Estimating Treatment Effects in the Presence of Overlapping Programs and Misreporting*

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November, 2024

Abstract

Non-mutually exclusive interventions are common features of programs implemented in developing and developed countries. Evaluating the impact of individual and joint participation in these programs may be of policy interest in guiding the cost-effective allocation of resources. However, program participation is substantially misreported in survey data, which may result in biased treatment effect estimates. While the literature has focused on misreporting in one program, our study proposes a method to consistently estimate individual and joint treatment effects of potentially misreported overlapping (and exogenous) programs. We focus on false negative cases which are more prevalent in observational studies. We derive the bias in the traditional ordinary least squares (OLS) estimator and show that it is not possible to determine the direction of the bias a priori. The joint treatment effect may also have an opposite sign to the true effect, which may have dramatic consequences if used to inform policy on whether the programs are complements or substitutes. As in the previous literature, we argue that any instrumental variable (IV) that meets relevant criteria fails to adhere to the exclusion restrictions, resulting in biased IV estimates. We then develop a consistent estimator of treatment effects using misclassification probabilities, available through validation studies and other external sources. When misclassification probabilities are unknown, we provide an approach to estimate and apply them in the proposed method. Monte Carlo simulations show that the estimator performs well in finite samples. Finally, we provide an empirical example, estimating the effect of two correlated and documented to be substantially misreported programs, the Supplemental Nutrition Assistance Program (SNAP) and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), on food security and healthy eating.

JEL Classification: C13, C31, H53, I38

Keywords: Misclassification, Least Square, Treatment Effects, Welfare programs

*We thank seminar participants at Georgia State University and conference participants of the Georgia Econometrics Workshop, the African Meeting of the Econometrics Society, and the Canadian Economics Association Meeting for their helpful comments.

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1 Introduction

This paper focuses on estimating individual and joint treatment effects in parametric regressions in the presence of non-mutually exclusive interventions and misreporting. Non-mutually exclusive interventions are a common feature of programs implemented in most countries. For instance, the U.S. Department of Agriculture (USDA) administers 15 domestic food assistance programs to eligible low-income households (Oliveira, 2018). These programs vary in terms of their sizes, benefits, and population targets, and low-income households may be eligible to participate in multiple programs. We can find similar overlapping programs in developing countries, such as Conditional Cash Transfers (CCTs) and Insecticide Treated Nets (ITNs) distribution programs. The CCTs require recipients to meet established criteria, including children's school attendance, up-to-date vaccinations, and regular health care visits, which may influence household's chances of receiving free or subsidized ITNs channeled through schools and antenatal Clinics (Fiszbein, 2009; Scates et al., 2020).

Estimating individual and joint treatment effects of overlapping programs may interest researchers and policymakers. First, participating in one program may increase individuals' awareness of other available programs, affecting their probability of participating in these programs. The transactional cost of participating in a program may also change conditional on participation in other programs, which may encourage uptake. For example, automatic or categorical eligibility allows households to be automatically eligible for a program without going through eligibility determination conditional on receiving other programs. Second, understanding whether parallel treatments are complements or substitutes is critical for the optimal combination of interventions and cost-effective allocation of resources. If two programs are complements, there are additional gains from implementing both programs relative to each, incentivizing the parallel implementation of the programs. In contrast, if the two programs are substitutes, the gains from implementing both are less than the sum of benefits from implementing either. The optimal policy, in this case, might be to implement one of the programs but not both.

However, participation in social programs is substantially misreported in survey data. False negatives occur when program participants report not receiving treatment when they did, and false positives when program nonparticipants report receiving treatment when they didn't. In this paper, we focus on false negatives, which are more prevalent in social programs in observational studies. For instance, Meyer et al. (2022) report up to 48.98%, 33.08%, and 22.82% rates of false negatives in Supplemental Nutrition Assistance Program (SNAP) participation in the Current Population Survey (CPS), the American Community Survey (ACS), and the Survey of Income and Program Participation (SIPP), while false positive is typically low, 0.73%, 0.84%, and 1.64%. While other error sources, such as recall, salience, and design of survey instruments, may result in either false negatives or positives, stigma or social image concern is likely to drive the high prevalence of underreporting in social programs (Celhay et al., 2022). Note that underreporting in surveys is not restricted to social program participation. Stigma-related misclassifications extend to other behaviors perceived as socially undesirable. For example, in health literature, social stigma may reduce the probability that a mother reports prenatal smoking in surveys (Brachet, 2008; Fertig, 2010).

Identification and estimation with single misclassified binary regressors have been extensively studied in different settings such as exogenous misreporting and treatment (e.g., Aigner 1973, Bollinger 1996, Black et al. 2000, Lewbel 2007, Chen et al. 2008b, Chen et al. 2008a, van Hasselt & Bollinger 2012, Nguimkeu et al. 2021), exogenous misreporting and endogenous treatment selection (e.g., Kane et al. 1999, Frazis & Loewenstein 2003, Brachet 2008, DiTraglia & García-Jimeno 2017, Bollinger & van Hasselt 2017, Ura 2018) and endogenous misreporting and treatment selection (e.g., Kreider et al. 2012, Hu et al. 2015, Hu et al. 2016, Nguimkeu et al. 2019). These studies have offered various approaches to recovering consistent treatment effect point estimates and, in some cases, parameter bounds. Some solutions involve adjusting OLS using knowledge of misclassification probabilities or estimating them in the data given the distribution assumptions (e.g., Aigner 1973, Nguimkeu et al. 2021). Other studies use instrumental variables (e.g., Black et al. 2000, Frazis & Loewenstein 2003, Mahajan 2006, Ura 2018, DiTraglia & García-Jimeno 2017, Ura 2018, Nguimkeu et al. 2019), and repeated measurements (e.g., Kane et al. 1999, Black et al. 2000, Chen et al. 2008b, van Hasselt & Bollinger 2012) , while others estimate parameter bounds (e.g., Bollinger 1996, Black et al. 2000).

Much less is known about estimation with overlapping and potentially misclassified binary regressors. Our study is related to Jensen et al. (2019), examining whether joint participation in Supplemental Nutrition Assistance Program (SNAP) and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) increases food security compared to participating in only SNAP. However, the study assumes household true participation status in one program, SNAP, is observed using auxiliary administrative data to validate the self-reported status. In contrast, the second program, WIC, is potentially misreported, whereas we allow both programs to be underreported. In addition, the study identifies ATE bounds under the assumption that the better food security outcome of the household is weakly increasing with household expenditure on food at home compared to total spending. Though this assumption is plausible, it seems relatively strong. The share of food expenditure to total spending may increase as households become poorer or as household size increase for a given income which may not necessarily translate to favorable food security outcome. Moreover, this study does not produce point estimates which could be of interest in policy. Garber & Klepper (1980) attempts to show bias in multiple mismeasured continuous regressors and show that at least one coefficient estimate will be attenuated. Little is known about our setting when measurement errors, non-classical in nature, occur in multiple binary regressors.

This paper proposes a solution to consistently estimate individual and joint treatment effects in a multivariate linear regression when binary regressors representing correlated and non-mutually exclusive interventions are plausibly mismeasured. We first derive the asymptotic bias in the naive OLS estimator and show that it is not possible to determine the direction of the bias a priori. We further show that OLS estimates of the joint treatment effect of the two mismeasured binary regressors can have an opposite sign to the true effect. In addition, any valid instrument will also be correlated with the measurement errors, failing to meet relevance criteria, resulting in biased treatment effects estimates. Our approach to correct for OLS bias uses known misreporting probabilities to express the correlation between true (unobserved) binary regressors and observed covariates as a function of observed covariates and misreporting probabilities. When the misclassification probabilities are unknown, we extend the maximum likelihood estimator of Hausman et al. (1998) to bivariate models and estimate them given the data. Estimated misclassification probabilities and treatment effects are consistent, given the correct distribution assumption of the true binary regressors. We then assess the finite sample performance of the proposed estimators in Monte Carlo simulations and show that the proposed estimator is superior to naive OLS. Finally, we provide an empirical example. We examine the impact of SNAP and WIC on food security and Healthy Eating Index (HEI) using the National Household Food Acquisition and Purchase Survey (FoodAPS) data.

The rest of the paper proceeds as follows. Section 2 reviews the literature on measurement errors in binary regressors. Section 3 describes our framework, a multivariate linear regression model with two correlated misclassified binary regressors and their interaction, and assesses the bias of the OLS estimator. Section 4 develops our proposed estimators when misclassification probabilities are available to researchers and when they are unknown. Section 5 provides Monte Carlo simulations. We present an empirical example in Section 6 and summarize our findings in Section 7. Mathematical proofs are in the appendix.

2 Literature Review

Literature on measurement errors in binary regressors mainly focuses on identification and estimation with single misclassified binary regressors under different assumptions regarding misreporting and treatment selection. A group of studies examine when misreporting and participation happen exogenously (Aigner, 1973; Bollinger, 1996; Black et al., 2000; Lewbel, 2007; Chen et al., 2008b,a; van Hasselt & Bollinger, 2012; Nguimkeu et al., 2021). Aigner (1973) is the first study to examine misclassification in binary regressors. The study demonstrates the attenuation bias in the OLS estimator and proposes an estimator to compute treatment effects using misclassification probabilities. Black et al. (2000) provide partial identification bounds in linear regressions such that the true value of the parameter is bounded between the OLS estimator, which can be improved further if two noisy measures exist, and the instrumental variable (IV) estimator. Nguimkeu et al. (2021), estimate linear regression models with misclassified binary regressor, potentially correlated with other regressors resulting in hidden bias. The study provides a way to consistently obtain treatment effects using misclassification probabilities in a bias-adjusted least-squares estimator (BALS). Lewbel (2007) uses an instrument for participation in nonparametric and semi-parametric regressions. Other studies take a partial identification approach and provide parameter bounds under a weak set of assumptions in linear regression models (Bollinger, 1996; van Hasselt & Bollinger, 2012), and others examine identification in nonparametric regression models (Chen et al., 2008b,a).

A related group of studies examine the case with exogenous misreporting but participation or treatment selection is endogenous (Kane et al., 1999; Frazis & Loewenstein, 2003; Brachet, 2008; DiTraglia & García-Jimeno, 2017; Bollinger & van Hasselt, 2017; Ura, 2018). Kane et al. (1999) propose a generalized method of moment (GMM) to simultaneously identify misreporting errors and parameters of interest when repeated measures are available. Brachet (2008) uses a two-step GMM procedure, estimating the probability of true status in the first stage by maximum likelihood and using the predicted probabilities to recover consistent estimates in the second stage. Mahajan (2006) provides nonparametric point estimates of homogenous average treatment effects using additional information or "instrument-like variable". Frazis & Loewenstein (2003) provide homogenous average treatment effects bounds using IV and GMM. Ura (2018) provides finite bounds of local heterogenous treatment effects in a nonparametric setting using IV. DiTraglia & García-Jimeno (2017) considers identification when a discrete-valued instrument is available. Bollinger & van Hasselt (2017) propose a Bayesian approach to identify parameter bounds.

Other studies provide solutions when a single misclassified and endogenous binary regressor is endogenously misreported. Kreider et al. (2012) shows the effect of SNAP on health outcomes by estimating average treatment effects bounds under increasingly stronger but plausible nonparametric assumptions. Hu et al. (2015) uses a local polynomial regression estimator to identify parameters in single-index models. Hu et al. (2016) examines a class of nonseparable index models with measurement errors and endogeneity. Nguimkeu et al. (2019) considers underreporting cases and shows that regardless of whether participation is endogenous, endogenous misreporting results in inconsistent OLS (may lead to sign switching) and IV estimators. The study proposes a two-step estimator that estimates the probability of true participation status in the first stage using information regarding participation and misreporting and identifies treatment effects in the second stage using the predicted participation status from the first stage.

Literature on regression frameworks with multiple misclassified binary regressors is scanty. Jensen et al. (2019) identify ATE bounds on whether joint SNAP and WIC participation reduces food insecurity compared with participating in only SNAP using a nonparametric approach. One limitation of this study is that it does not yield point estimates that could be relevant to policymakers. Additionally, it considers only one program, WIC, to be misreported, and true SNAP participation status is observed in the data. Moreover, the study imposes behavioral assumptions on the relationship between outcome, program participation, and other covariates. Other studies consider the case when continuous multiple variables are measured with errors (e.g., Garber & Klepper 1980). However, these theoretical conclusions may not necessarily extend to measurement errors in multiple binary regressors, considering the non-classical nature of misclassifications in binary regressors. Unlike Savoca (2000), we consider joint treatment effects and exploit the case when misclassification probabilities are unknown.

Our paper has two salient contributions. First, we propose a consistent estimator of treatment effects of overlapping and plausibly misreported programs when information about misclassification probabilities is available, for example, through validation studies or other relevant sources. Second, when information about misclassification probabilities is unavailable, we propose a framework to estimate them from the data. We also show the finite sample performance of our proposed solutions and provide an empirical example.

3 Framework

This section describes our regression framework and discusses the bias of the OLS estimator due to misreporting.

3.1 Model with overlapping treatments and misreporting

Our regression framework considers a multiple linear regression model with a scalar outcome, y_i, correlated and exogenous (true) participation indicators, t^*_{1i} and t^*_{2i} such that $Pr[t^*_{1i} =$

$1] = P_1^*$ and $Pr[t_{2i}^* = 1] = P_2^*$ with $P_1^* \in (0, 1)$ and $P_2^* \in (0, 1)$, and a $k \times 1$ vector of exogenous error-free covariates, x_i for each observation i in random sample size, n. The relationship between these entities is given by

$$y_i = \alpha_1 t_{1i}^* + \alpha_2 t_{2i}^* + \alpha_3 t_{1i}^* \times t_{2i}^* + x_i' \beta + \epsilon_i \tag{1}$$

Here, α_1 is the conditional average treatment effect of t_{1i}^* for subjects that are not participating in t_{2i}^*, and α_2 is the conditional average treatment effects of t_{2i}^* for subjects that are not participating in t_{1i}^*. The parameter α_3 captures the additional effect for participating in both t_{1i}^* and t_{1i}^* compared to just participating in one of them.

We assume the two true binary regressors of interest, t_{1i}^* and t_{2i}^* are correlated with $\text{Cor}(t_{1i}^*, t_{2i}^*) = \rho$. When $\rho > 0$, individuals who participate in one program are likely to also participate in the other program, for instance, as they become categorically eligible or more aware of other programs. However, if there are additional (explicit or implicit) costs or disincentives associated with participating in the other programs given participation in the first one, we expect $\rho < 0$. When $\rho = 0$, participation in these programs are independent to one another.

