Identifying the Effect of WIC on Very Low Food Security among Infants and Children

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Abstract: The Women, Infants, and Children Program (WIC) is considered a crucial component of the social safety net in the United States, yet there is limited supporting evidence on the effects of WIC on the nutritional well-being and food security of infants and young children. Two key identification problems have been especially difficult to address. First, the decision to take up WIC is endogenous as households are not randomly assigned to the program; recipients are likely to differ from nonrecipients in unobserved ways (e.g., prior health) that are related to associated outcomes. Second, survey respondents often fail to report receiving public assistance, and the existing literature has uncovered substantial degrees of systematic misclassification of WIC participation. Using data from the National Health and Nutrition Examination Survey (NHANES), we apply recently developed partial identification methodologies to jointly account for these two identification problems in a single framework. Under relatively weak assumptions, we find that WIC reduces the prevalence of child food insecurity by at least 5.5 percentage points and very low food security by at least 1.5 percentage points.

Keywords: Women, Infants, and Children (WIC) Program, food insecurity, partial identification, treatment effect, nonparametric bounds, nonclassical measurement error, classification error

JEL classification numbers: I12, I38, C14, C21

Executive Summary

The Women, Infants, and Children Program (WIC) is considered a crucial component of the social safety net in the United States, serving over eight million participants per month including nearly half of all infants and more than a quarter of all children younger than five (USDA, 1999). Given the size, importance, and continuing popularity of WIC, it is vital that policymakers and program administrators have a thorough understanding of the costs and benefits associated with the program. Yet, there is only modest supporting evidence on the efficacy of WIC benefits.

Two key identification problems have hampered progress. First, the decision to take up WIC is endogenous as households are not randomly assigned to the program; recipients are likely to differ from nonrecipients in ways (e.g., prior health status) that are related to outcomes even after controlling for observed characteristics. Second, survey respondents often fail to report receiving public assistance, and the existing literature has uncovered substantial degrees of systematic misclassification of WIC participation. Classification errors are thought to be nonrandom and pervasive.

Using data from the National Health and Nutrition Examination Survey (NHANES), we evaluate the efficacy of WIC in reducing the prevalence of very low food security among infants and children under the age of five. Previous studies have found that children growing up in food insecure families are at heighted risk of suffering from numerous health and nutrition deficiencies. This is particularly concerning given the unprecedented number of households classified as having very low food security in recent years – 6 percent and rising in 2008 (Nord et al., 2009). Our empirical results enable policymakers and program administrators to better understand the effects of WIC on very low food security.

We make several contributions to the literature evaluating WIC. First, we study the effects of WIC on food insecurity using recently developed partial identification bounding methods that jointly account for the endogenous selection and systematic classification error problems in a unified framework (Gundersen, Kreider, and Pepper, 2010; Kreider, Pepper, Gundersen, and Jolliffe, forthcoming). Importantly, these methods 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 consequences of WIC for food security. Second, while much of this literature has focused on the impact of WIC on the health of newborns (Currie, 2003), we also examine the impact of WIC on older infants and children eligible for assistance. Third, our analysis makes transparent how assumptions on the selection and reporting error processes shape inferences about the causal impacts of WIC.

Under relatively weak assumptions, we find that WIC substantially reduces the prevalence of food insecurity and very low food security. In particular, we find that the program reduces the prevalence of child food insecurity by at least 5.5 percentage points and very low food security by at least 1.5 percentage points. These impacts are substantial and significantly different from zero. Under sensible modeling restrictions, our evidence suggests that WIC leads to notable reductions in food insecurity and very low food security among income-eligible infants and children.

I.   Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is considered a crucial component of the social safety net in the United States designed to "provide supplemental and nutritious food as an adjunct to good health during such critical times of growth and development [during pregnancy, the postpartum period, infancy, and early childhood] in order to prevent the occurrence of health problems" (P.L. 94-105). Program participants receive a combination of monthly nutritious food packages, nutritional education, and access to health services. To evaluate the efficacy of the program, a literature has studied the impact of WIC on nutrient intake, various anthropometric measures, health outcomes, use of medical care, rates of breast-feeding, and other related outcomes (see, e.g., Currie (2003) and Hoynes et al. (2011) for overviews of this literature). These questions are especially pressing in light of an unprecedented 40% increase in the very low food security rate from 2007 to 2008 (Nord et al., 2009). In general, these papers find that WIC has a modest positive impact on well-being.

Yet, there are two important limitations with this literature. First, much of this literature has focused on the impact of WIC on the health of newborns but not on older infants and children who are also eligible for assistance (Currie, 2003). Second, existing research has struggled to identify the causal impacts of nutritional programs like WIC due to the presence of two important identification problems. A *selection problem* arises because counterfactual outcomes cannot be observed in the data, and participation in WIC is not randomly assigned. Instead, participation is endogenously determined: unobserved factors such as expected future health status, parents' human capital characteristics, and financial stability are thought to be jointly related to both WIC participation and associated outcomes. In addition, a nonrandom *classification error problem* arises because large fractions of WIC recipients fail to report their

program participation in household surveys. Comparisons of administrative and survey data have found that as many as one in three survey respondents misreport their WIC participation status, with the vast majority of these misreports reflecting errors of omission.[1,2] Clearly, many important questions about the efficacy of WIC remain unanswered. A recent Institute of Medicine (IOM) report argues that "the time has come to initiate a new program of research" (p. 1) and specifically highlights the importance of assessing whether WIC reduces the likelihood of food insecurity (p. 114) (IOM, 2011).

Why has it been so difficult to evaluate food assistance programs in the United States? Perhaps the most important reason is that standard instrumental variable approaches have been difficult to implement because most of the key policy rules have not varied significantly across states or over time. There have been significant indirect changes in WIC eligibility over time arising from changes in related programs like Medicaid and AFDC/TANF which confer adjunctive eligibility into WIC (see Swann, 2010). Such changes, however, may not be exogenous with respect to many health-related outcomes including food security.

While some analyses of WIC have attempted to address the selection problem,[3] we are unaware of any analyses of the impact of WIC on health or food security that attempt to account for the classification error problems. Classical measurement error models do not apply when the inaccurately measured covariate is discrete (see, e.g., Bollinger, 1996), when errors are thought

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[1] See, e.g., Meyer et al. (2009), Cole and Lee (2004), Bitler et al. (2003), Ver Ploeg and Betson (2003), and Cody and Tuttle (2002) for details on the measurement error problem.

[2] In addition, self-reports of food security may also be mismeasured. For example, some parents might misreport being food secure if they feel ashamed about heading a household in which their children are not getting enough food to eat (Hamelin et al., 2002). Alternatively, some WIC recipients might exaggerate food hardships if they believe that to report otherwise could jeopardize their eligibility for benefits. We do not address problems measuring food insecurity in this paper.

[3] For example, Hoynes et al. (2011) evaluate the performance of WIC at the time of its establishment as a pilot program in 1972. They address the selection problem by exploiting the plausibly exogenous variation in participation due to the staggered introduction of the program in the 1970s. Employing a difference-in-differences technique using WIC program data, they report that the introduction of WIC has a beneficial causal effect on infant health. However, the authors acknowledge that the number of the treated women (who received WIC benefits during its early years) is only an indirect estimate.

to be systematic in a particular direction (e.g., underreporting of public transfers), or when the errors may be correlated with other characteristics of the respondents.

In this paper, we evaluate the causal impacts of WIC on alleviating very low food security among infants and children under the age of five. Previous studies have found that children growing up in food insecure families are at heighted risk of suffering from numerous health and nutrition deficiencies.[4] This is particularly concerning given the unprecedented number of households (6% of all U.S. households) classified as having very low food security in 2008 (Nord et al., 2009). We are not aware, however, of any research that explicitly focuses on the impact of WIC on very low food security among infants and children.

