Salience, Food Security, and SNAP Receipt

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Abstract

Household food insecurity status in the United States is ascertained by a battery of close-ended questions. We posit that the monthly nature of benefit receipt from the Supplemental Nutrition Assistance Program (SNAP) creates experiences of food hardship, which become salient in the context of SNAP receipt, and in turn exert influence on the response to food security questions. We test this hypothesis by examining answers to a 30-day food security module in relation to when SNAP benefits are received. We find that for SNAP households near the end of or at the beginning of the benefit month, the probability of being classified as food insecure increases by 11 percentage points, over a baseline of 42 percent. We also find that the probability of responding affirmatively to any of the first five items in the module increases during this time. We discuss the importance of these findings for the estimation of food security and its implication on program evaluation. © 2018 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Food insecurity—defined as a lack of access to the kinds and amounts of food necessary for each member of a household to lead an active, healthy lifestyle—is a prominent health concern. In the United States, it is linked to a multitude of adverse health outcomes including anemia, diabetes, greater cognitive problems, higher levels of anxiety, a higher probability of mental health issues, asthma in children, as well as suicidal ideation, depression, and chronic disease in adults (Gregory & Coleman-Jensen, 2017; Gundersen & Ziliak, 2015). The nation's principal safeguard against food insecurity, the Supplemental Nutrition Assistance Program (SNAP), provides low-income households in-kind transfers to boost their food-purchasing power. In 2016, over $66 billion in benefits were distributed to more than 21 million households (Food and Nutrition Service [FNS], 2017). Although the rate of SNAP participation increases with the severity of food insecurity (Anderson et al., 2014), the literature suggests that participation in SNAP reduces instances of food insecurity.[1]

The characterization of households as food secure or insecure rests on their responses to the U.S. Department of Agriculture's (USDA) Core Food Security Module

[1] Among others, see Bitler (2016); Mykerezi and Mills (2010); Nord and Prell (2011); Ratcliffe, McKernan and Zhang (2011); DePolt, Moffitt, and Ribar (2009); and Yen et al. (2008).

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(FSM). In particular, the Core FSM fields 10 questions that each elicit a response to descriptions of progressively more severe states of food hardship. For example, a question that describes a relatively low level of hardship asks about whether the household respondent ever worried about household food stocks, while one that describes a more severe level asks whether a household member skipped eating for a whole day. Although the module has been validated as a reliable measure of latent food insecurity through psychometric studies (e.g., Cafiero et al., 2014; Hamilton et al., 1997; National Research Council, 2006), responses vary across otherwise identical households due to subjectivity of the questions (Gundersen & Ribar, 2011), social desirability bias or stigma (Hamelin, Beaudry, & Habicht, 2002), and the belief that answers may be related to eligibility of food assistance (Gundersen & Kreider, 2008).

We posit that the monthly nature of SNAP benefit receipt creates salient experiences relevant for the evaluation of food security status. Previous studies have found that as the benefit month draws to a close, SNAP beneficiaries spend considerably less on food (e.g., Smith et al., 2016), consume fewer calories (Shapiro, 2005; Wilde & Ranney, 2000), consume lower quality diets (Todd, 2015), and have an increased probability of hospital admission due to hypoglycemia (Seligman et al., 2014). We believe such recently experienced food hardships significantly change the probability of affirming module questions, which prompt respondents to recall experiences. Specifically, we examine responses to individual questions in the core FSM, as well as food security status, under the assumption that the module's prompts should elicit recalls of all food hardships experienced during the SNAP benefit month with equal probability, since the questions are cued to the last 30 days.

We draw on the conceptual framework of behavioral economics in discussing the mechanisms that could drive the aforementioned hypothesis. Prospect theory teaches us that decisionmaking is prone to regular and predictable changes in the calculus of utility based on context, framing, and recent experiences (Kahneman & Tversky, 1979). We pursue this intuition by allowing for a *salience effect* to exert influence on the response to questions in the FSM. Here, we define the salience effect as contextual. That is, how a household responds to the FSM depends upon the length of time since a food hardship was experienced, the recollection of which is tied to SNAP receipt.

Kahneman and Krueger (2006) discuss in general what we are calling the "salience effect" as the difference between experienced utility (i.e., the way households feel about some set of experiences as they occur in real-time) versus remembered utility (i.e., the way households remember or recall their experiences). In brief, this research indicates the recollection of past experiences is a weighted average of the moment-by-moment experiences: unpleasant or challenging experiences may be retrospectively down-weighted by more recent pleasant experiences, just as peak or trough experiences may be up-weighted (Kahneman & Krueger, 2006). While we do not have direct measures of all experienced food hardships as they occur (e.g., worrying about food stocks today), we do know the time since SNAP receipt; to the extent that SNAP receipt is correlated with variation in experienced food hardships, as shown in the literature, recollections of such experiences also should be tied to SNAP receipt.

Our data allow us to explore some of these nuances via the detailed module questions. Given that food hardship measures are elicited in a host of datasets across numerous countries and used in a vast number of applications by researchers, policymakers, and interest groups, it is important to better understand how household response patterns can vary based on when such a module is administered.

We employ several estimation strategies to examine the cyclical nature of responses to the FSM questions. Our identification strategy for all of our specifications relies on the fact that the timing of the FSM questionnaire in our data, relative to when SNAP benefits are received, is randomly distributed across households. We first use nonparametric smoothing regressions to plot out affirmations to module questions over the SNAP benefit month, in addition to indicators of *food insecurity* (i.e., three or more affirmations) and *very low food security* (i.e., six or more affirmations). Here, we find that SNAP participants are particularly likely to respond affirmatively, and thus be categorized as food insecure in the three days before and three days after benefit arrival. We term this seven-day period that centers on the day SNAP benefits arrive as the "salience window."

Next, we turn to parametric methods to point estimate the "salience effect": the impact of survey timing with regard to SNAP receipt. We first use simple logistic regressions for each FSM question, as well as categorizations of food insecurity and very low food security. Here, we find affirmations to less severe food security questions (i.e., the first five questions) increase significantly by 8 to 13 percentage points when SNAP households are surveyed during the salience window. Correspondingly, the categorization of food insecure increases by 11 percentage points (over a predicted probability of about 42 percent), whereas very low food security does not significantly change.

Our final approach allows for household-specific heterogeneity in response to module questions. In simple terms, this specification is a household random-effects logistic regression, which yields a single estimate of the salience effect when answering *any* question.[2] With regard to the behavioral mechanisms stated above, this approach allows for household-specific evaluations of past experiences, which are common to all questions within the household, to be randomly distributed across the SNAP population. For example, this model allows for the degree of subjectivity and the ability to recall experiences to vary in unobserved ways across households.[3] Here, we find the marginal effect of administering the FSM within the salience window significantly increases the probability of reporting any food hardship by 4.6 percentage points, over a baseline of 21 percent.

The policy implications of this finding extend to the measurement of food insecurity in the United States, both in terms of prevalence and, perhaps more importantly, how food security research might be affected by nonclassical mismeasurement. With regard to the former, the primary source in monitoring food insecurity in the United States is the Current Population Survey Food Security Supplement (CPS-FSS), which also fields the 30-day Core FSM. Importantly, the CPS-FSS is fielded over a one-week period in December, which can fluctuate from year to year. For example, in 2007 the FSS was administered December 9 through 15, whereas in 2008 the survey week was December 14 through 20. Even though most states stagger benefit disbursement over multiple days, the majority of benefits are issued toward the beginning of the month (Cotti, Gordanier, & Ozturk, 2016). This could mean that we are currently mismeasuring food insecurity in the United States in any given year due to nonuniform sampling. In light of these findings, we perform back-of-the-envelope calculations to show that mismeasurement of food insecurity could be as much as 0.09 percentage points per year, or about 150,000 households, depending on the calendar year.

[2] This is the conceptual and estimation framework behind the food security module, item response theory (IRT). IRT posits that latent traits (e.g., psychological wellness or mathematical ability) can be assessed through a series of questions pertaining to the trait. Prominent applications of IRT include the graduate record examination (GRE) and the scholastic aptitude test (SAT). We use an explanatory IRT model such as described in De Boeck and Wilson (2004).

[3] As further described below, each module question refers to a situation with regard to affordability or having "enough money." Interpreting this component, which is common to all questions, may be household-specific due to preferences or other constraints. Of course, other unobserved features of the household that are question invariant will also be accounted for in this specification.

The potential mismeasurement of food insecurity status has implications on its usage in policy analysis as either an outcome or as a regressor. In the case of the former, such as understanding how SNAP impacts food insecurity, if mismeasurement is classical (i.e., distributed with mean zero), then estimates will be unbiased but less precisely estimated. Our main results indicate this is not the case. Indeed, in our sample we find the upper bound on the effect of SNAP in decreasing food insecurity to be twice as high (15.5 percent) for those surveyed within the salience window as compared to those who are not (7.4 percent). When estimating the impact of food insecurity on an outcome of interest, statistical problems arise once again. In short, when a binary regressor is exogenously misreported, the measurement error is nonclassical and leads to attenuation bias (Aigner, 1973; Lewbel, 2007). When misreporting is endogenous, the bias can be severe (e.g., Kreider & Pepper, 2007). Again, using our sample, we find an associative effect of food insecurity on self-reported fair/poor health to be roughly 50 percent higher for the group of SNAP households surveyed within the salience window (11.2 percent) as opposed to those surveyed within the interior of the month (7.3 percent).

This paper is organized as follows: we first provide background on the FSM and other behavioral responses to the receipt of SNAP benefits. Following a description of our data, we present summary measures, both nonparametric and parametric, showing the cyclical nature of responses to module questions. We use this exploratory analysis to motivate our methodological approach, which encompasses conditional probability models, as well as an approach using methods from the food security measurement literature (i.e., a random effects logistic regression). We show that our results are robust to alternative definitions of the salience window, spanning from a single day when benefits are received to a nearly two-week period. We then discuss what our results imply for the measurement of food insecurity, as well as its usage in food security research.

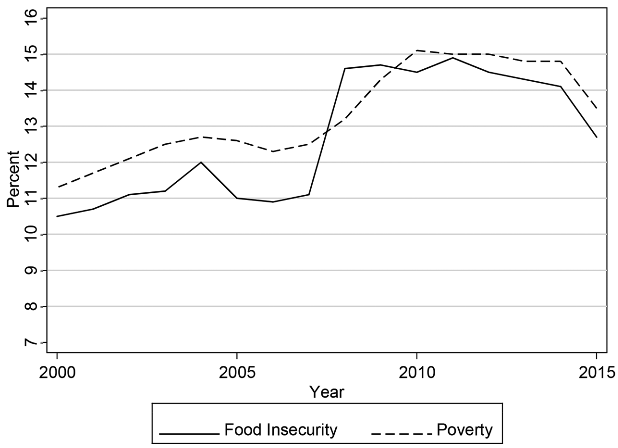
BACKGROUND

The United States Food Security Module

Since 1995, the USDA has measured food insecurity in a questionnaire administered as part of the CPS. The questionnaire, known as FSM,[4] has since also been included in numerous surveys that study aspects of the U.S. population health, labor market, educational attainment and other outcomes. The module has been validated by, among others, the National Academies of Science (National Research Council, 2006).[5]

It is well known that food insecurity, as measured by the FSM, tracks other kinds of economic hardship—for example, poverty. As shown in Figure 1, the official food security level in the United States has moved together with the poverty rate, especially since the Great Recession. Both prevalences remained stubbornly high in 2014, for example, despite an improving labor market, but fell in tandem in 2015. Although the module is understood to measure economic hardship well, responses to the module are inherently subjective: a study by Gundersen and Ribar (2011) found that food spending is not a very good predictor of food insecurity, even at low levels of food spending.