In the treatment effect literature, the interaction term captures the effect of one program that is influenced by participation status in the other program. In other words, it reflects cases when participation in two programs has a larger effect than the sum of effects of individual programs alone. When the interaction term is omitted or when $\alpha_3 = 0$, the specification imposes an additive assumption indicating that the effect of t_{1i}^* on y_i is independent of the effect of t_{2i}^*, and vice versa. Note that t_{1i}^* and t_{2i}^* could be independent to each other but still have dependent effects on the outcome y_i.

We aim to consistently estimate the model parameters, $\theta = [\alpha_1,\ \alpha_2,\ \alpha_3,\ \beta]'$. We are particularly interested in α_3, the key parameter capturing the additional effect of participating in both programs relative to either, which we term the 'joint effect'.

Assumption 1. $\mathbb{E}\left[\epsilon_i | t_{1i}^*, t_{2i}^*, x_i\right] = 0$

We assume that (true) program participation status, t_{1i}^* and t_{2i}^*, and other covariates, x_i, in the treatment effect model are orthogonal to the error term ϵ_i. Thus, the error term does not have any predictive power on our outcome of interest, y_i, conditional on covariates and correct model specification. This assumption is standard in linear regression models.

Assumption 2. $\text{Var}(x_i)$ *exists and is nonsingular (and finite).*

This assumption is the typical identification condition in treatment effect models. It requires a matrix $X = [x_1', x_2', \ldots, x_n']'$, to have a full rank, k, ruling out perfect multicollinearity among the covariates in X. Additionally, we assume that covariates follow a well-behaved distribution with finite moments, thereby eliminating extreme cases. Under Assumption (1) and (2), OLS estimator of the model parameters, (α_1, α_2, α_3, β), is unbiased and consistent. In particular, the probability limit of our coefficient of interest is equal to the true value, plim $\hat{\alpha}_{3LS} = \alpha_3$ and we can make correct policy prescriptions based on whether the two programs are complements or substitutes.

However, the econometrician does not observe the true participation status (t_{1i}^*, t_{2i}^*), but plausibly error-driven proxies, t_{1i} and t_{2i}, such that $Pr[t_{ji} = 1] = P_j$ with $P_j \in (0, 1)$. We model misreporting in the observed participation status using two unobserved binary indicators, δ_{1i} and δ_{2i}. Each of these misreporting (or reporting) indicators is such that a respondent correctly reports their treatment status if the indicator takes the value 1, and reports not receiving treatment otherwise.

Assumption 3. *The observed (error-ridden) binary regressors, (t_{1i}, t_{2i}), are functions of the true (unobserved) binary regressors and the misreporting indicators, (δ_{1i}, δ_{2i}), such that*

$$t_{1i} = t_{1i}^* \delta_{1i} \quad \textit{and} \quad t_{2i} = t_{2i}^* \delta_{2i}$$

This specification reflects a one-sided misreporting case commonly encountered in social programs especially those associated with stigma (such as SNAP and WIC), in risky behavior (such as smoking and drunk driving), and other types of responses that are prone to social desirability bias. In this case, the observed program participants truly received the treatment, but some of the observed nonparticipants also received treatment but reported no receipt, representing the false negatives. For the SNAP and WIC programs that we use in our applications, validation studies show that the prevalence of false negatives are substantial in survey data while false positives are negligible, consistent with Assumption 3 (e.g., Meyer et al. 2022). This measurement error framework resembles the partial observability model in Poirier (1980) where each of the observed binary regressors is determined jointly by the true underlying binary regressors and measurement error indicator, and it has been previously used in the measurement error literature (e.g., see Nguimkeu et al. 2019). Note that measurement errors in binary regressors are necessarily negatively correlated with true participation status (Aigner, 1973).

We allow misreporting to depend on covariates only through the true participation status. This implies that conditional on true participation status, the probability of misreporting in both programs is exogenous, hence constant and uncorrelated with ϵ_i. Specifically, we define misreporting probabilities as

Assumption 4.

$$\Pr(t_{1i} = 0 | t^*_{1i} = 1, t_{2i}, x_i) = a_1 \quad \textit{and} \quad \Pr(t_{2i} = 0 | t^*_{2i} = 1, t_{1i}, x_i) = a_2, \quad a_1, a_2 \in [0, 1). \quad (2)$$

This assumption introduces misclassification probabilities, a_1 and a_2, the probability of false negatives in t_1 and t_2, respectively. The assumption of constant misclassification probabilities is common in the literature of mismeasured binary regressors (e.g., see Bollinger 1996, Kane et al. 1999, Hausman et al. 1998, Black et al. 2000, Frazis & Loewenstein 2003, Brachet 2008, van Hasselt & Bollinger 2012, Bollinger & van Hasselt 2017, DiTraglia & García-Jimeno 2017, Nguimkeu et al. 2021). We further assume that $a_j \in [0, 1)$, ruling out severe cases of misreporting where the observed error-driven proxies are no longer informative of the true treatment status. In other words, the misclassification errors do not overwhelm their ability to signal true status, equivalent to saying that $\text{Cov}(t_{ji}, t^*_{ji}) \geq 0$, and t_{ji} is better proxy of t^*_{ji} compare to $1 - t_{1i}$ or a random guess. Hausman et al. (1998) termed this assumption the "Monotonicity condition."

The econometrician does not observe the data-generating process and estimates a model equivalent to that specified in Equation (1), referred to as the operation model, using observed treatment status contaminated by misreporting errors and other error-free covariates, given by:

$$y_i = \alpha_1 t_{1i} + \alpha_2 t_{2i} + \alpha_3 t_{1i} \times t_{2i} + x_i'\beta + \epsilon_i \tag{3}$$

Naive OLS estimator of the parameters in Equation 3 are asymptotically biased given the measurement errors in the regressors. Moreover, the misreporting errors in the two binary regressors contaminate the interaction term, which imposes additional complications in estimating the model parameters.

3.2 Bias of treatment effects due to misreporting

We first derive the bias in a naive OLS estimator of the treatment effects. We next show the joint treatment effects estimates may attain a sign opposite of the true treatment effect. To do this, we express our covariates in vector and matrix forms and define $Z^* = [t_1^*,\, t_2^*,\, t_3^*,\, x]$ and $Z = [t_1,\, t_2,\, t_3,\, x]$ where $t_{3i}^* = t_{1i}^* \times t_{2i}^*$ and $t_{3i} = t_{3i}^* \times \delta_{3i}$, with $\delta_{3i} = \delta_{1i} \times \delta_{2i}$. The following result follows.

Lemma 1. *Under Assumptions 1-2, the probability limit of the OLS estimator of the model parameters, $\theta = [\alpha_1,\, \alpha_2,\, \alpha_3,\, \beta]'$, is given by*

$$plim\, \hat{\theta}_{LS} = [\text{Var}(Z)]^{-1}\text{Cov}(Z, Z^*)\theta$$

Proof. See Appendix A.1.

A naive OLS estimator is biased and inconsistent because $\text{Var}(Z)] \neq \text{Cov}(Z, Z^*)$, unless there is no misreporting. To see this, consider the components of $\text{Var}(Z)$ and $\text{Cov}(Z, Z^*)$ featured in Lemma 1. First, $\text{Cov}(t_{ji}, t_{ji}^*) \neq \text{Var}(t_{ji})$, for $j \in \{1, 2, 3\}$ since the misreporting error in binary regressors is necessarily negatively correlated with true participation status, $\text{Cov}(t_{j1}, t_{j1}^* - t_{j1}) \neq 0$. Second, misreporting errors are necessarily correlated with other regressors in the model if the true (unobserved) binary regressor is correlated with those regressors (Nguimkeu et al., 2021). We also derive this result in our framework and show that $\text{Cov}(x_i, t_{ji}^*) \neq \text{Cov}(x_i, t_{ji})$ as long as $\text{Cov}(x_i, t_{ji}^*) \neq 0$. Other terms in Lemma 1 are driven by the correlation between the two binary regressors t_{1i} and t_{2i}, and their correlation with the interaction term, t_{3i}. Since $\rho^* \neq 0$, it follows that $\text{Cov}(t_{ki}, t_{ji}^*) \neq \text{Cov}(t_{ki}, t_{ji})$, for

$k \in \{1,2,3\}$ and $k \neq j$. In contrast, when there is no misreporting, $t^*_{ji} = t_{ji}$ and $Z^* = Z$, and the OLS estimator is unbiased and consistent.

In particular, under Assumptions 1-2, the asymptotic bias in the OLS estimator of the joint effect, α_{3LS}, is

$$\text{plim} \quad \hat{\alpha}_{3LS} - \alpha_3 = \frac{A - B\alpha_3}{Q}$$

Where $A = \mathbb{E}\left[t_{3i}\mathbf{z}'^{*}_{i,-t_{3i}}\theta_{-\alpha_3}\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}^{*}_{i,-t_{3i}}\theta_{-\alpha_3}\right]$,

$B = \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\left(t^*_{3i} - t_{3i}\right)\right]$,

$Q = \mathbb{E}\left[t_{3i}\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{-3i}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}t_{3i}\right]$,

$\theta_{-\alpha_3} = [\alpha_1,\, \alpha_2,\, \beta]'$, and $\mathbf{z}_{i,-t_{3i}} = [t_{1i}; t_{2i}; x_i]$

Proof. See Appendix A.1.

$A \neq 0$ and $B \neq 0$, if there is misreporting in at least one binary regressor, and Q, is always positive by Cauchy-Schwarz inequality. Therefore, the direction of the bias is driven by A and B, and we would overestimate the joint treatment effect, an expansion bias when $B > 0$ and $\alpha_3 < A/B$ or $B < 0$ and $\alpha_3 > A/B$. On the other hand, we will underestimate the joint treatment effect when $B > 0$ and $\alpha_3 > A/B$ or $B < 0$ and $\alpha_3 < A/B$. Thus, the direction of the bias cannot be determined a priori. Garber & Klepper (1980) reached a similar theoretical conclusion with multiple mismeasured continuous covariates where the study shows that their coefficients are not necessarily attenuated, but at least one of them is.

A naive OLS estimator of the joint treatment effect may also result in sign reversal. For example, with complement treatments, $\alpha_3 > 0$. However, when $A > 0$ and $B - Q > 0$, plim α_3 will always be negative. In a different setup, when $A < 0$ and $B - Q < 0$, plim α_3 will be negative when $0 < \alpha_3 < A/(B - Q)$. Without losing generality, noting that sign switching regions when $\alpha_3 < 0$ simply mirror those when $\alpha_3 > 0$, we only present the sign-switching regions when $\alpha_3 > 0$ in Figure 1. When there is no misreporting, $A = 0$ and $B = 0$, naive OLS estimator is unbiased, and we have plim $\hat{\alpha}_3 = \alpha_3$.

4 The proposed estimator

We propose an estimator that uses misclassification probabilities to correct for the OLS bias and attain consistent treatment effects. We first assume that econometricians or researchers have access to information regarding the misclassification probabilities. We also develop a procedure to estimate misclassification probabilities given the data and distributional assumptions about the true binary regressors.

4.1 Known misclassification probabilities

By inverting the relationship in Lemma 1, a consistent estimator of the model parameters can be obtained by defining an adjusted least squares estimator given by

$$\widehat{\theta}_{Adj} = \widehat{\mathrm{Cov}}(Z, Z^*)^{-1}\widehat{\mathrm{Var}}(Z)\widehat{\theta}_{LS}. \tag{4}$$

While $\widehat{\mathrm{Var}}(Z)$ can be obtained from the sample, $\widehat{\mathrm{Cov}}(Z, Z^*)$ on the other hand, the covariance between the observed regressors and their true (unobserved) counterparts, contains components that are not directly observed in the data. Our proposed estimator uses the assumptions made above for this framework to estimate $\widehat{\mathrm{Cov}}(Z, Z^*)$ from the data by expressing this quantity as a function of misclassification probabilities and sample statistics that can be computed from the data. This is provided by the following lemma.

Lemma 2. *Under Assumptions 1-2,*

$$\begin{aligned}
&\mathrm{Cov}(t_{2i}, t_{1i}^*) = \zeta_1 \mathrm{Cov}(t_{2i}, t_{1i}), && \mathrm{Cov}(t_{3i}, t_{1i}^*) = \zeta_1 \mathrm{Cov}(t_{1i}, t_{2i}), && \mathrm{Cov}(x_i, t_{1i}^*) = \zeta_1 \mathrm{Cov}(x_i, t_{1i}),\\
&\mathrm{Cov}(t_{3i}, t_{2i}^*) = \zeta_2 \mathrm{Cov}(t_{2i}, t_{1i}), && \mathrm{Cov}(t_{1i}, t_{2i}^*) = \zeta_2 \mathrm{Cov}(t_{1i}, t_{2i}), && \mathrm{Cov}(x_i, t_{2i}^*) = \zeta_2 \mathrm{Cov}(x_i, t_{2i})\\
&\mathrm{Cov}(t_{1i}, t_{1i}^*) = \eta_1 \mathrm{Var}(t_{1i}) && \mathrm{Cov}(t_{2i}, t_{2i}^*) = \eta_2 \mathrm{Var}(t_{2i}) &&\\
&\mathrm{Cov}(t_{1i}, t_{3i}^*) = \zeta_2 \eta_{1i} \mathrm{Cov}(t_{1i}, t_{3i}), && \mathrm{Cov}(t_{2i}, t_{3i}^*) = \zeta_1 \eta_2 \mathrm{Cov}(t_{2i}, t_{3i}), &&\\
&\mathrm{Cov}(x_i, t_{3i}^*) = \zeta_3 \mathrm{Cov}(x_i, t_{3i}), && \mathrm{Cov}(t_{3i}, t_{3i}^*) = \eta_3 \mathrm{Var}(t_{3i}) &&
\end{aligned}$$

Where

$$\begin{aligned}
&\zeta_1 = \frac{1}{1-a_1}, && \zeta_2 = \frac{1}{1-a_2}, && \zeta_3 = \frac{1}{1-a_3},\\
&\eta_1 = \frac{1-a_1-P_1}{(1-a_1)(1-P_1)} && \eta_2 = \frac{1-a_2-P_2}{(1-a_2)(1-P_2)} && \eta_3 = \frac{1-a_3-P_3}{(1-a_3)(1-P_3)},\\
&a_3 = \Pr(t_{3i} = 0 | x_i, t_{3i}^* = 1) && P_3 = \Pr(t_{3i} = 1) &&
\end{aligned}$$

Using Lemma 2, we now have an estimator of $\widehat{\text{Cov}}(Z, Z^*)$ from the data, which we denote $\widehat{W}(a_1, a_2)$ to express its dependency to misclassification probabilities only, and we can formally present our proposed estimator of the parameters in the model given by Equation 3. The following result follows if we denote the sample covariance between r_i and s_i by σ_{rs} and variance of r_i by σ_r^2. Specifically, $\sigma_{rs} = \dfrac{1}{n}\sum_{i=1}^n (r_i - \bar{r})(s_i - \bar{s})'$ and $\sigma_r^2 = \dfrac{1}{n}\sum_{i=1}^n (r_i - \bar{r})(r_i - \bar{r})'$where $\bar{r}$ and $\bar{s}$ are the sample mean of r_i and s_i.

