Investigating Treatment Effects of Participating Jointly in SNAP and WIC when the Treatment is Validated Only for SNAP*

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Abstract. USDA operates several food assistance programs aimed at alleviating food insecurity. Little is known about how they interact. We focus on SNAP and WIC, two of the largest means-tested programs that provide resources to low-income households to purchase food and differ in several respects. Our question is the extent to which participation in both programs alleviates food insecurity compared with participation in SNAP alone. We bound underlying causal effects by applying nonparametric treatment effect methods that allow for endogenous selection and underreported program participation to data from the National Household Food Acquisition and Purchase Survey (FoodAPS). FoodAPS contains administrative data to validate SNAP participation and data on the local food environment, including the cost of food, allowing us to tighten bounds on the causal effects. Under relatively weak assumptions about the selection process, combined with a food expenditure-based monotone instrumental variable, we identify that the marginal impact of participating in both programs is strictly positive. This finding provides evidence that the programs are nonredundant, which can aid policymakers in improving the design and targeting of food assistance programs. The methods showcase what can be learned about treatment effects when validation data are available for one program but not the other.

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1. Introduction

A household is food secure if it has access to enough food for an active, healthy life of all of its members; it is food insecure otherwise (NRC, 2006). Substantial prevalence of food insecurity in the low-income U.S. population is a matter of public concern, since food insecurity can be detrimental to the health and well-being of adults and children (for a literature review, see Gundersen, Kreider, and Pepper, 2011). In fact, among households with income below 130% of the federal poverty threshold in 2016, 35.7% experienced food insecurity (Coleman-Jensen et al., 2017a; 2017b).[1]

The U.S. Department of Agriculture (USDA) administers 15 domestic food assistance programs designed to alleviate food insecurity (Oliveira, 2017). Many low-income households are eligible for and participate in more than one program. The largest and third largest programs by expenditures are, respectively, the Supplemental Nutrition Assistance Program (SNAP; $68.1 billion spent in the fiscal year 2017, 42.2 million participants on average per month) and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC; $5.7 billion, 7.3 million participants).[2] These two programs are the focus of our paper.

While both SNAP and WIC are means-tested and aim to provide resources to low-income households for the acquisition of food, they differ in several respects (Hoynes and Schanzenbach, 2015; U.S. GAO, 2010). SNAP offers vouchers to eligible low-income households for the purchase of most food items. In contrast, WIC provides benefits to qualifying individuals in low-income households for the purchase of a restricted set of foods identified to meet the specific nutritional needs of pregnant, post-partum, and lactating women; infants; and children less than 5 years old. WIC also provides nutrition counseling and referrals for health services. Differences in the program design may lead to synergies between SNAP and WIC.[3] For example, WIC expands acquisition of specific foods and may lead to increased awareness of

[1] The rate of food insecurity among all U.S. households was 12.3% (Coleman-Jensen et al., 2017a).
[2] The second largest program is the National School Lunch Program (NSLP).
[3] Brien and Swann (2000) find that synergies can reinforce welfare programs' effects.

healthy food selection and food purchases that better meet household nutritional needs. In turn, SNAP provides a wider choice over foods. Investigating whether either program has a positive marginal effect on food security given participation in the other is informative about potential programmatic redundancies and contributes to a better understanding of the overall efficacy of the food safety net in the United States.

In practice, the rate of food insecurity among food program recipients is substantial. In particular, 51.2% of households on SNAP and 40.6% on WIC were food insecure in 2016. Moreover, the rate of food insecurity among SNAP (WIC) recipients was twice (1.4 times) that among potentially eligible, low-income nonrecipients (Coleman-Jensen et al., 2017a). These seemingly counterintuitive associations (especially in view of the programs' shared objective to reduce food insecurity) provide an additional motivation for studying SNAP and WIC effects.

Identifying causal, rather than associative, effects of any food program is challenging because of (*i*) endogenous self-selection of households into the program and (*ii*) pervasive underreporting of food assistance in national surveys. In particular, unobserved personal characteristics (e.g., expected future health status) are thought to be related to both food security and program participation. This simultaneity precludes the use of simple regression techniques (e.g., OLS) to estimate causal effects (see Gundersen and Oliveira, 2001; Jensen, 2002; Fox, Hamilton, and Lin, 2004; Wilde, 2007; Nord and Golla, 2009). Furthermore, households are thought to systematically underreport the receipt of food assistance (e.g., Bollinger and David, 1997; Meyer, Mok, and Sullivan, 2015a; 2015b), and the propensity to misreport may vary across households based on their observed and unobserved characteristics. For example, Meyer et al. (2015a) find that less than 60% of SNAP benefits are recorded in recent waves of the Current Population Survey (CPS). Bitler, Currie, and Scholz (2003) find evidence of "severe" underreporting of WIC benefits. Under such circumstances, all of the classical measurement error assumptions are violated, and it becomes particularly important to locate and use any available validation information as a means to mitigate the measurement error problem. In turn, analyzing not just one, but two programs adds an extra layer of complexity.[4]

In this paper, we quantify how participation in both SNAP and WIC affects a household's probability of being food secure compared with participation in SNAP alone. To estimate causal treatment effects, we apply nonparametric bounding methods developed by Jensen, Kreider, and Zhylyevskyy (2018; hereafter JKZ) to address joint program participation. These methods allow us to place bounds on the causal treatment effects and account for the dual identification problems of endogenous selection and potentially misreported program participation status when auxiliary information is available to validate self-reported participation in one program (e.g., SNAP) but not the other (e.g., WIC). The dual identification problems pose an obstacle to applying standard instrumental variable (IV) techniques to quantify treatment effects, since such techniques are known to produce inconsistent estimates when an endogenous binary treatment variable (e.g., program participation vs. nonparticipation) is mismeasured (e.g., Black et al., 2000; Frazis and Loewenstein, 2003).[5] If the self-reported indicator of participation is measured with error, it is likely that a valid instrument for receipt is correlated with the measurement error as well. Even in the absence of measurement error, it may still be difficult in practice to find IVs that are both valid and strong. For example, variation in policy instruments across states that affect food assistance participation rates may be endogenously related to food insecurity.[6]

The approach in JKZ generalizes methods in Kreider and Hill (2009) and Kreider et al. (2012), which accommodate binary treatments (e.g., SNAP vs. no SNAP), to handle the case of partially ordered multiple treatments. We derive sharp bounds on average treatment effects

[4] To avoid further complexity, we abstract away from the issue of potentially mismeasured food security status. To the extent that such misclassification exists, the identified bounds on treatment effects reported in this paper would become wider.
[5] Mismeasurement of a binary variable induces a nonclassical measurement error even if such errors occur randomly, and in our setting errors are systematic in one direction and likely related to household characteristics.
[6] The policies are not randomly assigned, and policies targeted towards participation (such as eligibility rules or ease of recertification) may be correlated with other state policies that could directly affect food security (e.g. policies that affect the financial well-being of poorer households and therefore their ability to buy food).

