Identifying the Effects of Food Stamps on Child Health Outcomes When Participation is Endogenous and Misreported*

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Abstract: The literature assessing the efficacy of the Food Stamp Program, now called the Supplemental Nutrition Assistance Program (SNAP), has long puzzled over positive associations between food stamp receipt and various undesirable health outcomes such as food insecurity. Assessing the impact of food stamps on outcomes is made difficult by endogenous selection into food stamp recipiency and extensive systematic misreporting of participation status. Using data from the National Health and Nutrition Examination Survey (NHANES), we apply and extend partial identification bounding methods to account for these two identification problems in a single unifying framework. Imposing relatively weak nonparametric assumptions on the selection and reporting error processes, we provide informative bounds on the impact of food stamps on child food insecurity, obesity, general health, and anemia. We find that commonly cited negative relationships between food stamps and health outcomes provide a misleading picture about the impact of the program. Without imposing any parametric assumptions, our tightest bounds identify modest favorable impacts of food stamps on child health.

Keywords: Food Stamp Program, Supplemental Nutrition Assistance Program, food insecurity, health outcomes, partial identification, treatment effect, nonparametric bounds, classification error

JEL classification numbers: C14, C21, I38

I. Introduction

The Food Stamp Program, now called the Supplemental Nutrition Assistance Program (SNAP), 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, the Food Stamp Program provided assistance to more than 15 million children (Wolkwitz and Trippe, 2009), 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 food stamps 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 food stamps on health reveals little supporting evidence regarding the efficacy of the program in promoting food security and alleviating health problems.[1] Children residing in households receiving SNAP are substantially more likely to suffer from an array of health-related problems, including food insecurity, than observationally similar nonparticipating children (see, e.g., Nord *et al*., 2008).

While food stamp receipt 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 the Food Stamp Program is unlikely to be exogenous. To the contrary, unobserved factors such as expected future health status, parents' human capital characteristics, financial stability, and attitudes towards 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 food stamp recipiency exceed 12 percent and are related to

[1] For a comprehensive review of this literature, see Currie (2003).

respondents' characteristics including their true participation status, health outcomes, and demographic attributes.[2] Bitler, Currie, and Scholz (2003) provide similar evidence of extensive underreporting in the Current Population Survey (CPS).

While these identification problems have long been known to confound inferences on the impact of the Food Stamp Program, credible solutions remain elusive. Most studies treat selection as exogenous and receipt as accurate, leading Currie (2003, p. 240) to assert that "many studies have […] simply 'punted' on the issue of identification."[3]

In this paper, we consider what can be inferred about impacts of the Food Stamp Program 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 identification problems within a single methodological framework. To do so, we apply and extend recently developed partial identification methods that allow one to consider weaker assumptions than required under conventional parametric approaches (see e.g., Manski, 1995; Pepper, 2000; Molinari, forthcoming; Kreider and Pepper, 2007 and 2008; Gundersen and Kreider, 2008; Kreider and Hill, 2009).

This partial identification approach is especially well-suited for studying the impact of the Food Stamp Program 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

[2] They provide a detailed discussion about potential causes of misreporting, such as a respondent's reluctance to reveal sensitive information that is possibly threatening or stigmatizing, confusion about the source of benefits, or telescoping past events forward or backward in time.

[3] There are some exceptions that address either the selection or misclassification problems. Hoynes and Schanzenbach (2009) and Gundersen and Oliveira (2001) address the selection problem using instrumental variables within a linear response model, and Devaney and Moffitt (1991) use a nonlinear selection model. Interestingly, Devaney and Moffitt (1991) found no significant evidence of selection bias in their consideration of the effect of food stamps on dietary intakes. Similar evidence has been found in some studies of the National School Lunch Program (see, for example, Millimet, Tchernis, and Husain, forthcoming). DePolt, Moffitt, and Ribar (2008) use panel data models to control for selection based on fixed but unobserved characteristics of the households. Moreover, with access to administrative data on program receipt, DePolt *et al*. do not face the classification error problem. The one study that explicitly addresses the classification error program is Gundersen and Kreider (2008) who formally allow for the possibility of misclassified program participation, but they focus on identifying descriptive statistics and do not attempt to identify counterfactual outcomes.

on the outcomes. Yet, the Food Stamp Program is mostly defined at the federal level and has not substantively changed since the early 1980s, so program rules and regulations are not as useful as instrumental variables.[4] 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 (see, e.g., Bollinger, 1996).

The methods applied in this paper 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 sharp bounds on the health consequences of the Food Stamp Program. Given the largely paradoxical findings in the existing literature coupled with 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 food stamps 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 food stamps on food insecurity, obesity, unfavorable general health status, and anemia.

After describing the data in Section 2, we formally define the empirical questions and the nonparametric models 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

[4] Geographical variation in rules has been used in some analyses including Hoynes and Schanzenbach (2009).

outcomes of a nonparticipant in an alternate state of the world in which food stamps are received) and (b) the measurement error problem that arises because the data cannot reveal respondents with misclassified food stamp participation. Departing from the usual treatment effects literature that formally acknowledges uncertainty associated with counterfactuals but not uncertainty associated with misreporting, our methods simultaneously account for both problems. In particular, we derive bounds on average treatment effects when the indicator of food stamp participation is subject to classification errors that can be arbitrarily related to true participation status and the health outcomes. Given classification errors in the realized treatment variable, Manski's (1995) basic selection bounds do not apply. Instead, we extend the results from Molinari (forthcoming) and Kreider and Hill (2009) to assess how identification decays with the nature and degree of uncertainty about the assigned treatment.

To account for nonclassical measurement error, we consider two scenarios. The first allows for arbitrary patterns of classification errors in a "corrupt sampling" framework. The second imposes an additional "no false positives" assumption that all reports of food stamp receipt are accurate while allowing for inaccurate reports of non-receipt. This assumption is consistent with validation studies indicating that errors of commission in food stamp reporting are rare (0.3 percent in Bollinger and David's (1997) analysis).

To account for the selection problem, we begin by examining what can be learned without imposing any assumptions on the selection process. After developing these bounds, we consider the identifying power of a number of alternative identification assumptions. In particular, we consider a *Monotone Instrumental Variable* (MIV) assumption that the latent probability of a poor health outcome is nonincreasing in household income (adjusted for family composition). As discussed below, the MIV assumption is less powerful but also considerably less restrictive than the standard IV assumption. Likewise, we formalize the idea of self-selection by considering a *Monotone Treatment Selection* (MTS) assumption that, under either treatment (becoming a food stamp recipient or becoming a nonrecipient), households that have selected themselves into the program are more likely to experience a negative health outcome than nonparticipants. Finally, in parts of the analysis, we consider a *Monotone Treatment*

Response (MTR) assumption that participation in the food stamp program cannot worsen health status. While recipients appear to be worse off on average than eligible nonrecipients, many have argued that participating in the Food Stamp Program would not cause health or food security to deteriorate (e.g., Currie, 2003).

Our empirical results are presented in Section 4, and we draw conclusions in Section 5. We emphasize three findings. First, the observed positive association between food insecurity and self-reported food stamp participation may be an artifact of reporting errors. If as few as 10 percent of households might misreport, the data do not support the conclusion of higher rates of food insecurity among food stamp participants. Second, under the MIV and MTS assumptions, we find that the expansion of food stamps to all eligible recipients would lead to declines in food insecurity rates. This result holds even for modest degrees of misclassification error. Finally, under the joint MIV-MTS-MTR assumption, we can conclude that such an expansion would lead to declines in food insecurity rates and other poor health outcomes even when allowing for high rates of classification error.

II. Data

To study the impact of food stamps on child nutritional health, we use data from the 2001-2006 NHANES.[5] 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

[5] We pool the 2001-2002, 2003-2004, and 2005-2006 two-year cycles of the NHANES. Weights are established within the NHANES for use when multiple cycles are combined.

research on health- and nutrition-related child outcomes (recent work includes, e.g., Bhattacharya *et al*. 2004 and Gundersen *et al*., 2008).

We focus our analysis on households with children that are eligible to receive food stamps. The Food Stamp Program is available to all families with children that meet income and asset tests. To be eligible for assistance, a household's gross income before taxes in the previous month cannot exceed 130 percent of the poverty line, net monthly income cannot exceed the poverty line, and assets must be less than $2,000.[6] Since the NHANES does not provide sufficient information to measure net income and assets, we focus on gross income eligibility.[7] Our preliminary sample is comprised of 4690 children between the ages of two 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 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 4,418 income-eligible children.

For each observation, we observe a number of socioeconomic and demographic characteristics, including the ratio of income to the poverty threshold adjusted for family composition. Our sample has an average household income level equal to 75 percent of the poverty line.[8]

[6] Net income is calculated by subtracting a standard deduction from a household's gross income. In addition to this standard deduction, households with labor earnings deduct 20 percent of those earnings from their gross income. Deductions are also taken for child care and/or care for disabled dependents, medical expenses, and excessive shelter expenses. Depending on the state, the value of a vehicle above a certain level may be considered an asset unless it is used for work or for the transportation of disabled persons.

[7] Given our focus on children, however, this data limitation should not lead to many errors in defining eligibility. Nearly all gross income-eligible households are also net income-eligible, and the asset test is generally not separately binding for households with children. Using combined data from 1989 to 2004 from the March CPS (which does have information on the returns to assets), Gundersen and Offutt (2005) find that only seven percent of households with children are asset ineligible but gross income eligible. In contrast, the asset test could be important for a sample that includes a high proportion of households headed by an elderly person (Haider *et al*., 2003).

[8] To assess the characteristics of our sample relative to other national estimates, we pool data from six rounds of the 2001-2006 CPS, March Supplement. These data indicate that during this same time period, income eligible children lived in families with average income equal to 70 percent of the poverty line.

A. Self-Reported Food Stamp Receipt Indicator

Beyond this demographic information, we also observe a self-reported measure of food stamp receipt over the past year. In this survey, about 46% of the eligible households claim to be receiving benefits. This participation rate is similar to those found in other surveys (e.g., the CPS in Gundersen and Kreider, 2008) but substantially lower than analogous rates found using administrative data.[9] Differences between the participation rates from administrative and self-reported surveys are thought to largely reflect classification errors in the self-reported survey data.