[4] The specifics of module questions are listed in the data section.
[5] See also Cafiero et al. (2014) and Hamilton et al. (1997).



Notes: Food insecurity is calculated from the Current Population Survey, December Supplement using the 12-month questionnaire based on ERS-USDA reports. Poverty is calculated from the Current Population Survey, Annual Social and Economic Supplement (CPS ASEC).

Figure 1. Food Insecurity and Poverty, 2000 to 2015.

The SNAP Benefit Cycle and Behavioral Responses

SNAP is the largest of the USDA's food assistance programs. During 2012, the time in which data collection for our primary data source took place, SNAP paid out $74.6 billion with an average monthly participation of 46.6 million persons. SNAP benefits are distributed once per month on schedules that differ state by state. According to data from the FNS cited in Cotti, Gordanier, and Ozturk (2016), in 2012, nine states distributed SNAP benefits on a single day during the month (usually the 1st): Alaska, Idaho, Nevada, New Hampshire (5th), North Dakota, Rhode Island, South Dakota (10th), Vermont, and Virginia (changed mid-year). A plurality of states distributed benefits over a 10-day period. A handful of states—Georgia, Illinois, Michigan, Missouri, New Mexico, North Carolina, Pennsylvania, South Carolina, and Tennessee—distributed benefits over more than 15 days. The periodicity of the distribution for individual households is what gives rise to the SNAP cycle phenomenon that this study is particularly interested in.[6]

There are several behavioral mechanisms at work with regard to SNAP receipt and household expenditure patterns over the benefit month. The first is time inconsistent preferences, whereby households discount consumption between two far-off days at a much lower rate than they discount tomorrow's consumption (Mastrobuoni & Weinberg, 2009; Shapiro, 2005). Administrative records reveal that more than 80 percent of SNAP benefits are redeemed within the first two weeks of issuance (Castner & Henke, 2011). Correspondingly, research has shown that participants not only purchase less food by month's end, but also consume fewer calories as compared to the beginning of the month (Fraker, 1990; Shapiro, 2005; Wilde & Ranney, 2000). Todd (2015) provides the most recent evidence of declining caloric consumption—recipients in 2007 to 2010 consumed 38 percent fewer calories in the

[6] A separate issue with regard to the cyclical nature of distribution is how markets (particularly, grocery stores) respond to benefits being distributed on a single day. See, for example, Hastings and Washington (2010).

final two days of the month as compared to the rest of the month. While not explicitly focused on SNAP, Seligman et al. (2014) finds that there is a 27 percent increase in hospital admissions due to hypoglycemia among low-income individuals at the end of the benefit month, with no observed increase in higher-income populations.

A second behavioral mechanism relates to the substitutability, or fungibility, of SNAP benefits for other sources of income. Standard economic theory, dating back to Southworth (1945), predicts that infra-marginal SNAP households (i.e., those who receive benefit income less than the food budget) should not treat SNAP differently from non-SNAP income. Richard Thaler referred to the imperfect fungibility of income sources as "mental accounting"—households assign income to specific mental accounts based on its usage (Thaler, 1999). Since SNAP is earmarked for grocery food, the "grocery food account" tends to be overinflated throughout the benefit month (Smith et al., 2016), leading to higher marginal propensities to spend on food out of SNAP rather than from cash income.

We believe a third behavioral mechanism, rooted in prospect theory, is at play for the self-evaluation of food security status: a salience effect. The salience effect refers to the way households' responses to the FSM are dependent upon the length of time since a food hardship was experienced. As mentioned in the introduction, this allows for a distinction between remembered and experienced food hardships. As discussed in Kahneman et al. (1993) and Kahneman and Krueger (2006), a key finding in this literature on subjective measures is that when asked to recall past events humans typically ignore the durations of events (e.g., the number of days one worried about having enough food) and place greater weight on more recent experiences (e.g., if they worried about food yesterday), peak or trough experiences (e.g., more extreme food hardships can stick in one's mind), or both. Our evidence suggests this type of response pattern is present: respondents tend to remember and report more recent food hardships at the expense of more temporally distant ones.

This study contributes to both the "SNAP cycle" and food security measurement literatures by bringing questions about context to bear on the measurement of food insecurity. With a few notable exceptions, there has been little work on behavioral aspects of food security measurement. Moreover, there has been no work that looks at how contextual issues might affect the calculus by which households respond to the food security questionnaire. This study does both of these.

DATA

We use the National Household Food Acquisition and Purchase Survey (FoodAPS), which is a stratified, multistage sample of the civilian population in the United States. The data collection for FoodAPS was sponsored by the USDA, Economic Research Service (ERS), and the FNS as a study of food acquisition behavior of U.S. households. Because of the interest in the food acquisition behavior of participants in federal food assistance programs, FoodAPS contains an oversample of low-income households who do and do not participate in the largest of these programs, the SNAP, as well as a sample of higher-income households who are not eligible for SNAP. In particular, the FoodAPS sampling frame has four target groups: SNAP participant households, non-SNAP participant households with incomes less than 100 percent of the federal poverty line (FPL), non-SNAP households with income between 100 and 185 percent of the FPL, and non-SNAP households with incomes above 185 percent of the FPL.

FoodAPS was conducted over nine months, between April 2012 and January 2013. Data collection revolved around food acquisitions over a seven-day period by all members of the household, and contains added information on topics related to program participation and food demand. Each FoodAPS household completed an initial interview, after which each member documented the places, dates, quantities, and paid amounts for all food acquired, either as food to be eaten at home or away from home. Each household member also recorded when a breakfast, lunch, dinner or three potential snacks (AM, PM, and evening) was consumed on each of the seven survey days. Following the seven days of food acquisition documentation, the household completed a final interview, at which time the 10-item, 30-day FSM was administered.[7]

Food Security Module Questions in FoodAPS

The 10-item FSM administered in FoodAPS has appeared in numerous other federal surveys, such as the Current Population Survey, and has been validated as an instrument (Bickel et al., 2000). We use the binary variables that indicate affirmation of conditions or behaviors as the independent variables of interest in our analysis.[8] The items in the FSM are listed below:

> *Now I'm going to read you several statements that people have made about their food situation. For these statements, please tell me whether the statement was often true, sometimes true, or never true for (you/your household) in the last 30 days.*

1. **Worried**: (I/We) worried whether (my/our) food would run out before (I/we) got money to buy more.
2. **NotLast**: The food that (I/we) bought just didn't last, and (I/we) didn't have money to get more.
3. **Meal**: (I/We) couldn't afford to eat balanced meals.
4. **CutSkip**: In the last 30 days did (you/you or other adults in your household) ever cut the size of your meals or skip meals because there wasn't enough money for food?
5. **CutSkipFreq**: In the last 30 days, how many days did (you/you or other adults in your household) cut or skip meals?
6. **EatLess**: In the last 30 days, did you ever eat less than you felt you should because there wasn't enough money for food?
7. **Hungry**: In the last 30 days, were you ever hungry but didn't eat because there wasn't enough money for food?
8. **LoseWeight**: In the last 30 days, did you lose weight because there wasn't enough money for food?
9. **WholeDay**: In the last 30 days, did (you/you or other adults in your household) ever not eat for a whole day because there wasn't enough money for food?
10. **WholeDayFreq**: In the last 30 days, how many days did (you/you or other adults in your household) not eat for a whole day?

We follow the standard protocol in the food security literature in creating our binary outcomes: an answer is considered affirmative if "often" or "sometimes true" is the response. For the two questions that ask about the number of days in the last 30 days in which a behavior occurred (i.e., questions 5 and 10), affirmatives are counted for any response of three or more days.

[7] Additional covariates in our model include: (from the initial interview) race/ethnicity, highest degree of educational attainment by an adult in the household, age composition of children in the household, marital status of respondent, number of disabled persons in the household, level of SNAP benefits normalized by household size, and its square, as well as (from the final interview) whether the household has at least $2,000 in liquid assets, home ownership, automobile ownership, whether the household has recently had a large expenditure, nonfood expenditures normalized by household size, and its square.
[8] For more on the 10-item food security module, see Coleman-Jensen et al. (2016).

The FSM questions are asked in the order of their (presumed) severity, with the least severe conditions pertaining to the state of household food stocks (questions 1 and 2) and the quality of food (question 3). The remaining questions pertain to more severe conditions that indicate disruptions of food intake. As such, in order to reduce respondent burden in FoodAPS, all households are asked the first three questions. If the response to any of the first three questions is affirmative (i.e., often or sometimes true), then households are screened into the next five questions. If there are any affirmatives among questions 4 through 8, they are asked the last two questions. Questions that households are not screened into are counted as nonaffirmations.[9]

Advantages of FoodAPS

Two distinctive features of FoodAPS make this study possible. The first is an administrative match of consenting households to SNAP participation records.[10] Even a small amount of endogenous misreporting is sufficient to overturn inferences (Gundersen & Kreider, 2008). The administrative match in FoodAPS uses two sources of data. The first, state SNAP participation records, allows us to verify household reports of SNAP participation or nonparticipation by matching administrative records furnished by state SNAP agencies. A second data source is the Anti-fraud Locator using EBT (Electronic Benefit Transfer) Retailer Transactions (ALERT) system data, which track households through the use of EBT cards at food retail/wholesale establishments. For 15 percent of the entire FoodAPS sample, the state did not furnish administrative data, and therefore the ALERT system data are used to verify SNAP participation (Clay et al., 2016). We limit our sample to households whose SNAP participation was administratively verified, although we do show results for households whose professed SNAP participation could not be verified.

The second distinctive feature is respondent accounts of the amount of time (in days) since the household has received SNAP. Of the 1,184 cases in our sample, 1,021 had dates of disbursement from state-level administrative data that matched the household reports. The other 163 had dates that were estimated from the ALERT data based on changes in balances on their SNAP-EBT card. For 77 of these households, the ALERT data agreed with the household reports. For the remaining 86 households in this sample, the ALERT data were relied on exclusively for establishing dates of disbursement.

[9] There are several differences in the way the food security module is administered in the CPS versus in FoodAPS. First, in the CPS, households that have income above 185 percent of the FPL are only screened into the module if they affirm either of the following screener questions:

- "People do different things when they are running out of money for food in order to make their food or their food money go further. In the last 12 months, since December of last year, did you ever run short of money and try to make your food or your food money go further?"
- "Which of these statements best describes the food eaten in your household: enough of the kinds of food we want to eat, enough but not always the kinds of food we want to eat, sometimes not enough to eat, or often not enough to eat?"

In FoodAPS, by contrast, all households are given the first three questions, as mentioned above. A second difference is that the 30-day questions in the December CPS are only administered if someone affirms one of the 12-month questions. Both of these differences, as well as survey context, likely contribute to the differences in measured 30-day food insecurity prevalence: in FoodAPS, it is 16.0 percent, while in the CPS for 2012 (about the time of the FoodAPS survey) it is 8.2 percent. See Clay et al. (2016) for further comparison between FoodAPS and other health and nutrition federal surveys.
[10] All but 122 households in FoodAPS gave consent for the administrative match.

