's gross income before taxes in the previous month cannot exceed 130% of the poverty line, net monthly income (gross income minus a standard deduction and expenses for care for disabled dependents, medical expenses, and excessive shelter costs) cannot exceed the poverty line, and assets must be less than $2000. Since the NHANES does not provide sufficient information to measure net income and assets, we focus on gross income eligibility. Given our focus on children, however, this data limitation should not lead to substantial errors in defining eligibility (Gundersen and Offutt 2005). In contrast, the asset test could be important for a sample that includes a high proportion of households headed by an elderly person (Haider, Jacknowitz, and Schoeni 2003). Virtually, all gross income eligible households are also net income eligible.

Our preliminary sample comprises 4690 children between the ages of 2 and 17 who reside in households with income less than 130% of the federal poverty line. Children under the age of two are not included in the sample because there is no commonly accepted way to establish body mass index (BMI) percentiles for children this young. After dropping additional observations for which information is missing about height and weight, we obtain our final sample of 4418 income-eligible children.

For each observation, we observe a number of socioeconomic and demographic characteristics including the ratio of income to the poverty line. Our sample has an average household income level equal to 75% of the poverty line. To assess the charac-

teristics of our sample relative to other national estimates, we examined data from the 2003 CPS, December Supplement (see Section 4.4 for further details). These data also indicate that income-eligible children lived in families with an average income equal to 75% of the poverty line.

2.1 Self-Reported SNAP Receipt Indicator

Beyond demographic information, we also observe a self-reported measure of SNAP receipt over the past year. SNAP participants receive benefits for the purchase of food in authorized retail food outlets where the benefit amount depends on net income. Households with a net income of zero receive the maximum benefit, and benefits decline with income. For every additional dollar, the amount of SNAP benefits is reduced by 30 cents (except earned income, in which case the reduction is 24 cents). In 2010, the average monthly benefit was $288/month for a family of four, with a maximum benefit of $668. These benefits can represent a substantial component of low-income households' total income.

In this survey, only 46% of the households classified as eligible for benefits claim to be participating in the program. In part, this might reflect errors in classifying eligibility status. Some respondents classified as eligible may, in fact, be ineligible (Gundersen, Kreider, and Pepper 2012 address this classification error problem in their evaluation of the NSLP). Even with classification errors, however, a large fraction of eligible households do not participate in SNAP. This nonparticipation is ascribed to four main factors. First, there may be a stigma associated with receiving SNAP. Stigma encompasses a wide variety of sources, including a person's own distaste for participation, fear of disapproval from others when redeeming food stamps, and the possible negative reaction of caseworkers (Moffitt 1983). Second, transaction costs can diminish the attractiveness of participation. To receive SNAP, households must personally verify their income and expenses and must visit a caseworker on a periodic basis to recertify their eligibility. The initial visit and subsequent recertifications can be time consuming. Third, the benefit level can be quite small for relatively higher income families–sometimes as low as $10 a month.

Finally, SNAP receipt is thought to be underreported. Evidence of pervasive underreporting has surfaced in two types of studies, both of which compare self-reported information with official records. The first type has compared aggregate statistics obtained from self-reported survey data with those obtained from administrative data. These studies suggest the presence of substantial underreporting in many different surveys including the CPS, the SIPP, the PSID, and the Consumer Expenditure Survey (CES) (Trippe, Doyle, and Asher 1992; Bitler, Currie, and Scholz 2003; Meyer, Mok, and Sullivan 2009). Meyer, Mok, and Sullivan (2009, Table 12), for example, find that self-reports in the CPS reflect just over half the number of food stamp recipients identified in administrative data. Other studies have compared individual reports of food stamp participation status in surveys with matched reports from administrative data. Using this method, researchers can identify both errors of commission (reporting benefits not actually received) and errors of omission (not reporting benefits actually received). As discussed earlier, Bollinger and David (1997, Table 2) find that 12.0% of responses

Table 1. Means by reported food stamp program participation

Variable	Income-eligible children	Recipients ($FS = 1$)	Nonrecipients ($FS = 0$)
Age in years	9.108 (0.099)	8.607*** (0.127)	9.527 (0.132)
Ratio of income to the poverty line	0.754 (0.011)	0.631*** (0.016)	0.857 (0.014)
Food stamp recipient	0.456 (0.022)		
Food insecure	0.400 (0.015)	0.450** (0.023)	0.357 (0.024)
Poor or fair health	0.080 (0.005)	0.088 (0.008)	0.073 (0.007)
Obese	0.185 (0.008)	0.191 (0.014)	0.179 (0.012)
Anemia[a]	0.012 (0.002)	0.013 (0.003)	0.010 (0.003)

NOTES: Sample estimates are weighted using the medical exam weight. Standard errors in parentheses account for the sample design using the synthetic strata and PSU variables. The estimated means for the SNAP recipient population are superscripted with *, **, or *** to indicate that they are statistically significantly different from the means for the nonrecipient population, with p-values less than 0.1, 0.05, 0.01, respectively, based on Wald statistics corrected for the sample design.
[a]The sample size for anemia is 3871 (with 1888 food stamp recipients) due to missing observations. The sample size for the other three outcomes is 4418.

in the SIPP involve errors of omission while only 0.3% involve errors of commission (see also Marquis and Moore 1990).

2.2 Outcomes

A primary strength of the NHANES is the detailed information provided on dietary- and health-related outcomes, with distinct components of the survey providing information from self-reports, medical examinations, physiological measurements, and laboratory tests. Since no single measure is thought to completely capture health and nutritional well-being, the detailed and varied health measures available in the NHANES make it a unique and important survey for studying the impact of nutritional programs on well-being.

Because alleviating food insecurity is the central goal of SNAP (Food and Nutrition Act of 2008, 7 U.S.C. § 2011, 2008), much of our attention focuses on this measure of nutritional health. The extent of food insecurity in the United States has become a well-publicized issue of concern to policymakers and program administrators. In 2010, 14.5% of the U.S. population reported that they suffered from food insecurity at some time during the previous year (Coleman-Jensen et al. 2011). These households were uncertain of having, or unable to acquire, enough food for all their members because they had insufficient money or other resources.

To calculate these official food insecurity rates in the U.S., defined over a 12-month-period, a series of 18 questions are posed in the Core Food Security Module (CFSM) for families with children. (For families without children, a subset of 10 of these 18 questions is posed.) Each question is designed to capture some aspect of food insecurity and, for some questions, the frequency with which it manifests itself. Examples include: "I worried whether our food would run out before we got money to buy more" (the least severe outcome); "Did you or the other adults in your household ever cut the size of your meals or skip meals because there wasn't enough money for food?" and "Did a child in the household ever not eat for a full day because you couldn't afford enough food?" (the most severe outcome). A complete listing of the food insecurity questions is presented in Appendix A, Table 1. Following official definitions, we use these 18 questions to construct a comparison of children in food-secure households (two or fewer affirmative responses) with children in food-insecure households (three or more affirmative responses).

In addition to studying the impact of SNAP on food insecurity rates, we also examine three other outcome variables: obesity, anemia, and an indicator of fair or poor general health. Based on guidelines provided by the Centers for Disease Control and Prevention, a child is classified as obese if his or her BMI (kg/m^2) is at or above the 95th percentile for his or her age and gender. In the NHANES, heights and weights used to calculate BMI are obtained by trained personnel (i.e., not self-reported). A child is classified as having anemia if, based on a blood test, the child is both iron-deficient and has an abnormally low hemoglobin level. The indicator of fair or poor general health is reported by the child's parent. In this article, we treat these health outcomes as accurately measured. While errors in measuring obesity and anemia are likely to be minimal (data on height and weight were collected by trained personnel and anemia is measured using a blood test), this assumption may be violated for the general health and food insecurity outcomes. In general, measurement error in the outcome variables would widen the bounds established in this article.

Together, these four measures reflect a wide range of health-related outcomes that might be impacted by SNAP. All four outcomes are also known to be associated with a range of negative physical, psychological, and social consequences that have current and future implications for health, including reduced life expectancy. With a maximum pairwise correlation of only 0.12 (between food insecurity and poor general health), these four outcomes are related but clearly measure different aspects of well-being. The outcomes have also attracted different levels of attention in the existing food stamp literature. Food insecurity and obesity are of central concern to policymakers and researchers studying the impact of SNAP on health (e.g., Kaushal 2007; Meyerhoefer and Pylypchuk 2008). To the best of our knowledge, this article is the first to investigate (using any methods) the impacts of SNAP on self-reported general health and anemia.

Table 1 displays means and standard errors for the variables used in this study. The estimates in this table (and elsewhere in the article) are weighted to account for the complex survey design used in the NHANES. Consistent with previous work on this topic, SNAP recipients tend to have worse health outcomes than eligible nonparticipants. For example, 45% of children reported as SNAP recipients are food-insecure, nine percentage points higher than the 36% food insecurity rate among eligible nonparticipants (a statistically significant difference at better

than the 5% level). Compared with eligible nonrecipients, SNAP recipients are also slightly more likely to be obese, be in fair or poor general health, and have anemia.

3. IDENTIFYING THE AVERAGE TREATMENT EFFECT

Our interest is in learning about the average treatment effect (ATE) of SNAP receipt on each of our health-related outcomes among food-stamp-eligible households. Focusing on binary outcomes, the ATE is given by

$$\text{ATE}(1, 0 | X \in \Omega) = P[H(1) = 1 | X \in \Omega] \\ - P[H(0) = 1 | X \in \Omega], \qquad (1)$$

where H is the realized health outcome, $H(1)$ denotes the health of a child if he or she were to receive food stamps, $H(0)$ denotes the analogous outcome if the child were not to receive food stamps, and $X \in \Omega$ denotes conditioning on observed covariates whose values lie in the set Ω. Thus, the ATE reveals how the mean outcome would differ if all eligible children received food stamps versus the mean outcome if all eligible children did not receive food stamps. In our analysis, $H = 1$ denotes a poor health outcome with $H = 0$ otherwise.

In what follows, we simplify the notation by suppressing the conditioning on subpopulations of interest captured in X. For this analysis, we focus on eligible children. In much of the literature examining the impact of SNAP, other observed covariates are motivated as a means of controlling for factors influencing a family's participation decision. In the usual regression framework, researchers attempt to "correctly" choose a set of control variables for which the exogenous selection assumption applies. Inevitably, however, there is much debate about whether the researcher omitted "important" explanatory variables. In contrast, conditioning on covariates in our approach serves only to define subpopulations of interest as there are no regression orthogonality conditions to be satisfied. The problem is well-defined regardless of how the subpopulations are specified (Pepper 2000).

As discussed earlier, two identification problems arise when assessing the impact of SNAP on children's health outcomes. First, even if participation were observed for all eligible households, the potential outcome $H(1)$ is counterfactual for all children who did not receive food stamps, while $H(0)$ is counterfactual for all children who did receive food stamps. This is referred to as the selection problem. Using the Law of Total Probability, this identification problem can be highlighted by writing the first term of Equation (1) as

$$P[H(1) = 1] = P[H(1) = 1 | FS^* = 1] P(FS^* = 1) \\ + P[H(1) = 1 | FS^* = 0] P(FS^* = 0), \quad (2)$$

where $FS^* = 1$ denotes that a child is in a household that truly receives food stamps and $FS^* = 0$ otherwise. If food stamp receipt is observed, the sampling process identifies the selection probability, $P(FS^* = 1)$, the probability that an eligible child does not receive food stamps, $P(FS^* = 0)$, and the expectation of outcomes, conditional on the outcome being observed, $P[H(1) = 1 | FS^* = 1] = P(H = 1 | FS^* = 1)$. Still, the sampling process cannot reveal the mean outcome conditional on the outcome being counterfactual, $P[H(1) = 1 | FS^* = 0]$.