Evidence of pervasive food stamp 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 of food stamp recipiency. Bitler *et al*. (2003, Table 3), for example, find that self-reports in the CPS reflected only about 85 percent of the true number of food stamp recipients identified in administrative data. Similar undercounts have been observed in the March Supplement of the CPS, the SIPP, the Panel Study of Income Dynamics (PSID), and the Consumer Expenditure Survey (CES) (Trippe, Doyle, and Asher 1992). 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 above, Bollinger and David (1997, Table 2) find that 12.0 percent of responses in the SIPP involve errors of omission while only 0.3 percent involve errors of commission (see also Marquis and Moore, 1990).

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[9] Wolkwitz (2008) finds, for example, that just over half of all eligible households and eighty percent of eligible children participate. Nonparticipation is ascribed to three main factors. First, there may be stigma associated with receiving food stamps. Stigma encompasses a wide variety of sources, from a person's own distaste for receiving food stamps to the fear of disapproval from others when redeeming food stamps to the possible negative reaction of caseworkers (Moffitt, 1983). Second, transaction costs can diminish the attractiveness of participation. A household faces these costs on a repeated basis when it must recertify its eligibility. Third, the benefit level can be quite small – for some families as low as $10 a month – especially for relatively higher income families (benefits are implicitly taxed at a rate of 30% for each additional dollar of net income).

B. 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 a central goal of the Food Stamp Program (U.S. Department of Agriculture, 1999), 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 2007, 11.1% of the U.S. population reported that they suffered from food insecurity at some time during the previous year (Nord *et al*., 2008). 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.[10] 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 Table 1.[11] Following official definitions, we use these 18 questions to construct a comparison of children

[10] For families without children and for one-person households, a subset of 10 questions are posed.
[11] In the NHANES, responses to individual questions from the CFSM are suppressed for confidentiality reasons.

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 the Food Stamp Program 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 Center for Disease Control and Prevention, a child is classified as obese if his or her body mass index (BMI) (kg/m^2) is at or above the 95th percentile for the child's age and gender.[12] 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 the child is both iron deficient and has an abnormally low hemoglobin level.[13] The indicator of fair or poor general health is reported by the child's parent.

Together, these four measures reflect a wide range of health related outcomes that might be impacted by the Food Stamp Program. 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 pair-wise 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 food stamps on health.[14] To the best of our knowledge, this paper is the first to investigate the impacts of food stamps on self-reported general health and anemia.[15]

Table 1 displays means and standard deviations for the variables used in this study. Consistent with previous work on this topic, food stamp recipients tend to have worse health outcomes than eligible nonparticipants. For example, 45% of children reported as food stamp recipients are also reported as food

[12] Children under the age of two were excluded because there is no commonly accepted way to establish BMI percentiles for children this young.
[13] Iron deficiency is defined as having an abnormal value, based on the results from a blood test, for at least two of the following three indicators: serum ferritin, transferrin saturation, and free erythrocyte protoporphyrin.
[14] Recent research that examines relationships between food stamps and obesity includes, e.g., Kaushal (2007) and Meyerhoefer and Pylypchuk (2008).
[15] Measures of general health status have been used as outcomes to examine the impact of a number of other factors including socioeconomic status (e.g., Case *et al*., 2002; and Currie *et al*., 2007).

insecure, nine percentage points higher than the 36% food insecurity rate among eligible nonparticipants (a statistically significant difference at better than the 5% level). Food stamp recipients are also slightly more likely to be obese, be in fair or poor general health, and have anemia compared with eligible nonrecipients.

III. The Selection and Measurement Problems

Our interest is in learning about the average and status quo treatment effects (ATE and SQTE) among food stamp eligible households. Focusing on binary outcomes, these treatment effects can be expressed as

$$\begin{aligned}\text{ATE}(1,0 \mid X \in \Omega) &= E[H(1) \mid X \in \Omega] - E[H(0) \mid X \in \Omega] \\ &= P[H(1)=1 \mid X \in \Omega] - P[H(0)=1 \mid X \in \Omega]\end{aligned} \tag{1a}$$

and

$$\begin{aligned}\text{SQTE}(1 \mid X \in \Omega) &= E[H(1) \mid X \in \Omega] - E(H \mid X \in \Omega) \\ &= P[H(1)=1 \mid X \in \Omega\ ] - P(H=1 \mid X \in \Omega)\end{aligned} \tag{1b}$$

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 average treatment effect 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. The status quo treatment effect compares the expected outcome when all eligible recipients receive food stamps with the realized mean outcome under the status quo. That is, the SQTE reveals how the prevalence of negative health outcomes would change if all eligible nonrecipients were to take up benefits.

In what follows, we will simplify notation by suppressing the conditioning on subpopulations of interest captured in X. For this analysis, we focus on the children who are eligible for food stamps. In much of the literature examining the impact of food stamp receipt, other observed covariates are motivated as a means of controlling for factors influencing a family's decision to take up food stamps. 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.[16] The problem is well-defined regardless of how the subpopulations are specified (Pepper, 2000).

Two identification problems arise when assessing the impact of food stamps on children's health outcomes. First, even if food stamp participation were observed for all eligible households, the 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 Equations (1a) and (1b) as

$$P[H(1)=1]=P[H(1)=1 \mid FS^{*}=1]P(FS^{*}=1)+P[H(1)=1 \mid FS^{*}=0]P(FS^{*}=0) \qquad (2)$$

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 censoring probability $P(FS^{*}=0)$, and the expectation of outcomes conditional on the outcome being observed, $P[H(j)=1 \mid FS^{*}=j],\ j=1,0$. Still, the sampling process cannot reveal the mean outcome conditional on censoring, $P[H(1)=1 \mid FS^{*}=0]$. Given this censoring, $P[H(1)=1]$ is not point-identified by the sampling process alone. Analogously, the second term in Equation (1a), $P[H(0)=1]$, is not identified.

Second, true participation status may not be observed for all respondents. This is referred to as the measurement or classification error problem. Instead of observing FS^{*}, we observe a self-reported

[16] In particular, there are no regression orthogonality conditions to be satisfied.

indictor, 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 Equations (1a) and (1b) as

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

$$= [P(H=1, FS=1) - \theta_1^+ + \theta_1^-] + P[H(1)=1 \mid FS^*=0][P(FS=0) + (\theta_1^+ + \theta_0^+) - (\theta_1^- + \theta_0^-)] \qquad (2')$$

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 term, $P[H(1)=1 \mid FS^*=1]P(FS^*=1)$, is not identified because of the classification error problem. The second term is not identified because of both the selection and classification error problems. As above, the data cannot reveal the counterfactual outcome distribution, $P[H(1)=1 \mid FS^*=0]$, 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, $P(FS^*)$.

A. Exogenous Selection Bounds

Much of the literature examining the impact of food stamps on health assumes that selection is exogenous so that $P[H(j) \mid FS^*] = P[H(j)]$ for $j = 1, 0$. Under this assumption, the average treatment effect can be written as

$$\text{ATE}(1,0) = P[H(1)=1 \mid FS^* = 1] - P[H(0)=1 \mid FS^* = 0] = P(H=1 \mid FS^* = 1) - P(H=1 \mid FS^* = 0). \qquad (3)$$

The appeal of the exogenous selection assumption is obvious: if selection is exogenous and food stamp receipt FS^* is observed, then the average treatment effect is identified by the sampling process. In this special case, the sample means displayed in Table 1 can be used to construct consistent estimates of ATE(1,0) which suggests that food stamps lead to a greater prevalence of food insecurity, poor health, and obesity.

If, however, one allows for the possibility of classification errors, the ATE and SQTE are not identified. To make progress in partially identifying these effects, we decompose the first term in Equation (3) into identified and unidentified quantities:

$$P(H=1 \mid FS^* = 1) = \frac{P(H=1, FS^* = 1)}{P(FS^* = 1)} = \frac{P(H=1, FS=1) + \theta_1^- - \theta_1^+}{P(FS=1) + (\theta_1^- + \theta_0^-) - (\theta_1^+ + \theta_0^+)} \qquad (4)$$

where $P(H=1, FS=1)$ and $P(FS=1)$ are identified by the data. In the numerator, $\theta_1^- - \theta_1^+$ reflects the unobserved excess of false negative versus false positive classifications among those whose health status equals 1. In the denominator, $\theta_1^- + \theta_0^- - \theta_1^+ - \theta_0^+$ reflects the unobserved excess of false negative versus false positive classifications within the entire population. The quantity $P(H=1 \mid FS^* = 0)$ can be decomposed analogously. Without assumptions restricting the nature or degree of classification errors, the data are uninformative.[17]

To address the classification error problem, we consider the following two assumptions (see Gundersen and Kreider, Proposition 1):

[17] One might be able to identify $P(FS^*)$ using auxiliary data. See Hotz, Mullins and Sanders (1997) for an illustration of how auxiliary data can be used to address similar measurement problems.

(A1) *Upper Bound Error Rate Assumption:* $P(Z^* = 0) \leq Q_u$

(A2) *No False Positives Assumption: If* $FS = 1$, *then* $FS^* = 1$

where Q_u places an upper bound on the degree of data corruption. If food stamp participation status is known to be accurately reported by all households, the researcher can set Q_u equal to 0. At the opposite extreme, Q_u can be set equal to 1 if nothing is known about the reliability of the participation responses. Assumption (A2) rules out false positive reports. That is, respondents reporting to have received food stamps are formally presumed to provide accurate reports. This identifying restriction is consistent with evidence from related validation studies that find errors of commission to be negligible (0.3%). Error rates among households reporting not to have received food stamps have been found to lie between 10% and 25%.

The literature evaluating the causal impacts of the Food Stamp Program has uniformly maintained the assumption of accurate reporting, in which case Q_u is implicitly assumed to be 0. To assess the sensitivity of inferences to classification errors in food stamp receipt, we vary Q_u between 0 and 0.25. As part of the sensitivity analysis, we compare cases where both A1 and A2 are imposed (the "no false positives" model) with cases in which only A1 is imposed (the " arbitrary errors" model).