SUMMARY MEASURES

Nonparametric Measures

While the literature on the SNAP benefit cycle focuses on differences between beginning- and end-of-month behaviors in consumption or purchases, it is not clear that responses to the FSN would be affected by the length of time since SNAP receipt in a similar manner: in particular, the prospect of SNAP receipt *in the near future* or experiences in the near past might affect responses to the module in a way that it cannot affect spending. With this in mind, we begin by running nonparametric regressions of the binary responses to each of the 10 food security questions, as well as food insecurity classifications, on the time (in days) since SNAP receipt. These regressions have the benefit of allowing us to look at a meaningful data summary without imposing any distributional form on the models (see Wasserman, 2006).

The nonparametric estimator is a local polynomial regression of order 2. In a neighborhood of a value of days since SNAP receipt (x_0), the estimator $\hat{m}(x_0)$ is equal to the constant term in the local quadratic regression with the following solution:

$$\beta = \left(\mathbf{X}'\mathbf{A}\mathbf{X}\right)^{-1} \left(\mathbf{X}'\mathbf{A}\mathbf{Y}\right). \tag{1}$$

A is the kernel weighting matrix, where the kernel weights in the neighborhood of x_0 are $K(\frac{x_i - x_0}{h})$, where h is the bandwidth and K is the Epanechnikov kernel. Rule-of-thumb bandwidths ranged from 4.08 to 6.03 depending on the survey question. For consistency, we chose a bandwidth of five for all regressions. Confidence bands on the nonparametric estimates are established by using a similarly weighted normalized residual sum of squares at each x_0.[11]

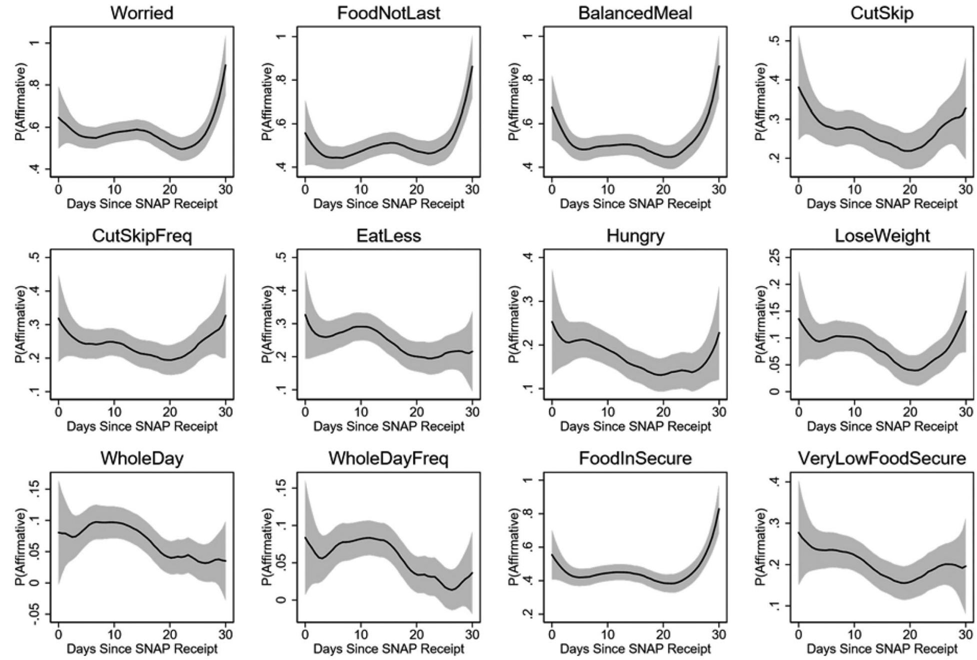
Figure 2 shows the results of our nonparametric regressions. For most of the questions, the predicted probabilities are highest at the beginning and the end of the month, with the exceptions of eating less and the two questions pertaining to not eating for a whole day. Even these latter questions appear to exhibit a decreasing trend over the month. Correspondingly, the classification of food insecure follows a U-shape, while the more severe classification of very low food security tends to decline.

A higher propensity to report food hardships at the end of the month is consistent with the existing "SNAP cycle" literature in the sense that we expect such experiences to occur with higher probability. We reemphasize, however, that the FSM elicits recalls of food hardships over the past 30 days, implying this increase in probability to affirm is a behavioral response in its own capacity. The more striking result is the higher propensity to affirm questions at the beginning of the month. This result could be because recent experiences of hardships at the end of the month are still fresh in mind even after benefits have been received, or due to the need to psychologically justify receipt of SNAP benefits, or perhaps both. With these descriptive results in mind, we define the salience window as being the last three days of the SNAP month (days since SNAP > 27) and the first four days of the beginning of the month, including the date of receipt itself (days since SNAP < 4.)

Samples Means and Identification

Column (1) of Tables 1 and 2 shows the means of our sample covariates and outcomes, respectively. Columns (2) and (3) divide the sample of SNAP participants

[11] We used Stata 14 for all estimates in the study. For more details on this particular regression, see Stata Corporation (2014) and Wasserman (2006, chapter 6).



Notes: Calculated from National Household Food Acquisition and Purchase Survey (FoodAPS). Each local polynomial regression is of degree two and uses the Epanechnikov kernel with a bandwidth of five. Shaded areas represent 95-percent confidence intervals.

Figure 2. Nonparametric Regressions: Effect of Days Since SNAP Disbursement on Food Security Module Questions and Classification.

whose responses are within the salience window as described above and those who are not, respectively, with the difference in column (4). *Prima facie* evidence of randomness in terms of survey timing is shown by the fact that the means for all of the observable covariates in our sample are not statistically different (Table 1). Indeed, the only variables for which the two samples differ are the first four questions of the FSM, as shown in Table 2. These differences show that the group surveyed in the salience window is much more likely to affirm these questions than those surveyed outside the window.

Clearly, an important assumption for our identification strategy is that the number of days since SNAP receipt is random. Table 1 shows that no single observable covariate is significantly different across treatment. In the Appendix, we show nonparametric regressions for each covariate using the same methods for our outcomes.[12] While there is no clear pattern, a small number of observable covariates do vary in some fashion over the month, while the remainder is relatively constant. For example, the proportion of Hispanics increases at the end of the month, while reports of having a large unexpected expenditure peak in the middle of the month. However, among the 21 covariates, we should expect some variation by sheer chance. Thus, to further test our identifying assumption with respect to our parametric estimations below, we ask if observable covariates are *jointly* correlated

[12] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 1. Means of covariates for estimation sample.

Covariates	Full sample	Outside window	In window	Difference
Hispanic	0.230	0.210	0.300	0.090
	(0.06)	(0.05)	(0.11)	(0.27)
Black	0.270	0.270	0.230	−0.050
	(0.05)	(0.04)	(0.07)	(0.33)
White	0.460	0.460	0.430	−0.030
	(0.04)	(0.04)	(0.07)	(0.61)
High school graduate	0.350	0.360	0.320	−0.030
	(0.02)	(0.02)	(0.04)	(0.48)
Some college	0.210	0.220	0.190	−0.030
	(0.02)	(0.02)	(0.03)	(0.45)
College graduate	0.210	0.220	0.170	−0.050
	(0.02)	(0.02)	(0.04)	(0.23)
No. of kids 0–5	0.400	0.390	0.460	0.070
	(0.04)	(0.04)	(0.08)	(0.36)
No. of kids 6–12	0.410	0.410	0.390	−0.020
	(0.03)	(0.03)	(0.05)	(0.72)
No. of kids 13–18	0.220	0.220	0.240	0.030
	(0.02)	(0.02)	(0.05)	(0.64)
Single head	0.460	0.460	0.450	−0.010
	(0.03)	(0.03)	(0.06)	(0.86)
No. of disabled persons	0.340	0.330	0.340	0.010
	(0.03)	(0.03)	(0.07)	(0.90)
>$2000 in liquid assets	0.110	0.110	0.070	−0.040
	(0.02)	(0.02)	(0.03)	(0.18)
Recent large expenditure	0.150	0.150	0.120	−0.030
	(0.02)	(0.02)	(0.03)	(0.36)
Own an auto	0.660	0.670	0.590	−0.080
	(0.03)	(0.03)	(0.06)	(0.18)
Normed[a] SNAP benefits	1.550	1.570	1.420	−0.150
	(0.04)	(0.04)	(0.09)	(0.10)
Normed[a] SNAP benefits^2	3.170	3.260	2.740	−0.520
	(0.20)	(0.21)	(0.29)	(0.11)
Normed[a] nonfood exp	0.470	0.470	0.470	0.000
	(0.02)	(0.03)	(0.04)	(0.94)
Normed[a] nonfood exp^2	0.500	0.540	0.310	−0.230
	(0.17)	(0.20)	(0.06)	(0.28)
N	1,184	976	208	

Notes: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. "In window" refers to the salience window: those households who completed the food security questionnaire either three days before or after SNAP benefits were received, including the day of receipt. All other households are "outside window."
[a]Nonfood expenditures and SNAP benefits are normed by the square root of household size. Standard errors are in parentheses in columns 1 through 3; p-value of test of the difference between the in-window and out-of-window samples is in parentheses in column 4.

with survey timing. To do so, we regressed an indicator for the salience window on all observables and conducted a joint test of significance: the F-stat is unsurprisingly small (0.86) with a p-value of 0.617.

METHODS

Since we have good reason to believe survey timing is random with respect to SNAP receipt, but do acknowledge some idiosyncrasies over the month in terms

Table 2. Means of outcomes for estimation sample.

Outcome	Full sample	Outside window	In window	Difference
Worried	0.570	0.540	0.700	0.150***
	(0.02)	(0.03)	(0.04)	(0.00)
FoodNotLast	0.490	0.470	0.600	0.130***
	(0.03)	(0.03)	(0.05)	(0.00)
BalancedMeal	0.510	0.480	0.630	0.150***
	(0.03)	(0.02)	(0.05)	(0.00)
CutSkip	0.270	0.250	0.340	−0.080*
	(0.02)	(0.03)	(0.04)	(0.07)
CutSkipFreq	0.240	0.220	0.290	0.070
	(0.02)	(0.02)	(0.04)	(0.16)
EatLess	0.250	0.250	0.240	0.000
	(0.02)	(0.02)	(0.03)	(0.95)
Hungry	0.170	0.170	0.200	0.040
	(0.01)	(0.01)	(0.04)	(0.38)
LoseWeight	0.080	0.080	0.090	0.010
	(0.01)	(0.01)	(0.03)	(0.84)
WholeDay	0.070	0.070	0.070	0.000
	(0.01)	(0.01)	(0.04)	(0.92)
WholeDayFreq	0.060	0.060	0.060	0.010
	(0.01)	(0.01)	(0.04)	(0.89)
Food insecure	0.450	0.420	0.570	0.150***
	(0.03)	(0.02)	(0.05)	(0.00)
Very low food security	0.200	0.200	0.220	0.030
	(0.02)	(0.02)	(0.03)	(0.59)
N	1,184	976	208	

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. "In window" refers to the salience window: those households who completed the food security questionnaire either three days before or after SNAP benefits were received, including the day of receipt. All other households are "outside window." Standard errors are in parentheses in columns 1 through 3; p-value of the test of the difference between in-window and out-of-window samples is in parentheses in column 4.

of household characteristics, we take a rather straightforward approach. First, we estimate logistic regressions for each of the 10 food security questions, in addition to the canonical classifications of food insecure (i.e., three or more affirmations) and very low food security (i.e., six or more affirmations). Our model is

$$Pr(Y_{ij} = 1 \,|W_i, Z_i) = G\left(\beta_j W_i + Z_i{}'\zeta_j\right) \tag{2}$$

where Y_{ij} equals one if household i responds affirmatively to the jth binary indicator for one of the 10 FSM questions, food insecurity, or very low food security. (To be clear, there 12 sets of parameters, one from each regression of the FSM questions, food insecurity, and very low food security.) W_i equals one if the household was surveyed within the salience window, Z_i are household and respondent characteristics, and β_j and ζ_{j} are parameters for outcome j. We include Z_i to increase the precision of our estimates. $G(.)$ is the logistic function. The coefficient estimate β_j is used to calculate the marginal effect of survey timing on each of the j food security responses and classifications.