Thus, $P[H(1) = 1]$ is not point-identified by the sampling process alone.

Second, true participation status may not be observed for respondents. This is referred to as the measurement or classification error problem. Instead of observing FS^*, we observe a self-reported indicator, FS, where $FS = 1$ if a child is in a household that reports receiving food stamps and 0 otherwise. Without assumptions restricting the nature or degree of classification errors, the sampling process does not reveal useful information on food stamp receipt, FS^*, and thus all of the probabilities on the right-hand side of Equation (2) are unknown.

To highlight this measurement problem, let the latent variable Z^* indicate whether a report is accurate, where $Z^* = 1$ if $FS^* = FS$ and $Z^* = 0$ otherwise. Using this variable, we can further decompose the first term of Equation (1) as

$$P[H(1) = 1] = P[H(1) = 1, FS^* = 1] - \theta_1^+ + \theta_1^- \\ + P[H(1) = 1 | FS^* = 0][P(FS = 0) \\ + (\theta_1^+ + \theta_0^+) - (\theta_1^- + \theta_0^-)], \qquad (3)$$

where $\theta_j^+ = P(H = j, FS = 1, Z^* = 0)$ and $\theta_j^- = P(H = j, FS = 0, Z^* = 0)$ denote the fraction of false positive and false negative classifications of food stamp recipients, respectively, for children realizing health outcome $j = 1, 0$. The first part of Equation (2), $P[H(1) = 1 | FS^* = 1] P(FS^* = 1)$, is not identified because of the classification error problem. The second part of Equation (2) is not identified because of both the selection and classification error problems. As discussed earlier, the data cannot reveal the counterfactual outcome distribution, $P[H(1) = 1 | FS^* = 0]$, regardless of whether participation is measured accurately, and, in the presence of classification errors, the sampling process does not reveal the proportion of respondents that received assistance, $P(FS^* = 1)$.

4. THE SELECTION PROBLEM

The literature evaluating the effect of SNAP on health has implicitly assumed that respondents accurately self-report program participation. To provide a direct comparison to the existing literature, we begin by focusing on this special case and study what can be learned about the ATE using existing methods. In Section 5 later, we develop new methods for simultaneously addressing the selection and classification error problems.

A natural starting point is to ask what can be learned in the absence of any assumptions invoked to address the selection problem (see Manski 1995; Pepper 2000). Since the latent probability $P[H(1) = 1 | FS^* = 0]$ must lie within [0,1], it follows that

$$P(H = 1, FS^* = 1) \leq P[H(1) = 1] \\ \leq P(H = 1, FS^* = 1) + P(FS^* = 0), \qquad (4)$$

where, in the absence of classification errors, the lower and upper bounds are identified by the sampling process. An analogous result applies for $P[H(0) = 1]$. In this worst-case scenario where there is no additional identifying information, the data alone cannot reveal whether SNAP leads to better or worse mean health outcomes (see Manski 1995 for further details).

4.1 MIV Models

To derive more informative inferences about the impact of SNAP on health, prior information to address the selection problem must be brought to bear. While the exogenous selection assumption $P[H(1) = 1] = P[H(1) = 1|FS^*]$ maintained in much of the literature seems untenable, there are a number of middle-ground assumptions that narrow the bounds by restricting the relationship between SNAP participation, health outcomes, and observed covariates. In this section, we apply two MIV assumptions that certain observed covariates are known to be monotonically related to the latent response variable.

First, we consider the MTS assumption (Manski and Pepper 2000) that children receiving food stamps are likely to have worse latent health outcomes, on average, than nonparticipants. MTS is a special case of MIV in which the treatment itself is a monotone instrument. This selection model formalizes the most common explanation for the positive association between participation and poor health: unobserved factors associated with poor health are thought to be positively associated with the decision to participate (e.g., Gundersen and Oliveira 2001; Currie 2003). For example, families may participate precisely because they expect to be food-insecure. Formally, the MTS assumption is given by

$$P[H(j) = 1|FS^* = 0] \leq P[H(j) = 1|FS^* = 1] \text{ for } j = 0, 1. \tag{5}$$

That is, for latent potential outcomes $H(0)$ and $H(1)$, eligible households that receive food stamps, $FS^* = 1$, have no better latent health outcomes on average than eligible households that do not receive food stamps, $FS^* = 0$. While the MTS assumption serves to reduce the upper bound on the ATE, the assumption alone does not identify the sign of the ATE (see Manski and Pepper 2000).

Second, we consider the relatively innocuous assumption that the latent probability of negative health outcomes weakly decreases with income adjusted for family composition. A large body of empirical research supports the idea of a negative gradient between reported income and the health outcomes studied in this article (e.g., Coleman-Jensen et al. [2011] for food insecurity, Case, Lubotsky, and Paxson [2002]; Deaton [2002] for general health, Newacheck [1994] for anemia, and Shrewsbury and Wardle [2008] and Jolliffe [2011] for obesity). To formalize this idea, let v be the MIV such that

$$u_1 < u < u_2 \text{ implies } P[H(t) = 1|v = u_2] \leq$$
$$P[H(t) = 1|v = u] \leq P[H(t) = 1|v = u_1] \quad \text{for } t = 1, 0. \tag{6}$$

These conditional probabilities can be bounded using the various nonparametric models described throughout this article. Let $LB(u)$ and $UB(u)$ be the known lower and upper bounds, evaluated at $v = u$, respectively, given the available information. Then, the MIV assumption formalized in Manski and Pepper (2000, Proposition 1) implies that

$$\sup_{u_2 \geq u} LB(u_2) \leq P[H(t) = 1|v = u] \leq \inf_{u_1 \leq u} UB(u_1).$$

Bounds on the unconditional latent probability, $P[H(t) = 1]$, can then be obtained using the law of total probability.

Following the approach developed in Kreider and Pepper (2007), we estimate these MIV bounds by first dividing the sample into equally sized groups (more than 200 observations per cell) delineated by an increasing ratio of income to the poverty line. Then, to find the MIV bounds on the rates of poor health outcomes, one takes the average of the plug-in estimators (weighted to account for the survey design) of lower and upper bounds across the different income groups observed in the data. We use 20 groups, although the qualitative results in this article are unchanged when we use 15 or 25 income groups. Since this MIV estimator is consistent but biased in finite samples (see Manski and Pepper 2000, 2009), we employ Kreider and Pepper's (2007) modified MIV estimator that accounts for the finite sample bias using a nonparametric bootstrap correction method. Chernozhukov, Lee, and Rosen (2011) formalize an entirely different approach for estimation, inference, and bias correction that involves applying a precision correction to their estimated boundary functions before taking the intersections. In the context of MIVs, their approach is designed to estimate the expected response functions conditional on a particular value of the instrumental variable (see Proposition 1 in Manski and Pepper 2000), but not the unconditional mean response function (see Corollary 1 in Manski and Pepper 2000) that is the focus of this article. Recent work by Hirano and Porter (2012) raises some concerns about applying bias corrections when estimating intersection bounds, showing that it may be impossible to completely eliminate bias and that reducing bias too much leads to large increases in variance. With more than 200 observations per cell, however, the bias correction in our application plays a relatively modest role in our estimates (see Table 2 and Figure 2).

4.2 Results for the No Errors Case

For each of the four outcomes, Table 2 presents bias-corrected bounds, confidence intervals, and estimated finite-sample biases under a variety of different models for the no errors case. In the first row, we make no assumptions about how eligible households select themselves into the program. The width of the ATE bounds always equals 1, and the bounds on the ATE always include 0 (see Manski 1995). These wide bounds highlight a researcher's inability to make strong inferences about the efficacy of the food stamps without making assumptions that address the problem of unknown counterfactuals. In the absence of restrictions that address the selection problem, we cannot rule out the possibility that SNAP has a large positive or negative impact on the likelihood of poor health outcomes. These bounds can be narrowed substantially, however, under common monotonic assumptions on treatment selection (MTS) and relationships between the latent outcome and observed instrumental variables (MIV).

To narrow the bounds, we apply the MTS and joint MTS–MIV assumptions. These results are presented in the middle two rows of Table 2. The MTS assumption alone is not strong enough to identify the sign of the impact of SNAP on the health outcomes, but it does dramatically reduce the upper bounds on the ATEs. For example, the upper bound for food insecurity falls from 0.555 to 0.093 such that ATE $\in [-0.445, 0.093]$. The upper bounds on the other three outcomes drop even further, falling to 0.015 for poor health, 0.012 for obesity, and 0.003 for anemia. Thus, under the MTS assumption alone, the estimated bounds

Table 2. Sharp bounds on the ATE of SNAP participation under no measurement error

	NHANES outcomes			
	Food insecurity	Poor health	Obesity	Anemia
Worst-case				
p.e.[a]	[−0.445, 0.555]	[−0.455, 0.545]	[−0.466, 0.534]	[−0.460, 0.540]
CI[b]	[−0.460 0.570]	[−0.470 0.559]	[−0.482 0.549]	[−0.474 0.555]
MTS				
p.e.	[−0.445, 0.093]	[−0.455, 0.015]	[−0.466, 0.012]	[−0.460, 0.003]
CI	[−0.460 0.139]	[−0.470 0.048]	[−0.482 0.050]	[−0.474 0.008]
MTS–MIV				
p.e.	[−0.366, −0.128]	[−0.398, −0.061]	[−0.411, −0.033]	[−0.391, −0.032]
CI	[−0.433 −0.034]	[−0.453 −0.005]	[−0.474 0.033]	[−0.450 0.006]
bias*	+0.027 −0.039	+0.019 −0.021	+0.022 −0.072	+0.008 −0.006
MTS–MIV–MTR				
p.e.	[−0.366, −0.149]	[−0.398, −0.061]	[−0.411, −0.041]	[−0.391, −0.034]
CI	[−0.433 −0.062]	[−0.453 −0.009]	[−0.474 0.000]	[−0.450 0.000]
bias	+0.027 −0.043	+0.019 −0.027	+0.022 −0.073	+0.008 −0.005
Food insecurity in the CPS				
Worst-case				
p.e.	[−0.399, 0.601]			
CI	[−0.412 0.614]			
MTS				
p.e.	[−0.399, 0.178]			
CI	[−0.501 0.406]			
MTS–MIV				
p.e.	[−0.376, 0.138]			
CI	[−0.412 0.205]			
bias	+0.025 −0.083			
MTS–MIV–MTR				
p.e.	[−0.376, −0.049]			
CI	[−0.412 0.000]			
bias	+0.025 −0.039			

NOTE: [a]Bias-corrected point estimates (p.e.) and [b]90% Imbens–Manski confidence intervals (CI) using 1000 pseudosamples.
*Estimated finite sample bias.

rule out the possibility that SNAP leads to large increases in poor health, obesity, and anemia. Instead, SNAP may lead to substantial reductions in these adverse health outcomes and, at worst, have slightly deleterious effects.

Perhaps the most important results are found when we combine the MTS assumption with the MIV assumption that the probability of poor health weakly decreases with family resources, as measured by the ratio of income to the poverty line. The upper bound is negative for all four outcomes in this joint MTS–MIV model, though the confidence interval includes zero for obesity and anemia. For example, the estimates suggest that SNAP reduces the prevalence of food insecurity by at least 12.8 points and poor health by 6.1 points. These numbers suggest that food stamps have substantial beneficial effects. In the absence of SNAP, our estimates of $P[H(0) = 1]$ indicate that at least 45.9% of eligible children would be food-insecure and 11.2% would be in poor health. Thus, the estimates indicate that the program has reduced the prevalence of food insecurity by at least 28% (= 12.8/45.9) and poor health by at least 54% (= 6.1/11.2).