These two assumptions imply informative restrictions on the unknown false reporting rates $\theta_1^-, \theta_0^-, \theta_1^+$ and θ_0^+. From Assumption (A1), we know

$$0 \leq \theta_1^- \leq \min\{Q_u, P(H=1, FS=0)\} \equiv \theta_1^{UB,-}, \qquad 0 \leq \theta_1^+ \leq \min\{Q_u, P(H=1, FS=1)\} \equiv \theta_1^{UB,+},$$

$$0 \leq \theta_0^- \leq \min\{Q_u, P(H=0, FS=0)\} \equiv \theta_0^{UB,-}, \qquad 0 \leq \theta_0^+ \leq \min\{Q_u, P(H=0, FS=1)\} \equiv \theta_0^{UB,+},$$

and

$$\theta_1^- + \theta_0^- + \theta_1^+ + \theta_0^+ \leq Q_u. \tag{5}$$

From Assumption 2, we know $\theta_1^+ = \theta_0^+ = 0$ so that

$$P(H=1 \mid FS^* = 1) = \frac{P(H=1, FS=1) + \theta_1^-}{P(FS=1) + \theta_1^- + \theta_0^-}. \tag{6}$$

Thus, we can sharply bound $P(H=1 \mid FS^*=1)$ and $P(H=1 \mid FS^*=0),$ respectively, as follows:

$$\frac{P(H=1,FS=1)}{P(FS=1)+\theta_0^{UB,-}} \leq P(H=1 \mid FS^*=1) \leq \frac{P(H=1,FS=1)+\theta_1^{UB,-}}{P(FS=1)+\theta_1^{UB,-}} \tag{7}$$

and

$$\frac{P(H=1,FS=0)-\theta_1^{UB,-}}{P(FS=0)-\theta_1^{UB,-}} \leq P(H=1 \mid FS^*=0) \leq \frac{P(H=1,FS=0)-\theta_1^{LB,-}}{P(FS=0)-\theta_0^{UB,-}}. \tag{8}$$ [18]

B. Worst-Case Selection Bounds

Rather than assume that selection is exogenous, 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 and Pepper, 2000). Since the latent probability $P[H(1)=1 \mid FS^*=0]$ must lie within [0,1], it follows that

$$P(H=1,FS^*=1) \leq P[H(1)=1] \leq P(H=1,FS^*=1)+P(FS^*=0). \tag{9}$$

Intuitively, the width of this bound on $P[H(1)=1]$ equals the censoring probability, $P(FS^*=1)$. Thus, if a large fraction of children receive food stamps, the width of the bound on $P[H(1)=1]$ is relatively narrow. In that case, the data cannot reveal much information about the distribution of $H(0)$, so the analogous bound of the quantity $P[H(0)=1]$ is larger. Taking the difference between the upper bound on $P[H(1)=1]$ and the lower bound on $P[H(0)=1]$ obtains a sharp upper bound on *ATE*, and analogously a sharp lower bound (Manski, 1995). As a result, the width of the bound on the average treatment effect

[18] A naive bound on the difference in mean outcomes between recipients and nonrecipients can be found by subtracting the upper (lower) bound on $P(H=1 \mid FS^*=0)$ from the lower (upper) bound on $P(H=1 \mid FS^*=1).$ These naive bounds on the ATE, however, do not account for the fact that the two conditional probabilities are linked by the unknown probabilities, θ_1^- and θ_0^-. Below, we compute sharp bounds on the mean difference in health outcomes using numerical methods that impose these constraints.

always equals 1. In the absence of identifying restrictions, the data cannot reveal the sign of the effect of food stamps on health outcomes.

If food stamp receipt, FS^*, is observed, then these bounds are identified by the sampling process. With measurement error, however, FS^* is not observed and the Manski worst-case selection bounds are not identified. In particular, we have

$$P(H=1,FS=1)-\theta_1^+ + \theta_1^- \leq P[H(1)=1] \leq P(H=1,FS=1)+P(FS=0)+\theta_0^+ - \theta_0^- . \tag{10}$$

Thus, without restrictions on the measurement error process, the false reporting rates θ are not identified and the data are uninformative about the ATE and SQTE. For example, we cannot rule out the possibility that respondents in poor health (H=1) and claiming to receive food stamps (FS=1) all misreport receipt so that the lower bound is 0. Likewise, we cannot rule out the possibility that the upper bound is 1.

However, given the Assumption 1 and 2 restrictions on the unknown classification error rates, θ, we can bound $P[H(1)=1]$ and $P[H(0)=1]$. Under Assumption 1, we have

$$P(H=1,FS=1)-\theta_1^{UB,+} \leq P[H(1)=1] \leq P(H=1,FS=1)+P(FS=0)+\theta_0^{UB,+} . \tag{11}$$

and

$$P(H=1,FS=0)-\theta_1^{UB,-} \leq P[H(0)=1] \leq P(H=1,FS=0)+P(FS=1)+\theta_0^{UB,-} . \tag{12}$$

When food stamp receipt is known to be fully accurately reported such that Q_u = 0, the bounds in Equations (11) and (12) simplify to the well-known worst-case selection bounds reported in Manski (1995). The width of the bounds on the ATE can be no smaller than 1, and these bounds expand with the degree of potential classification error. Without stronger assumptions on the selection process, the data cannot identify whether participation in the Food Stamp Program increases or decreases the prevalence of poor health outcomes.

Interestingly, under the A2 assumption of no false positive reports where $\theta_1^{UB,+} = \theta_0^{UB,+} = 0$, the bounds on $P[H(1)=1]$ in Equation (11) are identical to the Manski bounds, regardless of the value of Q_u. In this case, the latent food stamp receipt probability cannot be less than the reported probability,

$P(FS=1)$, and likewise, the latent outcome probability under full participation cannot be less than the observed joint probability of having poor health and receiving food stamps, $P(H=1, FS = 1)$.

C. Middle Ground Selection Models

To derive useful inferences about the impact of food stamps on health, prior information on the selection process must be brought to bear. While the exogenous selection assumption maintained in much of the literature is untenable, there are a number of middle ground assumptions that can narrow the bounds by restricting the relationship between food stamp participation, health outcomes, and observed covariates. We consider the identifying power of three monotonicity assumptions: one on treatment selection, one on an instrument, and one on treatment response.

The *Monotone Treatment Selection* (MTS) assumption (Manski and Pepper, 2000) places structure on the selection mechanism through which children become food stamp recipients. The Food Stamp Program literature suggests that unobserved factors associated with poor health are likely to be positively associated with the decision to take up the program. In this case, recipients have worse latent health outcomes than nonrecipients on average.[19] We formalize the MTS assumption as follows:

$$P[H(1)=1 \mid FS^*=0] \leq P[H(1)=1 \mid FS^*=1]$$

and

$$P[H(0)=1 \mid FS^*=0] \leq P[H(0)=1 \mid FS^*=1].$$

That is, conditional on either treatment $t=1$ or 0, eligible households that receive food stamps, $FS^*=1$, tend to have worse health outcomes than eligible households that have not taken up food stamps, $FS^* = 0$.

The *Monotone Instrumental Variable* (MIV) assumption (Manski and Pepper, 2000) formalizes the notion that the latent probability of a negative health outcome, $P[H(t)=1]$, varies monotonically with certain observed covariates. Arguably, for example, this probability decreases with the poverty income ratio (PIR), the ratio of a family's income to the poverty threshold set by the U.S. Census Bureau

[19] For information on differences between food stamp recipients and nonrecipients over commonly observed covariates, see Cunnyngham (2005). For speculation about differences over unobserved characteristics, see, e.g., Gundersen and Oliveira (2001) and Currie (2004).

accounting for the family's composition.[20] To formalize this idea, let v be the monotone instrumental variable such that

$$u_1 < u < u_2 \ \text{ implies } P[H(t)=1 \mid v=u_2] \le P[H(t)=1 \mid v=u] \le P[H(t)=1 \mid v=u_1] \text{ for } t=1,0.$$

While these conditional probabilities are not identified, they can be bounded using the various nonparametric models described above. 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:

$$\sup_{u \le u_2} LB(u_2) \le P[H(t)=1 \mid v=u] \le \inf_{u \ge u_1} UB(u_1).$$

In the absence of other information, these bounds on $P[H(t)=1|v=u]$ is sharp. Bounds on the unconditional latent probability, $P[H(t)=1]$, can then be obtained using the law of total probability.[21]

Finally, the *Monotone Treatment Response* (MTR) assumption (Manski, 1997) formalizes the common idea that food stamps cannot lead to a reduction in health status:

$$H(1) \le H(0).$$

In this case, the ATE and SQTE of receiving food stamps must be nonpositive (Manski, 1997 and Pepper, 2000).[22] For the food insecurity, general health, and anemia outcomes, this assumption is relatively

[20] Deaton (2002) provides evidence of a negative income gradient in realized health outcomes.

[21] To estimate these MIV bounds, we first divide the sample into 20 PIR groups. To find the MIV bounds on the rates of poor health, one takes the appropriate weighted average of the plug-in estimators of lower and upper bounds across the 20 different groups observed in the data. As discussed in Manski and Pepper (2000), this MIV estimator is consistent but biased in finite samples. We employ Kreider and Pepper's (2007) modified MIV estimator that accounts for the finite sample bias using a nonparametric bootstrap correction method.

innocuous in that it is difficult to imagine how receiving food stamps would lead to worse health outcomes (Currie, 2003). For obesity, however, the assumption is more tenuous. While better access to nutritious foods through the Food Stamp Program may lead to healthier eating and less obesity, potential increases in caloric intake could result in weight gains. Given this concern about the validity of the MTR assumption, we impose this assumption in some models but not others and we make transparent how the results vary across the different models.[23]

IV. Results

The analytical approach allows us to trace out sharp bounds on ATE and SQTE under different assumptions about the selection and measurement error problems. To do this, we evaluate the bounds as a function of the degree of uncertainty about the extent of food stamp reporting errors and layer on different types of restrictions aimed at addressing the selection problem. Our main set of empirical results, presented in Section IV. A, focuses on bounding the impact of food stamp participation on food insecurity. In Section IV. B, we extend the discussion to other health outcomes.