Using data from the National Health and Nutrition Examination Survey (NHANES), we apply recently developed nonparametric partial identification methodologies to jointly account for the selection and measurement problems. Importantly, these methods allow us to consider weaker assumptions than required under conventional parametric approaches.[5] By relaxing traditional assumptions, we shed light on the causal impacts of this key nutritional program. Specifically, we provide tight bounds on the average treatment effect of WIC on food security status for infants and children.

After describing the data in Section II, we formally define the empirical questions and the identification problems in Section III. Our analysis is complicated by two distinct identification problems: (1) 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 WIC benefits are received) and (2) the classification error problem that arises because the data

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[4] See Gundersen and Kreider (2008) for an overview.
[5] As foundation for this line of research, see, e.g., Kreider and Pepper (2007, 2008, 2011), Kreider, Pepper, Gundersen, and Jolliffe (2009), Kreider and Hill (2009), Gundersen and Kreider (2008, 2009), Pepper (2000), Molinari (2008, 2010), and Shaikh and Vytlacil (forthcoming). Related work on other nutrition programs (Gundersen, Kreider, and Pepper, 2011; Roy, 2012, and Kreider, Pepper, Gundersen, and Jolliffe, forthcoming) have found that these less restrictive assumptions are straightforward to motivate in practice and can result in informative bounds on the causal impacts of interest.

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cannot reveal respondents with misclassified participation status. We begin by examining what can be learned without imposing any assumptions on the selection process. We 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 WIC is monotonically related to poor latent health outcomes. We then 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). 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 WIC does not worsen health status. Many have argued that participating in food assistance programs would not cause health or food security to deteriorate (e.g., Currie 2003).

We then introduce classification errors in the model. Using the methods developed in Kreider et al. (forthcoming), we make two notable contributions to the WIC literature. First, we simultaneously account for both the selection and classification error problems. We also utilize administrative information on the size of the WIC caseload to derive informative constraints on the classification error problem.

We present our results in Section IV and draw conclusions in Section V. By layering successively stronger sets of assumptions, we provide tighter sets of bounds on the impacts of WIC on food security. In this vein, we make transparent to researchers and policymakers how the strength of the conclusions are tied to the strength of the assumptions the researcher is willing to make. Under the weakest assumptions, there is very little that can be inferred about the impact of WIC. Under a set of plausible assumptions, however, we provide narrow bounds on

average treatment effects which suggest that WIC notably reduces the prevalence of very low food security among infants and children.

II.   Data

To study the impact of WIC on measures of food security, we use data from the 1999-2008 NHANES.[6] 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).

Our analysis focuses on households with children who are age and income eligible to receive assistance from WIC. In particular, we restrict the sample to infants and children less than five years old with family incomes less than 185% of the Federal Poverty Level (FPL).[7,8] Our sample includes 4,614 children who reside in households with income less than 185% of the federal poverty line. On average, these households have income just over 90% of the FPL.

For each respondent, we observe a self-reported measure of WIC receipt over the past year. In total, respondents report that 62.9% of the households and 39.2% of the children report participating in WIC. Why might income-eligible households not participate in WIC? A

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[6] In future drafts of this paper, we intend to study the robustness of our results to data from the Current Population Survey (CPS).

[7] Several other features of the eligibility criteria are not incorporated into our sample restrictions. Most notably, participants in TANF, SNAP and Medicaid are eligible regardless of income. In addition, participants must be determined to be at nutritional risk (see Currie, 2003, p. 215 for details). In practice, however, this condition does not seem to be binding; nearly all income-eligible children are certified to be "at risk" (Currie, 2003, p. 215).

[8] Future drafts of this manuscript may include data on households with income just above the 185% FPL threshold in a type of modified regression discontinuity design similar to the one applied in Gundersen et al. (2012).

common explanation is that for some eligible households the costs of participating in the program outweigh the benefits (e.g., Moffitt 1983). Another part of the story is that not all income-eligible households can receive benefits; some may not meet other eligibility criteria, and even eligible households can be denied benefits when available resources are limited – WIC is not an entitlement program.

Finally, self-reported measures of WIC receipt are underreported. Comparing aggregate statistics obtained from self-reported survey data with those obtained from administrative data, numerous studies highlight substantial underreporting in many different surveys including the CPS, the Survey of Income and Program Participation (SIPP), the Panel Study of Income Dynamics (PSID), and the Consumer Expenditure Survey (CES) (e.g., Meyer et al., 2009; Cole and Lee, 2004; Bitler et al., 2003; Ver Ploeg and Betson, 2003; and Cody and Tuttle, 2002). For example, Meyer, Mok, and Sullivan (2009, Table 17) find that self-reports in CPS reflect just over half (57%) the number of WIC recipients identified in administrative data. Using data from the SIPP, Cody and Tuttle (2002) find error rates of up to 19 percent, and Trippe (2000) finds error rates of up to 30 percent.

In addition to observing a self-reported measure of WIC receipt, we also observe the information required to compute food security indicators. To calculate 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. Each question is designed to capture some aspect of food insecurity and, for some questions, the frequency with which it manifests itself (see Gundersen and Kreider, 2008).[9] Following official definitions, we use these 18 questions to construct a comparison of children in food secure households (two or fewer

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[9] Examples include "I worried whether our food would run out before we got money to buy more," "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?"

affirmative responses) with children in food insecure households (three or more affirmative responses) and those in very low food secure households (five or more affirmative responses).

Means and standard deviations for the variables used in this study are displayed in Table 1.[10] The rate of food insecurity is slightly higher among households that report participating in WIC. Not quite 17% of children reported as WIC recipients are food insecure compared with about 15% of income-eligible nonparticipants. At around 1.4%, the rates of very low food security are almost identical across reported participation status.

III. Identifying the Average Treatment Effect of WIC on Very Low Food Security

Under various sets of maintained assumptions, our analysis "partially identifies" parameters by deriving "identification regions" that specify sets of parameter values that are logically consistent with the observed data and imposed statistical and behavioral assumptions. Our approach allows for the possibility that respondents may misreport the household's true participation status and that selection into the program is nonrandom. Given classification errors in the treatment variable, Manski's (1995) basic selection bounds no longer apply. Instead, we apply recently introduced nonparametric methods (e.g., Molinari, 2010; Kreider and Hill, 2009; Kreider et al., forthcoming) to assess how identification depends on assumptions about data errors and the selection process. The data coupled with restrictions on the classification error and selection problems will allow us to provide narrow bounds on the impact of WIC participation in alleviating children's very low food security.

To make these ideas concrete, we introduce the following notation. Let $W^* = 1$ denote that a child truly receives WIC, with $W^* = 0$ if the child does not receive WIC. We observe a self-reported measure of participation, W, where $W = 1$ if the household reports receiving WIC

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[10] Estimates in this paper are weighted to account for the complex survey design used in the NHANES.

and $W = 0$ otherwise. [11] Finally, let $Y = 1$ indicate very low food security status, with $Y = 0$ otherwise.

Our primary interest is in learning about the average treatment effect (ATE) of a child's WIC participation on very low food security. The ATE is given by

$$ATE = P[Y(W^* = 1) = 1 | x \in \Omega] - P[Y(W^* = 0) = 1 | x \in \Omega] \tag{1}$$

where $Y(W^* = j)$ indicates the potential food security outcome under treatment j and $x \in \Omega$ denotes conditioning on observed covariates whose values lie in the set Ω. The average treatment effect reveals how the fraction of households suffering from very low food security would differ between the case in which all eligible persons received WIC versus the case that no eligible persons receive WIC. Conditioning on x allows us to focus on specific subpopulations of interest. We focus on the population of income-eligible households and subgroups based on age – i.e., infants and children under the age of five. For ease of notation, we suppress the conditioning on Ω in what follows.

A. *WIC and Very Low Food Security In the Presence of Selection*

Even if WIC participation is assumed to be accurately reported for all respondents $(W^* = W)$, one cannot identify the ATE without additional assumptions. The difficulty is that the potential outcome $Y(W^* = 1)$ is observed in the data only for households that chose to participate in WIC, while the potential outcome $Y(W^* = 0)$ is observed only for households that chose not to participate.