4.3 MTR Model

Despite the observed positive correlations in the data between SNAP participation and unfavorable outcomes, there is a

general consensus among policymakers and researchers that SNAP does not increase the rate of food insecurity (Currie 2003). Given this general consensus, we consider the identifying power of the MTR assumption (Manski 1995, 1997; Pepper 2000) that formalizes the common idea that SNAP does not lead to a reduction in health status: $H(1) \leq H(0)$. In this case, the ATE of receiving food stamps must be nonpositive.

Since MTR rules out the possibility of deleterious effects of food stamps on health by assumption, it is not helpful in resolving the health outcomes paradox. Nevertheless, for outcomes in which MTR is thought to be credible, it can help shed light on the magnitudes of any identified beneficial effects of the program. In particular, MTR may interact with the MTS and MIV assumptions to bound the ATE further away from 0 than can be attained under the MTS–MIV assumptions alone.

For the food insecurity, general health, and anemia outcomes, the MTR assumption seems relatively innocuous in that it is difficult to imagine how receiving food stamps would lead to worse health outcomes. Previous work has demonstrated that each additional dollar in benefits leads to marginal increases in food expenditures (e.g., Breunig and Dasgupta 2002; Levedahl 1995). If food is a normal good, SNAP should weakly increase the consumption of food and, in turn, decrease the incidence

of food insecurity, poor or fair health, and anemia. For obesity, however, the assumption is more tenuous. Better access to nutritious foods through SNAP may lead to healthier eating and less obesity, but potential increases in caloric intake could result in weight gains.

The bottom rows of Table 2 present results under the joint MTS–MIV–MTR assumption. While the MTR assumption is less credible for obesity than for the other outcomes, we present MTS–MIV–MTR estimates for each outcome to make transparent the identifying power of the MTR assumption in each case. Without measurement error, the MTR assumption does not notably reduce the estimated upper bounds on the ATE relative to the estimates derived under the MTS–MIV assumptions alone. With classification errors (see Section 5), however, the MTR assumption turns out to have substantial identifying power in this application.

4.4 Sensitivity Analysis Using Data From the CPS

To assess the sensitivity of our findings to the data source, we estimate these models using analogous data from the December Supplement of the 2003 CPS. The CPS has been widely applied to evaluate the association between SNAP and food insecurity (e.g., Jensen 2002; Wilde and Nord 2005; Gundersen and Kreider 2008) and is used by the U.S. Department of Agriculture (USDA) to establish the official food insecurity rates for the United States (e.g., Coleman-Jensen et al. 2011).

Using the sampling design in Gundersen and Kreider (2008), our data for this sensitivity analysis include 2707 households with children reporting incomes less than 130% of the poverty line. As with the NHANES, we observe a self-reported measure of food stamp receipt over the past year, food insecurity over the past year, and the ratio of income to the poverty line. The CPS does not include information on the other three outcomes (poor general health, obesity, and anemia) revealed by the NHANES data. The summary statistics from the CPS data are similar to what we find in the NHAMES (see Table 2 in Gundersen and Kreider 2008): just over 40% of the households report receiving food stamps, and the food insecurity rate among self-report recipients is 17.9 percentage points higher than among eligible nonrecipients (52.3% vs. 34.4%).

The bottom panel of Table 2 presents estimates using the CPS data. The estimates are similar to what we find using data from the NHANES, although the estimated upper bound under the MIV–MTS model is positive. The estimated upper bound remains negative, however, when we impose the MIV–MTS–MTR assumption. In part, differences in estimates reflect the smaller sample size in the CPS data that leads to less precise estimates and larger bias corrections in the MIV models. Differences may also reflect the fact that classification errors in the CPS have been found to be more extensive than in the NHANES (Meyer, Mok, and Sullivan 2009).

Although these data have very limited information on health outcomes compared with the NHANES, they are rich enough to allow us to construct some standard instrumental variables for SNAP participation used in the existing literature. In particular, state identifiers in the CPS allow us to apply a more traditional instrumental variable (IV) assumption based on cross-state variation in program eligibility rules. To do so, we merge the Ur-

ban Institute's database of state program rules (see Finegold, Margrabe, and Ratcliffe 2006) with the CPS data to create two instrumental variables: an indicator for whether the state uses a simplified semiannual reporting requirement for earnings (47%) and an indicator for whether cars are exempted from the asset test (30%). Suppose these two variables have no impact on the expected food insecurity status except indirectly through SNAP participation. When combined with the traditional linear response model, the ATE is point-identified and the Wald estimator of the ATE ranges from -0.23 (when the indicator for whether cars count in the asset test is used as an IV) to -0.62 (when the indicator for whether the state uses a simplified reporting requirement is used as an IV). Notice that the -0.62 estimate lies outside of the worst-case bounds reported in Table 2, suggesting that either the IV assumption or the linear response model assumption is invalid.

If instead of applying the linear response model we estimate Shaikh and Vytlacil's (2011) nonparametric threshold-crossing model, SNAP is found to reduce food insecurity by at least 3 percentage points when using the asset test instrument and at least 5 points when using the reporting requirement instrument. The estimate based on the reporting requirement instrument is significantly different than 0 at the five-percent level, but the asset test instrument is not statistically significantly different than 0.

Combining these traditional instruments with sufficiently strong assumptions reveals consistent evidence that SNAP reduces the rate of food insecurity. Yet, while these estimated negative ATEs are qualitatively similar to our primary results found using the MIV and MTS assumptions, we caution against drawing strong conclusions on the efficacy of SNAP based on these findings alone. In particular, there may be good reasons to doubt the excludability assumption that the instruments are mean-independent of the latent food insecurity outcome. State-specific food insecurity rates are well-documented (e.g. Coleman-Jensen et al. 2011), and states may choose SNAP rules and regulations within USDA guidelines partially in response to food insecurity levels. Thus, these state program rules may not be independent of the state food insecurity rate. Finally, while the Shaikh and Vytlacil threshold-crossing model provides a constructive middle-ground model that allows one to impose some additional structure, the linear response model imposes the strong homogeneity restriction on the response function that seems unlikely to hold in practice. The finding that one of the linear IV estimates lies outside of the worst-case bounds suggests that this assumption may be violated.

5. THE SELECTION AND CLASSIFICATION ERROR MODEL: A UNIFIED APPROACH

While our findings thus far imply that SNAP plays an important role in improving children's health, we have not yet accounted for classification errors. In this section, we introduce new methods that explicitly acknowledge the presence of SNAP reporting errors and incorporate auxiliary administrative data on the size of the SNAP caseload to restrict the magnitudes and patterns of such errors.

With classification errors, FS^* is not observed and the Manski (1995) worst-case selection bounds are not identified. In

particular, defining $\Theta \equiv (\theta_1^- + \theta_0^+) - (\theta_0^- + \theta_1^+)$, we augment the Manski bounds as follows:

$$[-P(H = 1, FS = 0) - P(H = 0, FS = 1)] + \Theta$$
$$\leq ATE(1, 0) \leq [P(H=1, FS=1)$$
$$+ P(H = 0, FS = 0)] + \Theta. \tag{7}$$

Thus, without restrictions on the measurement error process, the false reporting rates, θ, are not identified, and the data are uninformative about the ATE. We use two sources of information to restrict Θ. First, auxiliary data on size of the SNAP caseload provides informative restrictions on the classification error components, θ. Second, the relevant validation literature provides informative restrictions on the magnitude and patterns of the classification error problem.

5.1 The Classification Error Model

To draw inferences in light of the classification error problem, we exploit two sources of additional information. First, we combine readily available auxiliary data on the size of the caseload from the administrative data collected by the USDA with survey data from the NHANES to estimate the true participation rate, $P(FS^* = 1)$. In Proposition 1 below, we show how knowledge of the true and the self-reported rates implies meaningful restrictions on the classification error probabilities, θ. In particular, knowledge of $P^* \equiv P(FS^* = 1)$ and $P \equiv P(FS = 1)$ implies the following three restrictions:

$$(\theta_1^- + \theta_0^-) - (\theta_1^+ + \theta_0^+) = \Delta, \tag{8a}$$
$$\theta_i^- \leq \min\{P(H = i, FS = 0), P(FS^* = 1)\} \equiv \theta_i^{UB-},$$
$$i = 1, 0 \tag{8b}$$
$$\theta_i^+ \leq \min\{P(H = i, FS = 1), P(FS^* = 0)\} \equiv \theta_i^{UB+},$$
$$i = 1, 0 \tag{8c}$$

where $\Delta \equiv P^* - P$. Equation (8a) restricts the net fraction of false negative reports to equal the difference in the true and self-reported participation rates. Equations (8b) and (8c) place meaningful upper bounds on the fraction of false negative and positive reports.

Second, the range of studies examining the validity of self-reports provides additional information on the degree of misreporting. As discussed earlier, evidence from validation studies finds errors of commission to be negligible, with the overall rate of misreporting estimated to be no greater than about 25%. To incorporate information on the overall rate of misreporting, we consider the identifying power of a restriction on the maximum amount of data corruption in the spirit of Horowitz and Manski (1995). That is, let

$$P(Z^* = 0) \leq Q_u, \tag{9}$$

where Q_u is a known upper bound on the degree of SNAP misclassification. Given knowledge of P^*, this value must logically lie within the range $[|P^* - P|, 1]$. In the polar case where Q_u is set equal to 1, the researcher is setting no restriction on the proportion of false reports in the data beyond that implied by restrictions (8a)–(8c). We refer to this as the "arbitrary errors model." In the other polar case where Q_u is set equal to $|P^* - P|$, the researcher is imposing a "no excess errors" restriction that there are no data errors beyond the proportion necessary to generate the discrepancy (distance) between the true participation

rate, P^*, and the reported rate, P. For the case of systematic underreporting in our application, $\Delta \geq 0$, setting Q_u equal to its minimum allowed value, $|P^* - P|$, is equivalent to imposing a "no false positives" assumption that respondents do not falsely claim to participate in the program (and similarly a "no false negatives" assumption in applications involving $\Delta \leq 0$). The no false positives assumption serves as a useful benchmark for the receipt of SNAP in our application since validation data suggest very few instances of households falsely claiming to receive food stamps (e.g., Bollinger and David 1997; Marquis and Moore 1990). Middle-ground positions are obtained by setting Q_u between $|P^* - P|$ and 1.

The restrictions in Equations (8) and (9) imply informative bounds on the unknown parameter, Θ, where the upper bound is found by maximizing $(\theta_1^- + \theta_0^+)$ and minimizing $(\theta_0^- + \theta_1^+)$, and vice versa for the lower bound. In particular, we derive the following bounds on Θ:

Proposition 1. Given restrictions (8a)–(8c) and (9),

$$\Theta \in \big[\max\{-Q_u, -2\theta_1^{UB+} - \Delta, -2\theta_0^{UB-} + \Delta\},$$
$$\min\{Q_u, 2\theta_1^{UB-} - \Delta, 2\theta_0^{UB+} + \Delta\} \big].$$

See Appendix B for a proof of this result.