A. Food Insecurity

For the baseline case that selection into food stamp participation is exogenous, recall that the ATE is given by $\Delta = P(H=1 \mid FS^*=1) - P(H=1 \mid FS^*=0)$, the difference in the probability of being food

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[22] To see this result, notice that when MTR is imposed, observations where $FS^* \neq t$ may now be informative about the health outcome. For instance, children receiving food stamps would not have better health outcomes if they instead became nonrecipients. Thus, the observed outcome $P[H(1)=1 \mid FS^*=1]$ provides a lower bound for the unobserved outcome $P[H(1)=1 \mid FS^*=0]$. Given this MTR assumption, we know that $P[H(0)=1 \mid FS^*=0]$ $\geq P[H(1)=1 \mid FS^*=0]$ and $P[H(1)=1 \mid FS^*=1] \leq P[H(0)=1 \mid FS^*=1]$. Thus, $P[H(1)=1] \;\leq\; P(H=1)$ $\leq P[H(0)=1]$ (Manski, 1995; 1997). This bound is not sensitive to classification errors. It directly follows that the upper bound on the ATE is zero under the MTR assumption regardless of whether FS^* is accurately observed.

[23] Both the MTR and MTS assumptions improve the upper bound on $P[H(1)=1]$ and the lower bound on $P[H(0)=1]$. In fact, under the joint MTR-MTS assumption, we have $P[H(1)=1] \leq \min\{P(H=1 \mid FS^*=1), P(H=1)\}$ and $P[H(0)=1] \geq \min\{P(H=1 \mid FS^*=0), P(H=1)\}$. Thus, under this joint assumption, the upper bound on both the ATE and SQTE is nonpositive and the lower bound is unaffected.

insecure between children receiving and not receiving food stamps. Figure 1 traces out sharp bounds on Δ as Q_u varies between 0 and 0.25; throughout, we allow for the possibility that all food stamp recipiency reports are accurate. These bounds, which are estimated by replacing the population probabilities discussed in Section III with the corresponding sample probabilities, account only for identification uncertainty and abstract away from the additional layer of uncertainty associated with sampling variability. The associated table displays bounds for the selected values $Q_u = \{0, 0.05, 0.10, 0.25\}$ along with Imbens-Manski (2004) confidence intervals that cover the true value of Δ with 95% probability.

If all food stamp responses are known to be accurate ($Q_u = 0$), then Δ is point-identified in Figure 1 as 0.450 – 0.357 = 0.093 > 0 (consistent with the descriptive statistics in Table 1). This difference in children's food insecurity prevalence rates between food stamp recipients and nonrecipients is statistically significant with a p-value less than 0.01. When $Q_u > 0$, the food insecurity gap can only be partially identified. When an arbitrary 5 percent of households may misreport *FS*, for example, Δ can lie anywhere in the range [0.027, 0.158]. In this case, the 95 percent confidence interval for Δ is [0.001, 0.183]. These ranges narrow to [0.049, 0.158] and [0.023, 0.183], respectively, under the assumption of no false positive reports.

The key result in Figure 1 is that identification of Δ deteriorates with Q_u sufficiently rapidly, even under the no false positives assumption, that we cannot identify that the food insecurity gap is positive if more than 11 percent of households may have misreported *FS*. Thus, even assuming exogenous selection and ignoring the uncertainty associated with sampling variability, small levels of reporting error imply that the sign of ATE is not identified. Therefore, a conclusion that food insecurity is more prevalent among food stamp recipients than among eligible nonrecipients requires a large degree of confidence in self-reported food participation status, an assumption not supported by validation studies.[24]

Table 2 provides estimates of the bounds on ATE and SQTE when we relax the exogenous selection assumption. In the top panel, we make no assumptions about how eligible households select themselves into food stamp recipiency. When the food stamp participation indicator is presumed to be

[24] Gundersen and Kreider (2008) draw similar conclusions.

fully accurately reported ($Q_u = 0$), the bounds in Equations (12) and (13) simplify to the well-known Manski (1995) selection bounds reported in Equation (3). These wide bounds reveal the inherent ambiguity created by the selection problem. The width of the ATE bounds always equals 1, the width of the SQTE bounds always equals $P(FS{=}0) = 0.544$, and both treatment effect bounds always include zero.

Potential classification errors increase uncertainty about the ATE. When Q_u rises from 0 to 0.10, for example, the ATE bounds under no false positives (top panel of Table 2, Column (2)) expand from [-0.445, 0.555] with a width of 1 to [-0.500, 0.609] with a width of 1.11. Interestingly, however, the SQTE bounds are not sensitive to classification errors under the no-false positives assumption. This follows from the fact the outcome probability under full take-up, $P[H(1)=1]$, does not vary with Q_u (see Equation 11), and thus the bounds on the SQTE will not either. Regardless of the value of Q_u, SQTE is estimated to lie within the range [-0.194, 0.350].

These wide bounds presented in the top panel of Table 2 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 the Food Stamp Program has a large positive or negative impact on the likelihood of poor health. These bounds can be narrowed substantially, however, under common monotonicity assumptions on treatment response, treatment selection, and the relationship between the latent outcome and observed instrumental variables.

To narrow the bounds, we begin by considering the identification power of the MTS and joint MTS-MTR assumptions. These results are presented in the middle and bottom panel of Table 2, respectively, and traced out in Figure 2. The MTS assumption alone (middle panel of Table 2) is not strong enough to identify the sign of the impact of food stamps on food insecurity even if there is no uncertainty about misreporting. Still, the MTS assumption dramatically improves the upper bounds on both ATE and SQTE across all selected values of Q_u. When $Q_u = 0$, for example, the upper bound on the average treatment effect falls from 0.555 (top panel) to 0.093 (middle panel) such that

$\text{ATE} \in [-0.445, 0.093]$, while the upper bound on the status quo treatment effect falls from 0.350 to 0.050 such that $\text{SQTE} \in [-0.194,\ 0.050]$. Thus, relative to the 0.400 food insecurity prevalence rate under the status quo, an expansion of food stamps to all eligible children could lead to nearly a 50% decline (from 40% to 20%) in food insecurity – or it could alternatively lead to a modest (~12%) increase in food insecurity. The bottom panel of Table 2 presents corresponding results under the joint MTR-MTS assumption (see also Figure 2). This restriction serves to reduce the upper bound to 0 for ATE and SQTE regardless of the degree of possible misreporting.

Perhaps the most important results are found when we combine the MTS assumption with our MIV assumption that the probability of good health weakly increases with family resources, as measured by the PIR. In this joint MTS-MIV model, we can often sign the ATE as strictly negative without imposing the MTR assumption. Figure 3 traces out the bounds on the ATE under different restrictions on the upper bound misreporting rate, and Table 3 reproduces the bottom two panels of Table 2 under the additional assumption that the MIV restriction holds. The key finding in the top panel of Table 3 is that we can identify both the ATE and SQTE as strictly negative under the MTS-MIV model as long as the degree of food stamp misreporting is not too severe. Specifically, the corresponding Figure 3 reveals that we can identify ATE < 0 as long as food stamp misreporting is confined to no more than about 12% of households (under both arbitrary errors and no false positive errors).

As shown in the bottom panel of Table 3, the average treatment effect can be identified as strictly negative under the joint MTR-MTS-MIV assumption even for large degrees of arbitrary food stamp misreporting. Under this joint assumption, our estimated bounds on ATE vary from [-0.363, -0.130] when $Q_u = 0$ to [-0.623, -0.084] when $Q_u = 0.25$. Thus, under this model, we find that food stamps reduce the food insecurity rate by at least 8-10 percentage points and perhaps much more.

The analogous SQTE bounds range from [-0.157, -0.068] to [-0.271, -0.019] under arbitrary errors, with the lower bound improving to -0.157 when $Q_u = 0.25$ under no false positives. When $Q_u = 0$, these SQTE bounds imply that expanding food stamps to all eligible households would reduce the prevalence of food insecurity by at least 17% relative to the status quo rate of 0.400. At the other extreme, expanding food stamps could reduce the prevalence by up to 39%. When some households may misreport participation status, there is more uncertainty about the efficacy of the program. Still, we can identify that a full expansion of benefits would reduce the prevalence of food insecurity by at least 5% when up to a quarter of households may misreport; alternatively, the expansion could reduce the prevalence by up to 39% under the no false positives assumption and up to 68% under arbitrary misreporting. Thus, under the joint MTR-MTS-MIV assumption, the possibility of widespread participation reporting errors opens up the possibility that the Food Stamp Program dramatically improves households' chances of becoming food secure, with little downside likelihood that the program instead has a deleterious average effect on health.

B. Other Health Outcomes

We also consider what can be learned about the effects of food stamps on three other negative health outcomes: fair or poor general health, childhood obesity, and anemia. For brevity, we concentrate on results for cases when we impose MTS but not MTR (akin to the middle panel of Table 2), MTS-MIV (akin to the first panel of Table 3), and MTS-MTR-MIV (akin to the second panel of Table 3). These results are summarized in Tables 4-6, respectively, with the last set of results traced out in Figure 4 for the case of arbitrary errors. The lower bounds in this figure decay less rapidly with Q_u if the no false positives assumption is additionally imposed (not shown), while the upper bounds remain unchanged.

As above, imposing the MTS assumption alone (Table 4) does not allow us to bound the sign of the treatment effects. The upper bounds are close to zero for each health outcome when food stamp recipiency is measured accurately, but we are unable to conclude that expanding the program would lead to improvements in health. Thus, the selection problem alone is not sufficient to fully explain the paradoxical correlations in the data.

In contrast, we can identify strictly negative treatment effects for each health outcome under the joint MTS-MIV assumption (Table 5) for sufficiently small degrees of food stamp reporting error. At Q_u

= 0, for example, the average treatment effects for fair/poor health, obesity, and anemia are identified to lie within the ranges [-0.397, -0.062], [-0.410, -0.032], and [-0.397, -0.030], respectively.

The analogous status quo treatment effect ranges when $Q_u = 0$ are given by [-0.032, -0.029], [-0.079, 0.005], and [-0.003, -0.003]. For anemia, notice that the joint MTS-MIV assumption (combined with the assumption of fully accurate food stamp reporting) is strong enough to allow us to point-identify the impact of food stamps: expanding food stamps to all eligible households is estimated to reduce the prevalence of anemia by 25%, from 0.012 to 0.009. Likewise, the estimated bounds for fair/poor health are very narrow, suggesting that full participation in the food stamp program would reduce the rate of poor health from 8% to about 5%. Identification of both the ATE and SQTE decays rapidly with Q_u for each of the health outcomes, however, and each upper bound becomes positive if fewer than 5 percent of the households might misreport food stamp participation status (under either arbitrary errors or no false positives).