Theorem 1. *Under Assumptions 1-2, for given misclassification probabilities, a_1 and a_2, the adjusted least squares estimator is given by*

$$\begin{bmatrix} \hat{\alpha}_{1Adj} \\ \hat{\alpha}_{2Adj} \\ \hat{\alpha}_{3Adj} \\ \hat{\beta}_{Adj} \end{bmatrix} = \begin{bmatrix} \eta_1\sigma_{t_1}^2 & \zeta_2\sigma_{t_1t_2} & \eta_1\zeta_2\sigma_{t_1t_3} & \sigma_{t_1x} \\ \zeta_1\sigma_{t_2t_1} & \eta_2\sigma_{t_2}^2 & \eta_2\zeta_1\sigma_{t_2t_3} & \sigma_{t_2x} \\ \eta_1\sigma_{t_3t_1} & \eta_2\sigma_{t_3t_2} & \eta_3\sigma_{t_3}^2 & \sigma_{t_3x} \\ \zeta_1\sigma_{xt_1} & \zeta_2\sigma_{xt_2} & \zeta_3\sigma_{xt_3} & \sigma_x^2 \end{bmatrix}^{-1} \begin{bmatrix} \sigma_{yt_1} \\ \sigma_{yt_2} \\ \sigma_{yt_3} \\ \sigma_{yx} \end{bmatrix}$$

Where ζ_j and η_j for $j \in \{1, 2, 3\}$ are as defined in Lemma 2. It follows that:

(i) These estimators are consistent, that is, $\hat{\theta}_{Adj} \xrightarrow{p} \theta$, with $\theta = [\alpha_1,\, \alpha_2,\, \alpha_3,\, \beta]'$

When there is no misreporting, $\eta_j = 1$ and $\zeta_j = 1$, our estimator in Theorem 1 is quantitively similar to the OLS, and both are consistent. We can also see this by expressing the adjusted least squares estimator as $\hat{\theta}_{Adj} = \widehat{W}(a_1, a_2)^{-1}\widehat{\text{Var}}(Z)\hat{\theta}_{LS}$. When there is no misreporting, i.e. $a_1 = a_2 = 0$, then $\widehat{W}(0, 0) = \widehat{\text{Var}}(Z)$ and $\hat{\theta}_{Adj} = \hat{\theta}_{LS}$. When there is misclassification in at least one treatment status, solutions proposed for single misclassified regressors (e.g., modified least square estimator in Aigner (1973) and bias-adjusted least square in Nguimkeu et al. (2021)) do not account for the bias driven by misreporting in the second binary regressor. In contrast, the proposed estimator, the adjusted least squares, is consistent when misreporting exists in any or all binary regressors.

The variance of the proposed estimator can be estimated through bootstrapping techniques. Unless the researcher knows or makes assumptions on the distribution of ϵ_i, we suggest to use a non-parametric bootstrap procedure that jointly samples the outcome, y_i, program participation status $(t_{1i},\, t_{2i})$, and a vector of other covariates, x_i.

In our setting, the bootstrap procedure to estimate the covariance of our consistent estimator would involve M replications, samples n_m observations with replacement in each replication, m, and compute coefficient estimates using the adjusted least squares, $\hat{\theta}_{m,Adj}$ with each sample. Let $\bar{\hat{\theta}}_{M,Adj} = \frac{1}{M}\sum_{m=1}^{M}\left[\hat{\theta}_{m,Adj}\right]$, the average of the bootstrapped estimates of θ, the estimated asymptotic covariance matrix of the adjusted least squares estimator, $\hat{\theta}_{Adj}$, follows as

$$\hat{\text{Var}}\left[\hat{\theta}_{Adj}\right] = \frac{1}{M-1}\sum_{m=1}^{M}\left[\hat{\theta}_{m,Adj} - \bar{\hat{\theta}}_{M,Adj}\right]\left[\hat{\theta}_{m,Adj} - \bar{\hat{\theta}}_{M,Adj}\right]'$$

4.2 Estimation of misclassification probabilities

Our estimator so far uses known misreporting probabilities, a_1 and a_2, to estimate the treatment effects of interest. Hence, applying the proposed estimator in correcting the bias due to measurement error may be limited in the context where misclassification probabilities are unknown. One way of addressing this limitation is by estimating the misclassification probabilities in the data in the first step, $\hat{a}_1$ and $\hat{a}_2$, and using them in the proposed adjusted least squares estimator in the second step, $\hat{\theta}_{Adj} = \hat{W}(\hat{a}_1, \hat{a}_2)^{-1}\widehat{\text{Var}}(Z)\hat{\theta}_{LS}$.

The existing literature has taken this approach mainly in the context of single misreported binary regressors (e.g., Brachet 2008; Nguimkeu et al. 2021) and use the framework of estimating misclassification probabilities proposed by Hausman et al. (1998).

We can also extend the approach in Hausman et al. (1998) to a bivariate or multivariate framework to fit the context of multiple misclassified binary regressors. We model (true) binary regressors as

$$\begin{aligned} t_{1i}^* &= \mathbf{1}(x_i'\gamma_1 + u_{1i} > 0) \\ t_{2i}^* &= \mathbf{1}(x_i'\gamma_2 + u_{2i} > 0) \end{aligned} \tag{5}$$

and their interaction as

$$t_{3i}^* = \mathbf{1}(x_i'\gamma_1 + u_{1i} > 0, x_i'\gamma_2 + u_{2i} > 0) \tag{6}$$

We assume that the joint Cumulative Distribution Function (CDF) of $(-u_{1i}, -u_{2i})$ is known and defined by $F(x_i'\gamma_1, x_i'\gamma_2, \rho^*) = \Pr[-u_{1i} \leq x_i'\gamma_1, -u_{2i} \leq x_i'\gamma_2]$. In particular, if we assume that conditional on x_i, the disturbance terms (u_1, u_2) are drawn from bivariate normal distribution given by

$$\begin{pmatrix} u_{1i} \\ u_{2i} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho^* \\ \rho^* & 1 \end{pmatrix}\right), \tag{7}$$

their joint CDF would then be

$$\Pr[-u_{1i} \leq x_i'\gamma_1, -u_{2i} \leq x_i'\gamma_2] = \Phi_2(x_i'\gamma_1, x_i'\gamma_2; \rho^*),$$

where $\Phi_2(\cdot, \cdot, \rho^*)$ is the bivariate standard normal CDF associated with correlation coefficient ρ^*.

For given misclassification probabilities (a_1, a_2), the marginal likelihood of observed binary regressors, t_{1i} and t_{2i}, are given by

$$\begin{aligned} \Pr[t_{1i} = 1|x_i] &= (1 - a_1)\Phi(x_i'\gamma_1) \\ \Pr[t_{2i} = 1|x_i] &= (1 - a_2)\Phi(x_i'\gamma_2) \end{aligned} \tag{8}$$

and their joint probability can be obtained by

$$\Pr[t_{1i} = 1, t_{2i} = 1|x_i] = (1 - a_1 - a_2 + a_1 \times a_2)\Phi_2(x_i'\gamma_1, x_i'\gamma_2, \rho^*), \tag{9}$$

where $\Phi(\cdot)$ is the univariate standard normal CDF and $\Theta = (a_1, a_2, \gamma_1, \gamma_2, \rho)$ are vectors of unknown parameters. These unknown parameters can be estimated jointly through maximum likelihood method. The maximum likelihood estimators of Θ, denoted by $\hat{\Theta}$, can be obtained by maximizing the log-likelihood function given by

$$\begin{aligned} \mathcal{L}(\Theta) = \frac{1}{n}\sum_{i=1}^{n}\{ & t_{1i}t_{2i}\ln\omega_{i11}(\Theta) + t_{1i}(1 - t_{2i})\ln\omega_{i10}(\Theta) \\ & + (1 - t_{1i})t_{2i}\ln\omega_{i01}(\Theta) + (1 - t_{1i})(1 - t_{2i})\ln\omega_{i00}(\Theta)\} \end{aligned} \tag{10}$$

Where $\omega_{i11} = \Pr(t_{1i} = 1, t_{2i} = 1)$, $\omega_{i10} = \Pr(t_{1i} = 1, t_{2i} = 0)$, $\omega_{i01} = \Pr(t_{1i} = 0, t_{2i} = 1)$, and $\omega_{i00} = \Pr(t_{1i} = 0, t_{2i} = 0)$ are probabilities responding to the four possible realizations of t_1 and t_2, defined as

$$\omega_{i11}(\Theta) = (1 - a_1 - a_2 + a_1 \times a_2)\,\Phi_2(x_i'\gamma_1, x_i'\gamma_2; \rho^*)$$

$$\omega_{i10}(\Theta) = (1 - a_1)\,\Phi(x_i'\gamma_1) - (1 - a_1 - a_2 + a_1 \times a_2)\,\Phi_2(x_i'\gamma_1, x_i'\gamma_2; \rho^*)$$

$$\omega_{i01}(\Theta) = (1 - a_2)\,\Phi(x_i'\gamma_2) - (1 - a_1 - a_2 + a_1 \times a_2)\,\Phi_2(x_i'\gamma_1, x_i'\gamma_2; \rho^*)$$

$$\omega_{i00}(\Theta) = 1 - (1 - a_1)\,\Phi(x_i'\gamma_1) - (1 - a_2)\,\Phi(x_i'\gamma_2) + (1 - a_1 - a_2 + a_1 \times a_2)\,\Phi_2(x_i'\gamma_1, x_i'\gamma_2; \rho^*)$$

The maximum likelihood estimators, $\hat{\Theta}$, which includes estimators of misclassification probabilities, $\hat{a}_1$ and $\hat{a}_2$ can be obtained by maximizing likelihood function given by Equation 10 with respect Θ. Standard errors can be obtained, as usual, by computing the inverse of the observed information matrix. Given the model assumptions and correct specification of the cumulative distribution function, these maximum likelihood estimators are consistent.

4.3 Average marginal effects

Researchers and policymakers are often interested in the effect of participating in one program relative to not participating, that is, the average marginal effect of treatment 1 or 2. In our setting, as specified in Equation 1, the average marginal effects are given by

$$AME_j = \mathbb{E}_{t^*_{3-j}}\left[\frac{\partial \mathbb{E}[y_i|t^*_{1i}, t^*_{2i}, x_i]}{\partial t^*_j}\right] = \mathbb{E}_{t^*_{3-j}}\left[\alpha_j + \alpha_3 t^*_{(3-j)i}\right] = \alpha_j + \alpha_3 \frac{\mathbb{E}\left[t_{(3-j)i}\right]}{1 - a_{3-j}} \qquad for \quad j \in \{1, 2\} \tag{11}$$

We can recover consistent estimates of the average marginal effects by using the adjusted least squares estimator to obtain coefficient estimates in the treatment effect model, $(\hat{\alpha}_{1Adj}, \hat{\alpha}_{2Adj}, \hat{\alpha}_{3Adj})$ and the misclassification probabilities which may be available through validation datasets or estimated using the framework described in Section 4.2. Specifically, the estimation of average marginal effects, $\widehat{AME}$, follows as

$$\widehat{AME}_j = \mathbb{E}_{\hat{t}^*_{3-j}}\left[\frac{\partial \mathbb{E}[y_i|\hat{t}^*_{1i}, \hat{t}^*_{2i}, x_i]}{\partial \hat{t}^*_j}\right] = \hat{\alpha}_{j,Adj} + \frac{\hat{\alpha}_{3,Adj}}{1 - \tilde{a}_{3-j}}\bar{t}_{3-j} \qquad for \quad j \in \{1, 2\}$$

where $\bar{t}_{3-j} = \frac{1}{n}\sum_{i=1}^{n} \hat{t}_{(3-j)i,Adj}$ and $\tilde{a}_{3-j} = a_{3-j}$ or $\tilde{a}_{3-j} = \hat{a}_{3-j}$ depending on whether the misreporting rates are known or not. By applying the delta method, we can in turn estimate the asymptotic variance of the average marginal effects as

$$\widehat{\text{Var}}(\widehat{AME}_j) = \Pi_j \widehat{\text{Var}}[\hat{\theta}_{Adj}]\Pi_j' \qquad for \quad j \in \{1, 2\}$$

where $\Pi_j = [1 \;\; 0 \;\; \frac{\bar{t}_{3-j}}{1-a_{3-j}} \;\; 0_{k\times 1}]$, a $(k+3)\times 1$ vector obtained from $\frac{\partial \widehat{AME}_j}{\partial \hat{\theta}_{Adj}}$. This approach provides us with standard errors and confidence interval required in making inference on the average marginal effects.

4.4 Generalization to multivariate binary variables

Our proposed estimator can be extended to estimate individual and joint treatment effects of multiple and plausibly misreported binary variables. This framework is useful in the context of multiple programs where policymakers may be interested in understanding the joint effect of a pair of programs on outcomes of interest. Suppose the researcher is interested in evaluating the impact of p programs and considers potential pairwise interactions. There would be $\frac{p(p-1)}{2}$ such interactions terms which can be denoted $t_{I1}, \dots, t_{I\frac{p(p-1)}{2}}$. The adjusted least square estimator of the $\frac{p(p+1)}{2} + k$ model parameters in this case, $\hat{\theta}_{Adj} = (\hat{\alpha}_{1Adj}, \dots, \hat{\alpha}_{pAdj}, \hat{\alpha}_{I1Adj}, \dots, \hat{\alpha}_{I\frac{p(p-1)}{2}Adj}, \hat{\beta}_{Adj})$ is given by $\hat{\hat{\theta}}_{Adj} = \widehat{W}(a_1, \dots, a_p)^{-1}\widehat{\text{Var}}(Z)\hat{\theta}_{LS}$, such that $\widehat{W}(a_1, \dots, a_p) = \widehat{\text{Cov}}(Z, Z^*)$, and $Z = (t_1, \dots, t_p, t_{I1}, \dots, t_{I\frac{p(p-1)}{2}}, x)$. Here, the terms in $\widehat{W}(a_1, \dots, a_p)$ are obtained as for the binary case by applying the relationships of the types given in Lemma 2.

5 Monte Carlo simulations

In this section, we examine the finite sample performance of the proposed estimator and compare the proposed methods with OLS through Monte Carlo simulations. Our goal is to consistently estimate the parameters (α_1, α_2, α_3, β) of the model presented in Equation 1. Specifically, we aim to consistently estimate the individual treatment effect and the joint effect of participating in both programs relative to participating in only one program, under the assumption that the true participation status of both programs, t_1^* and t_2^*, are unobserved, but only, t_1 and t_2, their error-ridden surrogates driven, are observed. In addition, we assume that other observed covariates, x_i, are correctly measured.