To illustrate the selection problem, the first component of (1) can be written as:

$$P[Y(W^* = 1) = 1] = P[Y(W^* = 1) = 1 | W^* = 1]P(W^* = 1) \tag{2}$$

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[11] Throughout this discussion, an asterisk denotes that the variable is unobserved.

$$+ P[Y(W^* = 1) = 1 | W^* = 0]P(W^* = 0).$$

If reports of WIC participation are known to be accurate, we can identify $P(W^* = 1)$ and

$P(W^* = 0)$, the fractions of the eligible population receiving and not receiving WIC, and

$P[Y(W^* = 1) = 1 | W^* = 1]$, the fraction of WIC recipients with very low food security. Not

identified, however, is the counterfactual probability of very low food security among

nonrecipients had they received WIC, $P[Y(W^* = 1) = 1 | W^* = 0]$. Absent other information, this

value could lie anywhere between 0 and 1. Taking these extreme cases, we can bound (2) as:

$$P[Y(W^* = 1) = 1 | W^* = 1]P(W^* = 1)$$
$$\leq P[Y(W^* = 1) = 1] \leq P[Y(W^* = 1) = 1 | W^* = 1]P(W^* = 1) + P(W^* = 0). \qquad (3)$$

Each of the terms in these bounds is identified by the observed data. We can analogously bound

the quantity $P[Y(W^* = 0) = 1]$. Taking the difference between the upper bound on

$P[Y(W^* = 1) = 1]$ and the lower bound on $P[Y(W^* = 0) = 1]$ obtains a sharp upper bound on the

ATE, and analogously a sharp lower bound (Manski, 1995). Plug-in estmimators are used to

consistently estimate these "worst case" bounds, and confidence intervals are found using

methods developed in Imbens and Manski (2004).

　　To narrow the bounds on the impact of WIC on very low food security, prior information

to address the selection problem must be brought to bear. The exogenous selection assumption

$P[Y(1) = 1] = P[Y(1) = 1 | W^*]$ maintained in much of the literature, for example, point identifies the

average treatment effect, but this assumption seems untenable. Instead, we consider a number of

middle ground assumptions that narrow the bounds by restricting the relationship between WIC

participation, food security, and observed covariates. In particular, we apply three common

monotonicity assumptions.

First, we apply the *Monotone Treatment Selection* (MTS) assumption (Manski and Pepper, 2000) that households participating in WIC are likely to have worse latent food security outcomes on average than income-eligible nonparticipants. This selection assumption formalizes the idea that unobserved factors associated with food insecurity are thought to be positively associated with the decision to take up the program. In fact, the literature on WIC suggests that recipients are disadvantaged in comparison with nonrecipients across several economic characteristics (Bitler and Currie, 2004; Gundersen, 2005) and over food insecurity (Bitler et al., 2005). Thus, for these outcomes, we assume the following MTS restrictions hold:

$$P[Y(W^* = j) = 1 | W^* = 0] \leq P[Y(W^* = j) = 1 | W^* = 1], \; j = 1, 0.$$

That is, for latent potential outcomes $Y(0)$ and $Y(1)$, eligible households that receive WIC, $W^* = 1$, have no lower latent very low food security rates than eligible households that have not taken up WIC, $W^* = 0$. While the MTS assumption serves to reduce the upper bound on the ATE, the assumption alone does not identify the sign of the average treatment effect (see Manski and Pepper, 2000).

Second, we apply a *Monotone Instrumental Variable* (MIV) assumption (Manski and Pepper, 2000) that the latent probability of food insecurity weakly decreases with household income adjusted for family composition. A large body of empirical research supports the idea of a negative gradient between reported income and food insecurity (e.g., Nord *et al*. 2010). To formalize this idea, let v be the monotone instrumental variable such that

$$u_1 < u < u_2 \Rightarrow P[Y(W^* = 1) = 1 | v = u_1] \geq P[Y(W^* = 1) = 1 | v = u] \geq P[Y(W^* = 1) = 1 | v = u_2].$$

While these conditional probabilities are not identified, they can be bounded using the methods

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described above. Estimation details are provided in Kreider and Pepper (2007).[12] In other related applications, similar MIV assumptions have been effective in substantially narrowing the range of uncertainty about treatment effects (e.g., Kreider et al., forthcoming; Gundersen et al., 2011; Kang 2008).

Finally, we apply the *Monotone Treatment Response* (MTR) assumption that the food security status of a nonrecipient child would not deteriorate if the household took up WIC. This assumption, which implies that WIC cannot increase rates of very low food security, is fairly innocuous in the context of WIC participation given that food products purchased with WIC are nutritionally sound. This assumption, which was introduced by (Manski, 1995; 1997), implies

$$Y(W^* = 1) \leq Y(W^* = 0).$$

B.   *WIC and Food Security Outcomes Under Measurement Error and Selection*

The selection bounds in (3) presume that everyone reports WIC participation accurately. With reporting errors, however, these bounds are no longer valid because we are confronted with uncertainty not only about counterfactuals but also about the reliability of the data. In this case, we must incorporate the potential reporting errors as defined above. For example, the quantity $P[Y(W^* = 1) = 1]$ becomes:

$$P[Y(W^* = 1) = 1] = P[Y(W^* = 1) = 1, W^* = 1] + P[Y(W^* = 1) = 1 | W^* = 0] P(W^* = 0)$$

$$= [P(Y = 1, W = 1) - \theta_1^+ + \theta_1^-] \ + P[Y(W^* = 1) = 1 | W^* = 0] \Big[P(W = 0) + (\theta_1^+ + \theta_0^+) - (\theta_1^- + \theta_0^-) \Big] \ (4)$$

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[12] Following the approach developed in Kreider and Pepper (2007), we estimate these MIV bounds by first dividing the sample into 20 equally sized groups delineated by an increasing ratio of income to the poverty line (10 groups for the subsample of infants when assessing very low food security due to smaller sample sizes). Then, to find the MIV bounds on the rates of food insecurity, 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. Since this MIV estimator is consistent but biased in finite samples (see Manski and Pepper, 2000 and 2009), we employ Kreider and Pepper's (2007) modified MIV estimator that accounts for the finite sample bias using a nonparametric bootstrap correction method.

where $\theta_1^+ \equiv P(Y=1, W=1, Z^*=0)$ and $\theta_1^- \equiv P(Y=1, W=0, Z^*=0)$ denote the unobserved fraction of false positive and false negative WIC participation reports among very low food secure children. Similarly, $\theta_0^+ \equiv P(Y=0, W=1, Z^*=0)$ and $\theta_0^- \equiv P(Y=0, W=0, Z^*=0)$ denote the fraction of false positive and false negative WIC participation reports, respectively, among more food secure children. Without imposing assumptions on the measurement error probablities, θ, the conditional probability in (4) can lie anywhere between 0 and 1.

Given assumptions on the degree or pattern of reporting errors, however, we can restrict their logically feasible ranges. Following the approach developed in Kreider et al. (forthcoming), notice that

$$[-P(Y=1, W=0) - P(Y=0, W=1)] + \Theta$$

$$\leq ATE(1,0) \leq \tag{5}$$

$$[-P(Y=1, W=1) - P(Y=0, W=0)] + \Theta$$

where $\Theta \equiv (\theta_1^- + \theta_0^+) - \theta_0^- + \theta_1^+$. Two sources of information allow us to restrict Θ. First, suppose administrative data on WIC reveal $P^* \equiv P(W^* = 1)$. In particular, we estimate that the true participation rate of infants and children younger than five, P^*, equals 0.51.[13] The analogous self-reported rate, $P \equiv P(W=1)$, is 0.39 (see Table 1). Given this information, Proposition 1 in Kreider et al. (forthcoming) shows how knowledge of the true and self-reported rates imply meaningful restrictions on the classification error probabilities, Θ, and thus on the

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[13] Administrative data reported in Bitler et al. (2003) and Meyers et al. (2009) reveal that each year an average of about 5.8 million infants and children participated in WIC over our sample period. The NHANES data (see also Bitler et al., 2003, Table 6) reveals about 11.3 million infants and children (per year) are income-eligible for assistance. Thus, we estimate the true participation rate, P^*, equals 0.51.

average treatment effect.[14] For example, it follows that the net fraction of false negative reports must equal the difference in the true and self-reported participation rates $P(W^* = 1) - P(W = 1)$. So, in this application, at least 12 percent (0.52 – 0.39) of the respondents incorrectly report that their children do not participate in WIC.