Table 6 and Figure 4 present the MTR-MTS-MIV bounds on the treatment effects for these outcomes. Under this model, we estimate strictly negative impacts for each health outcome across all values of $Q_u \in [0,\ 0.25]$, even for the case of arbitrary reporting errors. For the food insecurity outcome, we previously estimated upper bounds on SQTE ranging from -0.068 to -0.019 as Q_u ranges from 0 to 0.25 (Table 3, Column 3 or 4). Based on these upper bounds, expanding food stamps to all eligible households would decrease the prevalence of food insecurity by at least 17% under no misreporting and by at least 4% if up to a quarter of the households may have misreported food stamp participation. Analogously in Table 6, we estimate upper bounds on SQTE ranging from -0.029 to -0.021 for the poor/fair health outcome as Q_u ranges from 0 to 0.25. Based on these upper bounds, expanding food stamps would decrease the prevalence of poor health by at least 36% when $Q_u = 0$ and by at least 26% when up to a quarter of the households may have misreported their participation status. For the selected upper bound misreporting rates, Q_u, the estimated lower bound for this outcome is constant at -0.032, suggesting a maximum 40% reduction in poor health. For the case of obesity, we estimate that SQTE lies within [-0.079, -0.015] regardless of the value of $Q_u \in [0,\ 0.25]$. These bounds translate into reductions in childhood obesity associated with full participation in the Food Stamp Program ranging from 8 to 43%. We previously estimated a 25% reduction in the prevalence of childhood anemia under the joint MTS-MIV assumption when $Q_u = 0$. Under the stronger MTR-MTS-MIV assumption, we can point-identify this 25% reduction across all values of $Q_u \in [0,\ 0.25]$.

V. Conclusion

The literature assessing the efficacy of the Food Stamp Program has long puzzled over positive associations between food stamp receipt and various undesirable health-related outcomes such as food insecurity. These associations are often ascribed to the self-selection of less healthy households into the Food Stamp Program. Misreporting of food stamp recipiency also confounds identification of the causal impacts of participation on health status. In this paper, we reconsidered the impact of food stamps on food insecurity and other health outcomes using a single unifying framework that formally accounts for both of these identification problems. Our partial identification approach is well-suited for this application where 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 the Food Stamp Program.

Using data from the National Health and Nutrition Examination Survey (NHANES), we make transparent how assumptions on the selection and reporting error processes shape inferences about the causal impacts of food stamp recipiency on health outcomes. The potentially troubling correlations in the data provide a misleading picture of the impacts of the Food Stamp Program. Without assumptions aimed to address the selection and measurement problems, the sampling process cannot identify the sign of the effect of food stamps on heath. The worst-case selection bounds always include zero, and even small amounts of measurement error are sufficient to cast doubt on the conclusion that food insecurity and other poor health outcomes are more prevalent among food stamp recipients than among eligible nonrecipients.

Moreover, the MTS assumption alone is not strong enough to identify the sign of the impact of food stamps on food insecurity even if there is no uncertainty about misreporting.

Combining the MTS and MIV assumptions, however, allows us sign the impact of the Food Stamp Program. Under this relatively weak nonparametric model used to address the selection problem, we find that the Food Stamp Program reduces the prevalence of food insecurity and other poor health outcomes. In the absence of measurement error, the joint MTS-MIV model reveals that expanding food stamps to all eligible households would reduce the prevalence of food insecurity by at least 17%, from 0.40 to 0.33, and by as much as 39%, from 0.40 to 0.24. Likewise, food stamps are estimated to reduce the rate of anemia from 0.012 to 0.009 and the rate of poor health from 0.08 to about 0.05. The Food Stamp Program has a substantial impact on the prevalence of food insecurity, anemia, and poor general health. When some households may misreport participation status, however, there remains uncertainty about the efficacy of the program.

Under the joint MTR-MTS-MIV assumption, the basic conclusion that the Food Stamp Program improves health outcomes holds even for large degrees of measurement error. For example, we find that a full expansion of benefits would reduce the prevalence of food insecurity by at least 4.5% when up to a quarter of households may misreport; alternatively, the expansion could reduce the prevalence by up to 68% under arbitrary misreporting. Under the joint MTR-MTS-MIV assumption, we find that that the Food Stamp Program will at least lead to modest reductions in poor health and may dramatically improve households' chances of becoming food secure.

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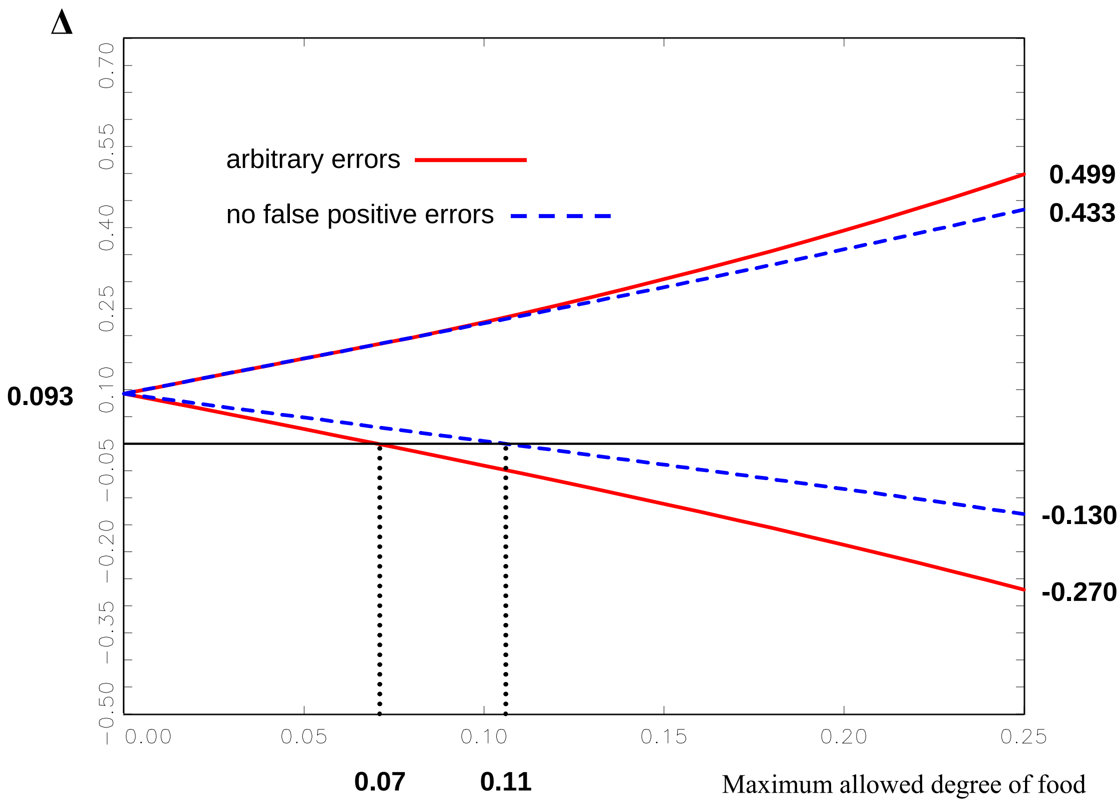
Table 1: Means by Reported Food Stamp Program Participation

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)
Income to poverty ratio	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)
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)
Poor or Fair Health	0.080 (0.005)	0.088 (0.008)	0.073 (0.007)
N	4418	2141	2277

Notes: Sample estimates weighted using the medical exam weight. Standard deviations in parentheses account for the sample design using the synthetic strata and PSU variables. The estimated means for the Food Stamp 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 3,871 (of which 1,888 are food stamp recipients) due to missing observations.

Figure 1. Sharp Bounds on Δ, the Difference in the Food Insecurity Prevalence Rate Between Food Stamp Participants and Nonparticipants [†]



Point Estimates (p.e.) of LB and UB and 95% I-M[†] Confidence Intervals (CI) Around the Unknown Parameter Δ

Q_u	Arbitrary Errors	No False Positives
0	[0.093, 0.093] p.e.	[0.093, 0.093] p.e.
	[0.060 0.126] CI[a]	[0.060 0.126] CI
0.05	[0.027, 0.158] p.e.	[0.049, 0.158] p.e.
	[0.001 0.183] CI	[0.023 0.183] CI
0.10	[-0.040, 0.225] p.e.	[0.005, 0.223] p.e.
	[-0.068 0.253] CI	[-0.021 0.248] CI
0.25	[-0.270, 0.499] p.e.	[-0.130, 0.433] p.e.
	[-0.307 0.537] CI	[-0.158 0.461] CI

[†] The figure traces out point estimates of the population bounds

[a] Confidence intervals around Δ are calculated using methods from Imbens-Manski (2004) with 1,000 pseudosamples.

