Measurement Error and the SNAP Benefit Cycle: Evidence from Supermarket Panel Data

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Abstract

This paper utilizes variation in the timing of benefit receipt that results from the semi-random assignment of SNAP distribution dates to evaluate the impact of SNAP issuance on household level spending patterns in retail scanner data. This approach is in contrast to recent research that utilizes variation in the timing of benefit receipt that stems from the proportion of SNAP benefits being issued over a given time period in a given state (i.e., variation in the likelihood of benefit receipt). We find that the likelihood of benefit receipt estimates are 2.0 to 2.9 times larger than the benefit receipt estimates. Benefit receipt estimates indicate a 13 percent increase in household level expenditure on the calendar week of benefit receipt, relative to calendar weeks of benefit non-receipt. We decompose the differences between these two sets of estimates and find that all of the difference is attributable to differences in endogeneity bias; explicitly, we find that the likelihood of benefit receipt estimates suffer endogeneity bias attributable to endogenous measurement error. Our findings illustrate that the utilization of group or time averages as proxies for individual stimuli may be subject to endogeneity concerns when the effect of treatment has both contemporaneous and lagged effects on the outcome of interest.

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1 Introduction

The Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program (FSP), is the largest food assistance program administered by the U.S. Department of Agriculture (USDA). In 2017, SNAP provided aid to 12.9% of the United States population and the average household received $254 in benefits per month (USDA). In addition to alleviating food insecurity, the SNAP program can also be utilized as an economic stabilizer during times of crisis.[1] However, monthly SNAP disbursement schedules have been found to generate cyclical expenditure and consumption cycles. Food expenditures of SNAP recipients spike on the day of benefit receipt (Wilde and Ranney (2000); Hastings and Washington (2010); Goldin et al. (2020); Castelatri et al. (2017)) and there is evidence that expenditure and consumption cycles are correlated (Kuhn (2018)). Furthermore, the SNAP disbursement schedule affects caloric intake (Shapiro (2005); Todd (2015); Kuhn (2018)), criminal activity (Foley (2011); Carr and Packham (2019); Carr and Packham (2021)) and standardized test scores (Cotti et al. (2018); Bond et al. (2021)). Given the magnitude and importance of this program for low-income families and the economy, it is important to understand how SNAP disbursement schedules influence the well-being and purchasing patterns of SNAP beneficiaries.

The day on which a household receives its monthly SNAP benefits is determined by their state of residence and is often assigned according to characteristics of the household that are not always known to the researcher (e.g., last digit of case number, social security number, first letter of last name, etc.). As a result, much of the work evaluating the effect of the SNAP benefit cycle has been conducted utilizing cross-sectional household level survey data in which participants report the day of the month on which they receive

[1] Ratcliffe and Mckerban (2010) show that monthly transfer of SNAP benefits reduces the likelihood that a household experiences food insecurity by roughly 30 percent. Furthermore, Hoynes and Schanzenbach (2009) find that food stamps reduce out-of-pocket food expenditures and increase overall food expenditures. Increased spending on SNAP benefits provides direct added income to the retailers where SNAP benefits are redeemed, which leads to further increases in spending among downstream suppliers and their employees (Canning and Stacy (2019)). Furthermore, Canning and Stacy (2019) estimate that an additional $1 billion dollars spent on SNAP benefits increases GDP by $1.54 billion which supports 13,560 new jobs. In addition, Steven Pennings (2021), finds that states that receive larger permanent transfers grow faster.

SNAP benefits or administrative data in which the exact day of benefit receipt is known.[2] However, there are many interesting data sets available to researchers in which the exact date of SNAP receipt is unknown. For example, recent literature makes use of the Kilts-Nielsen Retail Scanner data to understand the effect of SNAP issuance on retail sales, retailer responses and household level purchasing patterns over the course of the SNAP benefit cycle (Goldin et al. (2020), Castellari et al. (2017)). When the exact date of SNAP benefit distribution is unknown, researchers have utilized variation in the proportion of SNAP benefits issued in a given state on a particular day (Goldin et al. (2020); Cotti et al. (2020); Castellari et al. (2017); Cotti et al. (2016)). In other words, these researchers utilize variation in the likelihood of benefit receipt.