While our estimate of P^* is similar to findings in Bitler et al. (2003) and Meyers et al. (2009), there are reasons to question whether this is an accurate measure of the true participation rate. Participation rates computed from administrative data may be inaccurate, and the administrative data may not perfectly overlap the time periods covered in this study. As noted above, there are also likely to be errors in classifying eligible children. Finally, it is possible that a respondent could both be correctly coded as "receiving WIC" in the administrative records and "not receiving WIC" in the survey records. Administrative records count someone as "receiving WIC" whether or not they redeem their coupons, but many WIC coupons go unredeemed (especially for older children). Some recipients might report that they do not receive WIC if they do not redeem the coupons. Given these concerns, we will assess the sensitivity of our estimates to the value of the true participation rate.

IV.   Results

In this section, we present estimated bounds on the average effect of WIC on food insecurity and very low food security for infants and children. The methodological approach outlined in Section III allows us to estimate bounds under different sets of assumptions to address the selection and measurement error problems. A central objective is to reveal how the strength of the conclusions varies with the underlying identifying assumptions. In doing so, we provide a range of results that rely on different sets of assumptions. We begin in subsection A

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[14] Additional analysis is required to address the classification error problem under the MTS assumption. See Proposition 2 in Kreider et al. (forthcoming).

by focusing on the conventional assumption that self-reported measures of WIC participation are accurate. We relax this no-errors assumption in Section B, instead assuming that there are no false positive reports of WIC participation but that the true participation rate remains 0.51, which is 0.12 points higher than the self-reported rate of 0.39. In this case, we know that 12 percent of the income-eligible population provides false negative reports. We present our most preferred models for selection and classification error in subsection B. Results for these models indicate that WIC confers substantial benefits for infants and children. Finally, we conduct a sensitivity analysis in subsection C. Here, we allow for arbitrary classification errors and for the possibility that the true participation rate may differ from 0.51.

A. *No Errors Case*

Table 2A presents estimated upper bounds on the average treatment effect under the assumption that respondents accurately report participating in WIC.[15] To address the selection problem, we additionally apply the joint MTS-MIV assumption and the MTS-MIV-MTR assumption. Estimates are provided for both food insecurity and very low food security. Separate sets of estimates pertain to the full sample of children under five, the subpopulation of infants (younger than one), and the subpopulation of children aged one through four. To provide a basis for assessing the magnitude of these estimated upper bounds, we also display the estimated lower bound on the rate of food insecurity under the scenario that WIC did not exist, $P[Y(0) = 1]$.

In all cases, we find that the estimated upper bounds are negative and statistically different than zero (at the 10% significance level). For example, consider the impact of WIC on food insecurity under the MTS-MIV assumption. In this case, the estimates suggest that WIC

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[15] Lower bounds on the ATE are presented in Section IV.C. These estimated lower bounds are consistent with large reductions in the prevalence of food insecurity.

reduces the rate of food insecurity for infants by at least 0.145 and for children aged one through four by at least 0.097. These estimates are substantial. In the absence of WIC, our lower bound estimates of $P[Y(0)=1]$ indicate that food insecurity rates would be at least 22.3 percent for income-eligible infants and 19.7 percent for children. Thus, WIC appears to reduce the prevalence of food insecurity by at least 65 percent (=0.145/0.223) for infants and 49 percent for children. Most strikingly, under these models we find that WIC nearly eliminates very low food security for both infants and children.

B.   *No False Positive Errors, P* = 0.51*

    While these initial findings imply that WIC plays an important role in reducing the prevalence of food insecurity among infants and children, we have not yet accounted for classification errors. As noted above, validation studies suggest substantial reporting errors, particularly false negative reports. Table 2B presents estimated upper bounds under the assumptions that there are no false positive reports and that the true participation rate in WIC is 0.51, the estimated rate based on administrative data. Given that the self-reported rate is 0.39, this no-false positives classification error model implies that 12 percent of respondents incorrectly report that their children do not participate in WIC.

    Under this classification error model, we find that upper bounds are sensitive to the assumptions used to address the selection problem. Under the joint MTS-MIV restriction, the upper bounds are positive but statistically insignificant while the lower bounds (not shown) are negative and substantial. Thus, under this model, we cannot rule out the possibility that WIC has a large positive, large negative, or negligible impact on food insecurity. Adding the MTR assumption, however, reduces the upper bound such that the estimates are all negative and statistically significant. In particular, the estimates suggests that WIC reduces the rate of food insecurity for infants by at least 0.050 and for children by at least 0.054. In the absence of WIC,

our estimates of $P[Y(0)=1]$ indicate that food insecurity rates would be at least 16.2 percent for income-eligible infants and 20.2 percent for children age one through four. Thus, the estimates indicate that WIC reduces the prevalence of food insecurity by at least 31 percent (=0.145/0.223) for infants and 27 percent for the older subpopulation of children. In addition, we find that WIC appears to reduce very low food security for children by at least 60 percent and again nearly eliminate it for infants.

C.   *Sensitivity Analysis*

While the models considered so far appear to have strong foundations in the literature, it is constructive to consider the sensitivity of inferences to varying the underlying assumptions. 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 WIC participation rate, $P^* = P(W^* = 1)$, and under various assumptions about the selection process.

We begin by evaluating the bounds in the worst case in which there is no prior information that can be used to address the selection problem. Figures 1A and 1B trace out the estimated worst-case bounds for the ATE on the food insecurity and very low food security rates, respectively, across all values of P^* between 0 and 1. The accompanying tables highlight these results when the true WIC participation rate, $P(W^* = 1)$ equals (a) the NHANES self-reported participation rate of $P = 0.39$ and (b) our preferred estimated true participation rate of 0.51 based on administrative data. The solid lines in the figures trace out the estimated arbitrary error bounds in which there are no restrictions imposed on the nature or degree of errors except those implied by the knowledge of self-reported and true participation rates. The dashed lines display the estimated bounds under the further restriction of no excess errors – i.e., no false positives for

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the likely case that P^* exceeds P, and no false negatives for the less likely opposite case (see Kreider et al., forthcoming).

The worst-case bounds on the ATE if WIC receipt is accurately reported have a width of one and always include zero. For food insecurity, these worst-case no errors bounds are [-0.421, 0.579] as depicted in the figure by the solid vertical line at $P^* = P$. Allowing for classification errors notably increases the width of these bounds for food insecurity. For example, suppose the true participation rate remains equal to the self-reported rate of 0.390, but now one only imposes the assumption of no *net* reporting errors such that the rate of false positives equals the rate of false negatives. Then, as shown in Figure 1A and the accompanying table, the ATE bounds on the food insecurity rate expand from [-0.421, 0.579] to [-0.551, 0.766], with a width of 1.32. Interestingly, however, if the true participation rate is 0.51 (the rate consistent with administrative data) instead of 0.390, the width of the bounds does not notably change. Still, 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.

Introducing the MTS assumption notably reduces the upper bound. If WIC receipt is accurately reported, the estimated bounds are [-0.421, 0.012] for the food insecurity rate and [-0.396, 0.000] for the very low food security rate. Thus, under this model, we find that WIC may substantially reduce rates of food insecurity with very little downside risk of a harmful effect. Allowing for classification errors increases the upper bounds, but they are still notably smaller than the upper bounds found under the worst-case model. Consider the case of evaluating the impact of WIC on food insecurity when $P^* = 0.51$. The no false positives assumption decreases the ambiguity associated with measurement error from 1.32 to 1.09 points, and the MTS assumption further reduces the width of the bound to 0.85. Still, while these two assumptions have substantial identifying power, the wide bounds presented in Figure 1 highlight the difficulty of making strong inferences in light of the selection and measurement error

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problems. In the absence of additional restrictions that address the selection problem, we generally cannot rule out the possibility that WIC has a large positive or negative impact on the likelihood of food insecurity.