(ATEs) that are logically consistent with the observed data, available validation information, and imposed statistical and behavioral assumptions on the data-generating process. This methodology differs from the multiple-programs approaches of Fraker and Moffitt (1988), Keane and Moffitt (1998), and Brien and Swann (2001) who model participation decisions and program effects jointly using simultaneous equations in parametric settings. We also allow for underreported participation status.

We apply our methods to data from the USDA's National Household Food Acquisition and Purchase Survey (FoodAPS). Among existing nationally representative datasets, FoodAPS is particularly well suited for the analysis as it provides self-reported household participation in SNAP and WIC and, furthermore, contains auxiliary administrative data to validate self-reported SNAP participation.[7] In total, FoodAPS contains records for 4,826 households who participated in the survey during one week between April 2012 and January 2013. We focus only on those households that would be eligible to participate in SNAP and WIC concurrently. Given the eligibility restrictions associated with each of the two programs, we analyze households with income below 130% of the poverty threshold and containing a pregnant woman, or a child aged less than five years.[8] The analytical sample contains 460 households, 37% of whom report being on both programs.

Our empirical analysis starts by estimating the bounds on the ATE of participating in SNAP and WIC jointly vs. in SNAP alone under minimal assumptions. These worst-case bounds are wide and do not allow us to sign the ATE. We then investigate how several middle ground assumptions can narrow the worst-case bounds by restricting relationships between the latent food security outcomes, program participation, and observed covariates. For example, we study the identifying power of an assumption that, on average, the probability of a favorable food

[7] No administrative data to validate WIC participation are available in FoodAPS.
[8] It should be noted that while the income eligibility cutoff is 185% of the poverty threshold in the case of the WIC program, more than 86% of WIC households had incomes at 130% of poverty and below in 2014 (Thorn et al., 2015, Table III.6 on p. 44).

security outcome weakly rises with more household expenditures on food at home relative to expenditures consistent with the Thrifty Food Plan (Carlson et al., 2007). Unlike standard IVs, MIVs require no exclusion restrictions. The trade-off is that we are only able to bound and not point-identify causal effects. Still, when combined with the FoodAPS data, our assumptions are strong enough to identify substantial beneficial effects of participating in multiple food assistance programs.

The remainder of this paper is organized as follows. Section 2 describes the data and characteristics of our analytical sample. Section 3 lays out the methodological framework, formally defines the identification problems, and employs several new sets of closed-form analytical formulas derived in JKZ to bound ATEs given a potentially mismeasured, partially ordered, and partially verified treatment. Our empirical results highlight the identifying power of successively stronger nonparametric assumptions. Section 4 concludes.

2. Data

2.1. FoodAPS

Our main data source is the USDA's National Household Food Acquisition and Purchase Survey (FoodAPS), a recent nationally representative survey to collect comprehensive data about household food purchases and acquisitions.[9] The survey was administered on a stratified sample of 4,826 households between April 2012 and January 2013. The sample was drawn from three population groups: SNAP households, low-income households not participating in SNAP, and higher income households. Each household participated in the survey for one seven-day week.

FoodAPS captures detailed information about purchases and acquisitions of food items intended for consumption at home and away from home, including items acquired through USDA's food assistance programs, as well as the amount and source of payment for food. The survey also collects information about household and personal attributes, including demographic

[9] FoodAPS was co-sponsored by ERS and FNS and was conducted in the field by Mathematica Policy Research.

and socioeconomic characteristics, income, receipt of SNAP benefits, confirmation of SNAP receipt through an administrative match, and self-reported WIC receipt along with information to determine WIC eligibility.

Among other data reported, households filled out a 10-item food security questionnaire (referenced to the last 30 days), which is the basis for calculating raw food security scores and assigning households to categories of food security. Using the USDA's 30-day adult food security scale, "food insecure" households are those with the raw score of 3 or more.[10] Those with scores of 0, 1, or 2 are labeled as "food secure."

Through its Geography Component (FoodAPS-GC), the survey provides information about the local food environment, including the location of food retailers, measures of access to these retailers, food prices, and food-related public policies.[11] We employ FoodAPS-GC to construct variables that can be used as MIVs. In particular, we use local food price data to construct measures of the cost of TFP consistent with each household's size and composition. The TFP measures vary with respect to the geographic level of price aggregation: county vs. stores located within 20 miles of the household. We then construct a food expenditure MIV by dividing reported household expenditures on food at home by the TFP cost.[12]

2.2. Analytical Sample

We focus on FoodAPS households that would be eligible to participate in SNAP and WIC concurrently. Given the eligibility restrictions associated with these programs, our analytical sample is comprised of 460 households with income below 130% of the poverty threshold and

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[10] Such households can be further categorized as having "low food security" (score of 3-5) or "very low food security" (6-10).

[11] Many variables pertaining to these public policies come from the SNAP Policy Database (ERS, 2017b). They refer to SNAP policies and design features at the state level, including the magnitude of outreach expenditures, length of recertification periods, exemptions from the household asset test, reporting requirements, and fingerprinting of applicants, among others. The literature often uses these variables as IVs for SNAP participation (e.g., Gregory and Deb, 2015; Ratcliffe, McKernan, and Zhang, 2011; Yen et al., 2008).

[12] Our analysis involves using confidential geographic identifiers and other restricted-access FoodAPS data. We access them through a secure data enclave of the National Opinion Research Center (NORC). For details on publicly available FoodAPS data, see ERS (2017a).

containing a pregnant woman, or a child aged less than five years. All sample statistics and estimates incorporate FoodAPS household weights.