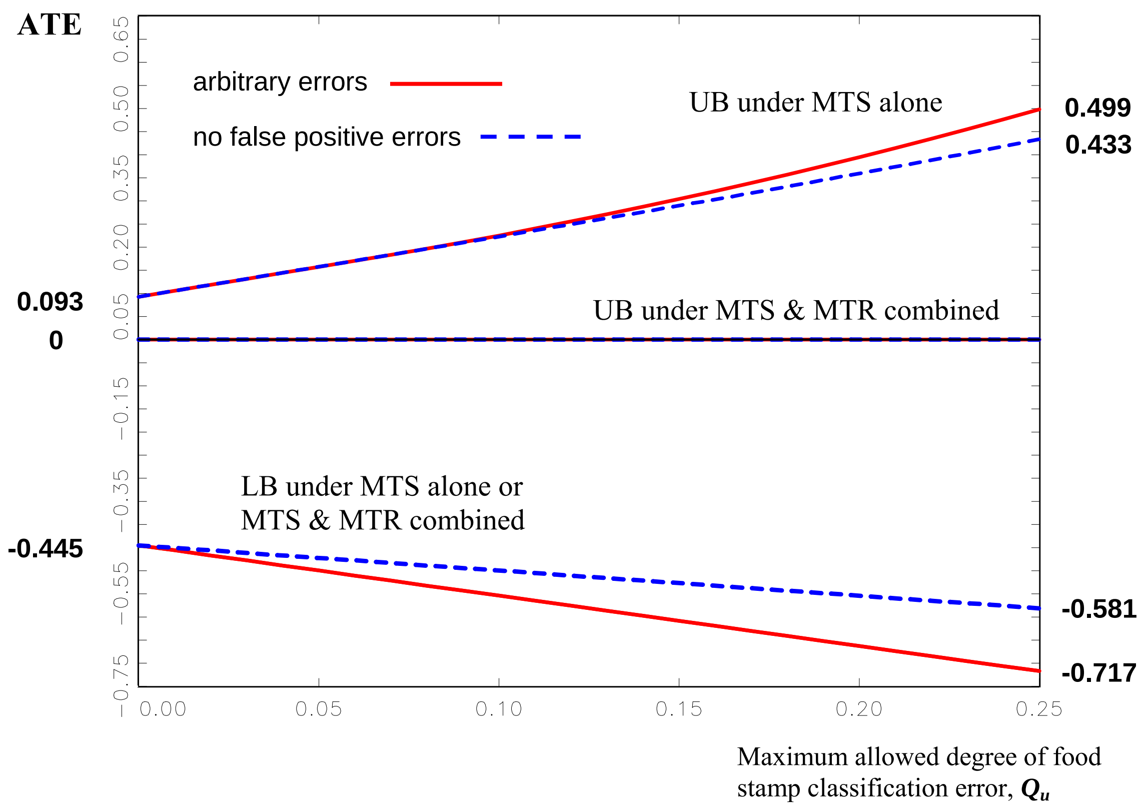
Table 2: Sharp Bounds on the ATE and SQTE of Food Stamp Participation on Food Insecurity Given Unknown Counterfactuals and Potentially Misclassified Participation Status: Various Assumptions about Selection

	ATE		SQTE	
	Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI) around the unknown parameter ATE		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI) around the unknown parameter SQTE	
	(1)	(2)	(3)	(4)
Q_u	Arbitrary Errors	No False Positives	Arbitrary Errors	No False Positives
	No Monotonicity Assumptions			
0	[-0.445, 0.555] p.e. [-0.458 0.568] CI	[-0.445, 0.555] p.e. [-0.458 0.568] CI	[-0.194, 0.350] p.e. [-0.205 0.363] CI	[-0.194, 0.350] p.e. [-0.205 0.363] CI
0.05	[-0.500, 0.609] p.e. [-0.512 0.623] CI	[-0.472, 0.582] p.e. [-0.485 0.595] CI	[-0.222, 0.377] p.e. [-0.232 0.391] CI	[-0.194, 0.350] p.e. [-0.205 0.363] CI
0.10	[-0.554, 0.664] p.e. [-0.567 0.677] CI	[-0.500, 0.609] p.e. [-0.512 0.623] CI	[-0.249, 0.404] p.e. [-0.260 0.418] CI	[-0.194, 0.350] p.e. [-0.205 0.363] CI
0.25	[-0.717, 0.827] p.e. [-0.731 0.843] CI	[-0.581, 0.691] p.e. [-0.594 0.705] CI	[-0.331, 0.486] p.e. [-0.343 0.502] CI	[-0.194, 0.350] p.e. [-0.205 0.363] CI
	MTS Assumption			
0	[-0.445, 0.093] p.e. [-0.458 0.118] CI	[-0.445, 0.093] p.e. [-0.458 0.118] CI	[-0.194, 0.050] p.e. [-0.205 0.064] CI	[-0.194, 0.050] p.e. [-0.205 0.064] CI
0.05	[-0.500, 0.158] p.e. [-0.512 0.183] CI	[-0.472, 0.158] p.e. [-0.485 0.183] CI	[-0.222, 0.081] p.e. [-0.232 0.095] CI	[-0.194, 0.081] p.e. [-0.205 0.095] CI
0.10	[-0.554, 0.225] p.e. [-0.567 0.253] CI	[-0.500, 0.223] p.e. [-0.512 0.248] CI	[-0.249, 0.111] p.e. [-0.260 0.127] CI	[-0.194, 0.109] p.e. [-0.205 0.122] CI
0.25	[-0.717, 0.499] p.e. [-0.731 0.537] CI	[-0.581, 0.433] p.e. [-0.594 0.461] CI	[-0.331, 0.242] p.e. [-0.342 0.268] CI	[-0.194, 0.177] p.e. [-0.205 0.190] CI
	MTR and MTS Assumptions			
0	[-0.445, 0.000] p.e. [-0.458 0.000] CI	[-0.445, 0.000] p.e. [-0.458 0.000] CI	[-0.194, 0.000] p.e. [-0.205 0.000] CI	[-0.194, 0.000] p.e. [-0.205 0.000] CI
0.05	[-0.500, 0.000] p.e. [-0.512 0.000] CI	[-0.472, 0.000] p.e. [-0.485 0.000] CI	[-0.222, 0.000] p.e. [-0.232 0.000] CI	[-0.194, 0.000] p.e. [-0.205 0.000] CI
0.10	[-0.554, 0.000] p.e. [-0.567 0.000] CI	[-0.500, 0.000] p.e. [-0.512 0.000] CI	[-0.249, 0.000] p.e. [-0.260 0.000] CI	[-0.194, 0.000] p.e. [-0.205 0.000] CI
0.25	[-0.717, 0.000] p.e. [-0.731 0.000] CI	[-0.581, 0.000] p.e. [-0.594 0.000] CI	[-0.331, 0.000] p.e. [-0.343 0.000] CI	[-0.194, 0.000] p.e. [-0.205 0.000] CI

† Confidence intervals around ATE and SQTE are calculated using methods from Imbens-Manski (2004) with 1000 pseudosamples.

Figure 2. Sharp Bounds on the ATE of Food Stamp Participation on Food Insecurity When Participation Status May be Misclassified

With MTS Alone or MTS & MTR Combined, No MIV†



Sharp Bounds on ATE Under the MTS Assumption Alone

Q_u	Arbitrary Errors	No False Positives
0	[-0.445, 0.093] p.e.	[-0.445, 0.093] p.e.
	[-0.458 0.118] CI[a]	[-0.458 0.118] CI
0.05	[-0.500 0.158] p.e.	[-0.472, 0.158] p.e.
	[-0.512 0.183] CI	[-0.485 0.183] CI
0.10	[-0.554, 0.225] p.e.	[-0.500, 0.223] p.e.
	[-0.567 0.253] CI	[-0.512 0.248] CI
0.25	[-0.717, 0.499] p.e.	[-0.581, 0.433] p.e.
	[-0.731 0.537] CI	[-0.594 0.461] CI

† The figure traces out point estimates of the population bounds

[a] Confidence intervals around ATE are calculated using methods from Imbens-Manski (2004) with 1,000 pseudosamples.

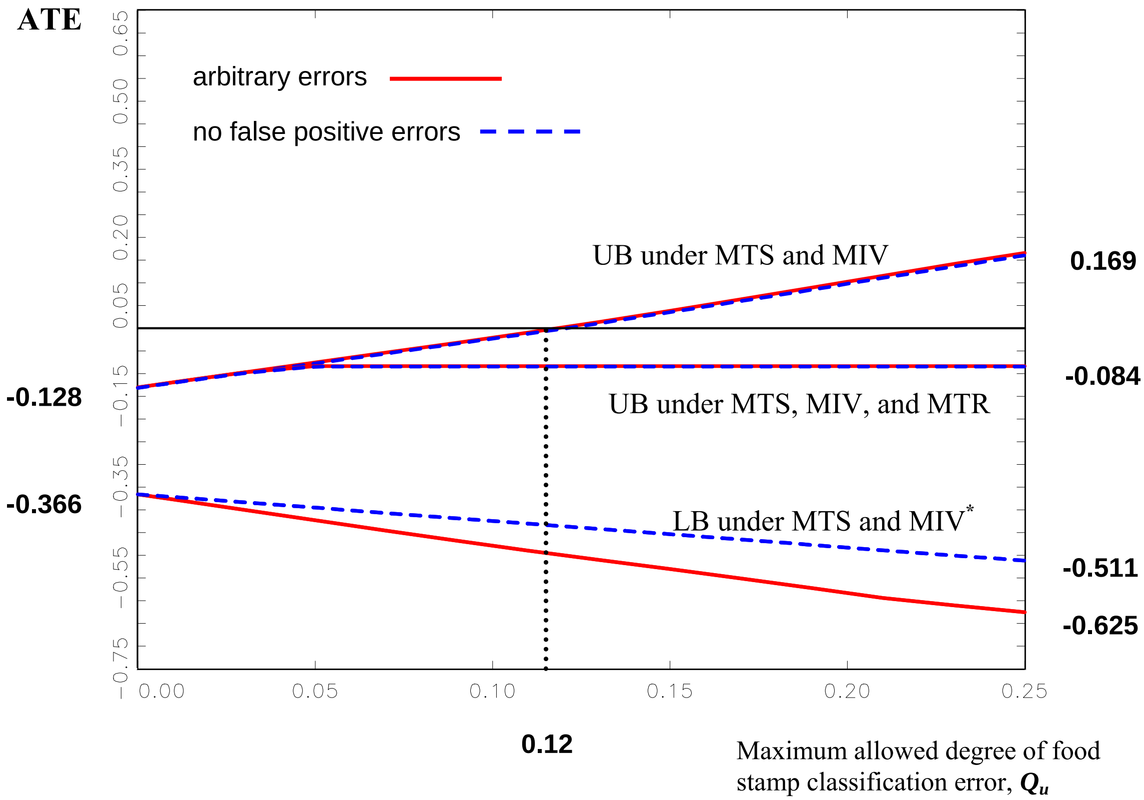
Table 3: Sharp Bounds on the ATE and SQTE of Food Stamp Participation on Food Insecurity Under Arbitrary Errors and No False Positives: With MIV