To further narrow the bounds, we assess the identifying power of the joint MTS-MIV and joint MTS-MIV-MTR assumptions. For the food insecurity outcome, these results are traced out in Figure 2A, which applies the MTS-MIV assumption, and Figure 2B, which layers on the MTR assumption. The associated tables present Imbens-Manski (2004) confidence intervals that cover the true value of the ATE with 90% probability and the estimated finite-sample biases using the methods described in Kreider and Pepper (2007).

Under the MTS-MIV model, we find that the bounds are strictly negative only if the degree of WIC misreporting is very small. Once we allow for any non-negligible reporting error, the estimated upper bound is positive suggesting the one cannot rule out the possibility that WIC increases food insecurity. Under the joint MTR-MTS-MIV assumption (see Figure 2B), however, the average treatment effect is strictly negative even for large degrees of arbitrary WIC misreporting. For example, the estimated bounds on the ATE vary from [-0.520, -0.055] when $P^* = 0.39$ to [-0.633, -0.055] when $P^* = 0.51$. In all cases, the estimates are statistically different than zero at the ten percent significance level. Thus, under this model, we find that WIC reduces the food insecurity rate by at least 6 percentage points and perhaps much more. These results suggest that WIC dramatically reduces the likelihood of becoming food insecure.

Figures 3A and 3B provide analogous results for the very low food security outcome. Since the prevalence of very low food security is relatively close to zero (see Table 1) and, as a result, the estimate upper bounds under an MTS restriction also tend to be close to zero (see Figure 1B), we rescale the figures to focus on the upper bounds and only on cases where P^* varies between 0.35 and 0.60.

The results for very low food security are qualitatively similar to those found for food insecurity. Under the MTS-MIV models, the upper bounds are negative only if one is willing to rule out any non-negligible amount of measurement error. Otherwise, the results do not rule out the possibility that WIC increases very low-food security. Under the joint MIV-MTS-MTR assumption, however, the estimated upper bounds are negative and substantial, suggesting that WIC reduces the prevalence on very low food security by at least 2 percentage points.

The results presented in Figures 1-3 clearly reveal the sensitivity of inferences to the underlying models used to address the measurement error and selection problems. Under the weakest models, we cannot rule out the possibility that WIC increases or decreases food insecurity, especially if there are classification errors. Yet, under the MTS-MIV-MTR model, the results suggest strictly beneficial effects even under large degrees of arbitrary classification error. In this case, we find that WIC reduces the rate of food insecurity by at least 5.5 percentage points, or nearly 30 percent, and the rate of very low food security by 1.5 percentage points, or nearly 70 percent.

VI.   Conclusion

We considered the impact of WIC on child food insecurity and very low food security by applying methods that account for both the endogenous selection and nonrandom classification error problems. The partial identification approach is well-suited for this application in which conventional assumptions strong enough to point-identify the causal impacts of WIC are not necessarily credible and there remains much uncertainty about even the qualitative impacts of WIC, especially for children.

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 WIC. The worst-case selection bounds on the average

treatment effect always include zero, and classification errors can generate substantial additional uncertainty about the efficacy of WIC in alleviating food insecurity. This ambiguity, however, is substantially mitigated by applying relatively weak assumptions on the selection and classification error processes.

Under our preferred joint MTS-MIV-MTR model, we find that WIC substantially reduces the prevalence of food insecurity and very low food security. Given errors of omission consistent with $P^* = 0.51$, the estimated true WIC participation rate for our sample, the program is estimated to reduce the prevalence of child food insecurity by at least 5.5 percentage points and very low food security by 1.5 percentage points. These impacts are significantly different from zero. Thus, although there remains considerable ambiguity induced by the selection and measurement error problems, we find evidence under sensible modeling restrictions that WIC leads to substantial reductions in food insecurity and very low food security among income-eligible infants and children.

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Table 1: Weighted Means and Standard Deviations by Program Participation

A. Eligible Infants and Children Younger than Age 5

	All	Child Recipients (W = 1)	Nonrecipients (W = 0)
WIC recipient (HH)	0.629 (0.483)	0.912 (0.293)	0.446 (0.497)
WIC recipient (child)	0.392 (0.488)	1.000 (0.000)	0.000 (0.000)
Food insecure child	0.159 (0.365)	0.166 (0.372)	0.154 (0.361)
Very low food secure child	0.014 (0.116)	0.013 (0.115)	0.014 (0.112)
N	4614	1981	2633

B. Infants (Age < 1)

	All	Child Recipients (W = 1)	Nonrecipients (W = 0)
WIC recipient (HH)	0.844 (0.363)	0.962 (0.192)	0.722 (0.448)
WIC recipient (child)	0.511 (0.500)	1.000 (0.000)	0.000 (0.000)
Food insecure child	0.134 (0.341)	0.127 (0.333)	0.142 (0.349)
Very low food secure child	0.013 (0.114)	0.009 (0.094)	0.017 (0.131)
N	1506	777	729

C. Children Aged 1-4

	All	Child Recipients (W = 1)	Nonrecipients (W = 0)
WIC recipient (HH)	0.576 (0.494)	0.895 (0.306)	0.394 (0.489)
WIC recipient (child)	0.363 (0.481)	1.000 (0.000)	0.000 (0.000)
Food insecure child	0.165 (0.371)	0.180 (0.384)	0.156 (0.363)
Very low food secure child	0.014 (0.116)	0.015 (0.121)	0.013 (0.114)
N	3108	1204	1904

Table 2: Bias-Corrected Upper Bound ATEs

A. No Classification Errors

	Food Insecurity		Very Low Food Security	
	LB $P[Y(0)=1]$	ATE	LB $P[Y(0)=1]$	ATE
Full Sample:				
MTS-MIV	0.199	-0.079*	0.028	-0.027*
MTS-MIV-MTR	0.202	-0.085*	0.028	-0.027*
Infants (age < 1):				
MTS-MIV	0.223	-0.145*	0.054	-0.054*
MTS-MIV-MTR	0.223	-0.145*	0.054	-0.054*
Children aged 1-4:				
MTS-MIV	0.197	-0.097*	0.024	-0.024*
MTS-MIV-MTR	0.206	-0.112*	0.025	-0.024*

B. No False Positive Errors

	Food Insecurity		Very Low Food Security	
	LB $P[Y(0)=1]$	ATE	LB $P[Y(0)=1]$	ATE
Full Sample:				
MTS-MIV	0.033	0.225	0.000	0.012
MTS-MIV-MTR	0.194	-0.055*	0.022	-0.015*
Infants (age < 1):				
MTS-MIV	0.000	0.117	0.000	0.008
MTS-MIV-MTR	0.162	-0.050*	0.013	-0.012*
Children aged 1-4:				
MTS-MIV	0.122	0.090	0.000	0.017
MTS-MIV-MTR	0.202	-0.054*	0.020	-0.012*