Table 1 provides the joint distribution of our analytical sample by self-reported, current household participation in SNAP and WIC.[13] The table shows that joint participation in the two programs is empirically relevant: 36.7% of the households report being on both SNAP and WIC. Also, 16.6% are reportedly on WIC but not SNAP, and 31.4% are on SNAP but not WIC. The remaining 15.3% indicate no participation in either program.

To ascertain SNAP participation, Mathematica Policy Research matched FoodAPS households to SNAP administrative records. Not all households could be matched in practice, however. Among the households in our sample, 57.6% are matched and confirmed SNAP participants, 2.6% are matched and confirmed nonparticipants, 37.5% could not be matched (most likely due to their genuine absence from the administrative records), and 2.3% withheld consent to be matched. Regrettably, FoodAPS contains no validation data on WIC participation. Thus, while we treat all sample households as "verified" with respect to SNAP participation status, no household is verified regarding WIC participation.

Table 2 is similar to Table 1, except that the SNAP participation indicator now incorporates administrative data in FoodAPS. In particular, for households that got matched to administrative records, SNAP status comes from the administrative record. In all other instances, SNAP participation is self-reported. Compared to the distribution in Table 1, the incorporation of administrative data about SNAP leads to an increase in the overall prevalence of SNAP participation by 5.2 percentage points. More specifically, the prevalence of households on SNAP but not on WIC increases by 2.2 percentage points (from 31.4% to 33.6%), while the prevalence of households on both SNAP and WIC rises by 3 percentage points (from 36.7% to 39.7%).

Table 3 provides the prevalence of food security in each of four subsamples defined according to self-reported household participation in SNAP and WIC. The rate of food security

[13] In FoodAPS, questions about SNAP and WIC refer to current participation.

exceeds 50% throughout, but somewhat varies across the subsamples. (The rate of food security in the analytical sample overall is 55.0%.) Given no WIC receipt, self-reported SNAP participation is associated with a decrease in the prevalence of food security from 53.2% to 52.2%, which is in line with a negative association between food security and SNAP found in the literature (see Gundersen et al., 2011). When WIC is in place, however, SNAP is associated with an increase in the food security rate from 54.5% to 58.5%. Perhaps the process of selecting into SNAP differs depending on whether the household participates in WIC, or perhaps there are synergies between the two programs in promoting food security. Also, the table shows that WIC participation is always associated with more food security given a self-reported SNAP status.

Table 4, which replaces the self-reported SNAP participation indicator with the administratively matched one, likewise shows food security rates in excess of 50% and varying a bit across the four subsamples. With one exception, the table indicates similar associations between the participation indicators and food security to those implied by Table 3. The only exception is that given no SNAP receipt, self-reported WIC participation is now associated with less (rather than more) food security.

Table 5 provides descriptive statistics for selected characteristics of the sample. On average, the households contain 4.5 members (of all ages), 2.3 children (aged < 18 years), and 1.6 young children (aged 0–6 years). Average monthly household income is about $1,600, income-to-poverty ratio is 0.75, and weekly expenditures on food at home are about $113. Twenty-one percent of the households live in rural areas, 78% rent their residence, 26% do not own or lease a vehicle, and 11% have used a food pantry in the past 30 days. Primary respondents in these households are predominantly female (88%) and just under 34 years old on average. Thirty-three percent are Hispanic, 55% are White, 29% are Black, 32% have no high school degree, 32% have a high school degree or GED, 28% have some college education but no bachelor’s degree, and 7% have a bachelor’s degree. Also, 44% are single, 29% are married, 25% are divorced or separated, and 2% are widowed; 43% are employed, 17% are looking for work, and 40% are not working.

3. Methodology and results

3.1. General Framework

Applying the methods developed in JKZ, let the outcome $Y = 1$ indicate that a household is food secure, with $Y = 0$ otherwise. Let S^* be an unobserved indicator of true program participation where $S^* = 0$ denotes no participation in SNAP or WIC, $S^* = 1$ denotes participation in SNAP alone, $S^* = 2$ denotes participation in WIC alone, and $S^* = 3$ denotes participation in both SNAP and WIC. This treatment variable is partially ordered: $S^* = 1$ or 2 denotes some participation in food assistance programs, while $S^* = 0$ does not, and $S^* = 3$ involves more participation. (Since $S^* = 1$ and $S^* = 2$ represent different programs, these two treatments are not ordered.)

Instead of observing S^* in the data, we observe a self-reported counterpart, S. We also observe FoodAPS administrative information on SNAP. Let $V_{SNAP} = 1$ denote verification that a household truly received SNAP (weighted 57.6% of the analytical sample, using the FoodAPS variable "*snapnowadmin*"), implying that $S^* = 1$ or 3, with $V_{SNAP} = 0$ (42.4% of the sample) conversely implying that $S^* = 0$ or 2.[14]

Using a potential outcomes framework, we focus on ATEs associated with participating in both food assistance programs versus a single program, or compared with no participation:

$$ATE_{jk} = P[Y(S^* = j) = 1 \mid X] - P[Y(S^* = k) = 1 \mid X] \text{ for } j,k \in \{0,1,2,3\},\ j \neq k \qquad (1)$$

where $Y(S^*)$ indicates the (latent) potential food security outcome under treatment S^*, X denotes any covariates of interest, and P denotes the probability of an outcome. Because there are no

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[14] Households with $V_{SNAP} = 0$ include verified SNAP non-participants (weighted 2.6% of the analytical sample), households that could not be matched to existing administrative records (26.8%; most likely due to true nonparticipation), households that could not be matched because states provided no or insufficient administrative data (10.7%), and households that withheld consent for the administrative match (2.3%). For simplicity, we treat all of these households as true nonparticipants.

regression orthogonality conditions to be satisfied in this framework, there is no need to include covariates as a means of avoiding omitted variable bias. Instead, covariates serve only to condition on subpopulations of interest.[15] To simplify notation, we suppress the conditioning on X and write $P[Y(S^* = j) = 1]$ more compactly as $P[Y(j) = 1]$.