Next, we supplement the 12 individual regressions by allowing for household-specific heterogeneity in response to module questions. This form of heterogeneity does not vary across questions, rather, it is "fixed" and is assumed to be randomly distributed across the population. This modification is simply a single household random effects logistic regression,

$$\Pr(Y_{ij} = 1 \,|W_i, Z_i, \varepsilon_i, \eta_j) = G\left(\beta W_i + Z_i{}'\zeta + \varepsilon_i - \eta_j\right) \quad (3)$$

where ε_i is a household-specific unobserved effect and η_j are item severity parameters for each of the 10 FSM questions. The severity parameters capture the scaled likelihood of affirming one question relative to another, conditional on household-specific effects. In this specification, the coefficient estimate $\hat{\beta}$ is used to calculate the marginal effect of affirming *any* question. All estimates are weighted.

We include the random effects logistic model primarily because it allows for correlations across survey questions and gives a different, but important interpretation. Moreover, this model belongs to the framework of measurement models used by the ERS to determine food security status. The framework is known as Item Response Theory (IRT), a field of measurement that began in educational testing in the 1960s. According to the theory, the measurement model should identify people on the scale of "ability" (i.e., via test questions) and on the scale of "difficulty," but that these scales should locate both persons and questions on the same underlying latent variable: for example, mathematical ability (Englehard, 2013). In our application, "difficulty" represents the severity of the behavior or attitude reflected in the question modeled by η_j. It indicates the difference between worrying about whether you would run out of food (less severe) and not eating for an entire day (more severe). Household "ability" is, in this case, a household's propensity for food hardship, which is modeled by ε_i.[13]

RESULTS

Main Results

Tables 3 and 4 show the parameter estimates of the individual logistic regressions for each FSM question as well as the classifications of food security status, respectively. Table 5 shows the corresponding marginal effects of being interviewed in the salience window. With respect to the coefficients in Tables 3 and 4, we notice that there are two covariates that are statistically significant for eight out of 10 questions and both classifications: number of disabled persons in the household and whether the household has had a recent large expenditure. For some, though not all regressions, the level of education, race, whether the household has at least $2,000 in liquid assets, and the presence of children under 5 in the home are significant.

The coefficient for the salience window is significant for the first five questions in the FSM and for food insecurity: the marginal effects are between 8 and 13 percentage points for the individual questions, while for food insecurity it is 11 percentage points (Table 5). The magnitudes of the marginal effects are large considering the unconditional means of these outcomes shown in Table 2. The marginal effect on very low food security is indistinguishable from zero, which is consistent with the results for the individual regressions, where survey timing primarily affects answers to less severe food hardships.

[13] Although there is an established literature that suggests the Rasch model, which is the underlying measurement model for food security, does not have optimal properties in the measurement modeling sense, alternatives in the context of food security measurement and classification have yet to be explored. See Nord and Coleman-Jensen (2014); Opsomer et al. (2002); Nord (2012); Sinharay (2006); Englehard, Rabbitt, and Englehard (2017).

Table 3. Parameter estimates from individual logistic regressions.

Variables	(1) Worried	(2) NotLast	(3) BalMeal	(4) CutSkip	(5) CutSkipF	(6) EatLess	(7) Hungry	(8) LoseWeight	(9) WholeDay	(10) WholeDayF
In window	0.592***	0.443***	0.604***	0.511**	0.487*	0.084	0.379	0.186	0.258	0.356
	(0.214)	(0.155)	(0.176)	(0.214)	(0.241)	(0.201)	(0.258)	(0.350)	(0.688)	(0.768)
Hispanic	0.875	1.290***	0.435	−0.164	−0.106	−0.144	−0.314	−0.330	−0.749	−0.342
	(0.543)	(0.382)	(0.396)	(0.452)	(0.495)	(0.420)	(0.475)	(0.929)	(0.707)	(0.740)
Black	0.381	0.709**	−0.328	0.173	0.124	0.070	−0.811*	−0.110	−1.535***	−1.922***
	(0.479)	(0.285)	(0.346)	(0.412)	(0.586)	(0.421)	(0.420)	(0.908)	(0.550)	(0.586)
White	0.593	1.001**	0.065	0.139	0.254	−0.130	−0.543	−0.137	−0.908	−0.503
	(0.520)	(0.395)	(0.416)	(0.405)	(0.444)	(0.398)	(0.520)	(0.876)	(0.554)	(0.509)
High school graduate	−0.490**	−0.501**	−0.297	0.303	0.317	0.251	0.789	0.228	0.635	0.971*
	(0.198)	(0.239)	(0.284)	(0.406)	(0.449)	(0.364)	(0.661)	(0.471)	(0.450)	(0.552)
Some college	−1.024***	−0.876***	−0.744***	−0.304	−0.240	−0.262	0.461	0.096	−0.133	0.357
	(0.187)	(0.295)	(0.202)	(0.314)	(0.337)	(0.323)	(0.508)	(0.478)	(0.530)	(0.615)
College graduate	−0.562**	−0.599**	−0.369	−0.069	−0.026	−0.094	0.585	−0.298	0.198	0.653
	(0.265)	(0.259)	(0.237)	(0.375)	(0.441)	(0.431)	(0.618)	(0.466)	(0.615)	(0.870)
No. of kids 0–5	0.007	−0.370***	−0.210*	−0.369**	−0.300*	−0.168	−0.238	−0.040	−0.793**	−0.454
	(0.125)	(0.130)	(0.104)	(0.150)	(0.170)	(0.148)	(0.165)	(0.210)	(0.364)	(0.340)
No. of kids 6–12	0.171	−0.084	−0.034	0.126	0.033	0.072	0.206	0.242	−0.231	−0.549
	(0.133)	(0.148)	(0.128)	(0.123)	(0.169)	(0.136)	(0.212)	(0.202)	(0.297)	(0.352)
No. of kids 13–18	0.112	−0.102	0.000	−0.068	−0.092	−0.111	0.031	−0.052	−0.120	−0.386
	(0.173)	(0.150)	(0.138)	(0.152)	(0.161)	(0.134)	(0.136)	(0.217)	(0.307)	(0.451)
Single head	0.007	−0.287	0.192	0.259	0.473**	0.131	0.661**	0.467	0.958***	1.317***
	(0.205)	(0.199)	(0.252)	(0.237)	(0.222)	(0.217)	(0.244)	(0.305)	(0.289)	(0.446)
No. of disabled persons	0.551***	0.639***	0.609***	0.447**	0.547***	0.553**	0.525**	0.659**	0.211	0.290
	(0.183)	(0.189)	(0.219)	(0.179)	(0.175)	(0.224)	(0.219)	(0.248)	(0.349)	(0.382)
>$2,000 in liquid assets	−0.936**	−1.142**	−0.948**	−0.458	−0.306	−0.502	−0.072	−0.216	0.666	1.124
	(0.371)	(0.452)	(0.392)	(0.606)	(0.612)	(0.497)	(0.583)	(0.479)	(0.899)	(0.840)

Continue

Table 3. Continued.

Variables	(1) Worried	(2) NotLast	(3) BalMeal	(4) CutSkip	(5) CutSkipF	(6) EatLess	(7) Hungry	(8) LoseWeight	(9) WholeDay	(10) WholeDayF
Recent large expenditure	1.174***	1.427***	1.139***	1.132***	1.036***	1.280***	1.068***	1.339***	1.845***	1.828***
	(0.277)	(0.265)	(0.305)	(0.209)	(0.195)	(0.236)	(0.224)	(0.251)	(0.305)	(0.400)
Own an auto	−0.427**	−0.598***	−0.098	−0.316	−0.238	−0.103	−0.185	0.026	−0.630**	−1.111***
	(0.188)	(0.180)	(0.145)	(0.221)	(0.221)	(0.206)	(0.285)	(0.253)	(0.257)	(0.340)
Normed[a] SNAP benefits	−0.212	−0.068	−0.286	−0.149	0.026	−0.025	1.640***	0.786	2.481***	2.929***
	(0.269)	(0.304)	(0.321)	(0.316)	(0.300)	(0.341)	(0.319)	(0.542)	(0.728)	(0.812)
Normed[a] SNAP benefits^2	0.039	0.009	0.066	0.055	0.038	0.048	−0.565***	−0.286*	−0.779***	−0.956***
	(0.068)	(0.084)	(0.085)	(0.097)	(0.097)	(0.098)	(0.098)	(0.158)	(0.281)	(0.306)
Normed[a] nonfood exp	−0.455	−0.696	−0.531	−0.344	−0.236	−0.813	−0.049	0.547	0.605	−0.179
	(0.570)	(0.567)	(0.401)	(0.536)	(0.606)	(0.764)	(0.519)	(0.413)	(0.710)	(0.789)
Normed[a] nonfood exp^2	0.169	0.159	0.103	0.103	0.098	0.316	−0.024	0.003	−0.088	−0.020
	(0.249)	(0.154)	(0.101)	(0.152)	(0.176)	(0.300)	(0.054)	(0.044)	(0.071)	(0.068)
Constant	0.443	0.210	0.484	−1.206*	−1.902***	−1.287**	−3.092***	−3.864***	−4.338***	−5.125***
	(0.611)	(0.479)	(0.495)	(0.661)	(0.687)	(0.612)	(0.646)	(1.077)	(0.765)	(0.845)
N						1,184				

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. "In window" refers to the salience window: those households who completed the food security questionnaire either three days before or after SNAP benefits were received, including the day of receipt. All other households are "outside window."
[a]Nonfood expenditures and SNAP benefits are normed by the square-root of household size. Standard errors are in parentheses.

Table 4. Parameter estimates for food security regressions.

Variables	Food insecure	Very low food security
In window	0.542***	0.341
	(0.169)	(0.279)
Hispanic	1.029**	−0.117
	(0.501)	(0.468)
Black	0.498	−0.251
	(0.455)	(0.416)
White	0.926*	−0.156
	(0.511)	(0.451)
High school graduate	−0.321	0.527
	(0.207)	(0.480)
Some college	−0.892***	−0.036
	(0.225)	(0.327)
College graduate	−0.618**	0.250
	(0.244)	(0.457)
No. of kids 0–5	−0.259*	−0.410**
	(0.131)	(0.160)
No. of kids 6–12	0.094	−0.011
	(0.123)	(0.220)
No. of kids 13–18	−0.090	−0.079
	(0.105)	(0.136)
Single head	0.070	0.424*
	(0.222)	(0.239)
No. of disabled persons	0.659***	0.588**
	(0.171)	(0.252)
>$2,000 in liquid assets	−1.219**	−0.043
	(0.534)	(0.568)
Recent large expenditure	1.303***	1.563***
	(0.242)	(0.230)
Own an auto	−0.372**	−0.362
	(0.182)	(0.258)
Normed[a] SNAP benefits	−0.301	0.008
	(0.344)	(0.352)
Normed[a] SNAP benefits^2	0.053	0.058
	(0.095)	(0.103)
Normed[a] nonfood exp	−0.637	−0.619
	(0.449)	(0.796)
Normed[a] nonfood exp^2	0.148	0.200
	(0.147)	(0.223)
Constant	−0.165	−1.881***
	(0.511)	(0.619)
N	1,184	

Notes: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. "In window" refers to the salience window: those households who completed the food security questionnaire either three days before or after SNAP benefits were received, including the day of receipt. All other households are "outside window."
[a]Nonfood expenditures and SNAP benefits are normed by the square-root of household size.