Using this proposition, we can directly bound the ATE when there are classification errors. In particular, bounds on the ATE follow directly by combining the Proposition 1 bound on Θ with Equation (7). Notice that allowing for ambiguity created by the reporting error problem (weakly) widens the treatment effect bounds. At the same time, Θ might be bounded to lie in a strictly positive or negative range. Thus, the upper bound on the ATE can decrease, or the lower bound can increase, even as the overall width of the ATE bound expands. Applying Proposition 1 above to evaluate the impacts of the NSLP, Gundersen, Kreider, and Pepper (2012) provide an example in which it is easier to identify the sign of their ATE of interest in the presence of classification errors (when the error patterns are constrained as described above) than under the standard implicit assumption of perfectly measured data.

Additional analysis is required to address the classification error problem under the MTS assumption. Under this assumption, the upper bound on the ATE can be written as the difference of conditional means:

$$ATE(1, 0) \leq P(H = 1|FS^* = 1) - P(H = 1|FS^* = 0).$$

In the absence of classification errors, this upper bound is simply the difference in the observed poor health rate among recipients and nonrecipients. With classification errors, we can write

$$ATE(1, 0) \leq \frac{P(H = 1, FS = 1) + \theta_1^- - \theta_1^+}{P(FS^* = 1)}$$
$$- \frac{P(H = 1, FS = 0) + \theta_1^+ - \theta_1^-}{P(FS^* = 0)}, \tag{10}$$

where information on the true participation rate, $P(FS^* = 1)$, implies bounds on these conditional probabilities. In particular, we can narrow the Proposition 1 bounds as follows:

Proposition 2. Given the MTS assumption in Equation (5) and the classification error model restrictions in Equations (8)

and (9), it follows that

$$
\begin{aligned}
&\text{ATE}(1,0) \\
&\leq
\begin{cases}
\dfrac{P(H=0)}{P(FS^*=0)}, & \text{if } 0 < P(FS^*=1) \\
& \quad < P(H=1, FS=1), \\[2ex]
\dfrac{P(H=1, FS=1) + \theta_1^{UB-*}}{P(FS^*=1)} & \text{if } P(H=1, FS=1) \\
\quad - \dfrac{P(H=1, FS=0) - \theta_1^{UB-*}}{P(FS^*=0)}, & \quad \leq P(FS^*=1) < 1,
\end{cases}
\end{aligned}
$$

where $\theta_1^{UB-*} \equiv \min\{Q_u, P(H=1, FS=0), P(FS^*=1) - P(H=1, FS=1)\}$.

See Appendix B for a proof of this result.

Except for the true participation rate, $P^* = P(FS^*=1)$, all of the probabilities in Propositions 1 and 2 can be consistently estimated using data from the NHANES. To infer $P(FS^*=1)$, we combine auxiliary data on the size of the caseload with data from the NHANES on the size of the eligible population. Administrative data from the USDA reveals that from 2001–2006 there was an average of nearly 11 million children receiving food stamps per year (calculated using annual reports from Rosso 2002; Genser 2003; Cunnyngham and Brown 2004; Poikolainen 2005; Barrett 2006; Wolkwitz 2007). From the NHANES, we estimate that 22 million children were
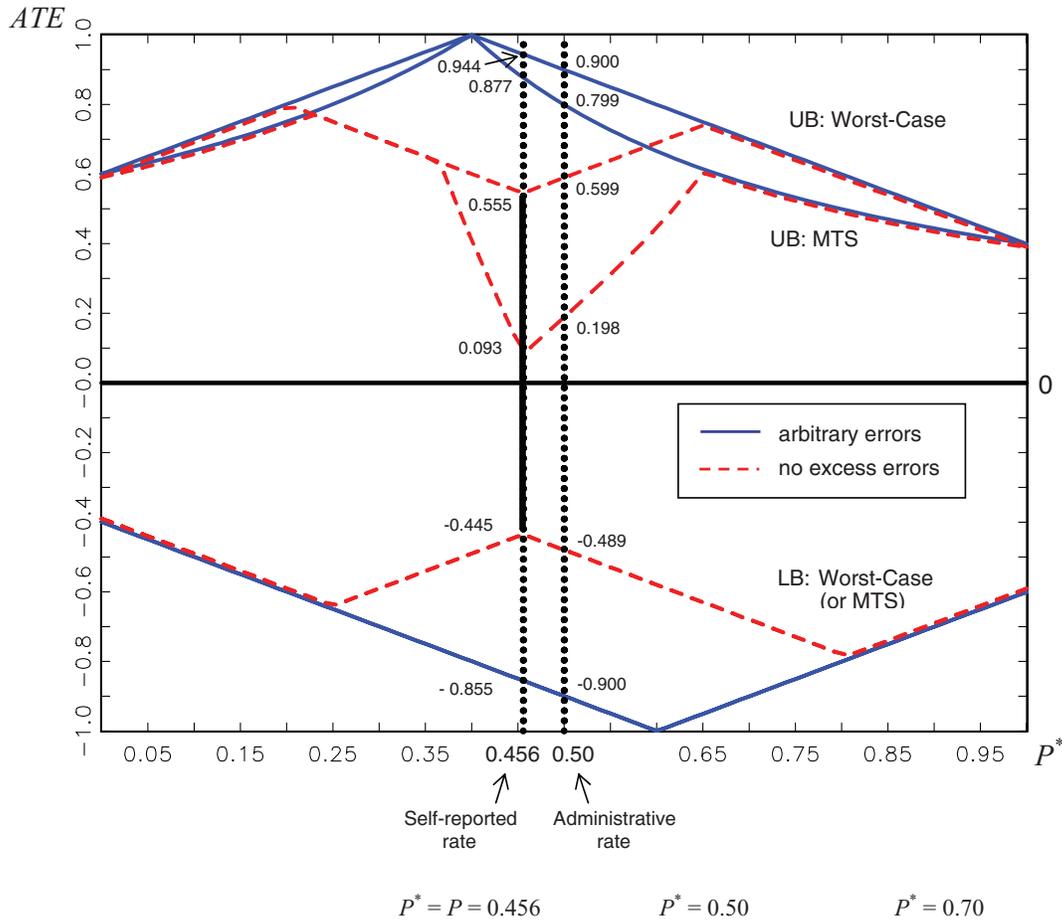
	$P^* = P = 0.456$	$P^* = 0.50$	$P^* = 0.70$
Worst-Case:			
Arbitrary errors p.e.[†]	[-0.855, 0.944]	[-0.900, 0.900]	[-0.900, 0.700]
CI[‡]	[-0.868 0.956]	[-0.912 0.912]	[-0.913 0.712]
No excess errors p.e.	[-0.445, 0.555]	[-0.489, 0.599]	[-0.689, 0.700]
CI	[-0.460 0.570]	[-0.506 0.619]	[-0.706 0.712]
MTS:			
Arbitrary errors p.e.	[-0.855, 0.877]	[-0.900, 0.799]	[-0.900, 0.571]
CI	[-0.868 0.904]	[-0.912 0.824]	[-0.913 0.589]
No excess errors p.e.	[-0.445, 0.093]	[-0.489, 0.198]	[-0.689, 0.571]
CI	[-0.460 0.139]	[-0.506 0.238]	[-0.706 0.589]

[†] Point estimates (p.e.) and [‡] 90% Imbens-Manski confidence intervals (CI) using 1,000 pseudosamples

Figure 1. Sharp bounds on the ATE for **food insecurity** as a function of P^*, the unobserved true SNAP participation rate: worst-case and MTS bounds. The online version of this figure is in color.

eligible to receive assistance. Thus, the implied participation rate is about 0.50, 4 points higher than the reported rate of 0.456. We note that a net false negative reporting rate of 4% is consistent with the results of Meyer, Mok, and Sullivan (2009) when evaluating misreporting of the SIPP, but much smaller

than found in the CPS and PSID. Using the CPS, for example, the participation rate is estimated to be around 70% (Cunnyngham 2005). Given this variability in the estimated participation rates and the possibility that errors in classifying eligible children may bias the estimated participation rates, we assess the
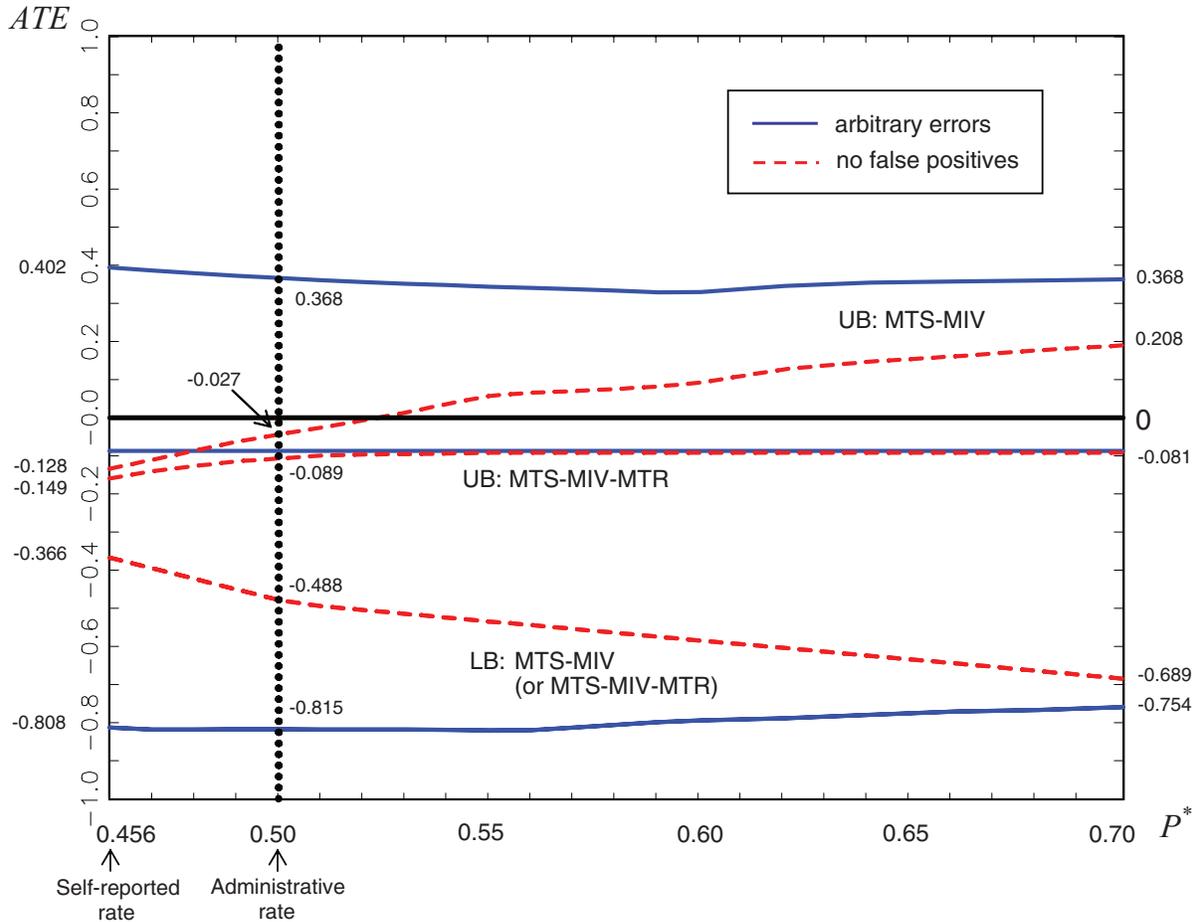
			$P^* = P = 0.456$	$P^* = 0.50$	$P^* = 0.70$
(a)	MTS-MIV, arbitrary errors	p.e.	[-0.808, 0.402]	[-0.815, 0.368]	[-0.754, 0.368]
		CI	[-0.874 0.543]	[-0.879 0.509]	[-0.816 0.511]
		bias†	+0.032 -0.051	+0.032 -0.046	+0.025 -0.054
(b)	MTS-MIV, no false positives	p.e.	[-0.366, -0.128]	[-0.488, -0.027]	[-0.689, 0.208]
		CI	[-0.433 -0.034]	[-0.553 0.074]	[-0.723 0.353]
		bias	+0.027 -0.039	+0.025 -0.039	+0.028 -0.063
(c)	MTS-MIV-MTR, arbitrary errors	p.e.	[-0.808, -0.081]	[-0.815, -0.081]	[-0.754, -0.081]
		CI	[-0.874 -0.015]	[-0.879 -0.015]	[-0.816 -0.014]
		bias	+0.032 -0.032	+0.032 -0.032	+0.025 -0.032
(d)	MTS-MIV-MTR, no false positives	p.e.	[-0.366, -0.149]	[-0.488, -0.089]	[-0.689, -0.081]
		CI	[-0.433 -0.062]	[-0.553 -0.013]	[-0.723 -0.013]
		bias	+0.027 -0.043	+0.025 -0.042	+0.028 -0.032

† corrected finite sample bias

Figure 2A. Sharp bounds on the ATE for **food insecurity** as a function of P^*, the unobserved true SNAP participation rate: MTS–MIV and MTS–MIV–MTR bounds. The online version of this figure is in color.

sensitivity of the bounds to variation in the true participation rate.