In this application, we are interested in the case of $j = 3$ vs. $k = 1$. Specifically, $ATE_{31} = P[Y(3) = 1] - P[Y(1) = 1]$ measures how the prevalence of food security would change if all eligible households participated in both SNAP and WIC rather than in SNAP alone.[16] One cannot identify ATE_{31} without additional assumptions, even if S is accurately reported, because the potential outcome $Y(S^* = 3)$ is observed only for households that chose to participate in both SNAP and WIC, while $Y(S^* = 1)$ is observed only for households that chose to participate in SNAP alone. The decomposition $P[Y(3) = 1] = P[Y(3) = 1 \mid S^* = 3]P(S^* = 3)$ $+P[Y(3) = 1 \mid S^* \neq 3]P(S^* \neq 3)$ highlights the selection problem: the term $P[Y(3) = 1 \mid S^* \neq 3]$ represents an unobserved counterfactual outcome, namely, the likelihood of food security when participating in SNAP and WIC jointly among households that actually chose not to be on both programs.

As a further identification problem, households are thought to systematically underreport program participation in national surveys, and such misreporting may be related to personal characteristics (including the food security outcome itself). Allowing S to deviate from S^*, let $\theta_i^{j,k} \equiv P(Y = i, S = j, S^* = k)$ for $j,k = \{0,1,2,3\}$ denote the fraction of households with food security status i reporting participation status j when true participation status is k. Using the law of total probability, the first term in ATE_{31} becomes $P[Y(3) = 1] = P(Y = 1, S = 3) + \theta_1^{-3,3} - \theta_1^{3,-3}$

$$+P[Y(3) = 1 \mid S^* \neq 3]\left\{P(S \neq 3) + \sum_{j \neq 3}(\theta_1^{-j,j} + \theta_0^{-j,j} - \theta_1^{j,-j} - \theta_0^{j,-j})\right\}, \text{ where}$$

[15] A tradeoff is that we cannot point-identify the ATE.

[16] Note that we are not restricting a treatment effect to be the same across households. As emphasized by Moffitt (2005), the classical linear response model assumption, for example, is difficult to justify in the case of government assistance programs that are thought to have heterogeneous effects.

$\theta_i^{-j,k} \equiv P(Y=i, S \neq j, S^* = k)$ and $\theta_i^{j,-k} \equiv P(Y=i, S=j, S^* \neq k)$. An analogous expression can be derived for $P[Y(1)=1]$.

Without further assumptions, propositions in JKZ can be used to show that the marginal impact on food security associated with participating in both SNAP and WIC, compared with participating in SNAP alone, is bounded as follows:

$$\begin{aligned} -1 + P(Y=1, S=3) + P(Y=0, S=1) + \Theta_{3,1}^{LB} \\ \leq ATE_{3,1} \leq \\ 1 - P(Y=0, S=3) - P(Y=1, S=1) + \Theta_{3,1}^{UB} \end{aligned} \tag{2}$$

where $\Theta_{3,1}^{LB} \equiv \theta_1^{-3,3} - \theta_1^{3,-3} + \theta_0^{-1,1} - \theta_0^{1,-1}$ and $\Theta_{3,1}^{UB} \equiv -\theta_0^{-3,3} + \theta_0^{3,-3} - \theta_1^{-1,1} + \theta_1^{1,-1}$ could each be positive or negative. Terms like $P(Y=1, S=3)$ are observed from the data, but the $\{\theta\}$ components are unobserved. Thus, the ATE bounds in Equation (2) are not yet operational.

In our FoodAPS sample, we have $P(Y=1, S=3) = 0.238$, $P(Y=0, S=1) = 0.159$, $P(Y=0, S=3) = 0.165$, and $P(Y=1, S=1) = 0.172$. Thus, in our application, the bounds in Equation (2) become

$$-0.603 + \Theta_{3,1}^{LB} \quad \leq ATE_{3,1} \leq \quad 0.663 + \Theta_{3,1}^{UB}.$$

If participation in SNAP and WIC were accurately measured, then setting $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$ equal to zero would reduce the bounds in Equation (2) to Manski's (1995) classic worst-case ATE bounds: $[-0.603, 0.663]$.[17] While obviously very wide, it is instructive to recognize that the data alone (if accurately measured) constrain the possible range of $ATE_{3,1}$ to improve on the range [-1,1]. Since $P(Y=1, S=3)$ in Equation (2) is greater than zero, we know that not all households participating in both SNAP and WIC are food insecure. Also, since $P(Y=0, S=1)$ is greater than zero, we know that participating in both programs does not cause all households to *become* food

[17] With a binary treatment, the Manski bounds would have a width equal to 1 (and always include 0). In the present context with multiple treatments, the Manski bounds have a width greater than 1.

secure; some were already food secure while participating in SNAP alone. These positive probabilities raise the lower bound above -1. Similar logic ensures that the upper bound is less than 1 (again, when SNAP and WIC participation are accurately measured).

In the context of food assistance programs, however, participation is thought to be underreported. Still, the error rates $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$ are logically bounded. For example, $\theta_1^{-1,1}$ cannot exceed $P(Y=1, S \neq 1) = 0.378$, another quantity directly observed in the data. Without knowledge about the nature and degree of reporting errors, however, nothing prevents the worst case bounds in Equation (2) from expanding to $[-1, 1]$, in which case they are completely uninformative. For the upper bound, for example, $\theta_0^{3,-3}$ could be as large as $P(Y=0, S=3)$ $= 0.165$, while $\theta_1^{1,-1}$ could be as large as $P(Y=1, S=1) = 0.172$. Since $\theta_0^{-3,3}$ and $\theta_1^{-1,1}$ could both be 0, the upper bound in Equation (2) attains 1. Analogously, the lower bound attains -1.

3.2. Partial Validation Data in FoodAPS

Partial validation data in FoodAPS allow us to place informative restrictions on the magnitudes of $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$. Knowledge of whether or not a household truly participates in SNAP is not enough to pinpoint the value of S^*, which represents the true *joint* participation status. In particular, confirmation of participation in SNAP merely identifies that $S^* \in \{1,3\}$; that is, the household might be participating in SNAP alone or in both SNAP and WIC. Similarly, confirmation of nonparticipation in SNAP merely identifies that $S^* \in \{0,2\}$; the household may have been participating in neither program or in WIC alone.