* = Statistically different than zero at the 10% level

Figure 1A. Sharp Bounds on the ATE for **Child Food Insecurity** as a Function of P^*
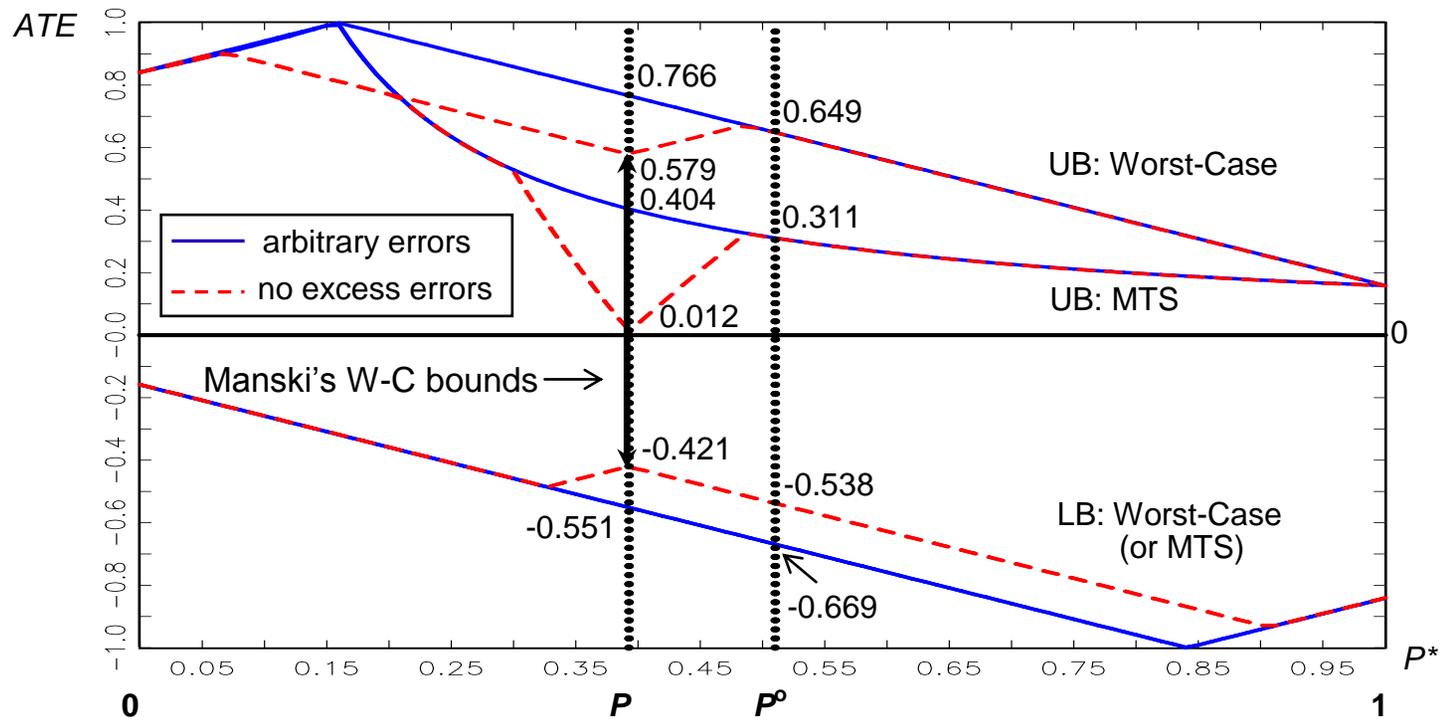
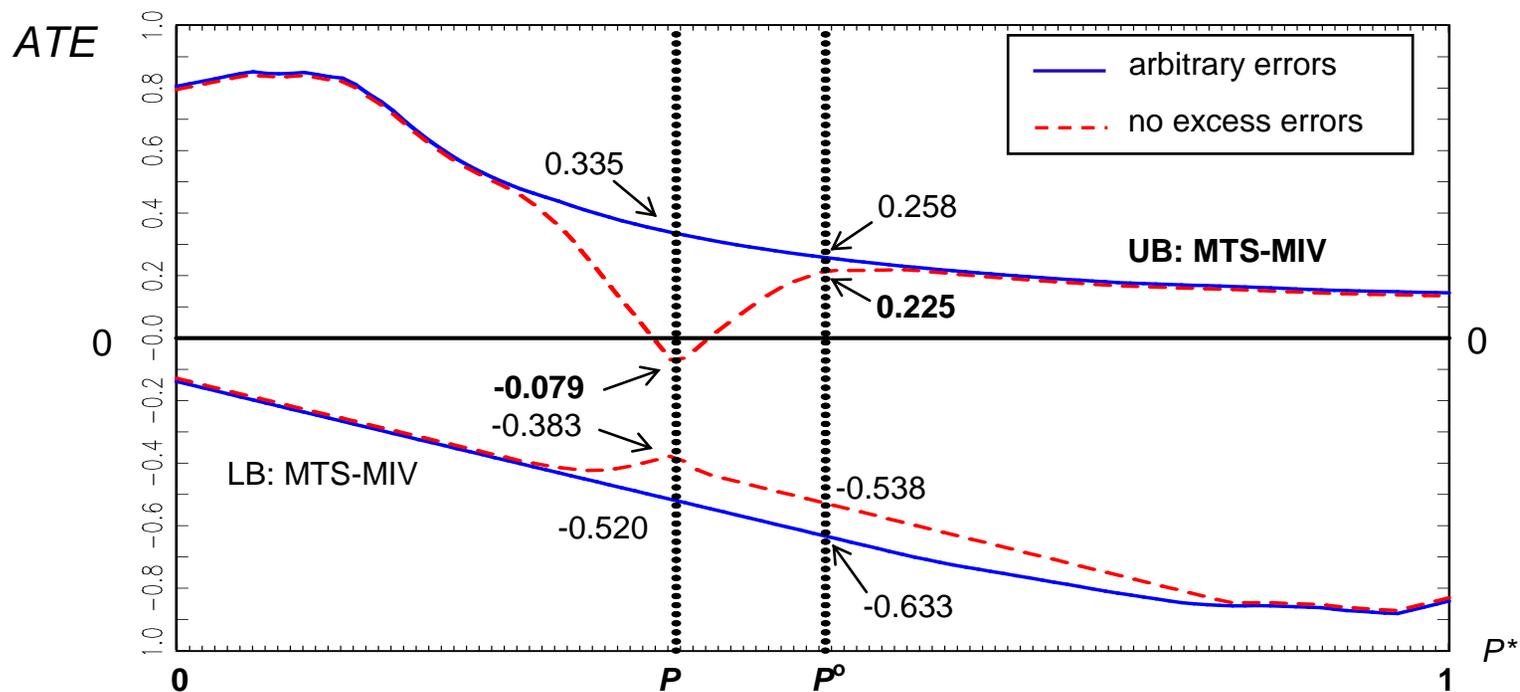|		$P^* = P = 0.39$	width	$P^* = P^o = 0.51$	width
Worst-Case:	——	[-0.551, 0.766]	1.32	[-0.669, 0.649]	1.32
	- - -	[-0.421, 0.579]	1.00 (Manski)	[-0.538, 0.649]	1.09
MTS:	——	[-0.551, 0.404]	0.96	[-0.669, 0.311]	0.98
	- - -	[-0.421, 0.012]	0.43	[-0.538, 0.311]	0.85

Note: These bounds apply to the full population of income-eligible infants and children.