5.1 Simulation setup

The data-generating process is simulated as follows. The true treatment indicators, t_1^* and t_2^*, are given by

$$t_{1i}^* = \mathbf{1}(\gamma_1 x_i + u_{1i} > 0) \text{ and } t_{2i}^* = \mathbf{1}(\gamma_1 x_i + u_{1i} > 0),$$

where $\gamma_1 = \gamma_2 = 1$ and $\begin{pmatrix} u_{1i} \\ u_{2i} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho^* \\ \rho^* & 1 \end{pmatrix}\right)$, $\rho^* = 0.2$. This setting can be extended to explore different degrees of correlation between the two treatment regressors by varying the values of ρ^*. The outcome equation y_i is given by

$$y_i = c + \alpha_1 t_{1i}^* + \alpha_2 t_{2i}^* + \alpha_3 t_{1i}^* \times t_{2i}^* + x_i\beta + \epsilon_i, \quad \text{where } \epsilon_i \sim N(0,1)$$

and $x_i \sim N(0,1)$. The true population regression parameters are $c = 1$, $\alpha_1 = 5.0$, $\alpha_2 = 2.8$, $\alpha_3 = 0.3$, and $\beta = 0.5$. We aim to estimate the model parameters consistently and of most interest, the individual (conditional) average treatment effects, α_1 and α_2, and the joint average treatment effect, α_3.

The econometrician does not observe the data-generating process and (true) treatment regressors defined above, but only their erroneous surrogates. The econometrician then estimates the following operational model:

$$y_i = c + \alpha_1 t_{1i} + \alpha_2 t_{2i} + \alpha_3 t_{1i} \times t_{2i} + x_i\beta + \varepsilon_i$$

Where t_{1i} and t_2 are error-driven proxies of true treatment status defined by:

$$t_{1i} = t_{1i}^* \mathbf{1}(\nu_{1i} > a_1)$$
$$t_{2i} = t_{2i}^* \mathbf{1}(\nu_{2i} > a_2)$$

The disturbance terms, $\nu_{1i} \sim \mathcal{U}(0,1)$ and $\nu_{2i} \sim \mathcal{U}(0,1)$, are drawn from uniform distribution and the parameters, a_1, and a_2, are the misreporting probabilities in t_{1i} and t_{2i}, such that $a_j \in [0, 1)$ for $j \in \{1, 2\}$. The misreporting probabilities determine the proportion of false negatives in observed binary regressors. For example, if there are no false negatives, $a_j = 0$, then $\mathbf{1}(\nu_{ji} > 0) = 1$ and $t_{ji} = t^*_{ji}$. When $a_j \neq 0$, we allow for $a_j \times 100\%$, for $j \in \{1, 2\}$ rate of false negatives. Specifically, we consider the following set of misclassification probabilities, $(a_1, a_2) \in \{(0, 0.1); (0, 0.2); (0, 0.4); (0.1, 0.1); (0.1, 0.2); (0.1, 0.4); (0.2, 0.1); (0.2, 0.2); (0.2, 0.4); (0.4, 0.1); (0.4, 0.2); (0.4, 0.4); \})$. In this way, we can examine OLS bias and the performance of the proposed estimator at different degrees of misreporting within and across programs.

We first estimate the model parameters, c, α_1, α_2, α_3, and β, using the OLS estimator and true treatment status, unobserved to the econometrician. We next report naive OLS estimates based on the observed misclassified binary regressors. We extend our OLS analysis to the problem of misspecification in the operational model, where we subsequently omit the interaction term, $t_{1i} \times t_{2i}$, and then the second treatment, t_{2i}.

We then use our proposed estimator to estimate the model parameters. Note that our estimator uses known misclassification probabilities, a_1 and a_2. Finally, we present estimates when misclassification probabilities are unknown. We estimate them from the simulated data using the framework proposed in Section 4.2.

5.2 Simulation results

In our Monte Carlo simulations, we execute 1000 replications using the sample size of $n = 5000$ observations. We report the averaged simulation results in Table 1 for the OLS and adjusted least squares estimators. "OLS True" columns show OLS estimates using true (unobserved) binary regressors. Our benchmark is the first column of "OLS True", which uses true binary regressors and has the correct model specification. Columns with "OLS Observed" present naive OLS estimates obtained using the observed data. Our simulation results are consistent with the theoretical discussion that misreporting errors may reverse the sign of the joint treatment effect. This sign switching can occur even at low misreporting rates, such as in cases where there is no misreporting in the first treatment and only a 10% false negative rate in the second treatment. The bias in the joint treatment effect worsens with increasing misreporting rates within and across programs. Misspecification introduces omitted variable bias, exacerbating the OLS bias in individual treatment effect estimates in some settings. Empirical studies may be affected by these inconsistencies, leading to policy prescriptions that are far from optimal.

We present the results of our proposed estimator, adjusted least squares, in the last three columns of Table 1. We first show the results when the interaction term is excluded from the model. As expected, these estimates are inconsistent due to omitted variable bias. However, these estimates are less biased than those from naive OLS, especially when misreporting in one or both programs is high. In contrast, when the model is correctly specified, the adjusted least squares estimator consistently estimates the treatment effects and other model parameters using known misreporting rates and when misreporting rates are unknown (and estimated from the data). Our proposed estimator performs similarly to our benchmark, the OLS estimator, when true (unobserved and error-free) binary regressors are used, and it is superior to naive OLS.

We next assess the sensitivity of our proposed solution to various forms of misspecification. First, we include false positives in our data-generating process by redefining the observed treatment status as $t_{ji} = t^*_{ji}\mathbf{1}(\nu_{ji} > a_j) + (1 - t^*_{ji})\mathbf{1}(\nu_{ji} < b_j)$ for $j \in (1, 2)$. Thus, the observed treatment indicator, t_j, has ($a_j \times 100$) rate of false negatives and ($b_j \times 100$) rate of false positive when $a_j > 0$ and $b_j > 0$. We consider 1%, 5%, and 10% rates of false positives in treatment one indicator and 1% and 5% in treatment one and two indicators. The results show that our proposed estimator performs relatively well when the false positive rate is low. The estimation bias worsens with higher rates of false positives and an increase in false negatives for a given rate of false positives. Further, the bias of the interaction term is relatively lower when misclassification probabilities are estimated rather than taken as given. Second, we examine the robustness of the proposed solution to endogeneity in treatment selection or program participation by allowing correlation between u_{ji} and ϵ_i for $j \in (1, 2)$. Specifically, we consider (0.2, 0.2), (-0.2,-0.2) and (0.2,-0.2) degrees of correlation, ($\text{Cor}(u_{1i}, \epsilon_i)$, $\text{Cor}(u_{2i}, \epsilon_i)$). Our estimator performs quite similarly to the benchmark OLS estimates that use true underlying treatment status, and both are slightly biased due to endogeneity. This finding is not surprising, considering that our proposed method does not correct for endogeneity.

Third, we allow for non-normal error terms, u_{1i}, u_{2i}, in our bivariate treatment selection model. We consider a number of distributions, including Gamma distribution, bivariate chi-squared distribution, Logistic distribution, Laplace distribution, and exponentially modified Gaussian distribution. The results suggest that the adjusted least squares estimator performs well when the true error terms distribution is symmetrical and poorly otherwise.

Fourth, we also consider the case where the treatment selection equation is misspecified by including w from a standard normal distribution, x^2, or x^3, in data generating process of t_1 and t_2 but omitting them in the estimation. The proposed estimator is robust to omitting w and x^3 but becomes inconsistent when x^2 is excluded. To minimize bias, we recommend researchers consider whether plausible distribution functions and non-linear model misspecification affect the symmetry of the bivariate error terms distribution in their settings. Lastly, the proposed estimator performs well and remains consistent when we allow the false negative rate in treatment one to be known, while in the second treatment, unknown and estimated using the proposed framework.

6 Empirical example

This section presents our empirical example. Our objective is to compute the adjusted least squares estimates of the effect of SNAP and WIC on food security and HEI. We use the publicly available version of the National Household Food Acquisition and Purchase Survey (FoodAPS). To illustrate the applicability of our methods, we assume other covariates apart from SNAP and WIC are error-free. We also do not account for potential endogeneity in SNAP and WIC participation. We first present naive OLS estimates of the effect of SNAP and WIC on food security. We then provide adjusted least squares estimates using misclassification probabilities of SNAP and WIC available in the literature. Finally, we also ignore the misclassification probabilities available in the literature and we estimate them from the data using our proposed method as a first step and we use them to estimate the treatment effects in the second step.

6.1 SNAP and WIC programs

Households are food secure if they have access to the kinds and quantities of foods needed for an active and healthy life at all times and for all its members, otherwise are food insecure. Food insecurity remains a public health concern in the United States, given the high prevalence among low-income households. Approximately 30.3% of households below 130% of the federal poverty threshold were food insecure in 2021 (Coleman-Jensen et al., 2022a,b). Food insecurity may also lead to poor diet quality, for example, food insecure households may replace fruits, vegetables, and whole grains with calorie-dense and other highly processed foods. In addition, food insecurity is associated with a broad spectrum of detrimental health outcomes (Gundersen et al., 2011; Gundersen & Ziliak, 2018). Inadequate economic resources for food may hinder households from obtaining adequate food or worsen food hardship for already food-insecure households.

SNAP is the largest and WIC the third largest of the 15 domestic food and nutritional assistance programs administered by USDA to address food insecurity and its consequences (Oliveira, 2018). The SNAP program, mean tested, provides nutrition benefits worth more than 60 billion US dollars a year and supplements the food budget of over 42 million individuals on average per month. WIC is also a means-tested federal program that provides nutritional benefits worth more than 5 billion US dollars annually to over 7 million participants on average per month. While SNAP gives vouchers to low-income households to purchase healthy food, WIC provides them vouchers to buy only a restricted set of foods for the nutrition requirements of pregnant, postpartum, and lactating women, infants, and children under five. Moreover, WIC provides counseling and referrals for health services (Bitler et al., 2003). This way, WIC may increase awareness of healthy food choices, and SNAP widens the food choice set.

Misclassification in self-reported SNAP participation status is substantially documented in the literature (e.g., Meyer et al. 2015, Courtemanche et al. 2019, Meyer et al. 2022). Our empirical example uses the misclassification probabilities from Courtemanche et al. (2019). Using FoodAPS data, Courtemanche et al. (2019) provide estimated participation and misclassification error rates for 12 different classification choices of administrative or "gold standard" measures, ADMIN and ALERT, in the survey. We illustrate our empirical application using ADMIN alternate 1, a gold standard measure with the highest misclassification probability, 32.31% false negatives. For WIC, we use misclassification probabilities in Fox & Hokayem (2022). These authors. link state administrative data on the WIC program to Current Population Survey Annual Social and Economic Supplement and report a false negative rate of 41.5%. Other empirical evidence about misreporting in WIC can be found in Bitler et al. (2003).

We aim to use the adjusted least squares estimator to account for misreporting in estimating the effect of participating in SNAP and WIC on food insecurity and HEI.

6.2 Data

FoodAPS is the first nationally representative survey of US households administered by USDA. The survey is designed to collect comprehensive data about household food purchases, including food obtained through food and nutrition assistance programs. FoodAPS surveys 4,826 households obtained through a multistage sampling design to include low-income households participating in SNAP, low-income households not in SNAP, and higher-income households. In addition to food acquisition, the survey contains self-reported SNAP and WIC participation status, administrative measures of SNAP participation, food security, HEI, income, and demographic characteristics.

We restrict the sample to households simultaneously eligible for SNAP and WIC. These are households with a pregnant woman, a child under five years old, and an income below 130% of the poverty threshold. We use USDA's 30-day food security scores to construct our outcome variable, food insecurity. Our food insecurity score comes from the affirmative responses to the ten items on the household food security questionnaire included in the FoodAPS. The food insecurity scores range from 0 to 10, with 0 representing high food security, and the higher the scores, the greater the degree of food insecurity. We also examine the impact of SNAP and WIC on HEI. Here the HEI index captures the healthfulness or nutritional quality of foods obtained by households by assessing whether they comply with the U.S. Dietary Guidelines for Americans (DGAs). We use the HEI-2010, obtained by summing 12 components and ranges from 0 to 100, where higher scores show greater compliance with the recommended dietary guidelines.

Our empirical example accounts for other control variables. These include dummy variables for gender, education attainment (less than high school, high school or GED, some college, and college degree or higher), ethnicity (Hispanic), race (white, black, or other), employment (employed, searching for a job, or unemployed), and marital status (married, previously married, or never married) of the primary respondent. We also include dummy variables for whether the household lives in the rural census track, whether there are children under five, whether they own any vehicle, and whether the primary store is SNAP-authorized. Our continuous variables include primary respondents' age, household size, household income, number of children, and distance to the primary store in miles. Table 3 present the summary statistics of our outcome variables. The average food insecurity and HEI scores in our sample are 2.7 and 47.5, respectively. In Table 4, we present our summary statistics of other covariates and show the overlap in WIC and SNAP participation. About 42.6% of the sample received both SNAP and WIC, 28.1% received only SNAP, 17.3% WIC only, and 12.1% did not participate in either program.

6.3 Results

We present the regression estimates that use given misclassification probabilities in our empirical example in Table 5. The "OLS" columns present estimates from a naive OLS estimator, and the "Adj. LS" columns present results from the adjusted least squares estimators proposed in our study. We use the SNAP false negative rate from Courtemanche et al. (2019), $a_1 = 32.31\%$, and WIC false negative from Fox & Hokayem (2022), $a_1 = 41.5\%$. We present estimates from food insecurity scores in the first two columns. Comparing the results from naive OLS and the adjusted least squares, we observe differences in the magnitude of coefficients across the two estimators. The coefficient of SNAP is almost 42% larger in naive OLS compared to the adjusted least squares. We also observe a plausible expansion bias in the estimated effect of WIC, where the naive OLS estimate coefficient is almost 62% higher than the proposed method estimate. The coefficient of the interaction term is downward biased in the naive OLS, indicating a stronger degree of complementarity than the proposed method. Note that the interaction term estimates are negative in both estimators but 90%

lower with naive OLS relative to the proposed method. The last two columns show results using HEI-2010. Here, SNAP and WIC naive OLS coefficient estimates are lower, about 89% and 60%, compared to the adjusted least squares estimates, whereas the coefficient for the interaction term is 90% larger relative to the adjusted least squares coefficient estimate. We also provide regression estimates using known misreporting probability and HEI components in Appendix Tables C1 and C2 where for some components, we observe differences in both magnitude and signs in the coefficient estimates of SNAP, WIC and the interaction term across naive OLS and our proposed methods.