This paper utilizes household level supermarket panel data, generated from roughly 850 SNAP households, to compare estimates that utilize variation in the likelihood of SNAP benefit receipt to estimates that utilize variation in the receipt of SNAP benefits. SNAP benefits, in the state of residence for our households, are distributed across all four calendar weeks of the month and the distribution date is determined by the first letter of the household's last name. Since the last name of the household is observed in the data, we are able to identify the specific day in the month that the household receives SNAP benefits. We estimate the effect of SNAP issuance on purchasing patterns by utilizing variation in the timing of benefit receipt; we call this estimate the benefit receipt estimate. For comparison, we estimate the effect of SNAP issuance in the counterfactual scenario when only the likelihood of SNAP benefit receipt is observed; we call this estimate the likelihood of benefit receipt estimate (Goldin et al. (2020); Cotti et al. (2020); Castellari et al. (2017); Cotti et al. (2016)).

We find that the likelihood of benefit receipt estimates are 2.0 to 2.9 times larger than the benefit receipt estimates. The benefit receipt estimates indicate a 13 percent increase in household level expenditure on the calendar week of benefit receipt, relative to calendar

[2]Spending and Consumption: Wilde and Ranney (2000), Shapiro (2005), Todd (2015), Kuhn (2018); Crime: Foley (2011), Carr and Packham (2019), Carr and Packham (2021); Test Scores: Cotti et al. (2018), Bond et al. (2021); Hospital Visits: Cotti et al. (2020).

weeks of benefit non-receipt.[3] We decompose the differences between the two estimates and find that all of the difference is attributable to differences in endogeneity bias; explicitly, we find that the likelihood of benefit receipt estimates suffer endogeneity bias attributable to endogenous measurement error. We suspect that the endogeneity concern for the likelihood of benefit receipt estimate arises due to the fact that the positive impact of SNAP benefit receipt on expenditures is not exclusively concentrated on the first day or even within the first week of benefit receipt. This becomes an endogenity concern because the likelihood of benefit receipt variable continues to vary in weeks after benefit receipt (i.e., the measurement error continues to vary); this leads the likelihood of benefit receipt estimate to capture both the contemporaneous effect of benefit receipt and the lagged effect of benefit receipt leading to a positive bias in the likelihood of benefit receipt estimate.

We evaluate alternative estimation strategies that can be utilized by researchers when only the likelihood of SNAP benefit receipt is known. In particular we evaluate the validity of only utilizing states where SNAP benefits are distributed on a specific day or week of the calendar month. In order to do so, we replicate Hastings and Washington (2010), who evaluate the impact of the SNAP benefit cycle on household spending in a state where all benefits are distributed on the first of the month. The first of the month also happens to be the same day that cash transfers are distributed and is also correlated with other regular monthly payments (e.g., rent, utilities, etc.); these confounders pose potential threats to the underlying identification assumptions and could lead to endogeneity bias. A unique advantage of our data is that the benefit distribution dates span the four calendar weeks of the month; this alleviates concerns that the day of SNAP distribution might be correlated with other monthly income shocks (e.g., cash welfare payments, paycheck receipt, rent payments, utility bills etc.) that occur on the first of the month.

We find moderate differences between our estimates and Hastings and Washington (2010). Specifically, our estimate of the effect of SNAP receipt indicates that (food) expen-

[3]The benefit receipt estimate predicts a 70 percent increase in household level expenditure on the day of benefit receipt, relative to days of benefit non-receipt.

diture in week one of the benefit cycle is 24% (27%) higher than week four expenditure.[4] These estimates are roughly 21% (10%) smaller in magnitude than the estimates presented in Hastings and Washington (2010), which indicate that expenditure in week one of the benefit cycle is 30% higher than week four expenditure. Regardless, the estimates presented in Hastings and Washington (2010) are comparable to our own and appear to be subject to less concern relative to the the likelihood of benefit receipt estimates. We utilize these findings to support our recommendation to researchers to utilize states where SNAP benefits are distributed on a specific day or within a specific week of the calendar month in the event that the exact SNAP distribution dates would otherwise be unknown.