Figure 1B. Sharp Bounds on the ATE for **Child Very Low Food Security** as a Function of P^*
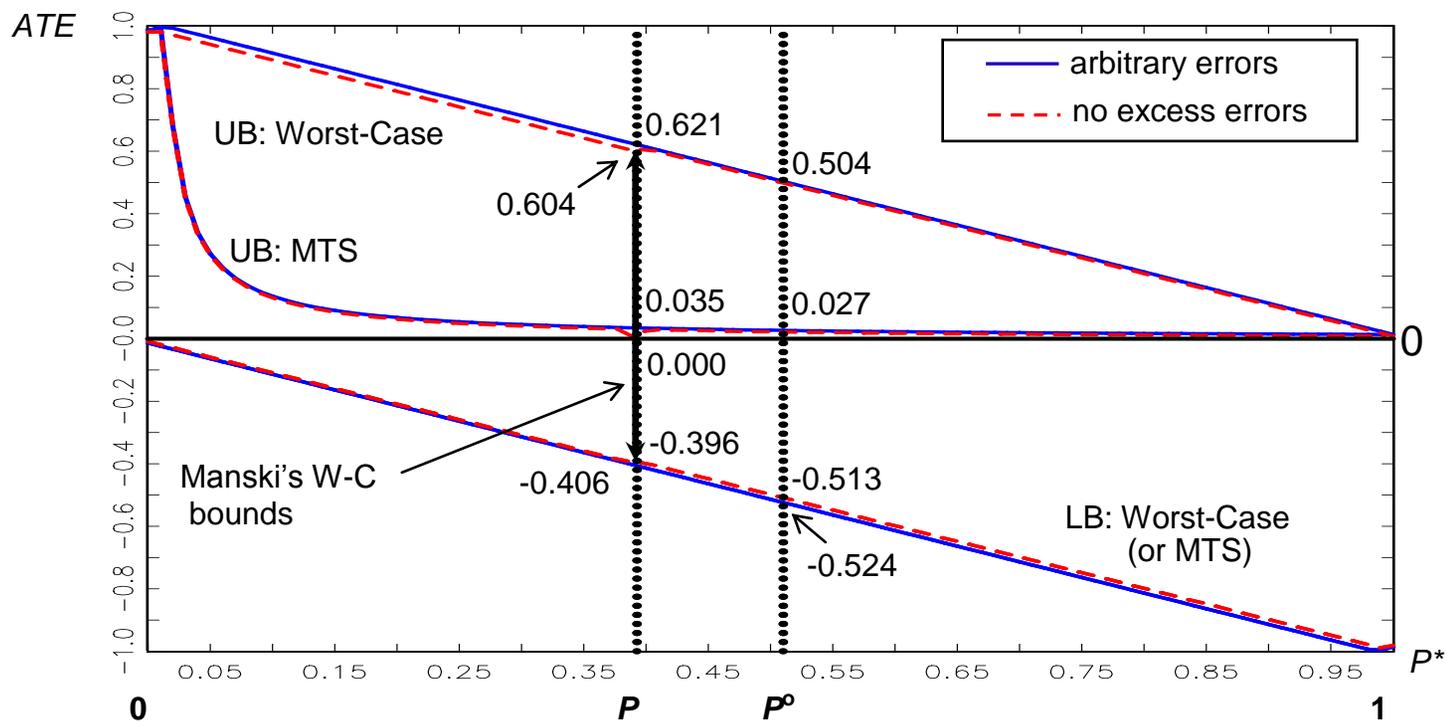
			$P^* = P = 0.39$	width	$P^* = P^o = 0.51$	width
Worst-Case:	———		[-0.406, 0.621]	1.03	[-0.524, 0.504]	1.03
	- - -		[-0.396, 0.604]	1.00 (Manski)	[-0.513, 0.504]	1.02
MTS:	———		[-0.406, 0.035]	0.44	[-0.524, 0.027]	0.55
	- - -		[-0.396, 0.000]	0.40	[-0.513, 0.027]	0.54

Figure 2A. Sharp Bounds on the ATE for **Child Food Insecurity: MTS-MIV**

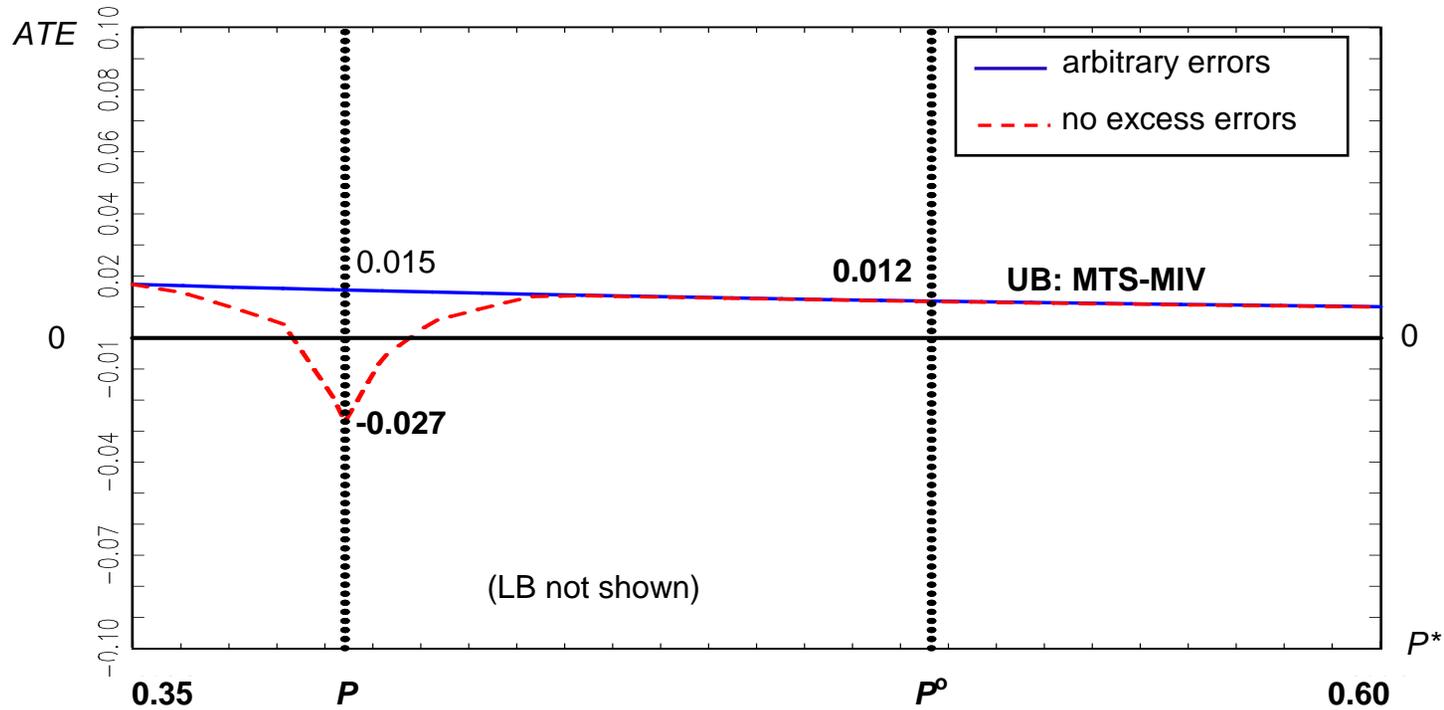|			$P^* = P = 0.39$	width	$P^* = P^o = 0.51$	width
MTS-MIV:	——	p.e.	[-0.520, 0.335]	0.86	[-0.633, 0.258]	0.89
		CI	[-0.545 0.369]		[-0.662 0.284]	
		bias	+0.012 -0.032		+0.016 -0.026	
	- - - -	p.e.	[-0.383, **-0.079**]	0.31	[-0.538, **0.225**]	0.76
		CI	[-0.407 -0.047]		[-0.550 0.264]	
		bias	+0.023 -0.036		+0.020 -0.050	

Notes: Bias-corrected point estimates (p.e.) and 90% Imbens-Manski confidence intervals (CI) using 1,000 pseudosamples. Bias measures the bootstrap bias correction described in Kreider and Pepper (2007).