Still, confirmation of SNAP participation status – and modifying the observed treatment indicator S accordingly to align with known values – allows us to eliminate many of the error components of $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$. Specifically, $\Theta_{3,1}^{LB} \equiv (\theta_1^{0,3} + \theta_1^{1,3} + \theta_1^{2,3}) - (\theta_1^{3,0} + \theta_1^{3,1} + \theta_1^{3,2})$ $+(\theta_0^{0,1} + \theta_0^{2,1} + \theta_0^{3,1}) - (\theta_0^{1,0} + \theta_0^{1,2} + \theta_0^{1,3})$ reduces to $\Theta_{3,1}^{LB} = \theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3}$ because $\theta_1^{0,3} = \theta_1^{2,3} = \theta_1^{3,0} = \theta_1^{3,2} = \theta_0^{0,1} = \theta_0^{2,1} = \theta_0^{1,0} = \theta_0^{1,2} = 0$. These eight (out of the 12) error components vanish using the FoodAPS validation information. For example, $\theta_1^{0,3} \equiv P(Y=1, S=0, S^*=3) = 0$

since SNAP validation rules out cases in which a household ends up falsely classified as participating in neither program since we have documentation that the household participated at least in SNAP. Similarly, $\Theta_{3,1}^{UB} \equiv -(\theta_0^{0,3} + \theta_0^{1,3} + \theta_0^{2,3}) + (\theta_0^{3,0} + \theta_0^{3,1} + \theta_0^{3,2}) - (\theta_1^{0,1} + \theta_1^{2,1} + \theta_1^{3,1})$ $+ (\theta_1^{1,0} + \theta_1^{1,2} + \theta_1^{1,3})$ reduces to $\Theta_{3,1}^{UB} = -\theta_0^{1,3} + \theta_0^{3,1} - \theta_1^{3,1} + \theta_1^{1,3}$ since $\theta_0^{0,3} = \theta_0^{2,3} = \theta_0^{3,0} = \theta_0^{3,2} = \theta_1^{0,1} = \theta_1^{2,1}$ $= \theta_1^{1,0} = \theta_1^{1,2} = 0.$ [18]

Using the FoodAPS validation data, the average treatment effect bounds in Equation (2) are thus narrowed as follows:

$$
\begin{aligned}
-1 + P(Y=1, S=3) + P(Y=0, S=1) + \theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3} \\
\le ATE_{3,1} \le \\
1 - P(Y=0, S=3) - P(Y=1, S=1) + \theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3}.
\end{aligned}
\tag{3}
$$

Note that the error components $\theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3}$ shift the lower and upper bound by the same unknown constant. In our application,

$$
-0.603 + \theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3} \quad \le ATE_{3,1} \le \quad 0.663 + \theta_1^{1,3} - \theta_1^{3,1} + \theta_0^{3,1} - \theta_0^{1,3}.
$$

Despite eliminating eight of the 12 error components in $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$, the bounds in Equation (3) are still uninformative: the ATE may still lie anywhere between -1 and 1. To see this, it is instructive to understand why the bounds in Equation (3) are informative in the absence of measurement error. In that case, the lower bound is elevated above -1 because some fraction of households $P(Y=1, S=3) = 0.238$ are known to be food secure while participating in both SNAP and WIC, while another fraction $P(Y=0, S=1) = 0.159$ are known to be food insecure while participating in SNAP alone. The presence of these groups reveals that, at least sometimes, participation in both programs is not harmful relative to participation in SNAP alone. Similarly,

[18] Although we formally treat administrative data in FoodAPS as the gold standard for SNAP participation, we recognize that these data may contain some errors themselves (e.g., if the household matching algorithm has imperfections).

the upper bound cannot attain 1 when some fraction of households $P(Y=0,S=3)=0.165$ are known to be food insecure despite participating in both programs, and some from fraction $P(Y=1,S=1)=0.172$ are food secure despite participating only in SNAP. Thus, at least sometimes, participation in both programs is not beneficial compared with participation in SNAP alone.

In the presence of classification error, the difficulty is that $\theta_1^{3,1}=P(Y=1,S=3,S^*=1)$ in the lower bound could be as large as $P(Y=1,S=3)=0.238$ while $\theta_0^{1,3}=P(Y=0,S=1,S^*=3)$ could be as large as $P(Y=0,S=1)=0.159$. Without further assumptions to constrain the patterns or degrees of misclassification, logically we cannot rule out the possibility that food secure households claiming to participate in both programs were actually participating only in SNAP. Nor can we rule out the possibility that food insecure households claiming to participate in SNAP alone were actually participating in both programs. Setting the other error components to zero as a worst case, the lower bound falls to -1, becoming uninformative. Similarly, the upper bound rises to 1. While these scenarios are extreme, they help crystalize how data must be combined with assumptions before we can make logical, informative inferences.

FoodAPS currently does not contain information that can be used to validate WIC participation status.[19] Thus, we cannot further constrain the four remaining error components in $\Theta_{3,1}^{LB}$ and $\Theta_{3,1}^{UB}$ using data alone. To learn anything about $ATE_{3,1}$, we need to impose assumptions about the magnitudes or patterns of reporting errors. We aim to strike a balance between making assumptions that are weak enough to be credible but strong enough to be informative.

3.3. No False Positives

We can make further progress in bounding $ATE_{3,1}$ by imposing a common "no false positives" assumption in the food assistance literature (e.g., Almada et al., 2016; Kreider, Pepper,

[19] For future research, there is information reported on food expenditures funded through WIC vouchers at purchase events that might be used to partially validate participation. Of concern is the potential for lags in the timing of using WIC benefits after no longer being certified as a program participant.

Gundersen, and Joliffe, 2012, referenced below as KPGJ) that households do not falsely report benefits they do not actually receive. Validation data from previous studies find only rare instances of these errors of commission (e.g., Bollinger and David, 1997; Marquis and Moore, 1990). In our FoodAPS sample, for example, only 1.8% of those reporting SNAP benefits were found not to be receiving them.