	ATE		SQTE	
	Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI) around the unknown parameter ATE		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI) around the unknown parameter SQTE	
	(1)	(2)	(3)	(4)
Q_u	Arbitrary Errors	No False Positives	Arbitrary Errors	No False Positives
	MTS Assumption			
0	[-0.363, -0.130] p.e. [-0.433 -0.041] CI	[-0.363, -0.130] p.e. [-0.433 -0.041] CI	[-0.157, -0.068] p.e. [-0.198 -0.007] CI	[-0.157, -0.068] p.e. [-0.198 -0.007] CI
0.05	[-0.420, -0.076] p.e. [-0.489 0.013] CI	[-0.393, -0.077] p.e. [-0.461 0.015] CI	[-0.184, -0.043] p.e. [-0.227 0.019] CI	[-0.157, -0.043] p.e. [-0.198 0.018] CI
0.10	[-0.477, -0.022] p.e. [-0.544 0.070] CI	[-0.422, -0.023] p.e. [-0.488 0.073] CI	[-0.212, -0.018] p.e. [-0.255 0.045] CI	[-0.157, -0.020] p.e. [-0.198 0.043] CI
0.25	[-0.623, 0.166] p.e. [-0.683 0.273] CI	[-0.509, 0.161] p.e. [-0.569 0.271] CI	[-0.271, 0.049] p.e. [-0.311 0.119] CI	[-0.157, 0.044] p.e. [-0.198 0.113] CI
	MTR and MTS Assumption			
0	[-0.363, -0.130] p.e. [-0.433 -0.046] CI	[-0.363, -0.130] p.e. [-0.433 -0.046] CI	[-0.157, -0.068] p.e. [-0.198 -0.011] CI	[-0.157, -0.068] p.e. [-0.198 -0.011] CI
0.05	[-0.420, -0.084] p.e. [-0.489 -0.012] CI	[-0.393, -0.084] p.e. [-0.461 -0.010] CI	[-0.184, -0.043] p.e. [-0.227 -0.000] CI	[-0.157, -0.043] p.e. [-0.198 -0.000] CI
0.10	[-0.477, -0.084] p.e. [-0.544 -0.018] CI	[-0.422, -0.084] p.e. [-0.488 -0.016] CI	[-0.212, -0.019] p.e. [-0.255 -0.000] CI	[-0.157, -0.020] p.e. [-0.198 -0.000] CI
0.25	[-0.623, -0.084] p.e. [-0.683 -0.018] CI	[-0.509, -0.084] p.e. [-0.569 -0.018] CI	[-0.271, -0.019] p.e. [-0.311 -0.000] CI	[-0.157, -0.019] p.e. [-0.198 -0.000] CI

†Confidence intervals around ATE and SQTE are calculated using methods from Imbens-Manski (2004) with 1000 pseudosamples.

Figure 3. Sharp Bounds on the ATE of Food Stamp Participation on Food Insecurity When Participation Status May be Misclassified

With MTS and MIV, With and Without MTR[†]



Sharp Bounds on ATE with MTS and MIV, No MTR

Q_u	Arbitrary Errors	No False Positives
0	[-0.363, -0.130] p.e. [-0.433 -0.041] CI[a]	[-0.363, -0.130] p.e. [-0.433 -0.041] CI
0.05	[-0.420 -0.076] p.e. [-0.489 0.013] CI	[-0.393, -0.077] p.e. [-0.461 0.015] CI
0.10	[-0.477, -0.022] p.e. [-0.544 0.070] CI	[-0.422, -0.023] p.e. [-0.488 0.073] CI
0.25	[-0.623, 0.166] p.e. [-0.683 0.273] CI	[-0.509, 0.161] p.e. [-0.569 0.271] CI

[†] The figure traces out point estimates of the population bounds.
[*] The MTR assumption has no identifying power for the lower bounds.
[a] Confidence intervals around ATE are calculated using methods from Imbens-Manski (2004) with 1,000 pseudosamples.

Table 4: Sharp Bounds on the ATE and SQTE of Food Stamp Participation on Health Outcomes
Under the Assumption of No False Positives: With MTS Assumption

		ATE		SQTE	
		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)	
		(1)	(2)	(3)	(4)
	Q_u	Arbitrary Errors	No False Positives	Arbitrary Errors	No False Positives
Poor or Fair Health	0	[-0.455, 0.015] p.e.	[-0.455, 0.015] p.e.	[-0.040, 0.008] p.e.	[-0.040, 0.008] p.e.
		[-0.469 0.030] CI	[-0.469 0.030] CI	[-0.044 0.016] CI	[-0.044 0.016] CI
	0.05	[-0.510, 0.115] p.e.	[-0.483, 0.115] p.e.	[-0.067, 0.060] p.e.	[-0.040, 0.060] p.e.
		[-0.523 0.130] CI	[-0.496 0.130] CI	[-0.072 0.068] CI	[-0.044 0.068] CI
	0.10	[-0.550, 0.166] p.e.	[-0.510, 0.161] p.e.	[-0.080, 0.086] p.e.	[-0.040, 0.081] p.e.
		[-0.563 0.178] CI	[-0.522 0.174] CI	[-0.087 0.093] CI	[-0.044 0.088] CI
	0.25	[-0.631, 0.200] p.e.	[-0.591, 0.161] p.e.	[-0.080, 0.120] p.e.	[-0.040, 0.081] p.e.
		[-0.642 0.215] CI	[-0.603 0.174] CI	[-0.087 0.130] CI	[-0.044 0.088] CI
Obese	0	[-0.466, 0.012] p.e.	[-0.466, 0.012] p.e.	[-0.097, 0.007] p.e.	[-0.097, 0.007] p.e.
		[-0.479 0.031] CI	[-0.479 0.031] CI	[-0.105 0.017] CI	[-0.105 0.017] CI
	0.05	[-0.520, 0.101] p.e.	[-0.493, 0.101] p.e.	[-0.125, 0.052] p.e.	[-0.097, 0.052] p.e.
		[-0.533 0.120] CI	[-0.506 0.120] CI	[-0.133 0.063] CI	[-0.105 0.063] CI
	0.10	[-0.575, 0.190] p.e.	[-0.520, 0.190] p.e.	[-0.152, 0.093] p.e.	[-0.097, 0.093] p.e.
		[-0.586 0.210] CI	[-0.533 0.210] CI	[-0.160 0.104] CI	[-0.105 0.104] CI
	0.25	[-0.689, 0.359] p.e.	[-0.602, 0.334] p.e.	[-0.185, 0.174] p.e.	[-0.097, 0.149] p.e.
		[-0.701 0.374] CI	[-0.613 0.350] CI	[-0.195 0.185] CI	[-0.105 0.158] CI
Anemic	0	[-0.456, 0.002] p.e.	[-0.456, 0.002] p.e.	[-0.005, 0.001] p.e.	[-0.005, 0.001] p.e.
		[-0.469 0.006] CI	[-0.469 0.006] CI	[-0.006 0.003] CI	[-0.006 0.003] CI
	0.05	[-0.488, 0.023] p.e.	[-0.483, 0.022] p.e.	[-0.010, 0.013] p.e.	[-0.005, 0.012] p.e.
		[-0.501 0.027] CI	[-0.495 0.026] CI	[-0.012 0.015] CI	[-0.006 0.014] CI
	0.10	[-0.516, 0.025] p.e.	[-0.510, 0.022] p.e.	[-0.010, 0.014] p.e.	[-0.005, 0.012] p.e.
		[-0.527 0.029] CI	[-0.522 0.026] CI	[-0.012 0.017] CI	[-0.006 0.014] CI
	0.25	[-0.597, 0.031] p.e.	[-0.592, 0.022] p.e.	[-0.010, 0.020] p.e.	[-0.005, 0.012] p.e.
		[-0.607 0.036] CI	[-0.602 0.026] CI	[-0.012 0.024] CI	[-0.006 0.014] CI

†Confidence intervals around ATE and SQTE are calculated using methods from Imbens-Manski (2004) with 1000 pseudosamples.

Table 5: Sharp Bounds on the ATE and SQTE of Food Stamp Participation on Health Outcomes Under the Assumption of No False Positives: With MTS and MIV Assumptions

		ATE		SQTE	
		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)	
		(1)	(2)	(3)	(4)
	Q_u	Arbitrary Errors	No False Positives	Arbitrary Errors	No False Positives
Poor or Fair Health	0	[-0.397, -0.062] p.e.	[-0.397, -0.062] p.e.	[-0.032, -0.029] p.e.	[-0.032, -0.029] p.e.
		[-0.461 -0.006] CI	[-0.461 -0.006] CI	[-0.044 -0.003] CI	[-0.044 -0.003] CI
	0.05	[-0.447, 0.025] p.e.	[-0.427, 0.024] p.e.	[-0.052, 0.011] p.e.	[-0.032, 0.011] p.e.
		[-0.508 0.085] CI	[-0.489 0.084] CI	[-0.072 0.035] CI	[-0.044 0.035] CI
	0.10	[-0.491, 0.084] p.e.	[-0.457, 0.081] p.e.	[-0.066, 0.023] p.e.	[-0.032, 0.020] p.e.
		[-0.548 0.144] CI	[-0.517 0.141] CI	[-0.087 0.050] CI	[-0.044 0.046] CI
	0.25	[-0.592, 0.118] p.e.	[-0.548, 0.100] p.e.	[-0.076, 0.039] p.e.	[-0.032, 0.020] p.e.
		[-0.637 0.150] CI	[-0.601 0.126] CI	[-0.087 0.071] CI	[-0.044 0.047] CI
Obese	0	[-0.410, -0.032] p.e.	[-0.410, -0.032] p.e.	[-0.079, 0.005] p.e.	[-0.079, 0.005] p.e.
		[-0.471 0.031] CI	[-0.471 0.031] CI	[-0.105 0.017] CI	[-0.105 0.017] CI
	0.05	[-0.468, 0.022] p.e.	[-0.440, 0.022] p.e.	[-0.108, 0.019] p.e.	[-0.079, 0.019] p.e.
		[-0.529 0.113] CI	[-0.500 0.114] CI	[-0.133 0.063] CI	[-0.105 0.063] CI
	0.10	[-0.521, 0.072] p.e.	[-0.471, 0.072] p.e.	[-0.130, 0.026] p.e.	[-0.079, 0.026] p.e.
		[-0.579 0.167] CI	[-0.529 0.167] CI	[-0.160 0.074] CI	[-0.105 0.074] CI
	0.25	[-0.643, 0.252] p.e.	[-0.561, 0.240] p.e.	[-0.161, 0.067] p.e.	[-0.079, 0.056] p.e.
		[-0.695 0.354] CI	[-0.613 0.341] CI	[-0.195 0.122] CI	[-0.105 0.109] CI
Anemic	0	[-0.397, -0.030] p.e.	[-0.397, -0.030] p.e.	[-0.003, -0.003] p.e.	[-0.003, -0.003] p.e.
		[-0.450 0.004] CI	[-0.450 0.004] CI	[-0.006 0.001] CI	[-0.006 0.001] CI
	0.05	[-0.434, 0.007] p.e.	[-0.427, 0.007] p.e.	[-0.010, -0.002] p.e.	[-0.003, -0.003] p.e.
		[-0.485 0.027] CI	[-0.478 0.026] CI	[-0.012 0.003] CI	[-0.006 0.003] CI
	0.10	[-0.464, 0.008] p.e.	[-0.457, 0.007] p.e.	[-0.010, -0.002] p.e.	[-0.003, -0.003] p.e.
		[-0.513 0.025] CI	[-0.505 0.024] CI	[-0.012 0.003] CI	[-0.006 0.003] CI
	0.25	[-0.554, 0.009] p.e.	[-0.547, 0.007] p.e.	[-0.010, -0.001] p.e.	[-0.003, -0.003] p.e.
		[-0.595 0.015] CI	[-0.587 0.013] CI	[-0.012 0.004] CI	[-0.006 0.003] CI