5.2 Results

Our analytical approach allows us to trace out sharp bounds on the ATE under different assumptions about selection and measurement error. To do so, we evaluate the bounds as a function of the unknown SNAP participation rate, P^*, under various assumptions about the selection process. By layering successively stronger assumptions, our analysis reveals how the strength of

the conclusions varies with the strength of the identifying assumptions. We begin in Section 5.2.1 by focusing on bounding the impact of SNAP participation on food insecurity. We then extend the discussion to the three other health outcomes in Section 5.2.2.

5.2.1 Food Insecurity. Figure 1 traces out the estimated Proposition 1 and 2 bounds—that is, the worst-case and MTS bounds—for the ATE on the food insecurity rate across all values of P^* between 0 and 1. The accompanying table highlights these results for P^* equal to (a) the NHANES self-reported
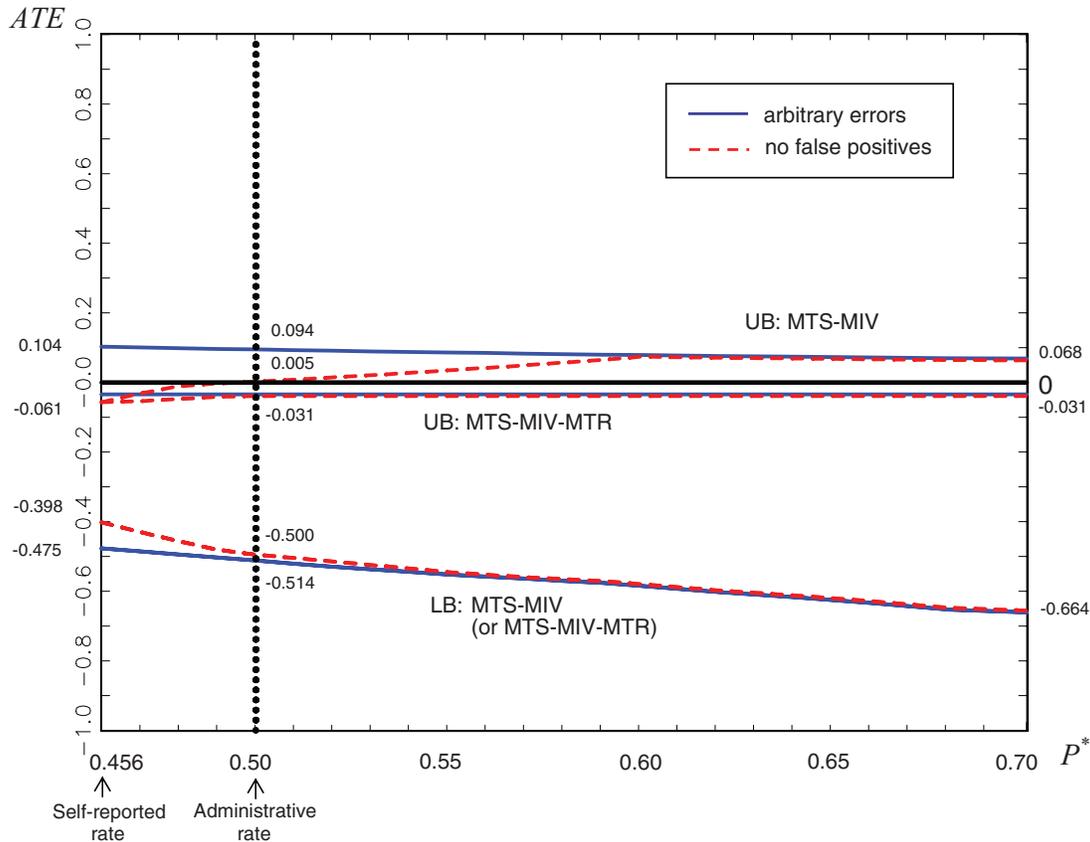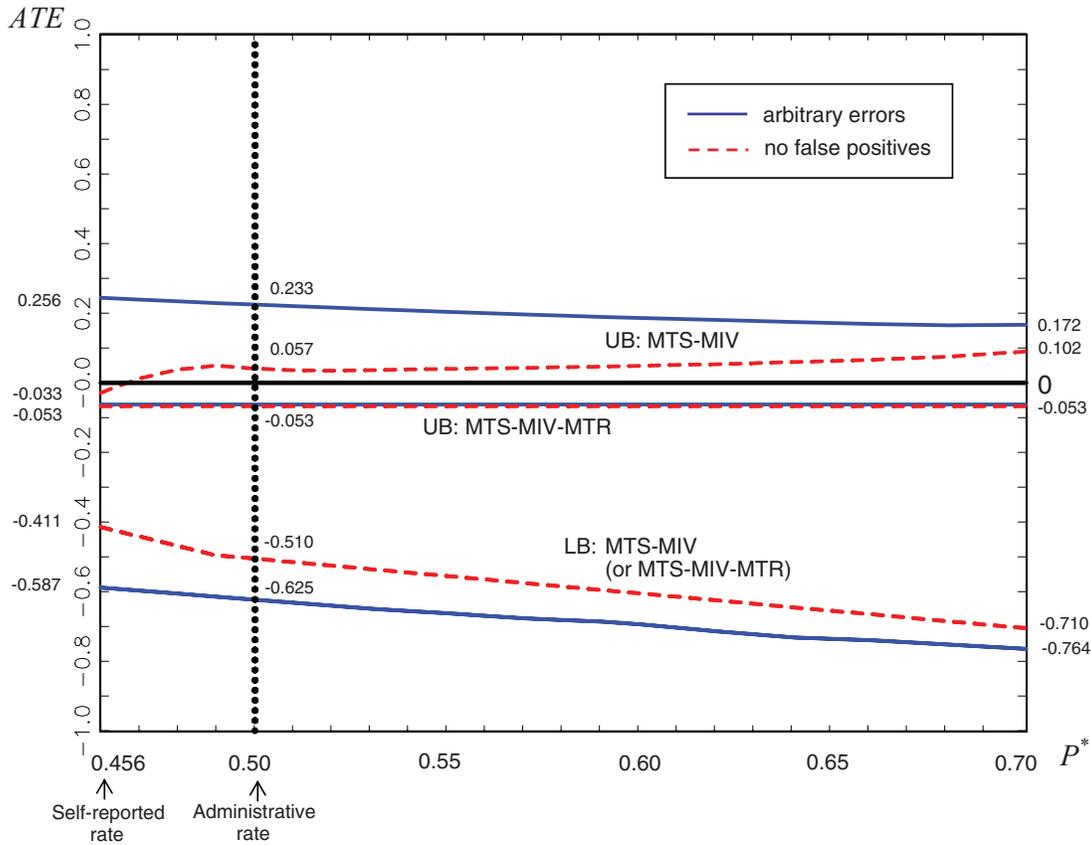

			$P^* = P = 0.456$	$P^* = 0.50$	$P^* = 0.70$
(a)	MTS-MIV, arbitrary errors	p.e.	[-0.475, 0.104]	[-0.514, 0.094]	[-0.664, 0.068]
		CI	[-0.529 0.133]	[-0.572 0.122]	[-0.745 0.088]
		bias	+0.008 -0.010	+0.010 -0.009	+0.017 -0.006
(b)	MTS-MIV, no false positives	p.e.	[-0.398, -0.061]	[-0.500, 0.005]	[-0.664, 0.068]
		CI	[-0.453 -0.005]	[-0.552 0.075]	[-0.739 0.100]
		bias	+0.019 -0.021	+0.022 -0.014	+0.017 -0.010
(c)	MTS-MIV-MTR, arbitrary errors	p.e.	[-0.475, -0.031]	[-0.514, -0.031]	[-0.664, -0.031]
		CI	[-0.529 0.000]	[-0.572 0.000]	[-0.745 0.000]
		bias	+0.008 -0.025	+0.010 -0.025	+0.017 -0.025
(d)	MTS-MIV-MTR, no false positives	p.e.	[-0.398, -0.061]	[-0.500, -0.031]	[-0.664, -0.031]
		CI	[-0.453 -0.009]	[-0.552 0.000]	[-0.739 0.000]
		bias	+0.019 -0.027	+0.022 -0.026	+0.017 -0.025

Figure 2B. Sharp bounds on the ATE for **poor health** as a function of P^*, the unobserved true SNAP participation rate: MTS–MIV and MTS–MIV–MTR bounds. The online version of this figure is in color.

participation rate of $P = 0.456$, (b) our preferred estimated true participation rate of 0.50 based on administrative data from the USDA, and (c) a higher rate of 0.70 chosen to be consistent with the participation rate found using the CPS (Cunnyngham 2005). The solid lines in the figures trace out the estimated arbitrary error bounds (i.e., $Q_u = 1$) in which there are no restrictions imposed on the nature or degree of errors except those implied by the knowledge of P and P^* as captured by Equations (8a)–(8c). The dashed lines display the estimated bounds under the further restriction of no excess errors: $Q_u = |P^* - P|$. Recall that for the underreporting cases (the most relevant cases in our application) in which P^* lies to the right of $P = 0.456$, this no excess

errors model is equivalent to no false positive reports. The table also provides Imbens and Manski (2004) confidence intervals that cover the true value of the ATE with 90% probability.