Under the no false positives assumption, the ATE bounds in Equation (3) shrink further and become informative:

$$\begin{gathered} -1+P(Y=1,S=3)+P(Y=0,S=1)+\theta_1^{1,3}-\theta_0^{1,3} \\ \leq ATE_{3,1} \leq \\ 1-P(Y=0,S=3)-P(Y=1,S=1)+\theta_1^{1,3}-\theta_0^{1,3}. \end{gathered} \tag{4}$$

Note that the error components $\theta_1^{1,3}-\theta_0^{1,3}$ shift the lower and upper bound by the same unknown constant. Taking worst cases across $\theta_1^{1,3}$ and $\theta_0^{1,3}$ in Equation (4), JKZ show that $ATE_{3,1}$ is sharply bounded as

$$-1+P(Y=1,S=3) \leq ATE_{3,1}^{WC} \leq 1-P(Y=0,S=3). \tag{5}$$

These worst-case bounds are presented in Panel A (*no additional assumptions*) of Table 6. The bounds are very wide, with the width of $2-P(Y=0,S=3)-P(Y=1,S=3)$. In our sample, $ATE_{3,1}^{WC}$ may lie anywhere in the range $[-0.762, 0.835]$ with a width of 1.60. We have made important progress, however, in moving away from the [-1, 1] uninformative bounds. Specifically, a fraction of households $P(Y=1,S=3)=0.238$ are food secure while claiming to participate in both programs, thus raising the lower bound away from -1. We trust their participation responses under the no false positives assumption. Similarly, a fraction of households $P(Y=0,S=3)=0.165$ are food insecure despite participating in both programs, lowering the upper bound away from 1.

To gain an understanding of how misreporting affects uncertainty about $ATE_{3,1}^{WC}$ beyond uncertainty due to unknown counterfactuals, we trace out the bounds in Equation (4) as a function of $P(S^* = 3 \mid Y = 0, S = 1)$ and $P(S^* = 3 \mid Y = 1, S = 1)$ in Figure 1. The figure utilizes a heat map. The blue surface depicts the lower bound on $ATE_{3,1}^{WC}$, while the yellow surface depicts the upper bound. The planes are parallel in this case given the structure of the worst-case bounds in Equation (4). The small red circles identify the bounds in the reference case of no classification error, with the dashed vertical red line spanning the range of these bounds. The worst-case lower bound in Equation (5) is attained at the bottom right corner of the blue surface, where $P(S^* = 3 \mid Y = 0, S = 1) = 1$ and $P(S^* = 3 \mid Y = 1, S = 1) = 0$. The worst-case upper bound is attained at the top left corner of the yellow surface, where $P(S^* = 3 \mid Y = 0, S = 1) = 0$ and $P(S^* = 3 \mid Y = 1, S = 1) = 1$. We will return to this figure when discussing an additional nondifferential errors assumption and its associated 45° solid blue lines.

One way to narrow the bounds in Panel A is to further restrict the nature of classification errors. Suppose, for example, that misreporting of SNAP or WIC participation arises independently of the household's food security status. This *nondifferential errors assumption* specifies that

$$P(S^* = j \mid S = k, Y = 1) = P(S^* = j \mid S = k, Y = 0). \tag{6}$$

Evidence from FoodAPS suggests that food secure and food insecure households are about equally likely to misreport the receipt of food assistance.[20] In this case, we can write $\theta_0^{1,3} = \kappa\theta_1^{1,3}$ in Equation (4), where $\kappa \equiv P(Y = 0, S = 1) / P(Y = 1, S = 1)$ is observed in the data:

$$\begin{aligned} -1 + P(Y = 1, S = 3) + P(Y = 0, S = 1) + (1-\kappa)\theta_1^{1,3} \\ \leq ATE_{3,1} \leq \\ 1 - P(Y = 0, S = 3) - P(Y = 1, S = 1) + (1-\kappa)\theta_1^{1,3}. \end{aligned} \tag{7}$$

[20] The chance of being found to participate in SNAP when claiming otherwise is about 49% among food secure households and 44% among food insecure households. The fractions are also similar to each other for the rare cases of reporting SNAP benefits not actually received.

This assumption has substantial identifying power in our application, especially when κ is close to 1. In fact, when $\kappa = 1$, which implies that half of the households reporting participation in SNAP alone are food secure, with the other half being food insecure, classification error ceases to be an issue. In that case, the worst-case bounds in Equation (7) reduce to Manski's (1995) classic worst-case bounds in Section 3.1. Otherwise, the bounds under the nondifferential errors assumption reduce to the bounds shown in Panel B of Table 6:

$$\begin{aligned} -1+P(Y=1,S=3)+\min\left\{P(Y=0,S=1),P(Y=1,S=1)\right\} \\ \leq ATE_{3,1}^{ND} \leq \\ 1-P(Y=0,S=3)-\min\left\{P(Y=0,S=1),P(Y=1,S=1)\right\}. \end{aligned} \tag{7'}$$

Notice the similarity between these bounds and Manski's worst-case bounds in Equation (2) under no measurement error ($\Theta_{3,1}^{LB} = \Theta_{3,1}^{UB} = 0$). When $\kappa = 1$, the bounds are identical: SNAP verification combined with no false positives and nondifferential errors is equivalent to no measurement error at all. When $\kappa > 1$ such that more than half of the households reporting participation in SNAP alone are food insecure, the Table 6 Panel B upper bound is identical to Manski's no-errors upper bound. When $\kappa < 1$ such that more than half of the households reporting participation in SNAP alone are food secure, the Panel B lower bound is identical to Manski's no-errors lower bound.

In our sample, $\kappa = 0.92 < 1$ which implies $P(Y=0,S=1) < P(Y=1,S=1)$. Thus, the Panel B bounds become

$$\begin{aligned} -1+P(Y=1,S=3)+P(Y=0,S=1) \\ \leq ATE_{3,1} \leq \\ 1-P(Y=0,S=3)-P(Y=0,S=1). \end{aligned} \tag{7''}$$

The worst-case bounds narrow from $[-0.762, 0.835]$ to $[-0.603, 0.676]$ with a width of 1.28, a 32 percentage point reduction in the width. The lower bound of -0.603 is identical to Manski's lower bound (which presumes accurate reporting). Returning to Figure 1, the worst-case bounds shrink as they become restricted to lie on the solid blue lines. These 45° lines impose the Equation (6) constraint that the true fraction of households participating in both SNAP and WIC among those reporting participation in SNAP alone, $P(S^* = 3 \mid S^* = 1)$, does not vary by food security status, Y. In particular, the lowest feasible value of $ATE_{3,1}$ is no longer at the bottom right corner of the blue plane, and the highest feasible value is no longer at the top left corner of the yellow plane.