We next estimate misreporting probabilities in SNAP and WIC from the data as a first step. Following the method that we proposed in Section 4.2, the estimated probabilities of underreporting is $\hat{a}_1 = 29.7\%$ in SNAP and is $\hat{a}_2 = 40.1\%$ in WIC. These estimates are very close to those obtained above from validation studies in the literature.

In the second step, we apply the estimated probability in the adjusted least square estimator to obtain the effect of SNAP and WIC on food insecurity and healthy eating. We present the results for food insecurity and HEI in Table 6 and for HEI components in Appendix Tables C3 and C4. Overall, our results and inference are qualitatively similar to when misreporting probabilities are known.

7 Conclusion

Literature on measurement errors in binary regressors mainly focuses on single misclassified binary regressors. Our paper exploits the estimation of individual and joint treatment effects in the presence of misclassification errors and other overlapping programs (also plausibly measured with errors). We consider the case of false negatives, which is more common in surveys, and allow the misreporting errors to depend on observable covariates through true binary regressors. We show that the naive OLS estimator is biased, and the joint treatment effect may have a sign opposite to the true effect (sign switching). This bias may have dramatic consequences if used to inform policy, for instance, whether two programs are complements or substitutes.

Our proposed estimator uses misclassification probabilities to estimate treatment effects consistently. When misclassification probabilities are unknown, we propose a method to estimate them from the data and then apply them in the proposed estimator. It is evident in Monte Carlo simulation that the proposed estimator consistently estimates treatment effects in finite samples and outperforms the naive OLS estimator. In our empirical application, the impact of SNAP and WIC on food security, we illustrate the application of our proposed method in empirical settings where we observe substantial differences in the magnitude of coefficient estimates in the naive OLS compared to the proposed method.

Note that our paper does not address the endogeneity in misclassified binary regressors and misreporting. These are potential limitations of our study that are beyond the scope of our current setting, and we leave them to be addressed in future research.

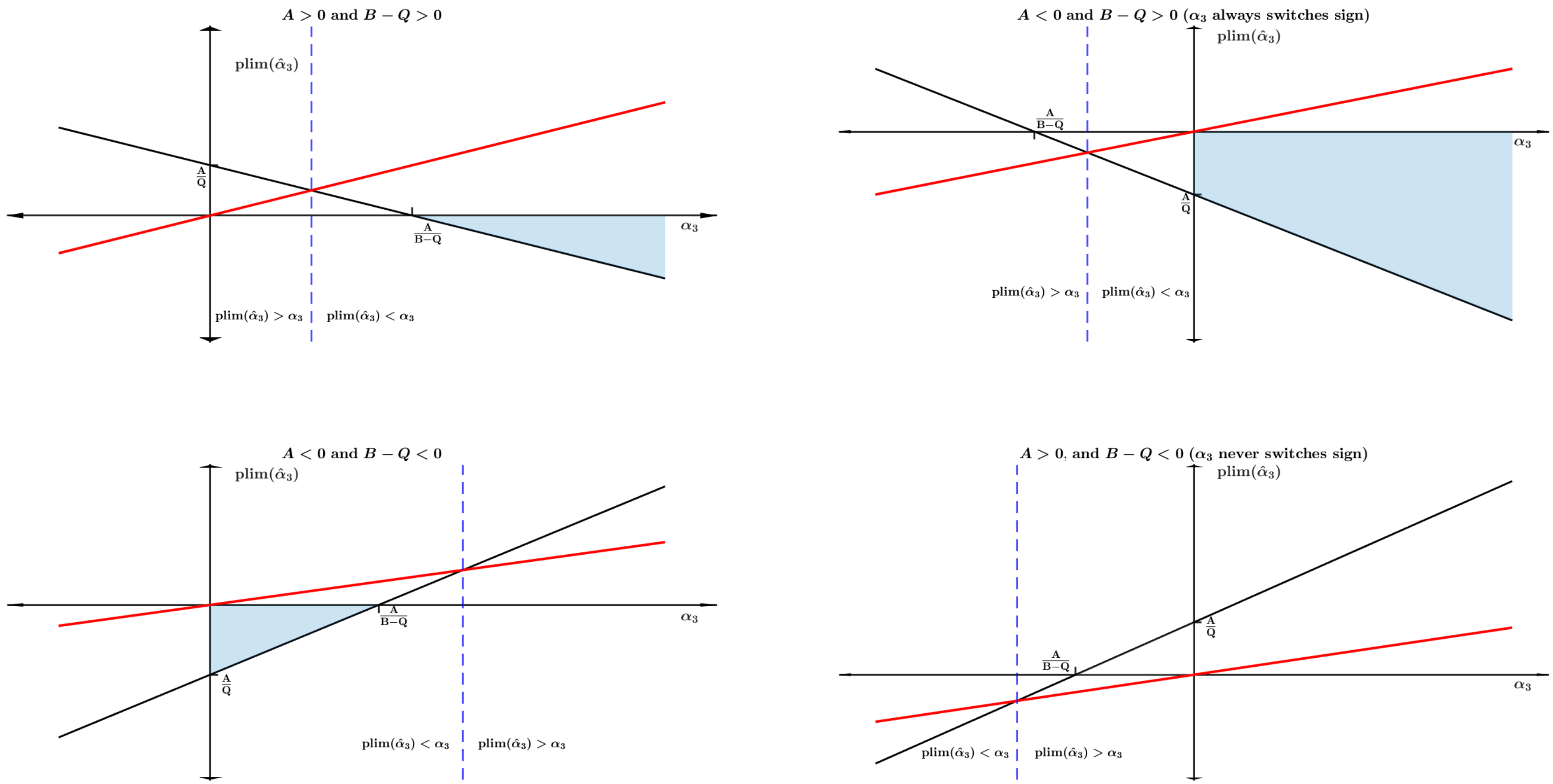

Figure 1: **Illustration of the sign-switching regions in OLS estimation of the joint treatment effect,** $\alpha_3 > 0$

Table 1: **Monte carlo simulation results**

a_1	a_2	Para.	True Values	OLS True	OLS Observed	OLS True	OLS Observed	OLS True	OLS Observed	Adj.LS - Known Misreporting Rates		Adj.LS - Unknown Misreporting Rates
0.0	0.1	α_1	5.0	5.000	5.323	5.151	5.251	5.548	5.548	5.152	5.000	5.013
		α_2	2.8	2.800	2.550	2.951	2.461			2.951	2.800	2.781
		α_3	0.3	0.302	-0.163						0.303	0.330
		β	0.5	0.499	0.677	0.499	0.679	1.220	1.220	0.499	0.499	0.489
		c	1.0	0.998	1.247	0.954	1.272	2.231	2.231	0.953	0.998	0.992
	0.2	α_1	5.0	5.000	5.484	5.150	5.318	5.543	5.543	5.149	4.998	5.012
		α_2	2.8	2.801	2.361	2.950	2.111			2.951	2.801	2.781
		α_3	0.3	0.299	-0.425						0.302	0.331
		β	0.5	0.499	0.800	0.499	0.808	1.220	1.220	0.499	0.499	0.488
		c	1.0	1.000	1.440	0.956	1.503	2.235	2.235	0.956	1.000	0.994
	0.4	α_1	5.0	5.001	5.611	5.151	5.414	5.547	5.547	5.150	4.996	5.008
		α_2	2.8	2.799	2.075	2.949	1.641			2.948	2.796	2.779
		α_3	0.3	0.301	-0.680						0.307	0.337
		β	0.5	0.500	0.967	0.500	0.981	1.221	1.221	0.501	0.501	0.489
		c	1.0	1.000	1.722	0.955	1.806	2.232	2.232	0.957	1.001	0.995
0.1	0.1	α_1	5.0	5.000	4.790	5.149	4.383	5.542	4.644	5.150	4.999	5.000
		α_2	2.8	2.802	3.015	2.951	2.607			2.952	2.801	2.799
		α_3	0.3	0.299	-0.830						0.302	0.308
		β	0.5	0.499	0.992	0.499	1.010	1.221	1.605	0.499	0.498	0.496
		c	1.0	1.000	1.729	0.955	1.859	2.234	2.914	0.954	0.998	0.998
	0.2	α_1	5.0	5.000	4.882	5.150	4.447	5.544	4.648	5.153	4.998	5.000
		α_2	2.8	2.801	2.764	2.951	2.237			2.949	2.795	2.792
		α_3	0.3	0.300	-1.004						0.310	0.314
		β	0.5	0.499	1.126	0.499	1.151	1.221	1.605	0.499	0.498	0.497
		c	1.0	1.000	1.958	0.955	2.110	2.234	2.914	0.954	0.999	1.000
	0.4	α_1	5.0	5.001	4.898	5.151	4.530	5.545	4.647	5.154	5.000	5.007
		α_2	2.8	2.798	2.393	2.948	1.744			2.957	2.804	2.798
		α_3	0.3	0.301	-1.140						0.310	0.304
		β	0.5	0.501	1.310	0.501	1.339	1.220	1.605	0.496	0.495	0.496
		c	1.0	1.000	2.298	0.955	2.442	2.232	2.913	0.947	0.991	0.993
0.2	0.1	α_1	5.0	4.999	4.383	5.149	3.763	5.542	3.997	5.157	5.001	4.996
		α_2	2.8	2.799	3.220	2.950	2.708			2.946	2.789	2.797
		α_3	0.3	0.301	-1.182						0.313	0.307
		β	0.5	0.500	1.218	0.500	1.248	1.221	1.882	0.498	0.498	0.499
		c	1.0	1.000	2.103	0.956	2.282	2.235	3.407	0.954	0.999	1.001
	0.2	α_1	5.0	5.000	4.423	5.150	3.817	5.545	3.997	5.149	4.998	5.003
		α_2	2.8	2.800	2.934	2.949	2.329			2.951	2.801	2.798
		α_3	0.3	0.299	-1.305						0.302	0.304
		β	0.5	0.500	1.361	0.500	1.398	1.220	1.883	0.501	0.500	0.498
		c	1.0	1.001	2.358	0.957	2.549	2.234	3.408	0.957	1.000	1.000
	0.4	α_1	5.0	4.999	4.359	5.151	3.894	5.547	3.999	5.155	5.000	5.001
		α_2	2.8	2.799	2.498	2.950	1.818			2.953	2.799	2.800
		α_3	0.3	0.303	-1.346						0.312	0.316
		β	0.5	0.500	1.561	0.500	1.599	1.220	1.882	0.497	0.495	0.492
		c	1.0	1.000	2.740	0.955	2.903	2.232	3.406	0.952	0.995	0.995
0.4	0.1	α_1	5.0	5.001	3.788	5.151	2.934	5.545	3.128	5.163	5.011	5.006
		α_2	2.8	2.798	3.330	2.948	2.848			2.944	2.788	2.797
		α_3	0.3	0.300	-1.496						0.310	0.299
		β	0.5	0.501	1.529	0.501	1.567	1.222	2.258	0.500	0.498	0.501
		c	1.0	1.000	2.657	0.956	2.845	2.233	4.069	0.951	0.995	0.999
	0.2	α_1	5.0	5.002	3.752	5.151	2.984	5.544	3.132	5.176	5.011	5.005
		α_2	2.8	2.800	2.972	2.950	2.446			2.940	2.772	2.785
		α_3	0.3	0.299	-1.519						0.336	0.315
		β	0.5	0.499	1.684	0.499	1.727	1.220	2.254	0.492	0.490	0.497
		c	1.0	0.999	2.947	0.955	3.131	2.234	4.065	0.943	0.989	0.996
	0.4	α_1	5.0	4.998	3.588	5.148	3.038	5.545	3.126	5.162	4.992	4.998
		α_2	2.8	2.800	2.461	2.950	1.912			2.950	2.780	2.784
		α_3	0.3	0.300	-1.453						0.347	0.327
		β	0.5	0.501	1.910	0.501	1.948	1.221	2.257	0.494	0.488	0.494
		c	1.0	1.001	3.376	0.957	3.521	2.234	4.068	0.947	0.989	0.996

Table 2: **Monte carlo simulation results: Average Marginal Effects**

a_1	a_2	OLS True	OLS Observed	Adj.LS known Misreporting Rates	Adj.LS - Unknown Misreporting Rates
0.0	0.1	5.150	5.383	5.147	5.154
		2.950	2.263	2.953	2.950
	0.2	5.150	5.514	5.146	5.153
		2.950	1.914	2.957	2.952
	0.4	5.150	5.667	5.142	5.151
		2.950	1.543	2.971	2.963
0.1	0.1	5.150	4.195	5.150	5.145
		2.950	2.680	2.952	2.953
	0.2	5.150	4.311	5.159	5.156
		2.950	2.306	2.951	2.950
	0.4	5.150	4.409	5.144	5.151
		2.950	1.874	2.981	2.960
0.2	0.1	5.150	3.565	5.175	5.157
		2.950	2.882	2.950	2.954
	0.2	5.150	3.636	5.160	5.161
		2.950	2.479	2.958	2.960
	0.4	5.150	3.677	5.157	5.166
		2.950	1.982	2.972	2.955
0.4	0.1	5.150	2.842	5.174	5.164
		2.950	3.080	2.952	2.955
	0.2	5.150	2.851	5.196	5.163
		2.950	2.599	2.943	2.953
	0.4	5.150	2.806	5.170	5.174
		2.950	2.014	2.961	2.949

Table 3: **Summary Statistics: Food Insecurity and Healthy Eating**

Statistic	Mean	St. Dev.	Min	Max
Food Insecurity Score	2.679	2.665	0	10
Health Eating Index	47.541	11.972	16.310	84.952
Total vegetables	2.525	1.459	0.000	5.000
Greens and beans	1.246	1.696	0.000	5.000
Total fruit	1.984	1.598	0.000	5.000
Whole fruit	2.146	1.871	0.000	5.000
Whole grains	1.650	2.265	0.000	10.000
Dairy	5.934	3.159	0.000	10.000
Total protein foods	3.890	1.380	0.000	5.000
Seafood and plant proteins	1.590	1.701	0.000	5.000
Fatty acids	4.565	3.341	0.000	10.000
Sodium	5.759	3.547	0.000	10.000
Refined grains	5.611	3.503	0.000	10.000
Empty calories	10.642	5.617	0.000	20.000

Table 4: **Summary Statistics: Supplemental Nutrition Assistance Program (SNAP), Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and Demographic Characteristics**