As noted above, the worst-case bounds on the ATE if SNAP receipt is accurately reported ($P^* = P$ and no excess errors), have a width of 1 and always include 0. For food insecurity, these worst-case no error bounds are $[-0.445, 0.555]$ as depicted in the figure by the solid vertical line at $P^* = P$ (see also Table 2). Allowing for classification errors notably increases the width of these bounds. For example, suppose the true participation rate remains equal to the self-reported rate of 0.456, but now one only imposes the assumption of no *net* reporting errors
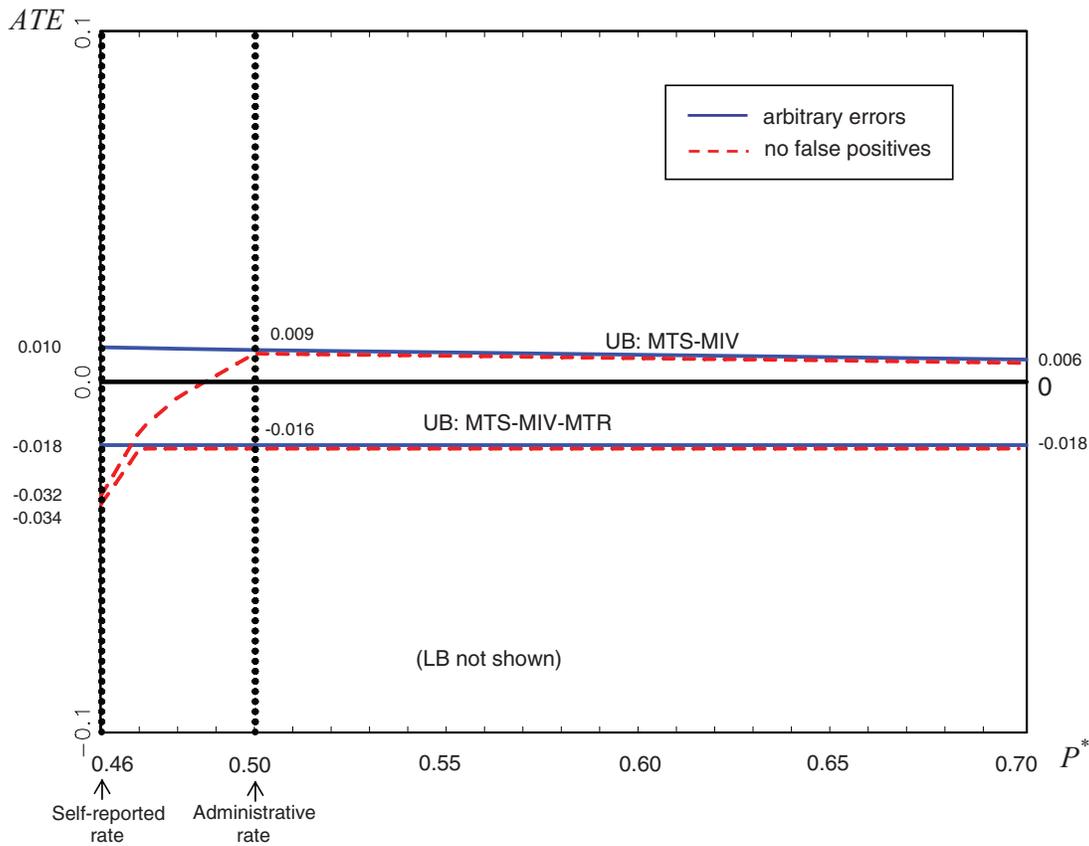

		$P^* = P = 0.456$	$P^* = 0.50$	$P^* = 0.70$
(a) MTS-MIV, arbitrary errors	p.e.	[-0.587, 0.256]	[-0.625, 0.233]	[-0.764, 0.172]
	CI	[-0.652 0.361]	[-0.695 0.329]	[-0.852 0.222]
	bias	+0.011 -0.074	+0.013 -0.056	+0.034 -0.020
(b) MTS-MIV, no false positives	p.e.	[-0.411, -0.033]	[-0.510, 0.057]	[-0.710, 0.102]
	CI	[-0.474 0.033]	[-0.564 0.153]	[-0.772 0.218]
	bias	+0.022 -0.072	+0.023 -0.066	+0.034 -0.047
(c) MTS-MIV-MTR, arbitrary errors	p.e.	[-0.587, -0.053]	[-0.625, -0.053]	[-0.764, -0.053]
	CI	[-0.652 0.000]	[-0.695 0.000]	[-0.852 0.000]
	bias	+0.011 -0.045	+0.013 -0.045	+0.034 -0.045
(d) MTS-MIV-MTR, no false positives	p.e.	[-0.411, -0.053]	[-0.510, -0.053]	[-0.710, -0.053]
	CI	[-0.474 0.000]	[-0.564 0.000]	[-0.772 0.000]
	bias	+0.022 -0.073	+0.023 -0.049	+0.034 -0.045

Figure 2C. Sharp bounds on the ATE for **obesity** as a function of P^*, the unobserved true SNAP participation rate: MTS–MIV and MTS–MIV–MTR bounds. The online version of this figure is in color.

such that the rate of false positives equals the rate of false negatives. Then, as shown in Figure 1 and the accompanying table, the ATE bounds on the food insecurity rate expand from $[-0.445, 0.555]$ to $[-0.855, 0.944]$, with a width of 1.799. If the true participation rate is 0.50 (the rate consistent with the USDA administrative data) instead of 0.456, the bounds change to $[-0.900, 0.900]$ with a width of 1.800. These findings reveal the important negative result that the ambiguity created by classification errors can be substantial even if the true and self-reported rates are similar.

The upper bounds when P^* is near P, however, are markedly reduced by introducing the MTS assumption, especially when

combined with the no excess errors assumption (no false-positives when $P^* \geq P$). With $P^* = 0.50$, for example, the no false positives bounds are $[-0.489, 0.599]$, with a width of 1.088. Adding the MTS assumption further reduces the upper bound to 0.198. Thus, the no false positives assumption decreases the ambiguity associated with measurement error from 1.800 to 1.088, a 40% reduction, and the MTS assumption further reduces the width of the bound to 0.687, more than 60% narrower than the width of the worst-case bounds when $P^* = 0.50$. While these two assumptions have substantial identifying power in this application, these wide bounds presented in Figure 1 highlight the difficulty of making strong inferences

			$P^* = P = 0.46$	$P^* = 0.50$	$P^* = 0.70$
(a)	MTS-MIV, arbitrary errors	p.e.	$[-0.402, 0.010]$	$[-0.437, 0.009]$	$[-0.601, 0.006]$
		CI	$[-0.460 \quad 0.016]$	$[-0.499 \quad 0.014]$	$[-0.689 \quad 0.010]$
		bias	$+0.005 \quad -0.003$	$+0.005 \quad -0.002$	$+0.007 \quad -0.002$
(b)	MTS-MIV, no false positives	p.e.	$[-0.391, -0.032]$	$[-0.437, 0.009]$	$[-0.601, 0.006]$
		CI	$[-0.450 \quad 0.006]$	$[-0.498 \quad 0.028]$	$[-0.688 \quad 0.010]$
		bias	$+0.008 \quad -0.006$	$+0.005 \quad -0.008$	$+0.007 \quad -0.002$
(c)	MTS-MIV-MTR, arbitrary errors	p.e.	$[-0.402, -0.018]$	$[-0.445, -0.016]$	$[-0.601, -0.018]$
		CI	$[-0.460 \quad 0.000]$	$[-0.499 \quad 0.000]$	$[-0.689 \quad 0.000]$
		bias	$+0.005 \quad -0.004$	$+0.005 \quad -0.004$	$+0.007 \quad -0.004$
(d)	MTS-MIV-MTR, no false positives	p.e.	$[-0.391, -0.034]$	$[-0.437, -0.018]$	$[-0.601, -0.018]$
		CI	$[-0.450 \quad 0.000]$	$[-0.498 \quad 0.000]$	$[-0.688 \quad 0.000]$
		bias	$+0.008 \quad -0.005$	$+0.005 \quad -0.004$	$+0.007 \quad -0.004$

Figure 2D. Sharp bounds on the ATE for **anemia** as a function of P^*, the unobserved true SNAP participation rate: MTS–MIV and MTS–MIV–MTR bounds. The online version of this figure is in color.

in light of the selection and measurement error problems. In the absence of additional restrictions that address the selection problem, we cannot rule out the possibility that SNAP has a large positive or negative impact on the likelihood of poor health.

To narrow the bounds, we assess the identifying power of the joint MTS–MIV and joint MTS–MIV–MTR assumptions. These results are traced out in Figure 2(A) and the corresponding table for the most relevant cases in our application in which P^* lies between 0.456 and 0.700. In drawing this figure, we assume that the fraction of misreporting does not vary across MIV groups. Focusing on the no-false positive classification error model, we begin by combining the MTS assumption with our MIV assumption that the probability of good health weakly increases with family resources, as measured by the ratio of income to the poverty line. In this joint MTS–MIV model, we can often sign the ATE as strictly negative without imposing the MTR assumption. Specifically, Figure 2(A) reveals that we can identify the ATE to be negative as long as food stamp misreporting is confined to no more than about 6% of the households, ranging from a 12.8% reduction at $P^* = 0.456$ to no effect at $P^* = 0.52$. When $P^* = 0.50$, the estimates imply that SNAP reduces food insecurity by at least 2.7 percentage points, although this upper bound is not statistically different than zero at the 10% significance level.

Under the joint MTR–MTS–MIV assumption, the ATE is strictly negative even for large degrees of arbitrary food stamp misreporting. Under this joint assumption, our estimated bounds on the ATE vary from $[-0.808, -0.081]$, when $P^* = 0.456$, to $[-0.754, -0.081]$, when $P^* = 0.70$. In all cases, the estimates are statistically different than zero at the 10% significance level. Thus, under this model, we find that SNAP reduces the food insecurity rate by at least 8 percentage points and perhaps much more. These results suggest that SNAP dramatically improves the likelihood of becoming food secure.

5.2.2 Other Health Outcomes. We also consider what can be learned about the effects of food stamps on the three other negative health outcomes: self-reported fair/poor general health, childhood obesity, and anemia. For brevity, we concentrate on results for cases when we impose the MTS–MIV and MTS–MIV–MTR models. These results are summarized in Figures 2(B)–2(D).

As above for the case of food insecurity, we can identify strictly negative ATEs for each health outcome under the joint MTS–MIV assumption for sufficiently small degrees of food stamp reporting error. For example, without any errors—that is, when $P^* = 0.456$ with no excess errors—the ATEs for fair/poor health, obesity, and anemia are identified to be no greater than -0.061, -0.033, and -0.032, respectively. Identification of the ATE decays rapidly with P^* for each of the health outcomes, however, and each upper bound becomes positive if fewer than 4% of the households might misreport food stamp participation status (under either arbitrary errors or no false positives). Still, if the true participation rate is 0.50 (the rate consistent with the USDA administrative data) under no false positives, the estimated upper bounds rule out the possibility that SNAP substantially increases the incidence of these poor health outcomes. For example, the estimated bounds on the rate of poor general health is $[-0.500, 0.005]$. Thus, these results imply that

SNAP may dramatically improve childhood health, as measured by poor health, obesity, and anemia, with little downside risk that the program instead has a deleterious average effect.

Finally, to shed additional light on the magnitudes of any identified beneficial effects of the program, we apply the MTR assumption that SNAP cannot lead to worse health outcomes. Under the joint MTS–MIV–MTR model, we estimate strictly negative (beneficial) and substantial impacts for each health outcome across all values of $P^* \in [0.456, 0.700]$, even for the case of arbitrary reporting errors. In particular, at $P^* = 0.50$ we find that SNAP reduces the rate of poor general health by at least 0.031 (from 0.089 to 0.058), obesity by at least 0.053 (from 0.218 to 0.165), and anemia by at least 0.016 (from 0.020 to 0.004).

6. CONCLUSION

The literature assessing the efficacy of SNAP has long puzzled over its positive associations with various undesirable health-related outcomes such as food insecurity. These associations are often ascribed to the self-selection of less healthy households into SNAP. Identification of the causal impacts of participation on health status is also confounded by systematic underreporting of food stamp recipiency. In this article, we reconsidered the impact of SNAP on child food insecurity and other health outcomes by developing methods that account for both of these identification problems in a single unifying framework. Within this framework, we combine information from household self-reports with administrative data on the size of the SNAP caseload to derive formal restrictions on the magnitudes and patterns of household reporting errors. While introducing measurement error widens Manski's (1995) classic worst-case selection bounds, we show how restricting the magnitudes and patterns of errors can, in some cases, make it easier to sign the ATE than under the standard implicit assumption of perfectly measured data. Acknowledging the presence of nonrandom measurement error need not necessarily hinder inference on the sign of the ATE.