As a polar case, Panel C of Table 6 and Figure 2A highlight the identifying power of an *exogenous selection* assumption that, on average, potential outcomes do not depend on the realized treatment:

$$P[Y(j)=1] = P[Y(j)=1 \mid S^* = k] \quad \forall j,k. \tag{8}$$

This assumption would make sense if households were randomly assigned to food assistance programs such that there were no systematic differences in household attributes across treatment groups. In this case, the lower and upper bound planes coincide. In the absence of classification error, the bounds collapse to a point, 0.0384, depicted by the small red circle. The closed-form bounds across all values of the error components are provided in Panel C of Table 6. In our application, the worst-case bounds narrow from $[-0.762, 0.835]$ to $[-0.576, 0.713]$ under exogenous selection. Even though the exogeneity assumption eliminates uncertainty associated with unknown counterfactuals, the bounds remain very wide due to potential measurement error in WIC participation status.

Now consider *exogenous selection with nondifferential errors* (Panel D of Table 6). Figure 2B shows how the exogenous selection bounds in our application become further constrained under the additional nondifferential errors assumption reflected by the 45° solid blue line. As above, this constraint imposes the restriction that $P(S^* = 3 \mid S^* = 1)$ does not vary with food security status, Y. The added horizontal zero plane helps to see that the ATE is strictly positive across all values of the error components. Using the closed-form bounds in Panel D of Table 6, the average treatment effect is isolated to lie in the narrow range $[0.0384, 0.0701]$ with width 0.0316, nearly point-identifying the parameter. This upper bound is computed as $P(Y=1|S=3)-P(Y=1|S=1)$ = 0.0701, the observed difference in food security rates between households reporting participation in both programs vs. those reporting participation in SNAP alone.[21]

3.4. Monotonicity Assumptions

Because households choose to participate in food programs on their own accord, exogenous selection, while an important reference case, is an untenable assumption in our setting. Therefore, we do not impose this independence assumption in the remainder of the analysis. Instead, we study how the Table 6 Panel A and B worst-case bounds can be narrowed under relatively weak monotonicity restrictions such as Monotone Treatment Selection (Manski and Pepper, 2000; KPGJ) and Monotone Treatment Response (Manski, 1997; KPGJ). The MTS assumption formalizes the notion that unobserved factors related to food insecurity are likely to be positively associated with the decision to take up food assistance. Under the MTR assumption, participating in SNAP and WIC would not harm food security, on average, conditional on treatment.

Formally, MTS in our partially-ordered treatment framework is specified as:

$$P[Y(j)=1|S^*=3] \le P[Y(j)=1|S^*=k] \le P[Y(j)=1|S^*=0] \quad \forall\, j \text{ and } k=1,2. \qquad (9)$$

For each potential treatment *j*, we posit that the latent food security probability is (weakly) less favorable among households that enrolled in both programs ($S^*=3$) compared with only one program ($S^*=1$ or 2), and similarly less favorable among households that enrolled in one program compared with no program ($S^*=0$). We impose no ordering between households that

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[21] JKZ show that either the lower or upper bound is given by this difference in conditional means depending on whether κ is less than or greater than 1.

enroll in only one program versus the other. The MTS assumption does not imply that any households would be better off changing their participation status—only that those who chose to participate in more programs start out relatively disadvantaged, on average, under any potential treatment. Returning to Table 6, the MTS lower bound is given by (see Panel E)

$$-1+\frac{P(Y=1,S=3)}{P(S=3)+P(Y=0,S=1)}\leq ATE_{3,1}^{MTS}$$

with the upper bound unchanged compared with the worst-case upper bound provided in Panel A. Using the FoodAPS data, the worst-case bounds shrink from $[-0.762, 0.835]$ to $[-0.576, 0.835]$. The lower bound is improved further by combining MTS with the nondifferential errors assumption as shown in Panel F:

$$-1+\max\left\{P(Y=1\,|\,S=3),\frac{P(Y=1,S=3)+P(Y=1,S=1)}{P(Y=3)+P(Y=1)}\right\}+P(Y=0\,|\,S=1)\big[P(S=3)+P(S=1)\big]$$

$$\leq ATE_{3,1}^{MTS}.$$

In our application, the improvement is dramatic. In particular, the Panel E bounds narrow from $[-0.576, 0.835]$ to $[-0.058, 0.676]$ in Panel F. The lower bound is improved 70 percentage points compared with the Frame B worst-case lower bound, and it is improved 54 percentage points compared with Manski's no-errors worst-case lower bound. Figures 3A and 3B reveal how the MTS bounds and the MTS bounds with nondifferential errors, respectively, vary with the values of $P(S^*=3\,|\,Y=0,S=1)$ and $P(S^*=3\,|\,Y=1,S=1)$.

To formally specify the MTR assumption, we slightly modify Manski's (1995; 1997) original approach. For a given realized program participation status, we suppose that potential participation in SNAP alone or WIC alone would not harm a household's food security on average compared with no participation, nor would participation in both programs be detrimental on average compared with participation in either program alone:

$$
\begin{aligned}
&P[Y(3)=1 \mid S^{*}] \geq P[Y(1)=1 \mid S^{*}] \geq P[Y(0)=1 \mid S^{*}] \\
&P[Y(3)=1 \mid S^{*}] \geq P[Y(2)=1 \mid S^{*}] \geq P[Y(0)=1 \mid S^{*}]. \qquad (10)
\end{aligned}
$$

In isolation, this assumption is uninformative, since it precludes strictly negative effects by construction. It can have useful identifying power, however, when combined with the instrumental variable assumptions described next. In particular, it assures that the effect is nonnegative across all values of the instrument. It is difficult to imagine that participation in more food assistance programs would itself cause more food insecurity, at least on average (Currie, 2003).