Estimated parameters and marginal effects from the fully-specified (saturated) random effects model can be found in Table 6, columns 1 and 2. The only significant covariates are those for some college education, liquid assets, number of disabled persons, recent large expenditure, and ownership of an auto. Therefore, in

Table 5. Marginal effect of salience window on individual responses and food security status.

Item/Food security	Marginal effect	SE
Worried	0.126***	0.044
FoodNotLast	0.091***	0.031
BalancedMeal	0.131***	0.038
CutSkip	0.089**	0.035
CutSkipFreq	0.078**	0.038
EatLess	0.014	0.033
Hungry	0.048	0.032
LoseWeight	0.013	0.024
WholeDay	0.014	0.038
WholeDayFreq	0.015	0.033
Food insecure	0.111***	0.034
Very low food security	0.047	0.038
N	1,184	

Notes: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. The salience window refers to those households who completed the food security questionnaire within three days before and three days after SNAP benefits were received, including the day of receipt.

the third and fourth column of Table 6, we estimate a sparse model with only those variables.[14]

Here again, the coefficient estimate for the salience window is large and significant. While the marginal effect appears smaller than the individual regressions, the interpretation has changed: the propensity to report *any* food hardship increases by about 4.5 percentage points in the salience window. To put this magnitude into perspective, notice that the predicted probability of an affirmative response to any question $\Pr(Y_{ij} = 1 \,|W_i, Z_i, \varepsilon_i, \eta_j)$ is 21 percent. In other words, being interviewed within the week that surrounds benefit receipt increases affirmation by about 25 percent. To further put the magnitude into perspective, we also report the marginal effects for the observed covariates that were found to be significant, as stated above. For example, it is known that disability is a strong predictor of food insecurity (Coleman-Jensen & Nord, 2013; Huang, Guo, & Kim, 2010), and we find that the addition of a disabled person in the household increases the probability of any food hardship by 7 to 8 percent. Access to liquid assets and incurring a recent large expenditure have larger impacts on the probability of reporting a food hardship at several orders of magnitude as compared to being surveyed within the salience window.

Robustness: Definition of Salience Window

One critique may be that the "size" of the salience window is somewhat arbitrary, although our choice was driven by the nonparametric regressions found in Figure 2. Thus, Figure 3 shows graphically how the size of the salience window affects our estimates for food insecurity (left panel) and very low food security (right panel). Each point estimate for different window widths is accompanied by 95 percent confidence intervals, beginning with the effect of responding to the FSM on the same day as one receives SNAP benefits (i.e., a window width of one day).

For the classification of food insecurity, the marginal effects are significant for window widths of five, seven, and nine days, each centered on the day of issuance.

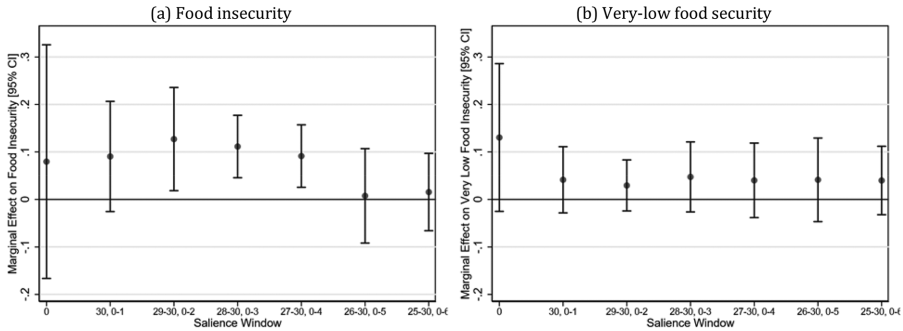
[14] Estimates of item parameters are available upon request.

Table 6. Parameter estimates and marginal effects: Random effects logistic regressions.

	Saturated		Sparse	
Variables	Parameters	Margins	Parameters	Margins
In window	0.661***	0.046**	0.653***	0.046***
	(0.256)	0.018	(0.251)	0.018
Hispanic	0.218	0.015		
	(0.536)	0.038		
Black	−0.050	−0.004		
	(0.510)	0.036		
White	0.419	0.029		
	(0.519)	0.037		
High school graduate	−0.437	−0.031		
	(0.275)	0.019		
Some college	−0.791**	−0.055**	−0.525**	−0.037**
	(0.330)	0.023	(0.263)	0.018
College graduate	−0.403	−0.028		
	(0.301)	0.021		
No. of kids 0–5	−0.178	−0.012		
	(0.138)	0.010		
No. of kids 6–12	0.217*	0.015*	0.138	0.010
	(0.131)	0.009	(0.135)	0.009
No. of kids 13–18	0.022	0.002		
	(0.136)	0.010		
Single head	0.013	0.001		
	(0.220)	0.015		
No. disabled persons	1.045***	0.073***	1.139***	0.080***
	(0.184)	0.012	(0.180)	0.012
>$2,000 in liquid assets	−2.619***	−0.183***	−2.569***	−0.181***
	(0.367)	0.027	(0.347)	0.026
Recent large expenditure	2.322***	0.162***	2.331***	0.164***
	(0.272)	0.017	(0.293)	0.019
Own an auto	−0.644***	−0.045***	−0.546***	−0.038***
	(0.214)	0.014	(0.197)	0.013
Normed[a] SNAP benefits	0.176	0.012		
	(0.388)	0.027		
Normed[a] SNAP benefits^2	−0.110	−0.008		
	(0.108)	0.008		
Normed[a] nonfood exp	0.076	0.005		
	(0.372)	0.026		
Normed[a] nonfood exp^2	0.029	0.002		
	(0.038)	0.003		
Predicted $P(Y_{ij} = 1 \mid W_i, Z_j, \varepsilon_i, \eta_i)$	0.211		0.210	
	(0.004)		(0.004)	
N	11,840			

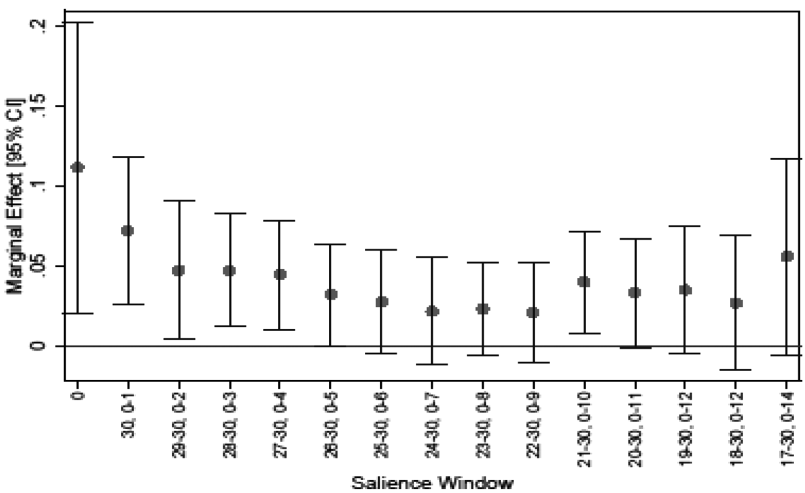
Notes: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. The salience window refers to those households who completed the food security questionnaire within three days before and three days after SNAP benefits were received, including the day of receipt.
[a]Expenditures and SNAP benefits normed by household size.

For the two smaller window sizes (i.e., only the day of issuance, and additionally including the day before and after issuance), the marginal effect is of similar magnitude but estimated with less precision. With regard to the more severe classification of very low food security, we find positive but insignificant effects of survey timing.



Notes: Salience window refers to the number of days since SNAP receipt, with day zero being the day of receipt. Food insecurity is defined by three or more affirmative responses; very-low food security is defined by six or more affirmative responses.

Figure 3. Marginal Effects of SNAP Receipt on Food Security Status by Size of Salience Window: Logistic Regression.

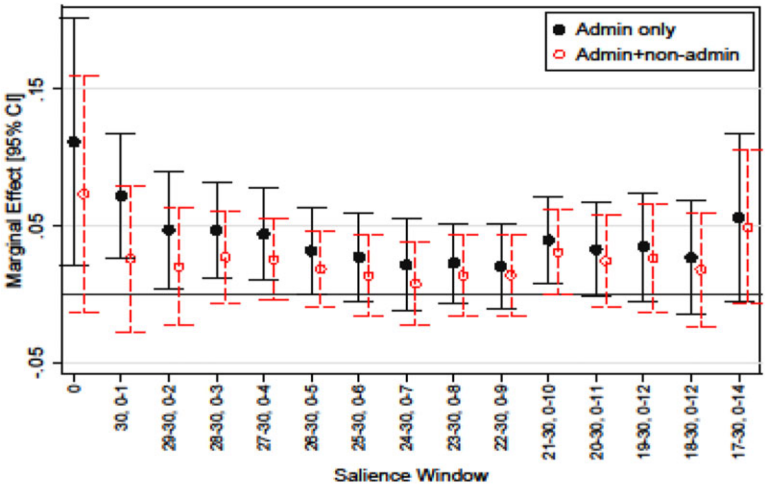


Notes: Salience window refers to the number of days since SNAP receipt, with day zero being the day of receipt. Estimates are from the random effects logistic regression (see Equation 3).

Figure 4. Marginal Effects of SNAP Receipt by Size of Salience Window Using Administratively Verified SNAP Households: Random Effects Logistic Regression.

Beyond the last windows shown here (at the far right of the figures, including days 25 to 30 and zero to six), marginal effects remain close to zero and insignificant.

Figure 4 performs a similar exercise by showing the marginal effects from the random effects model as they vary across even larger widths of the salience window. We show this because an interesting feature emerges that affirms the basic response pattern shown in Figure 2. At the far left of the graph, we see responding to the FSM on the same day that one receives SNAP increases the probability of affirming *any* of the conditions by 11 percentage points. The corresponding standard error is large due to the small number of households being surveyed on that day ($N = 26$). As we expand the window in both directions, still centered upon benefit receipt, estimates begin to attenuate, although with smaller standard errors. This pattern is to be



Notes: Salience window refers to the number of days since SNAP receipt, with day zero being the day of receipt. Bars represent 95 percent confidence intervals. Estimates are from the random effects logistic regression (see equation 3).