Our partial identification approach is well-suited for this application in which conventional assumptions strong enough to point-identify the causal impacts are not necessarily credible and there remains much uncertainty about even the qualitative impacts of SNAP. Using data from the NHANES, we make transparent how assumptions on the selection and reporting error processes shape inferences about the causal impacts of SNAP on health outcomes. The worst-case selection bounds always include zero, and classification errors can generate substantial additional uncertainty about the efficacy of SNAP in alleviating food insecurity and other health outcomes. This ambiguity, however, is substantially mitigated by applying relatively weak assumptions on the selection and classification error processes.

Our middle-ground MTS–MIV model allows us to identify strictly beneficial impacts of SNAP on food insecurity and other health outcomes as long as the degree of misreported participation status is not too large. In the absence of measurement error, the joint MTS–MIV model reveals that SNAP reduces the prevalence of food insecurity by at least 12.8 percentage points (28%), from 0.459 to 0.331. The strength of this finding naturally weakens when we allow for misclassified participation

status. Nevertheless, we can still identify that SNAP reduces the prevalence of food insecurity by at least 2.7 percentage points (6.5%), from 0.417 to 0.390, when allowing for errors of omission in participation status to be consistent with the estimated true participation rate of $P^* = 0.50$. Identification decays rapidly with the degree of misreporting, however, and confidence intervals for the ATE include 0 unless it can be known that only a small fraction of households misreport.

Under the stronger joint MTS–MIV–MTR model, which may be less credible for obesity than the other health outcomes, the basic conclusion that SNAP improves health outcomes holds even for large degrees of measurement error. Given that errors of omission are consistent with $P^* = 0.50$, SNAP is estimated to reduce the prevalence of child food insecurity by at least 8.1 percentage points, poor general health by 3.1 percentage points, obesity by 5.3 percentage points, and anemia by 1.6 percentage points. These impacts are significantly different from 0 for food insecurity and poor health. Our findings suggest that at least some of the potentially troubling correlations between SNAP and poor health outcomes provide a misleading picture of the true impact of SNAP. The program appears to lead to at least modest reductions in food insecurity and other poor health outcomes, with little downside risk that the program has significant deleterious effects. Our estimated bounds are also consistent with the possibility that SNAP dramatically improves the well-being of children in the United States.

APPENDIX A

Table A1. Food insecurity questions in the core food security module

(1) "We worried whether our food would run out before we got money to buy more." Was that **often, sometimes**, or never true for you in the last 12 months?

(2) "The food that we bought just didn't last and we didn't have money to get more." Was that **often, sometimes**, or never true for you in the last 12 months?

(3) "We couldn't afford to eat balanced meals." Was that **often, sometimes**, or never true for you in the last 12 months?

(4) "We relied on only a few kinds of low-cost food to feed our children because we were running out of money to buy food." Was that **often, sometimes**, or never true for you in the last 12 months?

(5) In the last 12 months, did you or other adults in the household ever cut the size of your meals or skip meals because there wasn't enough money for food? (**Yes**/No)

(6) "We couldn't feed our children a balanced meal, because we couldn't afford that." Was that **often, sometimes**, or never true for you in the last 12 months?

(7) In the last 12 months, did you ever eat less than you felt you should because there wasn't enough money for food? (**Yes**/No)

(8) (If yes to Question 5) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

(9) "The children were not eating enough because we just couldn't afford enough food." Was that **often, sometimes**, or never true for you in the last 12 months?

(10) In the last 12 months, were you ever hungry, but didn't eat, because you couldn't afford enough food? (**Yes**/No)

(11) In the last 12 months, did you lose weight because you didn't have enough money for food? (**Yes**/No)

(12) In the last 12 months, did you ever cut the size of any of the children's meals because there wasn't enough money for food? (**Yes**/No)

(13) In the last 12 months did you or other adults in your household ever not eat for a whole day because there wasn't enough money for food? (**Yes**/No)

(14) In the last 12 months, were the children ever hungry but you just couldn't afford more food? (**Yes**/No)

(15) (If yes to Question 13) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

(16) In the last 12 months, did any of the children ever skip a meal because there wasn't enough money for food? (**Yes**/No)

(17) (If yes to Question 16) How often did this happen—**almost every month, some months but not every month**, or in only 1 or 2 months?

(18) In the last 12 months did any of the children ever not eat for a whole day because there wasn't enough money for food? (**Yes**/No)

NOTE: Responses in bold indicate an affirmative response.

APPENDIX B

Proof of Proposition 1. Subject to the restrictions in Equations (8) and (9), the upper bound is found by maximizing $(\theta_1^- + \theta_0^+)$ and minimizing $(\theta_0^- + \theta_1^+)$, and vice versa for the lower bound.

For $\Delta \geq 0$: For the upper bound, first consider the case that $\theta_1^{UB-} \geq \Delta$. Then, $(\theta_0^- + \theta_1^+)$ is minimized at zero, and Equation (8a) simplifies to $\theta_1^- - \theta_0^+ = \Delta$. Given Equations (8a)–(8c), we know that θ_0^+ cannot exceed $\min\{\theta_0^{UB+}, \theta_1^{UB-} - \Delta\}$ and θ_1^- cannot exceed $\min\{\Delta + \theta_0^{UB+}, \theta_1^{UB-}\}$. From Equation (9), we know that $\theta_1^- + \theta_0^+$ cannot exceed Q_u. The upper bound follows directly. Second, consider the case that $\theta_1^{UB-} < \Delta$. From Equation (8b), we know that θ_1^- cannot exceed θ_1^{UB-} and, to satisfy the restriction in Equation (8a), θ_0^- must be no less than $\Delta - \theta_1^{UB-}$. As before, θ_1^+ is minimized at 0. From Equation (8c), we know that θ_0^+ can exceed 0 but Equation (8a) implies that that any conjectured increase in the false-positive error rate must be offset by an equivalent increase in the false-negative error rate. So, in this case, the upper bound would remain unchanged by increasing θ_0^+ above zero. Thus, we have the upper bound of $2\theta_1^{UB-} - \Delta$ which can be shown to be no greater than $2\theta_0^{UB+} + \Delta$.

For the lower bound, first consider the case that $\theta_0^{UB-} \geq \Delta$. Then $(\theta_1^- + \theta_0^+)$ is minimized at zero, and Equation (8a) simplifies to $\theta_0^- - \theta_1^+ = \Delta$. Given (8a)–(8c), we know that θ_1^+ cannot exceed $\min\{\theta_1^{UB+}, \theta_0^{UB-} - \Delta\}$ and θ_0^- cannot exceed $\min\{\theta_1^{UB+} + \Delta, \theta_0^{UB-}\}$. From Equation (9), we know that $\theta_0^- + \theta_1^+$ cannot exceed Q_u. It follows that $\max\{-Q_u, -2\theta_1^{UB+} - \Delta, -2\theta_0^{UB-} + \Delta\}$ provides a lower bound on Θ. Second, consider the case that $\theta_0^{UB-} < \Delta$. From Equation (8b), we know that θ_0^- cannot exceed θ_0^{UB-} and, to satisfy the restriction in Equation (8a), θ_1^- must be no less than $\Delta - \theta_0^{UB-}$. As before, θ_0^+ is minimized at zero. From Equation (8c), we know that θ_1^+ can exceed zero but Equation (8a) implies that any conjectured increase in the false-positive error rate must be offset by an equivalent increase in the false-negative error. So, in this case, the lower bound would be unchanged by increasing θ_1^+ above zero. Thus, we have the lower bound of $-2\theta_0^{UB-} + \Delta$ which can be shown to be no smaller than $-2\theta_1^{UB+} - \Delta$.

For $\Delta \leq 0$: For the upper bound, first consider the case that $\theta_0^{UB+} \geq -\Delta$. Then, $(\theta_0^- + \theta_1^+)$ is minimized at zero, and Equation (8a) simplifies to $\theta_1^- - \theta_0^+ = \Delta$. Given Equations (8a)–(8c), we know that θ_1^- cannot exceed $\min\{\theta_1^{UB-}, \theta_0^{UB+} + \Delta\}$ and θ_0^+ cannot exceed $\min\{\theta_1^{UB-} - \Delta, \theta_0^{UB+}\}$. From Equation (9), we know that $\theta_1^- + \theta_0^+$ cannot exceed Q_u. The upper bound follows directly. Second, consider the case where $\theta_0^{UB+} < -\Delta$. From Equation (8b), we know that θ_0^+ cannot exceed θ_0^{UB+} and, to satisfy the restriction in Equation (8a), θ_1^+ must be no less than $-\Delta - \theta_0^{UB+}$. As before, θ_0^- is minimized at 0. From Equation (8c), we know that θ_1^- can exceed 0 but the restriction in Equation (8a) implies that that any conjectured increase in the false-negative error rate must be offset by an equivalent increase in the false-positive error rate. So, in this case, the upper bound would be unchanged by increasing θ_1^- above 0. Thus, we have the upper bound of $2\theta_0^{UB+} + \Delta$, which can be shown to be no greater than $2\theta_1^{UB-} - \Delta$.

For the lower bound, first consider the case that $\theta_1^{UB+} \geq -\Delta$. Then $(\theta_1^- + \theta_0^+)$ is minimized at 0 and Equation (8a) simplifies to $\theta_0^- - \theta_1^+ = \Delta$. Given (8a)–(8c), we know that θ_0^- cannot exceed $\min\{\theta_0^{UB-}, \theta_1^{UB+} + \Delta\}$ and θ_1^+ cannot exceed $\min\{\theta_0^{UB-} - \Delta, \theta_1^{UB+}\}$. From Equation (9), we know that $(\theta_0^-) + (\theta_1^+)$ cannot exceed Q_u. It follows that $\max\{-Q_u, -2\theta_1^{UB+} - \Delta, -2\theta_0^{UB-} + \Delta\}$ provides a lower bound on Θ. Second, consider the case that $\theta_1^{UB+} < -\Delta$. From Equation (8b), we know that θ_1^+ cannot exceed θ_1^{UB+} and, to satisfy the restriction in Equation (8a), θ_0^+ must be no less than $-\Delta - \theta_1^{UB+}$. As before, θ_1^- is minimized at zero. From Equation (8c), we know that θ_0^- can exceed zero but Equation (8a) implies that any conjectured increase in the false-negative error rate must be offset by an equivalent increase in the false-positive error rate. So, in this case, the lower bound would be unchanged by increasing θ_0^- above zero. Thus, we have the lower bound of $-2\theta_1^{UB+} - \Delta$, which can be shown to be no smaller than $-2\theta_0^{UB-} + \Delta$.

Proof of Proposition 2. The objective is to maximize θ_1^- and minimize θ_1^+, subject to each conditional probability lying between 0 and 1 as well as the constraints in Equations (8) and (9).