We can further narrow the bounds by employing MIVs. Monotone instruments are often easier to motivate than standard IVs because they do not require any orthogonality/exclusion restrictions. In our application, we merely require that the instrument leads to a weakly improved latent food security outcome, on average, conditional on the treatment. As MIVs, we use variables reflective of important aspects of local food environment, as recorded in FoodAPS-GC.[22] In particular, we employ the ratio of actual household expenditures on food at home to food expenditures consistent with the TFP recommendations and local food prices (the expenditure MIV).[23] We also investigate the usefulness of a conventional income-to-poverty MIV based on household income and composition considered in KPGJ. An assumption underlying these monotone instruments is that, broadly speaking, more resources in the household and access to cheaper food cannot harm food security. Unlike a standard IV, there is no exclusion restriction that the monotone instrument can affect food security only through its

[22] Previous studies have shown that the local food environment is an important contributor to food security and health through differences in access, availability, and cost of food (e.g., Rose and Richards, 2004; Ver Ploeg, 2010; Bonanno and Goetz, 2012; Lee, 2012). In particular, the relative cost of food in the area can substantially affect a low-income household's ability to provide an adequate diet to its members. Zhylyevskyy et al. (2013) find that lower relative fruit and vegetable prices positively affect the selection of these foods in a study of African American youths and parents.

[23] We report the results for the MIV based on food prices at food stores located within 20 miles of the household's place of residence.

effect on program participation. The MTS assumption described above is a special case of the MIV assumption in which the treatment S^* itself is the instrument.

Let u represent a monotone instrument. The MIV assumption specifies that higher values of u lead to weakly improved food security outcomes, on average, under each treatment:

$$u_1 \le u \le u_2 \Rightarrow P[Y(j)=1 | v = u_1] \le P[Y(j)=1 | v = u] \le P[Y(j)=1 | v = u_2] \text{ for each } j.$$

While these conditional probabilities are not identified, they can be bounded as described by Manski and Pepper (2000). Bounds on the unconditional latent probability, $P[Y(j)=1]$, can, in turn, be obtained by applying the law of total probability and calculating a weighted average of the bounds on $P[Y(j)=1 | v = u]$ over different values of u.[24] When combined with MTS or MTR, those restrictions are assumed to apply at each value of the instrument, v.

Table 7 demonstrates the identifying power of combinations of the MTS, MTR, MIV, and nondifferential errors assumptions when SNAP participation status is known (though administrative data) and WIC participation may be underreported. Point estimates (p.e.) of the bounds are provided along with Imbens-Manski (2004) confidence intervals (CI) that cover the true value of the ATE with 95% probability. Strictly positive estimated average treatment effects are highlighted in bold. The key finding is that we can identify the ATE as strictly positive and statistically significant when combining the MTS, MTR, expenditure MIV, and measurement error assumptions. Participating in both SNAP and WIC compared with participating in SNAP alone is estimated to increase the food security rate among low-income households in our sample by at least 24 percentage points. Accounting for sampling variability reflected in the confidence interval, food security would rise by at least 1.9 percentage points (see the bottom right cell in Panel A of Table 7).

The large difference between the point estimate of the lower bound and the confidence interval lower bound reflects our relatively small sample size of 460 households. A larger sample

[24] As noted by Manski and Pepper (2000), the MIV estimator is consistent but biased in finite samples. We employ Kreider and Pepper's (2007) modified MIV estimator that accounts for the finite sample bias using a nonparametric bootstrap correction method.

size would not necessarily shrink the width of the estimated bounds on the average treatment effect since the point estimates of the bounds are consistently estimated. However, these bounds would be more precisely estimated. In our setting, the availability of carefully constructed SNAP validation data outweighs our concern about a relatively small sample size.

4. Conclusion

Low-income households in the United States often receive benefits from more than one food assistance program administered by USDA, which raises the question of whether these programs could have meaningful synergies or might be redundant. We investigate the issue by focusing on two popular programs, SNAP and WIC, and apply a novel nonparametric bounding methodology to handle a multinomial, partially ordered treatment, endogenous household selection into assistance programs, and underreported program participation in a unifying framework. The literature has shown that even small amounts of misreporting in surveys can lead to substantial identification decay of treatment effect parameters of interest (e.g., Millimet, 2011). This paper traces out how the availability of validation data, even for only one of the potential treatments, has the potential to substantially sharpen what can be known about the causal effects of multiple program participation.

We draw on a unique aspect of FoodAPS in that it provides auxiliary administrative data on SNAP participation, which allows us to partially validate the treatment variable. Under endogenous household selection into the programs, Manski's (1995) classical treatment effect bounds are wide and contain zero, which makes it impossible to sign the causal effects. The bounds become even wider in our environment of systematically underreported program participation. However, we are able to substantially narrow the bounds by combining conventional, relatively mild monotonicity assumptions on the selection process and restrictions on the patterns of WIC misclassification. Our objective is to strike a balance between making assumptions that are weak enough to be credible but strong enough to be informative.

The methods showcase what can be learned about treatment effects regarding multiple programs when validation data are available for one program but not the other. Exploiting the administratively validated SNAP data in FoodAPS, our key finding is that we can identify the average treatment effect as strictly positive under relatively weak assumptions on the selection process combined with a food expenditure monotone instrumental variable. Monotone instrumental variables are weaker than standard IVs in that they require no *a priori* exclusion restrictions. Among eligible households, we estimate that participating in both SNAP and WIC compared with participating in SNAP alone would increase the food security rate by at least 24 percentage points. While this estimated lower bound is large and statistically significant, it is not precisely estimated. With our relatively small sample size of 460 households, food security might rise by as little as 1.9 percentage points after accounting for sampling variability reflected in the 95 percent confidence interval.

Overall, our results provide evidence that SNAP and WIC are not redundant. These findings have direct policy relevance in that they inform policymakers about the existence of complementarities between SNAP and WIC, which can help contribute to designing a more efficient food safety net in the United States. The degree of the complementarity remains an open question owing in part to the relatively small sample size in our analysis. Moreover, the SNAP administrative data could themselves contain errors (e.g., if the household matching algorithm has imperfections), and we do not have direct evidence about the nature of WIC classification errors. Validation of participation status for WIC and other assistance programs beyond SNAP would allow for narrower and more reliable bounds on the average treatment effects of interest.

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