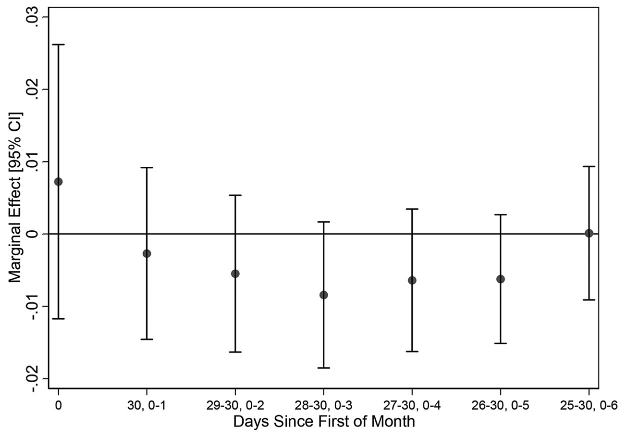
Figure 5. Marginal Effects of SNAP Receipt by Size of Salience Window Using Administratively Verified and Nonadministratively Verified SNAP Households: Random Effects Logistic Regression. [Color figure can be viewed at wileyonlinelibrary.com]

expected as we redefine the salience group to include more households within the interior of the month. Point estimates reach their lowest point once the salience window includes one-half of the month (24 to 30, zero to seven); that is, the first and last week of the benefit month including the day of receipt. As the window expands beyond two weeks, estimates begin to become larger, although still mostly insignificant. This is because the nonsalience group (i.e., $W = 0$) becomes even more narrowly defined as those within the very interior of the month, where affirmations are least likely.

Robustness: Alternative Samples

Figures 5 and 6 reestimate the random effects model using two alternative samples. In Figure 5, we show a comparison between our main sample (denoted by the solid black circles, which duplicate Figure 4) and one in which we include an additional 244 households that were not administratively verified but reported that they were SNAP participants (denoted by the open circles and dashed lines). While the inclusion of nonadministratively confirmed households yields results that are qualitatively similar to those in Figure 4, estimation size and precision are attenuated. This is to be expected from the sample for whom there is a greater probability of misreporting participation (Gundersen & Kreider, 2008).

In Figure 6, we address the concern that other forms of monthly cycles exist (e.g., rent and other bills, or paychecks). Since we do not have any direct information pertaining to paycheck receipt, or when bills are due/paid, we focus on calendar days. In particular, we show the results of the random effects model for non-SNAP households, using the first day of the month as the reference day. As we expect, the marginal effects are zero. This is suggestive evidence that our results are tied to the SNAP cycle in particular, and not to other cyclical effects related to the calendar month.



Notes: Day zero is the first day of the month. Bars represent 95 percent confidence intervals. Estimates are from the random effects logistic regression (see equation 3).

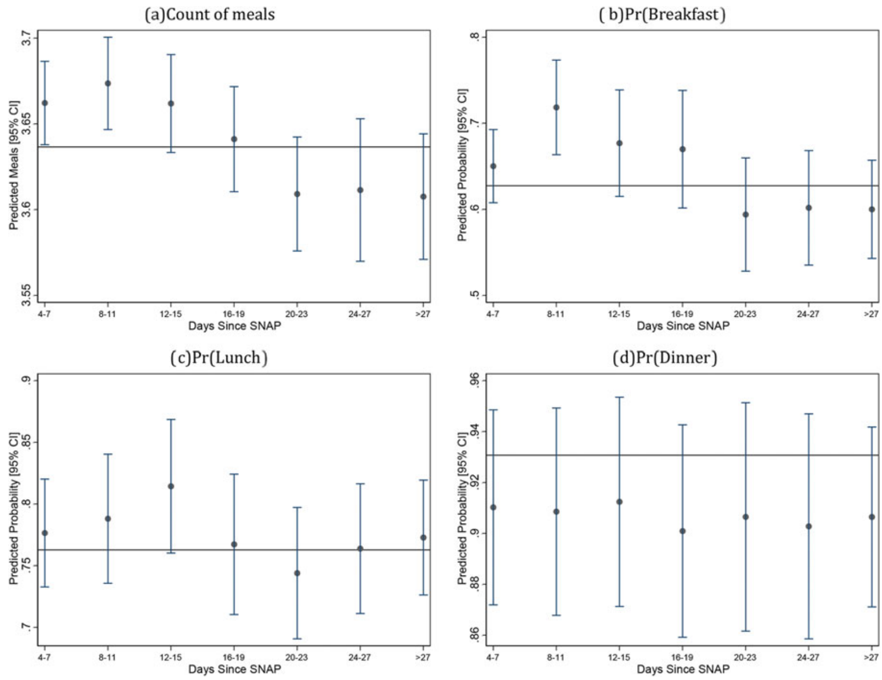
Figure 6. Marginal Effects of First of the Month by Size of Salience Window Using Non-SNAP Households: Random Effects Logistic Regression.

Mechanisms

As mentioned above, there might be (at least) two different mechanisms at play with respect to the responses that we document above, although the two are not mutually exclusive. First, there is good evidence that households experience food hardship toward the end of the month (Seligman et al., 2014; Shapiro, 2005; Todd, 2015; Wilde & Ranney, 2000), and it is likely that these episodes are most salient even after benefits have been received. Second, households may have in mind the need to justify the receipt of SNAP, both in anticipation of receiving SNAP in the near future and with respect to just receiving SNAP in the near past. While we cannot test for the latter hypothesis, we can look at whether there are changes in meal consumption that could explain the former.[15]

In FoodAPS, each household member reports when a breakfast, lunch, dinner, or three potential snacks (AM, PM, and evening) were consumed on each of the seven survey days. We focus on the number and types of meals consumed by adults in the sample.[16] Specifically, we estimate the predicted counts of adult meal consumption using a Poisson regression and the predicted probabilities of breakfast, lunch, and dinner using a logit regression. Panel (a) of Figure 7 shows there is a clear pattern of decline in the total number of meals eaten by adults. The subsequent three panels show that the results are driven by a reduction in the probability of adults eating breakfast. This suggests that our results are at least partly driven by one particular food hardship near the end of the month: adults skipping breakfast. In general, these results are also consistent with the SNAP cycling hypothesis, according to

[15] Keep in mind that meal consumption is unrelated to a subset of FSM questions, such as worrying, balanced meals and cutting portions of meals. Moreover, module questions related to meal consumption are predicated upon a lack of money. That is, individuals may skip meals for reasons unrelated to income.
[16] There is prior evidence using the FoodAPS data that adults, rather than children, cut back on meal consumption (Kuhn, 2017). In results not shown, we too find no decrease in meal consumption for children.



Notes: The horizontal line represents the sample average for the reference period, days zero to three in panel (a). Panel (a) reports predicted number of adult meals/day from a Poisson regression. Panels (b) through (d) report the predicted probabilities of an adult consuming a breakfast, lunch, and dinner using a logistic regression, respectively. In these figures the horizontal line represents the probability of eating the respective meal in the reference period, days zero to three.

Figure 7. Person-Level Meal Consumption and Days Since SNAP Receipt. [Color figure can be viewed at wileyonlinelibrary.com]

which SNAP recipients are more likely to face food shortages at the end of the month.[17]

DISCUSSION

Our results show a significant change in the response to food insecurity questions in relation to the receipt of SNAP benefits. The immediate implication is the potential for biased sample estimates of food insecurity rates from surveys that do not uniformly sample throughout the benefit month. We give an example using the CPS-FSS. We then apply the formulation to the 30-day food security questionnaire in the CPS to adjust national rates and account for the nonuniform sampling nature.[18]

[17] One might be inclined to include a measure of meal consumption directly in our regression to help "explain away" one mechanism or another. We highly caution against doing so, as this would introduce a form of selection bias (in our case, upward bias). Angrist and Plischke (2008) succinctly articulate this problem and call these "bad controls": "Bad controls are variables that are themselves outcome variables in the notional experiment at hand. That is, bad controls might just as well be dependent variables, too" (p. 64). In fact, if we do include a measure of meal consumption, our results are biased upwards to such a high degree that all questions, as well as both classifications of food insecurity, are significant—with the exception of the item about losing weight.

[18] The most cited national rates of food insecurity pertain to the 12-month questionnaire, such as those reported in Figure 1.

Second, we discuss implications for food security research when food security status is used as either an outcome or regressor.

Theoretical bias: Point Estimates of Food Insecurity

There are three intuitive implications for estimating food security prevalence in surveys with nonuniform (over the month) survey administration.[19]

1. A trivial implication is that if there is no salience window—that is, no contextual effect of SNAP receipt on responses to the FSM—there is no bias in prevalence estimates.
2. Because SNAP recipients have a higher propensity to affirm the FSM items (in general), the magnitude of the bias will be larger when SNAP enrollment (as a proportion of the population) increases.
3. The direction of bias will depend on the relative number of SNAP households surveyed within the salience window. Put differently, if module questions are uniformly administered throughout the benefit month (or all States switch to a uniform distribution of SNAP benefits), then the ratio of SNAP recipients in the salience window to those outside of it has a known value ($7/31 = 0.226$) and there is no bias. Deviations from this ratio in the sample imply bias: upward bias if this ratio is greater than 0.226, and downward bias if it is less than 0.226.

Application to CPS

In this section, we show how implications 2 and 3 are operational in the CPS-FSS. We focus on the 10-item, 30-day household FSM in the 2005 to 2010 CPS-FSS since these are the only years that contain a self-reported SNAP issuance date.[20] Using these dates, we can determine each respondent's salience window—three days prior and three days after issuance. However, because the public-use file does not contain the date of the interview, we do not know for certain if the interview occurred within the salience window. We can, however, calculate the probability that a respondent was surveyed during each household's salience window. For example, in 2005 when the 12th fell on a Monday, we know that interviews occurred over December 11 to 17. Thus, the salience window perfectly overlays with the interview week for anyone reporting receipt of benefits on the 13th (i.e., Wednesday of the survey week): their probability equals one. For respondents reporting receipt of benefits on either the 12th or the 14th (i.e., Tuesday or Thursday of the survey week), the salience window overlaps the interview week for six out of the seven days, and their probabilities are equal to 6/7. Using this logic, we can calculate each respondent's probability of being surveyed while in the salience window based on the known CPS survey week and the self-reported issuance date.

Table 7 shows the CPS interview dates from 2005 to 2010.[21] This table also shows estimates of the proportion of the SNAP population surveyed within the salience window along with standard errors, 95 percent confidence intervals, and sample

[19] Details and derivations are in the Appendix. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[20] The 2002-04 CPS-FSS have self-reported issuance dates, but the manner in which households were screened into the 30-day food security questionnaire was different.
[21] The CPS December supplement contains more than just the food security questions. The labor questions pertain to the previous week, which needs to wholly contain days in December. Thus, the reason 2008 and 2009 do not contain December 12 is because for those years the reference week would have contained some days of November. For example, in 2008 the 12th fell on a Friday, meaning that the reference week would have been November 30 through December 6.

Table 7. CPS-FSS interview dates and estimates of proportion of SNAP population surveyed during the salience window.

Year	Interview dates	$\frac{n_1}{n_0+n_1}$	SE	[95 percent CI]		Sample size
2005	December 11–17	0.22	0.03	0.16	0.28	131
2006	December 10–16	0.25	0.04	0.18	0.32	78
2007	December 9–15	0.33	0.03	0.28	0.38	174
2008	December 14–20	0.13	0.02	0.09	0.17	155
2009	December 13–19	0.17	0.02	0.12	0.21	190
2010	December 12–18	0.23	0.03	0.18	0.28	183

Notes: Current Population Survey Food Security Supplement (CPS-FSS). $\frac{n_1}{n_0+n_1}$ is the fraction of SNAP households surveyed within the salience window. Only those households who responded affirmatively to receiving SNAP benefits in November were asked about the date that benefits were received.

sizes. We can see that, except for 2008 and 2009, the 95 percent confidence intervals contain the uniform-sampling ratio of 0.226. We use the information in Table 7, along with our estimates of the effect of survey timing, to calculate adjusted rates of affirmation for each FSM question in the CPS (see Appendix for details).[22] Table 8 displays our results.