Case (1): When $P^* < P(H = 1, FS = 1)$, the first ratio in Equation (10) exceeds 1 unless θ_1^+ is at least as large as $\theta_1^{+*} \equiv P(H = 1, FS = 1) - P^*$. At this value, the first ratio still exceeds 1 unless $\theta_1^{-*} = 0$. The upper bound for this case is maximized at θ_1^{+*} and θ_1^{-*}, with the difference in Equation (10) reducing to $\frac{P(H=0)}{P(FS^*=0)}$. All constraints in Equations (8) and (9) are satisfied. Case (2): When $P^* \geq P(H = 1, FS = 1)$, set $\theta_1^{+*} = 0$ and recall that $\theta_1^- \leq \min\{Q_u, P(H = 1, FS = 0), P(FS^* = 1)\}$ by Equations (8b) and (9). Also, the first ratio in Equation (10) exceeds 1 unless $\theta_1^- \leq P(FS^* = 1) - P(H = 1, FS = 1)$, which is nonnegative by assumption in Case (2). Thus, set

$$\theta_1^{-*} = \min\{Q_u, P(H = 1, FS = 0), P(FS^* = 1), P(FS^* = 1)$$
$$- P(H = 1, FS = 1)\}$$
$$= \min\{P(H = 1, FS = 0), P(FS^* = 1) - P(H = 1, FS = 1)\}.$$

Again, all of the constraints in Equations (8) and (9) are satisfied.

[Received December 2009. Revised February 2011.]

REFERENCES

Barrett, A. (2006), "Characteristics of Food Stamp Households: Fiscal Year 2005," Report, U.S. Department of Agriculture, Food, and Nutrition Service, Alexandria, VA, Report No. FSP-06-CHAR. [967]

Bitler, M., Currie, J., and Scholz, J. (2003), "WIC Eligibility and Participation," *Journal of Human Resources*, 38, 1139–1179. [960]

Bollinger, C. (1996), "Bounding Mean Regressions When a Binary Variable Is Mismeasured," *Journal of Econometrics*, 73 (2), 387–399. [959]

Bollinger, C., and David, M. (1997), "Modeling Discrete Choice With Response Error: Food Stamp Participation," *Journal of the American Statistical Association*, 92 (439), 827–835. [958,960,966]

Breunig, R., and Dasgupta, I. (2002), "Do Intra-Household Effects Generate the Food Stamp Cash-Out Puzzle?" *American Journal of Agricultural Economics*, 84 (4), 552–568. [964]

Case, A., Lubotsky, D., and Paxson, C. (2002), "Economic Status and Health in Childhood: The Origins of the Gradient," *American Economic Review*, 92 (5), 1308–1334. [963]

Chernozhukov, V., Lee, S., and Rosen, A. M. (2009), "Intersection Bounds: Estimation and Inference," Working Paper, Department of Economics, Massachusetts Institute of Technology, Institute for Fiscal Studies, UCL, CEMMAP working paper 31/11. [963]

Coleman-Jensen, A., Nord, M., Andrews, M., and Carlson, S. (2011), "Household Food Security in the United States in 2010," Economic Research Report ERR-125, Report, U.S. Department of Agriculture, Economic Research Service, Washington, DC. [958,961,963,965]

Cunnyngham, K. (2005), "Food Stamp Program Participation Rates: 2003," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Reference No. 6044-209. [968,970]

Cunnyngham, K., and Brown, B. (2004), "Characteristics of Food Stamp Households: Fiscal Year 2003," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. FSP-06-CHAR. [967]

Currie, J. (2003), "U.S. Food and Nutrition Programs," in *Means Tested Transfer Programs in the United States*, ed. R. Moffitt, Chicago, IL: University of Chicago Press, pp. 199–289. [958,959,963,964]

Deaton, A. (2002), "Policy Implications of the Gradient of Health and Wealth," *Health Affairs*, 21 (2), 13–30. [963]

DePolt, R. A., Moffitt, R. A., and Ribar, D. C. (2009), "Food Stamps, Temporary Assistance for Needy Families and Food Hardships in Three American Cities," *Pacific Economic Review*, 14, 445–473. [959]

Finegold, K., Margrabe, W., and Ratcliffe, C. (2006), "Food Stamp Program Rules Database Documentation," Report, Department of Agriculture, The Urban Institute for the Economic Research Service. [965]

Food and Nutrition Act of 2008, 7 U.S.C. §2011 (2008), available at *http://www.fns.usda.gov/snap/rules/Legislation/pdfs/PL_110-246.pdf*. [961]

Genser, J. (2003), "Characteristics of Food Stamp Households: Fiscal Year 2002," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. FSP-04-CHAR. [967]

Gundersen, C., and Kreider, B. (2008), "Food Stamps and Food Insecurity: What Can Be Learned in the Presence of Nonclassical Measurement Error?" *Journal of Human Resources*, 43 (2), 352–382. [959,965]

Gundersen, C., Kreider, B., and Pepper, J. (2012), "The Impact of the National School Lunch Program on Child Health: A Nonparametric Bounds Analysis," *Journal of Econometrics*, 166 (1), 79–91. [959,960,966]

Gundersen, C., Lohman, B., Garasky, S., Stewart, S., and Eisenmann, J. (2008), "Food Security, Maternal Stressors, and Overweight Among Low-Income U.S. Children: Results From NHANES 1999–2002," *Pediatrics*, 122, e529–e540. [960]

Gundersen, C., and Offutt, S. (2005), "Farm Poverty and Safety Nets," *American Journal of Agricultural Economics*, 87 (4), 885–899. [960]

Gundersen, C., and Oliveira, V. (2001), "The Food Stamp Program and Food Insufficiency," *American Journal of Agricultural Economics*, 84 (3), 875–887. [959,963]

Haider, S., Jacknowitz, A., and Schoeni, R. (2003), "Food Stamps and the Elderly: Why Is Participation so Low?" *Journal of Human Resources*, 38 (S), 1080–1111. [960]

Hirano, K., and Porter, J. R. (2012), "Impossibility Results for Nondifferentiable Functionals," *Econometrica*, 80 (4), 1769–1790. [963]

Horowitz, J. L., and Manski, C. F. (1995), "Identification and Robustness With Contaminated and Corrupted Data," *Econometrica*, 63 (2), 281–302. [966]

Hoynes, H., and Schanzenbach, D. (2009), "Consumption Responses to In-Kind Transfers: Evidence From the Introduction of the Food Stamp Program," *American Economic Journal: Applied Economics*, 1 (4), 109–139. [959]

Imbens, G., and Manski, C. (2004), "Confidence Intervals for Partially Identified Parameters," *Econometrica*, 72 (6), 1845–1857. [970]

Jensen, H. (2002), "Food Insecurity and the Food Stamp Program," *American Journal of Agricultural Economics*, 84 (5), 1215–1228. [965]

Jolliffe, D. (2011), "Overweight and Poor? On the Relationship Between Income and the Body Mass Index," *Economics and Human Biology*, 9 (3), 342–355. [963]

Kaushal, N. (2007), "Do Food Stamps Cause Obesity? Evidence From the Immigrant Experience," *Journal of Health Economics*, 26, 968–991. [961]

Kreider, B., and Hill, S. (2009), "Partially Identifying Treatment Effects With an Application to Covering the Uninsured," *Journal of Human Resources*, 44 (2), 409–449. [959]

Kreider, B., and Pepper, J. (2007), "Disability and Employment: Reevaluating the Evidence in Light of Reporting Errors," *Journal of the American Statistical Association*, 102 (478), 432–441. [959,963]

——— (2008), "Inferring Disability Status From Corrupt Data," *Journal of Applied Econometrics*, 23 (3), 329–349. [959]

Leftin, J., Gothro, A., and Eslami, E. (2010), "Characteristics of Supplemental Nutrition Assistance Program Households: Fiscal Year 2009," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. SNAP-10-CHAR. [958]

Levedahl, J. (1995), "A Theoretical and Empirical Evaluation of the Functional Forms Used to Estimate the Food Expenditure Equation of Food Stamp Recipients," *American Journal of Agriculture Economics*, 77, 960–968. [964]

Manski, C. (1995), *Identification Problems in the Social Sciences*, Cambridge, MA: Harvard University Press. [959,962,963,964,965,972]

——— (1997), "Monotone Treatment Response," *Econometrica*, 65 (6), 1311–1334. [964]

Manski, C., and Pepper, J. (2000), "Monotone Instrumental Variables: With an Application to the Returns to Schooling," *Econometrica*, 68 (4), 997–1010. [959,963]

——— (2009), "More on Monotone Instrumental Variables," *The Econometrics Journal*, 12, S200–S216. [963]

Marquis, K., and Moore, J. (1990), "Measurement Errors in SIPP Program Reports," in *Proceedings of the Bureau of the Census Annual Research Conference*, *Bureau of the Census*, Washington, DC, pp. 721–745. [961,966]

Meyer, B., Mok, D. W., and Sullivan, J. X. (2009), "The Under-Reporting of Transfers in Household Surveys: Its Nature and Consequences," NBER Working Paper No. 15181, National Bureau of Economic Research, Cambridge, MA. [958,960,965,968]

Meyerhoefer, C., and Pylypchuk, V. (2008), "Does Participation in the Food Stamp Program Increase the Prevalence of Obesity and Health Care Spending?" *American Journal of Agricultural Economics*, 90 (2), 287–305. [961]

Moffitt, R. (1983), "An Economic Model of Welfare Stigma," *American Economic Review*, 73, 1023–1035. [960]

——— (2005), "Remarks on the Analysis of Causal Relationships in Population Research," *Demography*, 42 (1), 91–108. [959]

Molinari, F. (2008), "Partial Identification of Probability Distributions With Misclassified Data," *Journal of Econometrics*, 144 (1), 81–117. [959]

——— (2010), "Missing Treatments," *Journal of Business and Economic Statistics*, 28 (1), 82–95. [959]

Newacheck, P. (1994), "Poverty and Childhood Chronic Illness," *Archives of Pediatrics and Adolescent Medicine*, 148, 1143–1149. [963]

Nicoletti, C., Peracchi, F., and Foliano, F. (2011), "Estimating Income Poverty in the Presence of Missing Data and Measurement Error," *Journal of Business and Economic Statistics*, 29 (1), 61–72. [959]

Pepper, J. (2000), "The Intergenerational Transmission of Welfare Receipt: A Nonparametric Bounds Analysis," *The Review of Economics and Statistics*, 82 (3), 472–488. [959,962,963,964]

Poikolainen, A. (2005), "Characteristics of Food Stamp Households: Fiscal Year 2004," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. FSP-05-CHAR. [967]

Rank, M. R., and Hirschl, T. A. (2009), "Estimating the Risk of Food Stamp Use and Impoverishment During Childhood," *Archives of Pediatrics and Adolescent Medicine*, 163 (11), 994–999. [958]

Rosso, R. (2002), "Characteristics of Food Stamp Households: Fiscal Year 2001," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. FSP-04-CHAR. [967]

Shaikh, A., and Vytlacil, E. (2011), "Partial Identification in Triangular Systems of Equations With Binary Dependent Variables," *Econometrica*, 79 (3), 949–956. [965]

Shrewsbury, V., and Wardle, J. (2008), "Socioeconomic Status and Adiposity in Childhood: A Systematic Review of Cross-Sectional Studies 1990–2005," *Obesity*, 16, 275–284. [963]

Trippe, C., Doyle, P., and Asher, A. (1992), "Trends in Food Stamp Program Participation Rates, 1976 to 1990," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Reference No. 7925-03. [960]

Wilde, P., and Nord, M. (2005), "The Effect of Food Stamps on Food Security: A Panel Data Approach," *Review of Agricultural Economics*, 27 (3), 425–432. [965]

Wolkwitz, K. (2007), "Characteristics of Food Stamp Households: Fiscal Year 2006," Report, U.S. Department of Agriculture, Food and Nutrition Service, Alexandria, VA, Report No. FSP-07-CHAR. [967]