Figure 2B. Sharp Bounds on the ATE for **Child Food Insecurity: MTS-MIV-MTR**
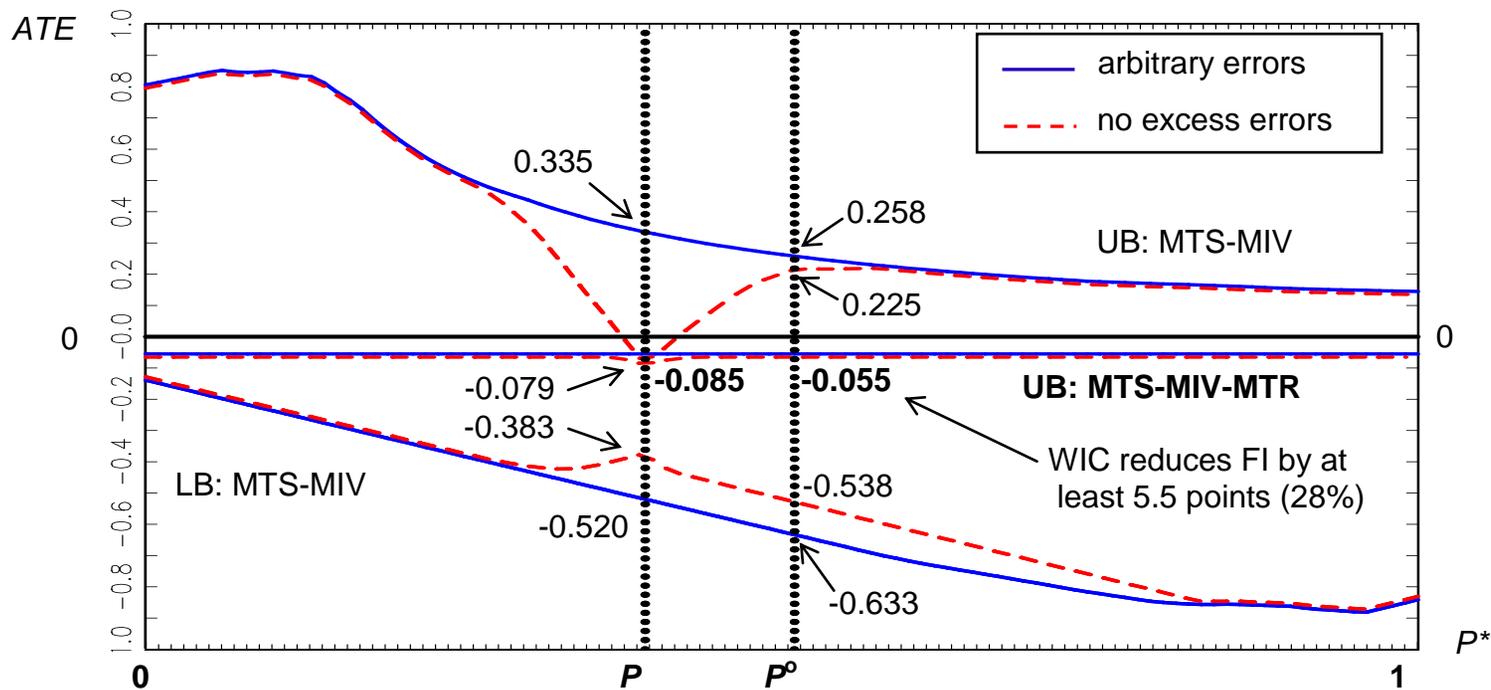
			$P* = P = 0.39$	width	$P* = P^o = 0.51$	width
MTS-MIV-MTR:	———	p.e.	[-0.520, -0.055]	0.47	[-0.633, **-0.055**]	0.58
		CI	[-0.545 -0.034]		[-0.662 -0.035]	
		bias	+0.012 -0.020		+0.016 -0.019	
	– – –	p.e.	[-0.383, **-0.085**]	0.30	[-0.538, **-0.055**]	0.48
		CI	[-0.407 -0.055]		[-0.550 -0.035]	
		bias	+0.023 -0.038		+0.020 -0.016	

Figure 3A. Sharp Bounds on the ATE for **Child Very Low Food Security: MTS-MIV**

			$P* = P = 0.39$	width	$P* = P^o = 0.51$	width
MTS-MIV:	_____	p.e.	[-0.391, 0.015]	0.38	[-0.503, **0.012**]	0.52
		CI	[-0.410 0.020]		[-0.527 0.016]	
		bias	+0.016 -0.004		+0.019 -0.003	
	- - - - -	p.e.	[-0.372, **-0.027**]	0.35	[-0.503, **0.012**]	0.76
		CI	[-0.394 -0.019]		[-0.526 0.016]	
		bias	+0.020 -0.003		+0.020 -0.003	

Figure 3B. Sharp Bounds on the ATE for **Child Very Low Food Security: MTS-MIV-MTR**
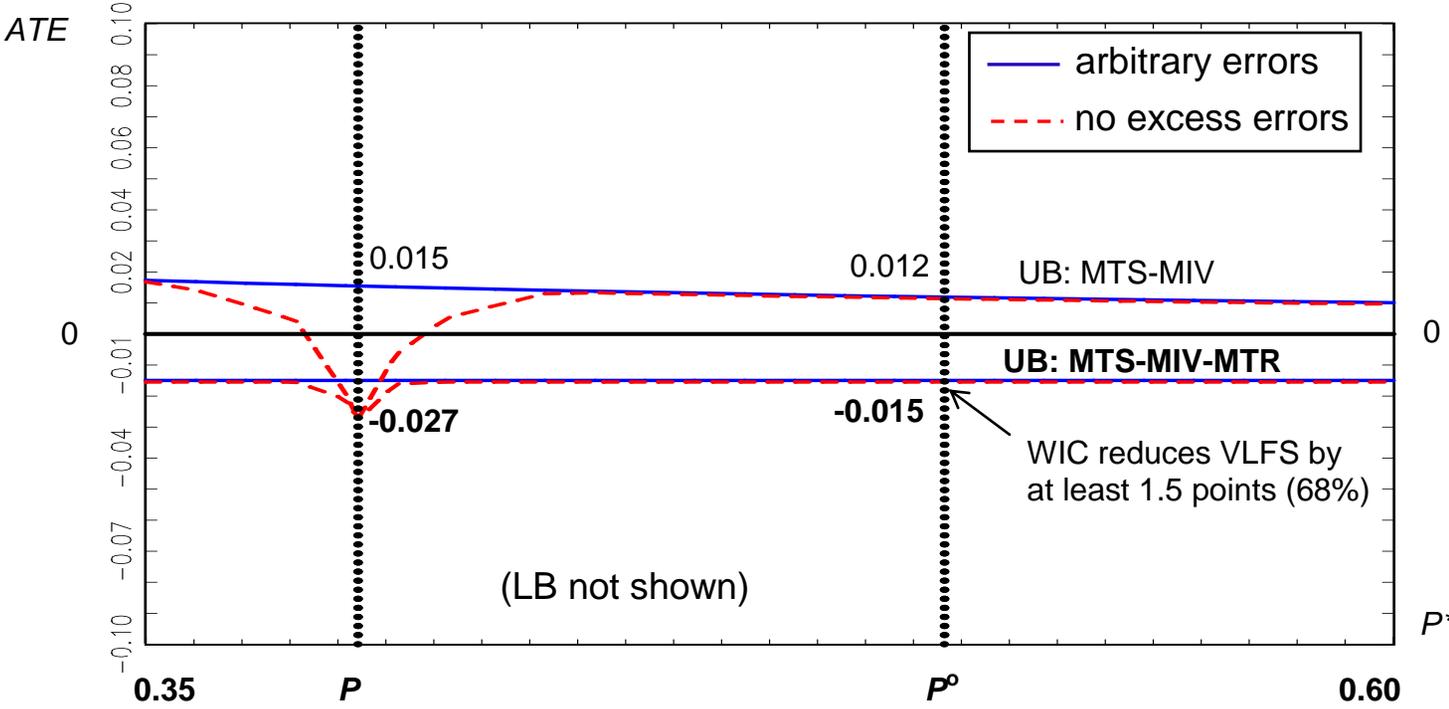
			$P^* = P = 0.39$	width	$P^* = P^o = 0.51$	width
MTS-MIV-MTR:	——	p.e.	[-0.391, -0.015]	0.38	[-0.503, **-0.015**]	0.49
		CI	[-0.410 -0.008]		[-0.527 -0.008]	
		bias	+0.016 -0.006		+0.019 -0.007	
	- - - -	p.e.	[-0.372, **-0.027**]	0.35	[-0.503, **-0.015**]	0.49
		CI	[-0.394 -0.019]		[-0.526 -0.007]	
		bias	+0.020 -0.006		+0.020 -0.007	