Statistic	Mean	St. Dev.	Min	Max
SNAP	0.703	0.458	0	1
WIC	0.599	0.491	0	1
SNAP × WIC	0.425	0.495	0	1
Female	0.889	0.315	0	1
Age (years)	35.056	11.110	16.500	85.000
Less than high school	0.321	0.468	0	1
High school or GED	0.321	0.468	0	1
Some college	0.273	0.446	0	1
College or more	0.085	0.279	0	1
Ethnicity - Hispanic	0.348	0.477	0	1
Race - white	0.611	0.488	0	1
Race - black	0.203	0.403	0	1
Race - others	0.186	0.390	0	1
Employed	0.343	0.475	0	1
Searching for job	0.147	0.355	0	1
Unemployed	0.510	0.501	0	1
Married	0.355	0.479	0	1
Previously married	0.263	0.441	0	1
Never married	0.382	0.486	0	1
Rural	0.232	0.423	0	1
Number of children under 5	1.498	0.793	0	6
Household size	4.746	1.878	1	14
Household income (monthly, 1000$)	1.621	0.922	0.000	5.970
Income to poverty ratio	0.735	0.352	0.000	1.504
Any vehicle indicator	0.749	0.434	0	1
House renting	0.766	0.424	0	1
Food pantry use	0.109	0.312	0	1
Primary store distance (miles)	2.821	4.168	0.078	48.967
Primary store SNAP authorized	0.990	0.098	0	1

Table 5: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Food Insecurity and Healthy Eating using Misclassification Probabilities from Validation Data**

Dependent Variables:	**Food Insecurity Score**		**Health Eating Index**	
	OLS	Adj.LS	OLS	Adj.LS
SNAP	0.211	0.122	-3.102	-0.352
	(0.465)	(0.087)	(2.023)	(0.339)
WIC	0.355	0.135	-1.968	-0.793*
	(0.493)	(0.102)	(2.144)	(0.445)
SNAP × WIC	-0.752	-0.072	4.328*	0.428*
	(0.583)	(0.062)	(2.538)	(0.241)
Female	0.625	0.560	1.458	1.463
	(0.434)	(0.387)	(1.887)	(1.781)
Age	-0.002	-0.002	0.118*	0.124**
	(0.014)	(0.013)	(0.061)	(0.062)
Rural	0.127	0.114	-4.911***	-4.837***
	(0.364)	(0.370)	(1.583)	(1.647)
Household size	0.063	0.060	-0.491	-0.523
	(0.164)	(0.147)	(0.714)	(0.748)
Household income	0.413	0.470	-2.649	-2.737
	(0.596)	(0.533)	(2.595)	(2.441)
Food pantry use	1.101**	1.155**	-0.901	-1.221
	(0.441)	(0.500)	(1.920)	(1.702)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP comes from Courtemanche et al. (2019), $a_1 = 32.31\%$, and in WIC comes from Fox & Hokayem (2022), $a_2 = 41.5\%$. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1

Table 6: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Food Insecurity and Healthy Eating using Estimated Misclassification Probabilities**

Dependent Variables:	**Food Insecurity Score**		**Health Eating Index**	
	OLS	Adj.LS	OLS	Adj.LS
SNAP	0.211	0.138	-3.102	-0.392
	(0.465)	(0.099)	(2.023)	(0.387)
WIC	0.355	0.148	-1.968	-0.869*
	(0.493)	(0.111)	(2.144)	(0.485)
SNAP × WIC	-0.752	-0.083	4.328*	0.491*
	(0.583)	(0.071)	(2.538)	(0.278)
Female	0.625	0.557	1.458	1.468
	(0.434)	(0.387)	(1.887)	(1.782)
Age	-0.002	-0.002	0.118*	0.124**
	(0.014)	(0.013)	(0.061)	(0.062)
Rural	0.127	0.115	-4.911***	-4.840***
	(0.364)	(0.370)	(1.583)	(1.648)
Household size	0.063	0.060	-0.491	-0.523
	(0.164)	(0.147)	(0.714)	(0.748)
Household income	0.413	0.472	-2.649	-2.738
	(0.596)	(0.533)	(2.595)	(2.441)
Food pantry use	1.101**	1.154**	-0.901	-1.217
	(0.441)	(0.500)	(1.920)	(1.703)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP is $a_1 = 29.7\%$, and in WIC is $a_2 = 40.1\%$. The misclassification probabilities are estimated by extending parametric procedure of Hausman et al. (1998) to bivariate models. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1

Appendix A. Mathematical Proofs

A.1. Proof of Lemma 1

Proof.

OLS estimation Biasdness: least squares estimates of the parameters, $\theta_{LS} = [\alpha_1,\ \alpha_2,\ \alpha_3,\ \beta]'$, in the operation model in equation (3) is given by

$$\hat{\theta}_{LS} = (Z'Z)^{-1}Z'y$$

where $Z = [t_1,\ t_2,\ t_3,\ x]$ and $t_3 = t_1 \times t_2$. It follows that

$$\hat{\theta}_{LS} = (Z'Z)^{-1}Z'(Z^*\theta + \epsilon)$$

such that $Z = [t_1^*,\ t_2^*,\ t_3^*,\ x]$. $Z'Z$ is positive-semi definite by Cauchy-Schwarz inequality. Therefore, the bias term in OLS bias, given by $\hat{\theta}_{LS} = (Z'Z)^{-1}Z'Z^*\theta + (Z'Z)^{-1}Z'\epsilon$, is determined by $Z'Z^*$ and $Z'\epsilon$.

OLS estimation Inconsistency: We can express $\hat{\theta}_{LS} = (Z'Z)^{-1}Z'Z^*\theta + (Z'Z)^{-1}Z'\epsilon$ as

$$\hat{\theta}_{LS} = \left(\frac{Z'Z}{n}\right)^{-1}\left(\frac{Z'Z^*}{n}\right)\theta + \left(\frac{Z'Z}{n}\right)^{-1}\frac{Z'\epsilon}{n}$$

By taking the probability limit and applying Slutsky's Lemma, we have

$$\text{plim}\ \hat{\theta}_{LS} = \text{plim}\left(\frac{Z'Z}{n}\right)^{-1}\text{plim}\left(\frac{Z'Z^*}{n}\right)\theta + \text{plim}\left(\frac{Z'Z}{n}\right)^{-1}\text{plim}\frac{Z'\epsilon}{n}$$

By the Weak Law of Large Numbers and continuous mapping theorem, we have

$$\text{plim}\left(\frac{Z'Z}{n}\right)^{-1} \xrightarrow{p} (\text{Cov}\,(Z, Z))^{-1}$$

Following similar argument as above, we have

$$\text{plim}\frac{Z'Z^*}{n} \xrightarrow{p} \text{Cov}\,(Z, Z^*)$$

and by exogeneity of covariates in Z, we have

$$\text{plim}\frac{Z'\epsilon}{n}\xrightarrow{p}\text{Cov}\left(Z,\epsilon\right)=0$$

Combining the expressions above, we can easily obtain the results in Lemma 1, that is

$$\text{plim }\hat{\theta}_{LS}=\left(\text{Cov}\left(Z,Z\right)\right)^{-1}\text{Cov}\left(Z,Z^{*}\right)\theta$$

□

Proof.

Joint treatment effect OLS estimation Biasdness: The least squares estimates of α_3 is given by

$$\hat{\alpha}_{3LS}=\left(t_3'M_{-t_3}t_3\right)^{-1}t_3M_{-t_3}y$$

Where $M_{-t_3}=1-Z_{-t_3}\left(Z_{-t_3}'Z_{-t_3}\right)^{-1}Z_{-t_3}'$ and $Z_{-t_3}=[t_1,\ t_2,\ x]$. It follows that

$$\hat{\alpha}_{3LS}=\left(t_3'M_{-t_3}t_3\right)^{-1}t_3M_{-t_3}\left(Z_{-t_3}^{*}\theta_{-\alpha_3}+\alpha_3t_3^{*}+\epsilon\right)$$

and

$$\hat{\alpha}_{3LS}-\alpha_3=\left(t_3'M_{-t_3}t_3\right)^{-1}t_3M_{-t_3}\left(Z_{-t_3}^{*}\theta_{-\alpha_3}+\alpha_3\left(t_3^{*}-t_3\right)+\epsilon\right)$$

By Cauchy-Schwarz inequality, $\left(t_3'M_{-t_3}t_3\right)^{-1}$ is positive semi-definite. Hence, the bias in OLS estimates, given by $\hat{\alpha}_{3LS}-\alpha_3=\left(t_3'M_{-t_3}t_3\right)^{-1}\left(t_3M_{-t_3}Z_{-t_3}^{*}\theta_{-\alpha_3}+t_3M_{-t_3}\left(t_3^{*}-t_3\right)\alpha_3+t_3M_{-t_3}\epsilon\right)$, is driven by the remaining terms, that is, $t_3M_{-t_3}Z_{-t_3}^{*}$, $t_3M_{-t_3}t_3^{*}$, and $t_3M_{-t_3}\epsilon$.

Joint treatment effect OLS estimation Inconsistency: We can expressed the bias term in least squares estimator as

$$\hat{\alpha}_{3LS}-\alpha_3=\left(\frac{t_3'M_{-t_3}t_3}{n}\right)^{-1}\left(\frac{t_3M_{-t_3}Z_{-t_3}^{*}}{n}\theta_{-\alpha_3}+\frac{t_3M_{-t_3}\left(t_3^{*}-t_3\right)}{n}\alpha_3+\frac{t_3M_{-t_3}\epsilon}{n}\right)$$

Taking probability limit and using Slutsky Lemma, we have

$$\text{plim } \hat{\alpha}_{3LS} - \alpha_3 = \text{plim}\left(\frac{t_3' M_{-t_3} t_3}{n}\right)^{-1}\left(\text{plim}\frac{t_3 M_{-t_3} Z^*_{-t_3}}{n}\theta_{-\alpha_3} + \text{plim}\frac{t_3 M_{-t_3}\left(t_3^* - t_3\right)}{n}\alpha_3 + \text{plim}\frac{t_3 M_{-t_3}\epsilon}{n}\right)$$

Expanding the projection matrix M_{-t_3}, we can then express plim $\frac{t_3' M_{-t_3} t_3}{n}$ as

$$\begin{aligned}
\text{plim } \frac{t_3' M_{-t_3} t_3}{n} &= \text{plim } \frac{t_3'\left(1 - Z_{-t_3}\left(Z'_{-t_3} Z_{-t_3}\right)^{-1} Z'_{-t_3}\right) t_3}{n} \\
&= \text{plim } \frac{t_3' t_3}{n} - \text{plim } \frac{t_3' Z_{-t_3}\left(Z'_{-t_3} Z_{-t_3}\right)^{-1} Z'_{-t_3} t_3}{n}
\end{aligned}$$

By Weak Law of Large Numbers, we have

$$\text{plim } \frac{t_3' M_{-t_3} t_3}{n} = \mathbb{E}\left[t_{3i}\right] - \mathbb{E}\left[t'_{3i}\mathbf{z}_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}'_{i,-t_{3i}}\mathbf{z}_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}} t_{3i}\right]$$

By continuous mapping theorem, we have

$$\left(\text{plim } \frac{t_3' M_{-t_3} t_3}{n}\right)^{-1} = \left(\mathbb{E}\left[t_{3i}\right] - \mathbb{E}\left[t'_{3i}\mathbf{z}_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}'_{i,-t_{3i}}\mathbf{z}_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}} t_{3i}\right]\right)^{-1}$$

Applying the arguments above to plim $\frac{t_3 M_{-t_3} Z^*_{-t_3}}{n}\theta_{-\alpha_3}$ and plim $\frac{t_3 M_{-t_3}\left(t_3^* - t_3\right)}{n}$, It follows that

$$\text{plim } \frac{t_3 M_{-t_3} Z^*_{-t_3}}{n}\theta_{-\alpha_3} = \mathbb{E}\left[t_{3i}\mathbf{z}'^*_{i,-t_{3i}}\theta_{-\alpha_3}\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}^*_{i,-t_{3i}}\theta_{-\alpha_3}\right]$$

and

$$\begin{aligned}
\text{plim } \frac{t_3 M_{-t_3}\left(t_3^* - t_3\right)}{n} &= \mathbb{E}\left[t_{3i}\left(t_{3i}^* - t_{3i}\right)\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\left(t_{3i}^* - t_{3i}\right)\right] \\
&= -\mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\left(t_{3i}^* - t_{3i}\right)\right]
\end{aligned}$$

Finally, following the exogenoity of t_3 and other covariates in the model, we have

$$\text{plim } \frac{t_3 M_{-t_3}\epsilon}{n} = \mathbb{E}\left[t_{3i}\epsilon'_i\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\epsilon_i\right] = 0$$

We get the desired results, the asymptotic bias of the joint treatment effect OLS estimator, α_{3LS}, by combining all the terms above, that is

$$\text{plim} \quad \hat{\alpha}_{3LS} - \alpha_3 = \frac{A - B\alpha_3}{Q}$$

Where $A = \mathbb{E}\left[t_{3i}\mathbf{z}'^{*}_{i,-t_{3i}}\theta_{-\alpha_3}\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}^{*}_{i,-t_{3i}}\theta_{-\alpha_3}\right]$,

$B = \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{i,-t_{3i}}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\left(t^{*}_{3i} - t_{3i}\right)\right]$,

$Q = \mathbb{E}\left[t_{3i}\right] - \mathbb{E}\left[t_{3i}\mathbf{z}'_{i,-t_{3i}}\right]\left(\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}\mathbf{z}'_{-3i}\right]\right)^{-1}\mathbb{E}\left[\mathbf{z}_{i,-t_{3i}}t_{3i}\right]$,

$\theta_{-\alpha_3} = [\alpha_1,\, \alpha_2,\, \beta]'$, and $\mathbf{z}_{i,-t_{3i}} = [t_{1i}; t_{2i}; x_i]$

□

A.2. Proof of Lemma 2

Proof.

Components of estimator of W, $\hat{W}(a_1, a_2)$: W, variance-covariance matrix $\text{Cov}(Z, Z^*)$, is given by

$$W = \begin{bmatrix} \text{Cov}(t_{1i}, t^*_{1i}) & \text{Cov}(t_{1i}, t^*_{2i}) & \text{Cov}(t_{1i}, t^*_{3i}) & \text{Cov}(t_{1i}, x_i) \\ \text{Cov}(t_{2i}, t^*_{1i}) & \text{Cov}(t_{2i}, t^*_{2i}) & \text{Cov}(t_{2i}, t^*_{3i}) & \text{Cov}(t_{2i}, x_i) \\ \text{Cov}(t_{3i}, t^*_{1i}) & \text{Cov}(t_{3i}, t^*_{2i}) & \text{Cov}(t_{3i}, t^*_{3i}) & \text{Cov}(t_{3i}, x_i) \\ \text{Cov}(x_i, t^*_{1i}) & \text{Cov}(x_i, t^*_{2i}) & \text{Cov}(x_i, t^*_{3i}) & \text{Var}(x_i) \end{bmatrix}$$

Given the data, all terms in W except those in the last column, are not directly observed by the researcher since they are determined by true (and unobserved) participation status. Given the misclassification probabilities, a_1 and a_2, the probability of false negative in t_{1i} and t_{2i}, we can obtain the probability of false negative in the interaction term, t_{3i}, as follows.