According to implication 2, we should expect the difference between the CPS estimate and our adjusted rate to increase in absolute value when SNAP participation increases, such as in 2008 to 2014 (see Figure 1). Indeed, for all questions, we see the magnitude in absolute value increases with participation rates, especially in 2008, when SNAP participation rose due to the recession. According to implication 3, we should observe an upward bias in 2006, 2007, and 2010 when the ratio of SNAP recipients in the salience window to those outside it exceeds 0.226. Put simply, implication 2 pertains to the magnitude of bias (in absolute value) and implication 3 pertains to its direction.

Table 8 shows that our expectations, as outlined in implications 2 and 3, are shown in the CPS. We note that the differences are not statistically significant, but this is to be expected: all of the components used to estimate these quantities are measured with error and thus exhibit substantial variance. However, the magnitude of the potential misclassification can be substantial, especially when considering trends over time. For example, considering the classification of food insecurity (i.e., three or more affirmatives): out of roughly 116 million households in 2007/2008, the number of food insecure households is overestimated in 2007 by 92,000 and underestimated by 105,000 in 2008. Put differently, when the recession hit in 2008, the increase in the number of food insecure households could have been over 197,000 more households than estimated by the CPS data.

We note that changes in state-level SNAP distribution schedules could affect the degree of bias.[23] Since 2012, 15 states have modified their SNAP issuance schedules. The general pattern among states is toward disbursements that are spread out more fully throughout the month, but the change in bias is unclear.[24] For example, a few states have increased the span of the first to last day by small amounts (e.g., Mississippi has gone from issuance on the 5th through the 19th day of the month to

[22] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[23] The data cited in this paragraph come from a comparison of FNS disbursement data for 2012 (available from the authors) and current FNS disbursement schedules, available at https://www.fns.usda.gov/snap/snap-monthly-benefit-issuance-schedule. These data are also cited in Cotti, Gordanier, and Ozturk (2016).
[24] Delaware has changed distribution schedules twice in this time.

Table 8. Observed and adjusted rates of affirmation for CPS-FSS module questions, 2005 to 2010.

	2005	2006	2007	2008	2009	2010
Food insecure						
Adjusted	5.893	5.759	4.622	8.900	8.623	8.180
CPS	5.884	5.777	4.701	8.810	8.562	8.186
Difference	0.009	−0.018	−0.079	0.090	0.061	−0.006
Very-low food security						
Adjusted	2.214	2.398	1.743	3.620	3.348	3.127
CPS	2.211	2.404	1.769	3.590	3.328	3.129
Difference	0.003	−0.006	−0.026	0.030	0.020	−0.002
Worried						
Adjusted	6.909	6.726	7.114	10.06	9.757	9.529
CPS	6.901	6.736	7.182	9.981	9.702	9.531
Difference	0.008	−0.010	−0.068	0.079	0.055	−0.003
FoodNotLast						
Adjusted	5.952	5.803	6.198	8.631	8.314	7.943
CPS	5.947	5.812	6.248	8.573	8.275	7.945
Difference	0.005	−0.009	−0.050	0.059	0.040	−0.002
BalancedMeal						
Adjusted	6.105	6.054	6.994	9.307	8.730	8.59
CPS	6.098	6.068	7.066	9.225	8.673	8.594
Difference	0.008	−0.014	−0.072	0.082	0.057	−0.003
CutSkip						
Adjusted	4.017	4.068	4.445	5.603	5.460	5.827
CPS	4.011	4.079	4.498	5.542	5.417	5.83
Difference	0.006	−0.012	−0.053	0.060	0.043	−0.003
CutSkipFreq						
Adjusted	3.004	3.005	3.033	3.945	3.797	4.120
CPS	2.999	3.015	3.075	3.897	3.763	4.123
Difference	0.005	−0.009	−0.043	0.048	0.034	−0.002
EatLess						
Adjusted	3.730	3.683	4.097	5.908	5.656	5.375
CPS	3.728	3.684	4.102	5.900	5.652	5.374
Difference	0.001	0.000	−0.005	0.008	0.005	0.001
Hungry						
Adjusted	2.937	2.892	3.256	4.962	4.654	4.286
CPS	2.933	2.897	3.282	4.93	4.633	4.286
Difference	0.003	−0.005	−0.026	0.031	0.021	0.000
LoseWeight						
Adjusted	1.241	1.278	1.381	2.115	2.008	1.729
CPS	1.241	1.279	1.389	2.105	2.001	1.728
Difference	0.001	−0.001	−0.008	0.010	0.007	0.000
WholeDay						
Adjusted	0.830	0.940	0.861	1.130	1.111	1.010
CPS	0.829	0.942	0.869	1.121	1.105	1.011
Difference	0.001	−0.002	−0.008	0.009	0.006	0.000
WholeDayFreq						
Adjusted	0.510	0.564	0.536	0.832	0.778	0.623
CPS	0.509	0.566	0.544	0.822	0.771	0.624
Difference	0.001	−0.002	−0.009	0.010	0.007	−0.001
U.S. households (millions)	113.343	114.384	116.011	116.783	117.181	117.538

Notes: The total number of U.S. households was obtained from the U.S. Census Bureau, Current Population Survey, March and Annual Social and Economic Supplements.

the 4th through the 21st day): this ought to reduce the probability of survey during the salience window. However, other states like Virginia and Idaho have gone from disbursement on the first of the month to multiple-day disbursement—10 days in the case of Idaho and three days in the case of Virginia: this should increase the probability of being surveyed during the salience window, and therefore increase the bias in food security estimates, both state and national. One problem with simply applying the known state-level distribution dates to the CPS is that we still do not know the exact day of the survey, which, as mentioned above, introduces measurement error in any analysis.

Potential Effects on Food Security Research

The strong correlation between SNAP receipt and food security reporting raises two questions for food security research: (1) Does the usage of food security status as a regressor become more problematic than it already is (due to endogeneity) when there are nonclassical misclassifications of food security status among SNAP participants? (2) Is the effect of SNAP on food insecurity prone to biases when the SNAP sample is drawn within the salience window?

To examine the first of these effects, we follow an approach similar to Gregory and Coleman-Jensen (2017). We estimate the probability of reporting fair or poor self-assessed health (as opposed to excellent, very good, or good) as a function of food insecurity and demographics (as in our previous specifications). We find the marginal (associative) effect of food insecurity on fair/poor health is about 55 percent higher for the sample surveyed within the salience window (11.18 percent) as opposed to those surveyed outside the window (7.29 percent). Although this difference is not significant in this model, it does point to potential issues in using food insecurity as a right-hand-side variable dependent upon survey timing.

The second problem—the effect of SNAP on food security—–is more difficult to address as it requires a fully articulated identification strategy for estimating treatment effects, such as the justification of an instrumental variable, which is beyond the scope of this paper. Instead, as suggested by Gundersen and Ziliak (2015), we adopt an estimator that places bounds on the potential effect of SNAP on food insecurity rather than point identifying an effect (Kreider et al., 2012). As is typical in this literature, the comparison group is nonparticipating low-income households, where we look at three income thresholds: 130, 185, and 200 percent of the federal poverty levels (FPL). We rely on two (relatively weak) assumptions: (1) enrolling in SNAP does not make a household more food insecure, and (2) households that choose to participate in SNAP have marginally worse food security outcomes than those who do not (i.e., there exists adverse selection). These assumptions are called monotone treatment response (MTR) and monotone treatment selection (MTS), respectively. (See Manski, 2003; Kreider et al., 2012; and Morgan & Winship, 2015, for more on these assumptions.) The second assumption (MTR) implies that the lower bound cannot be negative, and we therefore focus on the upper bound, which is interpreted as the largest potential effect of SNAP on food insecurity rates.

As shown in Table 9, when using the threshold for SNAP eligibility of 130 percent FPL, the upper bound on the effect of SNAP in decreasing food insecurity is twice as high for those surveyed within the salience window (15.5 percent) as compared to those who are surveyed outside the window (7.4 percent). When we increase the comparison group to include those with higher levels of income (i.e., 185 and 200 percent FPL), the potential to reduce food insecurity increases because the comparator group is arguably better off due to higher income. Even so, the relative magnitude between the SNAP groups within and outside the salience window

Table 9. Estimated upper bounds of the potential reduction in food insecurity rates due to SNAP participation.

Comparison group	In window	Outside window
FPL 130 ($N = 816$)	0.155	0.074
FPL 185 ($N = 1,435$)	0.233	0.137
FPL 200 ($N = 1,577$)	0.262	0.146

Notes: The comparison group is nonparticipating low-income households at three federal poverty levels (FPLs) in percentages: 130, 185, and 200 percent. "In window" refers to the salience window: those households who completed the food security questionnaire either three days before or after SNAP benefits were received, including the day of receipt. All other households are "outside window." The lower bound is assumed to be zero.

remains. These results indicate the scope for finding reductions in food insecurity is greater for those sampled within the salience window.

CONCLUSION

In this study, we have examined the sensitivity of responses to the 10-item FSM administered in FoodAPS to the timing of SNAP benefits. We find that SNAP receipt has a significant effect on responses to less severe 30-day FSM individual items, food insecurity status, and the probability of an affirmative response to *any* of the FSM questions. In particular, we find the probability of responding affirmatively to any of the first five questions in the FSM increases between 8 and 13 percentage points when respondents are surveyed in the week (three days before and three days after) of SNAP receipt. We call this time frame the "salience window" and use this term to designate when food hardships are particularly highlighted in respondents' minds and therefore lead to increased affirmations to the FSM. Importantly, our results show that the probability of food insecurity is 11 percentage points higher in the salience window, on a base prevalence of 46 percent; the prevalence of an affirmation to *any* question is 4.6 percentage points over a base prevalence of 21 percent. Our results are corroborated by the cyclical consumption of meals for adults in households.

There are two areas of research in which these results are particularly pertinent. The first is the estimation of food security prevalence. Our results are consistent with an interpretation of survey response in which respondents are particularly sensitive to more recent food hardships—for example, those in the previous week. From this point of view, overall prevalence estimates—at least in the case of uniform survey administration over the month (as in FoodAPS and most other surveys)—are likely underestimates, as recalls at the end and beginning of the month would be viewed as "accurate" while responses in the middle of the SNAP month would reflect the forgetting of food hardships that happened more than one week ago.

As tempting as this interpretation is, there is no counterfactual against which to measure the difference between real-time experiences and their associated recollections. Moreover, even if we could measure worrying about food stocks or skipping meals, for example, the fact remains that all of the conditions of hardship are conditioned upon not having sufficient financial resources. This judgment depends heavily on household preferences, which are not observable. At a bare minimum, our results seem to highlight the fact that the questions in the FSM give us access to *remembered* food hardships. Our results also seem to reaffirm the suggestion that the FSM, to use the words of Kahneman and Krueger (2006), "measures features of individuals' perceptions of their experiences, not their utility as economists typically conceive of it. Those perceptions are a more accurate gauge of actual feelings if they are reported closer to the time of, and in direct reference to, the actual experience." Certainly, further research into the possible biases of the FSM due to the time frame of the questions and possibility of recall bias would be warranted.