†Confidence intervals around ATE and SQTE are calculated using methods from Imbens-Manski (2004) with 1000 pseudosamples.

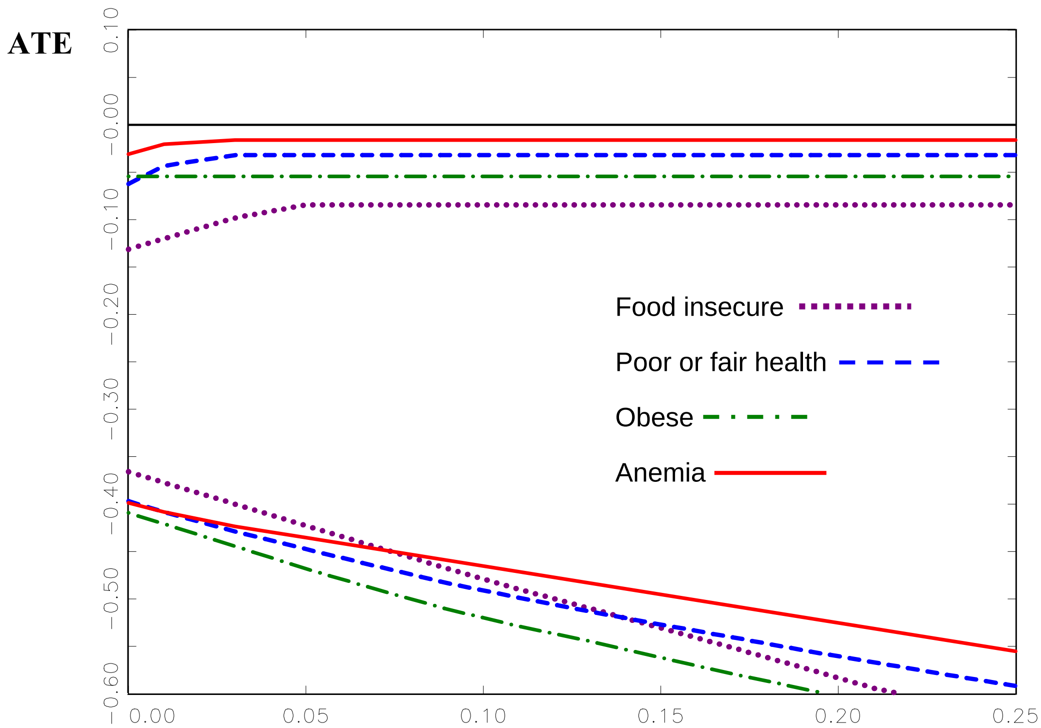
Table 6: Sharp Bounds on the ATE and SQTE of Food Stamp Participation on Health Outcomes
Under the Assumption of No False Positives: With MTS, MTR, and MIV Assumptions

		ATE		SQTE	
		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)		Point estimates (p.e.) of LB and UB and 95% I-M† confidence intervals (CI)	
		(1)	(2)	(3)	(4)
	Q_u	Arbitrary Errors	No False Positives	Arbitrary Errors	No False Positives
Poor or Fair Health	0	[-0.397, -0.062] p.e.	[-0.397, -0.062] p.e.	[-0.032, -0.029] p.e.	[-0.032, -0.029] p.e.
		[-0.461 -0.011] CI	[-0.461 -0.011] CI	[-0.044 -0.004] CI	[-0.044 -0.004] CI
	0.05	[-0.447, -0.032] p.e.	[-0.427, -0.032] p.e.	[-0.052, -0.021] p.e.	[-0.032, -0.021] p.e.
		[-0.508 0.000] CI	[-0.489 0.000] CI	[-0.072 -0.003] CI	[-0.044 -0.001] CI
	0.10	[-0.491, -0.032] p.e.	[-0.457, -0.032] p.e.	[-0.066, -0.021] p.e.	[-0.032, -0.021] p.e.
		[-0.548 0.000] CI	[-0.517 0.000] CI	[-0.087 -0.003] CI	[-0.044 -0.001] CI
	0.25	[-0.592, -0.032] p.e.	[-0.548, -0.032] p.e.	[-0.076, -0.021] p.e.	[-0.032, -0.021] p.e.
		[-0.637 0.000] CI	[-0.601 0.000] CI	[-0.087 -0.003] CI	[-0.044 -0.001] CI
Obese	0	[-0.410, -0.053] p.e.	[-0.410, -0.053] p.e.	[-0.079, -0.015] p.e.	[-0.079, -0.015] p.e.
		[-0.471 0.000] CI	[-0.471 0.000] CI	[-0.105 0.000] CI	[-0.105 0.000] CI
	0.05	[-0.468, -0.053] p.e.	[-0.440, -0.053] p.e.	[-0.108, -0.015] p.e.	[-0.079, -0.015] p.e.
		[-0.529 0.000] CI	[-0.500 0.000] CI	[-0.133 0.000] CI	[-0.105 0.000] CI
	0.10	[-0.521, -0.053] p.e.	[-0.471, -0.053] p.e.	[-0.130, -0.015] p.e.	[-0.079, -0.015] p.e.
		[-0.579 0.000] CI	[-0.529 0.000] CI	[-0.160 0.000] CI	[-0.105 0.000] CI
	0.25	[-0.643, -0.053] p.e.	[-0.561, -0.053] p.e.	[-0.161, -0.015] p.e.	[-0.079, -0.015] p.e.
		[-0.695 0.000] CI	[-0.613 0.000] CI	[-0.195 0.000] CI	[-0.105 0.000] CI
Anemic	0	[-0.397, -0.030] p.e.	[-0.397, -0.030] p.e.	[-0.003, -0.003] p.e.	[-0.003, -0.003] p.e.
		[-0.450 0.000] CI	[-0.450 0.000] CI	[-0.006 -0.001] CI	[-0.006 -0.001] CI
	0.05	[-0.434, -0.016] p.e.	[-0.427, -0.016] p.e.	[-0.010, -0.003] p.e.	[-0.003, -0.003] p.e.
		[-0.485 0.000] CI	[-0.478 0.000] CI	[-0.012 -0.002] CI	[-0.006 -0.001] CI
	0.10	[-0.464, -0.016] p.e.	[-0.457, -0.016] p.e.	[-0.010, -0.003] p.e.	[-0.003, -0.003] p.e.
		[-0.513 0.000] CI	[-0.505 0.000] CI	[-0.012 -0.002] CI	[-0.006 -0.001] CI
	0.25	[-0.554, -0.016] p.e.	[-0.547, -0.016] p.e.	[-0.010, -0.003] p.e.	[-0.003, -0.003] p.e.
		[-0.595 0.000] CI	[-0.587 0.000] CI	[-0.012 -0.002] CI	[-0.006 -0.001] CI

†Confidence intervals around ATE and SQTE are calculated using methods from Imbens-Manski (2004) with 1000 pseudosamples.

Figure 4. Sharp Bounds on the ATE of Food Stamp Participation on Food Insecurity When Participation Status May be Misclassified

Arbitrary errors with MTS, MIV, and MTR[†,*]



Maximum allowed degree of food stamp classification error, $\boldsymbol{Q_u}$

Arbitrary errors with MTS, MIV, and MTR

Q_u	Food Insecure	Poor or Fair Health	Obese	Anemia
0	[-0.363, -0.130] p.e.	[-0.397, -0.062] p.e.	[-0.410, -0.053] p.e.	[-0.397, -0.030] p.e.
	[-0.433 -0.046] CI[a]	[-0.461 -0.011] CI	[-0.471 0.000] CI	[-0.450 0.000] CI
	[-0.420 -0.084] p.e.	[-0.447 -0.032] p.e.	[-0.468 -0.053] p.e.	[-0.434, -0.016] p.e.
0.05	[-0.489 -0.012] CI	[-0.508 0.000] CI	[-0.529 0.000] CI	[-0.485 0.000] CI
	[-0.477, -0.084] p.e.	[-0.491, -0.032] p.e.	[-0.521, -0.053] p.e.	[-0.464, -0.016] p.e.
0.10	[-0.544 -0.018] CI	[-0.548 0.000] CI	[-0.579 0.000] CI	[-0.513 -0.000] CI
	[-0.623, -0.084] p.e.	[-0.592, -0.032] p.e.	[-0.643, -0.053] p.e.	[-0.554, -0.016] p.e.
0.25	[-0.683 -0.018] CI	[-0.637 0.000] CI	[-0.695 0.000] CI	[-0.595 -0.000] CI

[†] The figure traces out point estimates of the population bounds.

[*] The lower bounds decay less rapidly with Q_U if the no false positive errors assumption is additionally imposed, while the upper bounds remain unchanged.

[a] Confidence intervals around ATE are calculated using methods from Imbens-Manski (2004) with 1,000 pseudosamples.

Appendix Table 1

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.