By the Law of Iterated Expectations, $\mathbb{E}[t^*_{3i}] = \mathbb{E}[t^*_{1i}t^*_{2i}] = \mathbb{E}[\mathbb{E}[t^*_{1i}|t^*_{2i}]\ \mathbb{E}[t^*_{2i}|t^*_{1i}]]$. Considering that $\Pr(t_{ji} = 1) = (1 - a_j)\Pr(t^*_{ji} = 1)$ for $j \in \{1, 2\}$, it follows that $\mathbb{E}[t^*_{3i}] = \mathbb{E}\left[\frac{\mathbb{E}[t_{1i}|t^*_{2i}]}{1 - a_1}\,\frac{\mathbb{E}[t_{2i}|t^*_{1i}]}{1 - a_2}\right] = \frac{\mathbb{E}\left[t_{1i}\, t_{2i}\right]}{(1 - a_1)(1 - a_{2i})} = \frac{\mathbb{E}\left[t_{3i}\right]}{1 - a_3}$, where a_3 is the probability of false negative in t_{3i} and is given by $a_3 = a_1 + a_2 - a_1 \times a_2$.

Let's first consider the covariance between observed (and plausibly error-driven) program participation status and the underlying (true) status, that is, $\text{Cov}(\mathbf{t_{ji}}, \mathbf{t^*_{ji}})$, for $j \in \{1,2,3\}$. We can write

$$
\begin{aligned}
\text{Cov}(t_{ji}, t^*_{ji}) &= \mathbb{E}\left[t_{ji}, t^*_{ji}\right] - \mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t^*_{ji}\right] \\
&= \Pr\left[t_{ji} = 1, t^*_{ji} = 1\right] - \Pr\left[t_{ji} = 1\right]\Pr\left[t^*_{ji} = 1\right] \\
&= \Pr\left[t_{ji} = 1 | t^*_{ji} = 1\right]\Pr\left[t^*_{ji} = 1\right] - \Pr\left[t_{ji} = 1\right]\Pr\left[t^*_{ji} = 1\right] \\
&= \left(1 - \Pr\left[t_{ji} = 0 | t^*_{ji} = 1\right]\right)\Pr\left[t^*_{ji} = 1\right] - \Pr\left[t_{ji} = 1\right]\Pr\left[t^*_{ji} = 1\right] \\
&= \left(1 - a_j - \Pr\left[t_{ji} = 1\right]\right)\Pr\left[t^*_{ji} = 1\right]
\end{aligned}
$$

From $\Pr\left[t^*_{ji} = 1\right] = (1 - a_j)\, Pr\left[t_{ji} = 1\right]$, it follows that

$$
\begin{aligned}
\text{Cov}(t_{ji}, t^*_{ji}) &= \frac{\left(1 - a_j - \Pr\left[t_{ji} = 1\right]\right)\Pr\left[t_{ji} = 1\right]}{(1 - a_j)} \\
&= \frac{\left(1 - a_j - \Pr\left[t_{ji} = 1\right]\right)\text{Var}\left(t_{ji}\right)}{(1 - a_j)\left(\Pr\left[t_{ji} = 1\right]\right)} \\
&= \frac{(1 - a_j - P_j)}{(1 - a_j)(1 - P_j)}\text{Var}\left(t_{ji}\right)
\end{aligned}
$$

Hence, for $j \in \{1,2\}$, $\text{Cov}(t_{ji}, t^*_{ji})$ can be expressed as

$$
\text{Cov}(t_{ji}, t^*_{ji}) = \eta_j \text{Var}\left(t_{ji}\right)
$$

where $\eta_j = \dfrac{1 - a_j - P_j}{(1 - a_j)(1 - P_j)}$

Next, we examine covariance between true (unobserved) participation status in one program, t^*_{ji} , and the observed participation status in the other program, t_k, that is, $\text{Cov}(\mathbf{t_{ki}}, \mathbf{t^*_{ji}})$ where $j, k \in \{1,2\}$ and $j \neq k$. By applying the Law of Iterated Expectations, we have

$$
\text{Cov}(t_{ki}, t^*_{ji}) = \text{Cov}\left(t_{ki}, \mathbb{E}[t^*_{ji} | t_{ki}]\right)
$$

It follows that $\mathbb{E}[t_{ji}^*|t_{ki}] = \dfrac{\mathbb{E}[t_{ji}|t_{ki}]}{1-a_j}$, which implies that

$$
\begin{aligned}
\text{Cov}(t_{ki}, t_{ji}^*) &= \text{Cov}\left(t_{ki}, \frac{\mathbb{E}[t_{ji}|t_{ki}]}{1-a_j}\right) \\
&= \frac{1}{1-a_j}\text{Cov}\left(t_{ki}, \mathbb{E}[t_{ji}|t_{ki}]\right) \\
&= \frac{1}{1-a_j}\text{Cov}\left(t_{ki}, t_{ji}\right)
\end{aligned}
$$

We now have the results in Lemma 1, that is, for $j, k \in \{1, 2\}$ and $j \neq k$,

$$\text{Cov}(t_{ki}, t_{ji}^*) = \zeta_j \text{Cov}\left(t_{ki}, t_{ji}\right)$$

where $\zeta_j = \dfrac{1}{1-a_j}$

We then turn to the covariance between individual program participation and the interaction term given by $\text{Cov}(\mathbf{t_{ji}}, \mathbf{t_{3i}^*})$, for $j \in \{1, 2\}$.

$$\text{Cov}(t_{ji}, t_{3i}^*) = \mathbb{E}\left[t_{ji}t_{3i}^*\right] - \mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}^*\right]$$

We know that $\mathbb{E}\left[t_{3i}^*\right] = \dfrac{\mathbb{E}[t_{3i}]}{1-a_3}$ and $\mathbb{E}\left[t_{ji}t_{3i}^*\right] = \mathbb{E}\left[t_{ji}\, t_{ki}^*\right]$, so that, by the law of Iterated expectation,

$$\mathbb{E}\left[t_{ji}t_{3i}^*\right] = \mathbb{E}\left[t_{ji}\mathbb{E}[t_{ki}^*|t_{ji}]\right] = \mathbb{E}\left[t_{ji}\frac{\mathbb{E}[t_{ki}|t_{ji}]}{1-a_k}\right] = \frac{\mathbb{E}\left[t_{ji}t_{ki}\right]}{1-a_k} = \frac{\mathbb{E}\left[t_{3i}\right]}{1-a_k}$$

where $k = \{1, 2\}$ and $k \neq j$. Since $t_{3i} = t_{ji} \times t_{ki} = t_{ji} \times t_{3i}$, it follows that

$$\begin{aligned}
\text{Cov}(t_{ji}, t_{3i}^{*}) &= \frac{\mathbb{E}\left[t_{ji}t_{3ii}\right]}{1 - a_k} - \frac{\mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}\right]}{1 - a_3} \\
&= \frac{\mathbb{E}\left[t_{ji}t_{3i}\right]}{(1 - a_k)} - \frac{\mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}\right]}{(1 - a_j)(1 - a_k)} \\
&= \frac{\mathbb{E}\left[t_{ji}t_{3ii}\right] - \mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}\right] - a_j\mathbb{E}\left[t_{ji}t_{3i}\right]}{(1 - a_j)(1 - a_k)} \\
&= \frac{\text{Cov}\left(t_{ji}, t_{3i}\right) - a_j\mathbb{E}\left[t_{3i}\right]}{(1 - a_j)(1 - a_k)} \\
&= \left[\frac{\text{Cov}\left(t_{ji}, t_{3i}\right) - a_j P_3}{(1 - a_j)(1 - a_k)\text{Cov}\left(t_{ji}, t_{3i}\right)}\right]\text{Cov}\left(t_{ji}, t_{3i}\right)
\end{aligned}$$

We get the results in Lemma 2 if we rewrite the $\text{Cov}\left(t_{ji}, t_{3i}\right)$ terms in the bracket as $\text{Cov}\left(t_{ji}, t_{3i}\right) = \mathbb{E}\left[t_{ji}t_{3i}\right] - \mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}\right] = \mathbb{E}\left[t_{3i}\right] - \mathbb{E}\left[t_{ji}\right]\mathbb{E}\left[t_{3i}\right] = P_3\left(1 - P_j\right)$, that is

$$\begin{aligned}
\text{Cov}(t_{ji}, t_{3i}^{*}) &= \left[\frac{P_3\left(1 - P_j\right) - a_j P_3}{(1 - a_j)(1 - a_k)P_3\left(1 - P_j\right)}\right]\text{Cov}\left(t_{ji}, t_{3i}\right) \\
&= \left[\frac{1 - a_j - P_j}{\left(1 - a_j\right)\left(1 - P_j\right)\left(1 - a_k\right)}\right]\text{Cov}\left(t_{ji}, t_{3i}\right)
\end{aligned}$$

so that, for $j, k \in \{1, 2\}$ and $j \neq k$,

$$\text{Cov}(t_{ji}, t_{3i}^{*}) = \zeta_k \eta_j \text{Cov}(t_{ji}, t_{3i})$$

where $\zeta_k = \dfrac{1}{1 - a_k}$ and $\eta_j = \dfrac{1 - a_j - P_j}{1 - a_j}$

The remaining terms in W are determined by covariance between x_i, assumed to be error-free, and underlying (true) program participation status, t_{1i}^{*} and t_{2i}^{*}, and the interaction term, t_{3i}^{*}. Likewise, we can express these terms as functions of misreporting probabilities and sample statistics. For $j \in \{1, 2, 3\}$, it follows that

$$\begin{aligned}
\text{Cov}(x_i, t^*_{ji}) &= \mathbb{E}[x_i t^*_{ji}] - \mathbb{E}[x_i]\mathbb{E}[t^*_{ji}] \\
&= \mathbb{E}[x_i \mathbb{E}[t^*_{ji}|x_i]] - \mathbb{E}[x_i]\mathbb{E}[\exp[t^*_j|x_i]] \quad \text{by the Law of Iterated Expectations} \\
&= \mathbb{E}\left[\frac{x_i \mathbb{E}[t_{ji}|x_i]}{1-a_j}\right] - \mathbb{E}[x_i]\mathbb{E}\left[\mathbb{E}[\frac{t_{ji}|x_i]}{1-a_j}\right] \\
&= \frac{\mathbb{E}\left[x_i t_{ji}\right] - \mathbb{E}[x_i]\mathbb{E}\left[t_{ji}\right]]}{1-a_j} \\
&= \frac{\text{Cov}(x_i, t_{ji})}{1-a_j} = \zeta_j \text{Cov}(x_i, t_{ji})
\end{aligned}$$

We obtain Lemma 2 by combining the results above, giving us the estimator of W, $\hat{W}(a_1, a_2)$, which is a function of misclassification probabilities and other sample statistics that can easily computed in the data. □

A.3. Proof of Theorem 1

***Proof*.**

Consistency: The Adjusted Least Squares Estimator is given by

$$\begin{bmatrix} \hat{\alpha}_{1Adj} \\ \hat{\alpha}_{2Adj} \\ \hat{\alpha}_{3Adj} \\ \hat{\beta}_{Adj} \end{bmatrix} = \begin{bmatrix} \eta_1 \sigma^2_{t_1} & \zeta_2 \sigma_{t_1 t_2} & \eta_1 \zeta_2 \sigma_{t_1 t_3} & \sigma_{t_1 x} \\ \zeta_1 \sigma_{t_2 t_1} & \eta_2 \sigma^2_{t_2} & \eta_2 \zeta_1 \sigma_{t_1 t_3} & \sigma_{t_2 x} \\ \eta_1 \sigma_{t_3 t_1} & \eta_2 \sigma_{t_3 t_2} & \eta_3 \sigma^2_{t_3} & \sigma_{t_3 x} \\ \zeta_1 \sigma_{x t_1} & \zeta_2 \sigma_{x t_2} & \zeta_3 \sigma_{x t_3} & \sigma^2_x \end{bmatrix}^{-1} \begin{bmatrix} \sigma_{yt_1} \\ \sigma_{yt_2} \\ \sigma_{yt_3} \\ \sigma_{yx} \end{bmatrix}$$

which can be expressed in matrix and vector notations as:

$$\hat{\theta}_{Adj} = W\left(a_1, a_2\right)^{-1} \Sigma_{Zy}$$

where $Z = [t_1, t_2, t_3, x]$, $\Sigma_{Zy} = [\sigma_{yt_1}\, \sigma_{yt_2}\, \sigma_{yt_3}\, \sigma_{yx}]'$ and for any covariates, say r_i and s_i, $\sigma_{rs} = \dfrac{1}{n}\sum_{i=1}^n (r_i - \bar{r})(s_i - \bar{s})'$ and $\sigma^2_r = \dfrac{1}{n}\sum_{i=1}^n (r_i - \bar{r})(r_i - \bar{r})'$where $\bar{r}$ and $\bar{s}$ are the sample mean of r_i and s_i. It follows that $\hat{\theta}_{Adj} = W\left(a_1, a_2\right)^{-1}\left(\Sigma_{ZZ^*}\theta + \Sigma_{Z\epsilon}\right)$.

We already know from Lemma 2 that Σ_{ZZ^*} can be expressed in terms of misclassification probabilities as $W(a_1, a_2)$, so $\hat{\theta}_{Adj} = \theta + W(a_1, a_2)^{-1} \Sigma_{Z\epsilon}$. Hence, it follows that

$$\hat{\theta}_{Adj} - \theta = W(a_1, a_2)^{-1} \Sigma_{Z\epsilon}$$

By taking the probability limits and applying Slutsky's Lemma, we have

$$\begin{aligned} \text{plim } \hat{\theta}_{Adj} - \theta &= \text{plim } W(a_1, a_2)^{-1} \text{ plim } \Sigma_{Z\epsilon} \\ &= \Gamma^{-1}\Lambda \end{aligned}$$

where, by the Weak Law of Large numbers, we have

$$\Gamma = \begin{bmatrix} \eta_1 \text{Var}(t_{1i}) & \zeta_2 \text{Cov}(t_{1i}, t_{2i}) & \eta_1\zeta_2 \text{Cov}(t_{1i}, t_{3i}) & \text{Cov}(t_{1i}, x_i) \\ \zeta_1 \text{Cov}(t_{2i}, t_{1i}) & \eta_2 \text{Var}(t_{2i}) & \eta_2\zeta_1 \text{Cov}(t_{1i}, t_{3i}) & \text{Cov}(t_{2i}, x_i) \\ \eta_1 \text{Cov}(t_{3i}, t_{1i}) & \eta_2 \text{Cov}(t_{3i}, t_{2i}) & \eta_3 \text{Var}(t_{3i}) & \text{Cov}(t_{3i}, x_i) \\ \zeta_1 \text{Cov}(x_i, t_{1i}) & \zeta_2 \text{Cov}(x_i, t_{2i}) & \zeta_3 \text{Cov}(x_i, t_{3i}) & \text{Var}(x_i) \end{bmatrix}$$

and $\Lambda = [\text{Cov}(t_{1i}, \epsilon_i),\ \text{Cov}(t_{2i}, \epsilon_i),\ \text{Cov}(t_{3i}, \epsilon_i),\ \text{Cov}(x_i, \epsilon_i)]'$. By the Law of Iterated Expectations and under Assumption 1, $\mathbb{E}[\epsilon_i | t_{1i}, t_{2i}, t_{3i}, x_i] = \mathbb{E}[\mathbb{E}[\epsilon_i | t^*_{1i}, t^*_{2i}, t^*_{3i}, x_i] | t_{1i}, t_{2i}, t_{3i}, x_i] = 0$, therefore $\text{Cov}(t_{1i}, \epsilon_i) = \text{Cov}(t_{2i}, \epsilon_i) = \text{Cov}(t_{3i}, \epsilon_i) = \text{Cov}(x_i, \epsilon_i) = 0$, implying that $\Lambda = 0$, and $\hat{\theta}_{Adj} - \theta = 0$. This is equivalent to $\theta_{Adj} \xrightarrow{p} \theta$, which translates to, $\alpha_{1Adj} \xrightarrow{p} \alpha_1$, $\alpha_{2Adj} \xrightarrow{p} \alpha_2$, $\alpha_{3Adj} \xrightarrow{p} \alpha_3$, and $\beta_{Adj} \xrightarrow{p} \beta$.