Our results also imply that, under nonuniform survey administration, the biases in prevalence estimates could be significant. In the CPS, currently the source of official statistics on food security and used in many program evaluation studies, the FSM is typically administered in December in the week that contains the 12th. We have shown that, depending on the date of administration and the state of residence of the respondent, this could lead to an under- or over-estimate of food insecurity prevalence. While our results concerning biases in prevalence estimates are small, they are estimated with considerable measurement error. Nonetheless, our results highlight the need for closer examination of possible biases in both national- and state-level estimates of food insecurity due to the administration of the CPS. One question that remains is the possibility of monthly cycles in survey response to questions about 12-month food security status.

We are not the first to point out that the FSM is not without potential problems: for example, Nord and Hanson (2012) suggest that adults' and adolescents' experiences of food security conditions in the same household rarely agree. Our research suggests systematic contextual issues in the response to the FSM items. These results indicate the need for further research into what the FSM captures and how it captures it.

The potential effects of the timing of the FSM with respect to SNAP receipt also have implications for further research on food insecurity. In particular, we show that the measured effect of SNAP on food insecurity is greater for households who respond in the week of SNAP receipt compared to those who do not. An unanswered question is whether responses to this module show similar cyclicality with respect to receipt of other sources of income. Additionally, these results point to the possibility that the measured effect of SNAP on food insecurity would be systematically different when the fraction of low-income households participating in SNAP increases. These are also questions for further research.

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APPENDIX

THEORETICAL BIAS DUE TO NONUNIFORM SAMPLING

We can decompose the unconditional probability of affirming a food insecurity question into three mutually exclusive conditional probabilities:

$$P\left(Y_{10}\right)=\frac{n_0}{N}\ \Pr\left(Y=1|S=1,W=0\right) \tag{A.1}$$

$$P\left(Y_{11}\right)=\frac{n_1}{N}\Pr\left(Y=1|S=1,W=1\right) \tag{A.2}$$

$$P\left(Y_o\right)=\frac{n_2}{N}\Pr\left(Y=1|S=0\right) \tag{A.3}$$

$$P\left(Y=1\right)=P\left(Y_{10}\right)+P\left(Y_{11}\right)+P\left(Y_0\right) \tag{A.4}$$

where $Y = 1$ represents affirmation to an individual food security question or classification of food security (i.e., food insecure or very-low food security), N is the population size, which is equal to the sum of n_0 (the number of households on SNAP, $S = 1$, but not in the salience window, $W = 0$), n_1 (the number of households on SNAP, $S = 1$, and in the salience window, $W = 1$), and n_2 (the number of households not on SNAP, $S = 0$, who are all trivially outside the salience window by definition of not being on SNAP).[25]

Since equation (A3) is unaffected by the timing of the food security questionnaire in relation to SNAP receipt, we focus on the first two equation lines (A1) and (A2). Notice that (A1) plus (A2) is equivalent to the probability of an affirmative answer within the SNAP population Pr($Y = 1|S = 1$) multiplied by its respective population weight, $\frac{n_0+n_1}{N}$. Thus, biases will arise solely through a bias in the estimation of food security rates within the SNAP population Pr($Y = 1|S = 1$). This gives rise to the three implications outlined in the text.

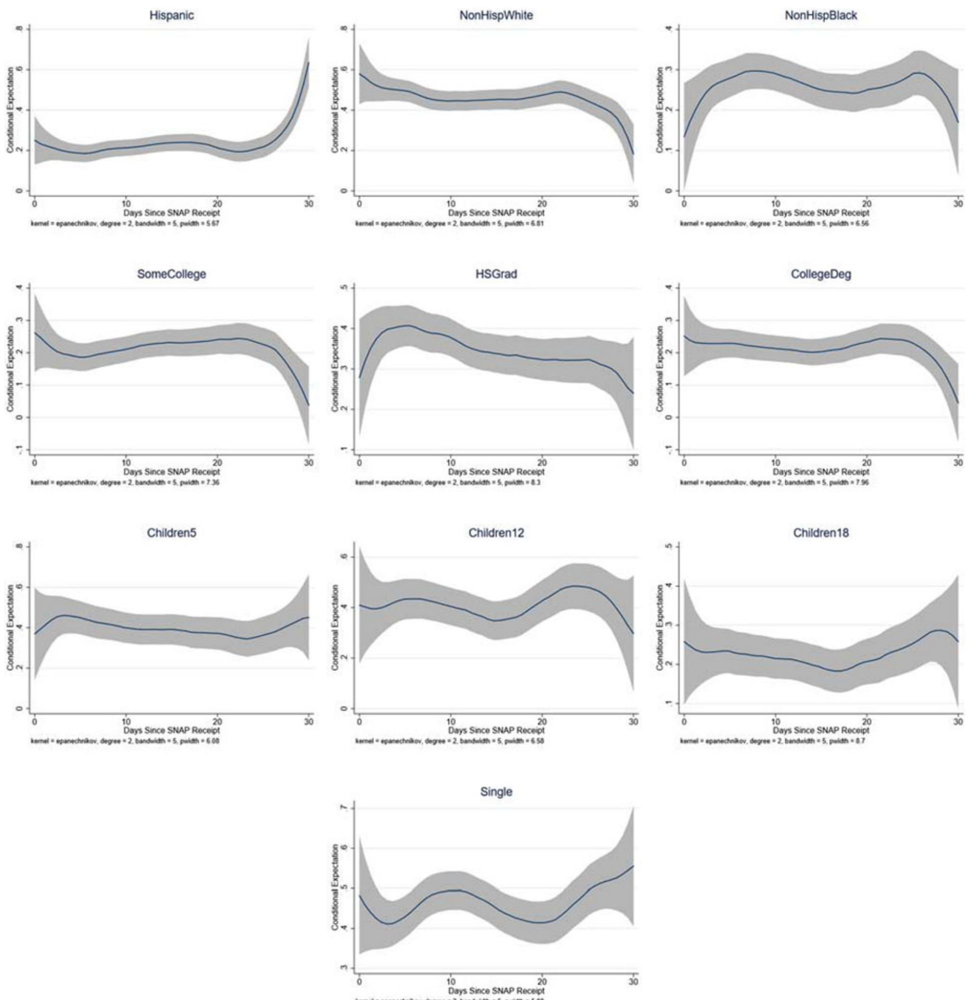
Implication 1: If Pr($Y = 1|S = 1, W = 0$) = Pr($Y = 1|S = 1, W = 1$), the timing of the food security module will not matter. This implication is rather trivial, but simply states that if there is no salience effect, then there is no bias.

Implication 2: Because SNAP recipients have a higher propensity to affirm food security questions, the magnitude of the bias will be larger in absolute value when SNAP enrollment as a proportion of the population increases. In other words, if a bias exists, it will be larger (in absolute value) in years when SNAP enrollment increases because more weight is placed on the SNAP population. Note, this implication assumes that new SNAP recipients are also prone to the salience effect.

Implication 3: The direction of the bias will depend on the relative number of SNAP households surveyed within the salience window, that is, n_1 to n_0. Put differently, if module questions are uniformly administered throughout the benefit month (or all States switch to a uniform distribution of SNAP

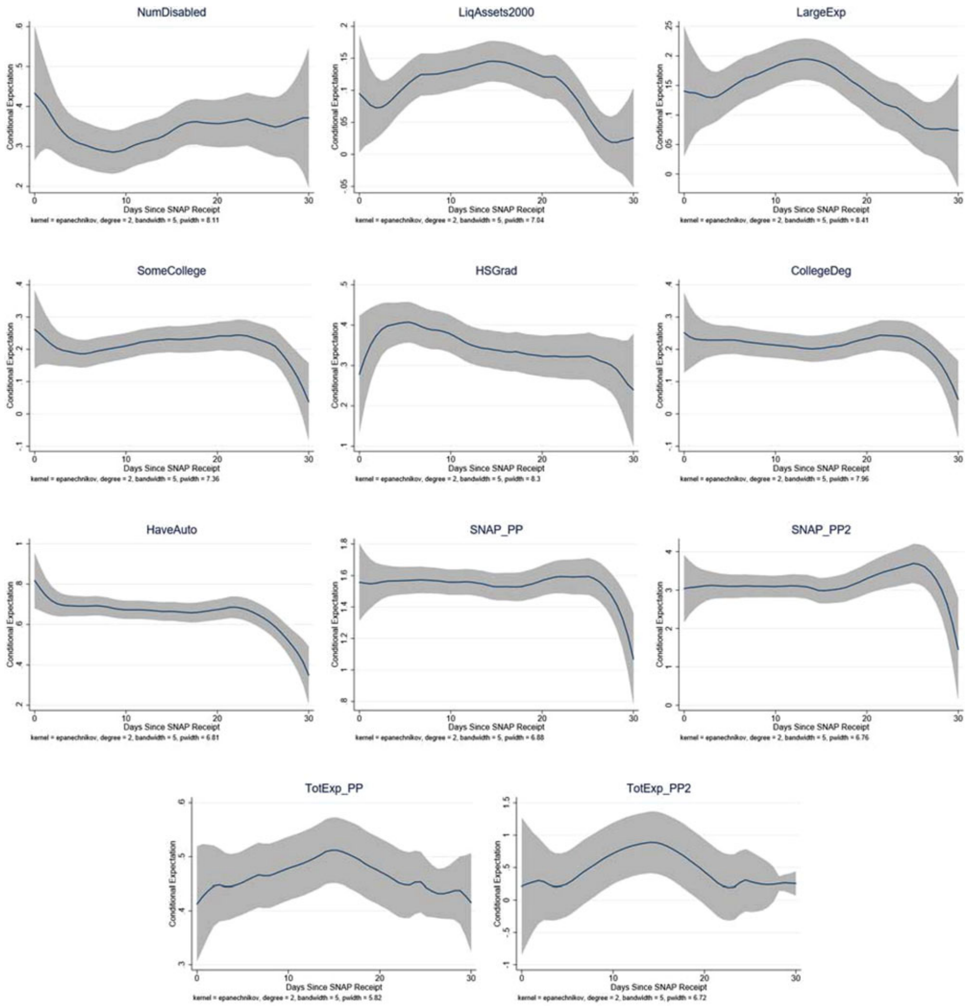
[25] The theoretical bias can be applied more generally to subjective well-being by noting that group membership in S represents those who are potentially affected by context/framing, and $W = 1$ for those who are "treated" with context/framing.

benefits), then the ratio of n_1 to n_0 has a known value and there is no bias (e.g., $\frac{n_0+n_1}{N} = \frac{7}{31} = 0.226$). Therefore, deviations from this ratio in the sample imply a bias relative to the true population. In particular, when using the one-week salience period, as is done in this study, if $\frac{n_0+n_1}{N} > 0.226$, then the bias will be upwards because there exists an oversampling of households during a time in which household are more likely to affirm questions.



Notes: Calculated from National Household Food Acquisition and Purchase Survey (FoodAPS). Each local polynomial regression is of degree two and uses the Epanechnikov kernel with a bandwidth of five. Shaded areas represent 95 percent confidence intervals.

Figure A1. Nonparametric Regressions of First Set of Observables on Days Since SNAP Receipt. [Color figure can be viewed at wileyonlinelibrary.com]



Notes: Calculated from National Household Food Acquisition and Purchase Survey (FoodAPS). Each local polynomial regression is of degree two and uses the Epanechnikov kernel with a bandwidth of five. Shaded areas represent 95 percent confidence intervals.

Figure A2. Nonparametric Regressions of Second Set of Observables on Days Since SNAP Receipt. [Color figure can be viewed at wileyonlinelibrary.com]