□

Appendix B. Data

B.1 Food Insecurity Score

The food Insecurity score in our analysis represents the USDA's 30-day Adult Food Security Scale obtained from the 10 questions (E2-E9a) included in the last interview of the National Household Food Acquisition and Purchase Survey (FoodAPS) to examine household food security status. The questions take into account severity of conditions and behaviors that characterize food insecurity. We represent the 10 questions in Appendix B Table B1.

Appendix Table B1: **Food Security Score Questions in National Household Food Acquisition and Purchase Survey (FoodAPS)**

Variable	Definition
Question E2	In last 30 days, worried food would run out before we got more money 1 - Often True, 2 - Sometimes True, 3 - Never True
Question E3	Food ran out and had no money to buy more, in last 30 days 1 - Often True, 2 - Sometimes True, 3 - Never True
Question E4	Couldn't afford to eat balanced meals, in last 30 days 1 - Often True, 2 - Sometimes True, 3 - Never True, -997 - Don't Know
Question E5	Adults skipped or cut size of meals b/c not enough money, in last 30 days (Y/N) 0 - No, 1 - Yes, -998- Refused, -996 - Valid Skip
Question E5a	Number of days adults skipped/cut meal size b/c not enough money, last 30 days
Question E6	Eat less than felt you should b/c not enough money, in last 30 days (Y/N) 0 - No, 1 - Yes, -998- Refused, -997 - Don't Know, -996 - Valid Skip
Question E7	Ever hungry but didn't eat b/c not enough money, in last 30 days (Y/N) 0 - No, 1 - Yes, -996 - Valid Skip
Question E8	Lose weight b/c not enough money for food, in last 30 days (Y/N) 0 - No, 1 - Yes, -997 - Don't Know, -996 - Valid Skip
Question E9	Skip food all day b/c not enough money for food, in last 30 days (Y/N) 0 - No, 1 - Yes, -996 - Valid Skip
Question E9a	How often adults skipped food all day b/c not enough money, in last 30 days -997 - Don't Know, -996 - Valid Skip

The Food Security Scale codes "Yes," "Often," "Sometimes," and "three or more days" responses to E2 - E9a questions described above as affirmative responses. The Food Security Score is obtained by summing up the affirmative responses, ranging from 0 to 10. In our analysis, we term it Food Insecurity Score to reflect that 0 represents high food security and increasing values indicate increasing food inadequacy.

B.2 Health Eating Index-2010

National Household Food Purchase and Acquisition Survey (FoodAPS) also aimed to provide data that can be used to evaluate the nutrition quality of food acquired by households. For one week, between April 2012 and January 2013, the survey collected detailed information regarding the types of food obtained by households that can be used in computing the Healthy Eating Index (HEI). The Healthy Eating Index (HEI) measures overall diet quality and the quality of several dietary components, which can be used to examine compliance with the U.S. Dietary Guidelines for Americans (DGAs). Several iterations of HEI have been developed by the U.S. Department of Health and Human Services National Cancer Institute (NCI) and the U.S. Department of Agriculture (USDA) researchers since 2005. Following Mancino et al. (2018), we consider 2010 Health Eating Index (HEI-2010) scores to assess the quality of food items reported in FoodAPS. HEI-2010 scores range from 0 to 100 and have 12 components, as presented in Appendix Table B2 . The calculation of HEI-2010 accounts for the variation in individual total caloric needs. The process matches the reported food items in Food APS with Food Pattern Equivalent Database for each food item and the USDA nutrient food code from USDA Food and Nutrient Database for Dietary Studies (FNDDS). We obtain the method, and the code to compute the 2010 HEI from Mancino et al. (2018).

Appendix Table B2: **HEI–20101 Components and Scoring Standards**

Component	Maximum Points	Standard For Maximum Score	Standard For Minimum Score of Zero
Adequacy:			
Total Fruit[a]	5	$\geq$ 0.8 cup equiv. per 1,000 kcal	No Fruit
Whole Fruit[b]	5	$\geq$ 0.4 cup equiv. per 1,000 kcal	No Whole Fruit
Total Vegetables[c]	5	$\geq$ 1.1 cup equiv. per 1,000 kcal	No Vegetables
Greens and Beans[c]	5	$\geq$ 0.2 cup equiv. per 1,000 kcal	No Dark Green Vegetables or Beans and Peas
Whole Grains	10	$\geq$ 1.5 oz equiv. per 1,000 kcal	No Whole Grains
Dairy[d]	10	$\geq$ 1.3cup equiv. per 1,000 kcal	No Dairy
Total Protein Foods	5	$\geq$ 2.5 oz equiv. per 1,000 kcal	No Protein Foods
Seafood and Plant Proteins[e, f]	5	$\geq$ 0.8 oz equiv. per 1,000 kcal	No Seafood or Plant Proteins
Fatty Acids[g]	10	(PUFAs + MUFAs)/SFAs $\geq$ 2.5	(PUFAs + MUFAs)/SFAs $\leq$ 1.2
Moderation:			
Refined Grains	10	$\leq$ 1.8 oz equiv. per 1,000 kcal	$\geq$ 4.3 oz equiv. per 1,000 kcal
Sodium	10	$\leq$ 1.1 gram per 1,000 kcal	$\geq$ 2.0 grams per 1,000 kcal
Empty Calories[h]	20	$\leq$ 19% of energy	$\geq$ 50% of energy

Note: Intakes between the minimum and maximum standards are scored proportionately. The total HEI score is the sum of the adequacy components (i.e. foods to eat more of for good health) and moderation components (i.e. foods to limit for good health).

[a] Includes 100% fruit juice.

[b] Includes all forms except juice.

[c] Includes any beans and peas not counted as Total Protein Foods.

[d] Includes all milk products, such as fluid milk, yogurt, and cheese, and fortified soy beverages.

[e] Beans and peas are included here (and not with vegetables) when the Total Protein Foods standard is otherwise not met.

[f] Includes seafood, nuts, seeds, soy products (other than beverages) as well as beans and peas counted as Total Protein Foods.

[g] Ratio of poly- and monounsaturated fatty acids (PUFAs and MUFAs) to saturated fatty acids (SFAs).

[h] Calories from solid fats, alcohol, and added sugars; threshold for counting alcohol is $>$ 13 grams/1000 kcal.

Appendix C. Empirical Example Additional Results

Appendix Table C1: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Healthy Eating - Adequacy component using Misclassification Probabilities from Validation Data**

	OLS	Adj.LS	OLS	Adj.LS	OLS	Adj.LS
Panel 1						
Dependent Variables:	**Total Fruit**		**Whole Fruit**		**Total Vegetables**	
SNAP	0.065	-0.022	-0.331	-0.038	-0.013	0.048
	(0.273)	(0.047)	(0.317)	(0.053)	(0.255)	(0.043)
WIC	0.089	-0.043	-0.475	-0.003	0.084	0.044
	(0.289)	(0.058)	(0.336)	(0.063)	(0.270)	(0.055)
SNAP × WIC	0.044	0.003	0.470	0.048	-0.212	-0.020
	(0.342)	(0.032)	(0.398)	(0.038)	(0.320)	(0.033)
Panel 2						
Dependent Variables:	**Greens and Beans**		**Whole Grains**		**Dairy**	
SNAP	-0.543*	0.008	-0.263	-0.042	-0.184	-0.155
	(0.296)	(0.051)	(0.395)	(0.068)	(0.551)	(0.099)
WIC	-0.114	-0.104*	-0.133	-0.096	-0.486	-0.138
	(0.314)	(0.060)	(0.419)	(0.080)	(0.584)	(0.127)
SNAP × WIC	0.431	0.044	0.420	0.041	0.886	0.085
	(0.371)	(0.036)	(0.496)	(0.048)	(0.691)	(0.070)
Panel 3						
Dependent Variables:	**Total Protein Foods**		**Seafood and Plant Proteins**		**Fatty Acids**	
SNAP	-0.129	-0.049	0.112	-0.016	-0.930	0.105
	(0.247)	(0.040)	(0.296)	(0.053)	(0.579)	(0.107)
WIC	-0.254	-0.032	0.146	-0.038	-0.160	-0.050
	(0.262)	(0.057)	(0.314)	(0.063)	(0.614)	(0.129)
SNAP × WIC	0.342	0.033	-0.027	-0.004	0.317	0.036
	(0.310)	(0.033)	(0.371)	(0.037)	(0.727)	(0.073)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP comes from Courtemanche et al. (2019), $a_1 = 32.31\%$, and in WIC comes from Fox & Hokayem (2022), $a_1 = 41.5\%$. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1

Appendix Table C2: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Healthy Eating - Moderation component using Misclassification Probabilities from Validation Data**

	OLS	Adj.LS	OLS	Adj.LS	OLS	Adj.LS
Dependent Variables:	**Refined Grains**		**Sodium**		**Empty Calories**	
SNAP	0.422	-0.120	0.199	0.076	-1.506	-0.147
	(0.621)	(0.115)	(0.627)	(0.108)	(0.987)	(0.181)
WIC	-0.308	0.029	0.311	0.075	-0.667	-0.439**
	(0.658)	(0.134)	(0.664)	(0.136)	(1.046)	(0.222)
SNAP × WIC	0.202	0.017	-0.529	-0.051	1.985	0.195
	(0.779)	(0.074)	(0.787)	(0.072)	(1.239)	(0.127)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP comes from Courtemanche et al. (2019), $a_1 = 32.31\%$, and in WIC comes from Fox & Hokayem (2022), $a_1 = 41.5\%$. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table C3: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Healthy Eating - Adequacy component using Estimated Misclassification Probabilities**

	OLS	Adj.LS	OLS	Adj.LS	OLS	Adj.LS
Panel 1						
Dependent Variables:	**Total Fruit**		**Whole Fruit**		**Total Vegetables**	
SNAP	0.065	-0.025	-0.331	-0.042	-0.013	0.055
	(0.273)	(0.053)	(0.317)	(0.060)	(0.255)	(0.049)
WIC	0.089	-0.047	-0.475	-0.005	0.084	0.048
	(0.289)	(0.063)	(0.336)	(0.069)	(0.270)	(0.060)
SNAP × WIC	0.044	0.004	0.470	0.055	-0.212	-0.023
	(0.342)	(0.037)	(0.398)	(0.043)	(0.320)	(0.037)
Panel 2						
Dependent Variables:	**Greens and Beans**		**Whole Grains**		**Dairy**	
SNAP	-0.543*	0.011	-0.263	-0.047	-0.184	-0.176
	(0.296)	(0.059)	(0.395)	(0.077)	(0.551)	(0.113)
WIC	-0.114	-0.114*	-0.133	-0.105	-0.486	-0.151
	(0.314)	(0.066)	(0.419)	(0.088)	(0.584)	(0.139)
SNAP × WIC	0.431	0.050	0.420	0.047	0.886	0.097
	(0.371)	(0.042)	(0.496)	(0.056)	(0.691)	(0.081)
Panel 3						
Dependent Variables:	**Total Protein Foods**		**Seafood and Plant Proteins**		**Fatty Acids**	
SNAP	-0.129	-0.055	0.112	-0.018	-0.930	0.123
	(0.247)	(0.046)	(0.296)	(0.060)	(0.579)	(0.122)
WIC	-0.254	-0.035	0.146	-0.041	-0.160	-0.054
	(0.262)	(0.062)	(0.314)	(0.069)	(0.614)	(0.140)
SNAP × WIC	0.342	0.038	-0.027	-0.005	0.317	0.042
	(0.310)	(0.038)	(0.371)	(0.043)	(0.727)	(0.084)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP is $a_1 = 29.7\%$, and in WIC is $a_2 = 40.1\%$. The misclassification probabilities are estimated by extending parametric procedure of Hausman et al. (1998) to bivariate models. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1

Appendix Table C4: **Impact of Supplemental Nutrition Assistance Program (SNAP) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) on Healthy Eating - Moderation component using Estimated Misclassification Probabilities**

	OLS	Adj.LS	OLS	Adj.LS	OLS	Adj.LS
Dependent Variables:	**Refined Grains**		**Sodium**		**Empty Calories**	
SNAP	0.422	-0.139	0.199	0.086	-1.506	-0.164
	(0.621)	(0.131)	(0.627)	(0.123)	(0.987)	(0.206)
WIC	-0.308	0.031	0.311	0.083	-0.667	-0.479**
	(0.658)	(0.146)	(0.664)	(0.148)	(1.046)	(0.242)
SNAP × WIC	0.202	0.020	-0.529	-0.059	1.985	0.225
	(0.779)	(0.085)	(0.787)	(0.083)	(1.239)	(0.146)

Note: Standard errors in parenthesis and bootstrapped. The analytical sample comes from FoodAPS and consists of households with a pregnant woman or child under five years old and below 130% of the federal poverty threshold. The probability of false negatives in SNAP is $a_1 = 29.7\%$, and in WIC is $a_2 = 40.1\%$. The misclassification probabilities are estimated by extending parametric procedure of Hausman et al. (1998) to bivariate models. Regressors not reported include primary respondent education, ethnicity, race, employment, marital status, age, rural household residency, indicator and count of children below the age of five, number of children, income to poverty ratio, primary store distance, whether the primary store is SNAP authorized, whether the household has any vehicle and whether the household is renting the house. Significance codes: *** p<0.01, ** p<0.05, * p<0.1

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