We make four contributions to the existing literature. Our primary contribution to the literature is a comparison of the following two estimates: the SNAP benefit receipt estimate and the likelihood of SNAP benefit receipt estimate (Goldin et al. (2020); Cotti et al. (2020); Castellari et al. (2017); Cotti et al. (2016)). Given the large discrepancies between these two sets of estimates, we believe that understanding the magnitude and sources of their differences is important. Our second contribution arises from the decomposition of the difference between the estimates, which applies more broadly to any methodological approach in which group or time averages are utilized as proxies for individual level stimuli.[5] Our decomposition illustrates that this methodology need not lead to attenuation bias and can provide consistent estimates, under some assumptions.[6] However, our findings also reveal that the assumptions necessary for consistency may be subject to violation when the effect of treatment or exposure has lagged effects on the outcome of interest. Our third contribution to the literature arises from the fact that SNAP benefits, in this state, are distributed across the four calendar weeks of the month. Our analysis allows us to control for intramonthly shocks to spending and relaxes concerns that the SNAP distribution date may be correlated with other monthly income shocks. Our final contribution are our suggestions to researchers and policy makers when implementing

[4]These estimates are generated measuring weeks relative to the day of SNAP disbursement. Explicitly, week one is defined as the day of benefit receipt and the six days that follow.

[5]School food policy: Handbury and Moshary (2021); Health insurance: Chen (2019)

[6]These findings that are well established in the measurement error literature but are often not immediately obvious to applied researchers as measurement error is often assumed to be classical.

and interpreting research that utilizes variation in the likelihood of SNAP benefit receipt to evaluate the effect of SNAP issuance on various outcomes.

While our data has unique advantages relative to other data sources, it is not without its own challenges. The first challenge arises from falsely assigning a SNAP distribution date to a specific household. The disbursement schedule of the state of residency allows us to infer the disbursement date based on the last name of the household. This procedure is worrisome if the household member who receives SNAP benefits has a different last name than the household member whose name is on file with the retailer. To address this concern, we perform a sensitivity analysis with SNAP households that consist of only one member and continue to find large and statistically significant discrepancies between the likelihood of benefit receipt estimate and the benefit receipt estimate. The second challenge is to establish the representativeness of our data set. Comparisons of SNAP participation rates nationally with SNAP participation rates in the areas surrounding the store locations indicate that the retailer is located in more affluent neighborhoods. Consistent with this finding, the households in our sample spend approximately $44 more per week with the retailer than the average SNAP household spends on food for at home consumption. Finally, Hastings and Shapiro (2018) point out the challenge of potential correlation between SNAP participation and the choice of the retailer. If SNAP issuance affects the intramonthly choices of where to shop, our study will only be able to provide insight into how SNAP issuance affects the intramonthly purchasing patterns of a household with the retailer of study. Given that our primary research objective is to better understand the difference between the likelihood of benefit receipt estimate and the benefit receipt estimate, we don't believe that single retailer data should prevent us from exploring the differences between these two methodological approaches in a meaningful way.

The remainder of the paper is structured as follows. Section 2 provides a brief background of the SNAP program. Section 3 discusses the data obtained and provides summary statistics. Section 4 presents comparisons of the likelihood of benefit receipt estimates and the benefit receipt estimates; in this section, we also decompose and quantify the sources of the differences between these two estimates. In Section 5 we replicate

Hastings and Washington (2010) in order evaluate the validity of an alternative strategy that utilizes only states that distribute benefits on one day or on a particular week of the month. Section 6 discusses the robustness of our findings to single person households and to an alternative estimation strategy which directly models lagged effects of SNAP issuance on expenditure. Section 7 discusses our findings and concludes with recommendations for researchers and policy makers.

2 SNAP Background

The Supplemental Nutrition Assistance Program (SNAP), formerly known as food stamps, is a federal aid program that is administered by the USDA. The stated objectives of the SNAP program are to reduce hunger, malnutrition and poverty through the provision of in-kind transfers to households who are eligible for benefits.[7] Current federal eligibility guidelines indicate that a household is eligible to receive SNAP benefits if they have less than $2,250 in savings, their income is less than 130 percent of the poverty rate and, in general, households must be engaged in employment activity in order to receive benefits (USDA-FNS).[8]

Although SNAP is a federal program, each state is responsible for distributing benefits to its residents.[9] Since 2004, benefits have been delivered electronically to households via the Electronic Benefits Transfer (EBT) system. Each SNAP household receives a card, similar to a debit card, that is electronically loaded with benefits on the appropriate distribution date for the household. Distribution dates for each household are determined

[7]Recent literature that considered the stated objectives of SNAP found that youth form SNAP receiving households are more likely to underconsume vitamins, be obese and suffer food insecurity (Battacharya and Currie (2001)), while an increase in benefits mitigates reduced caloric intake of SNAP recipients (Todd (2015)).

[8]There are some exceptions to the work requirement.

[9]States are given the ability to accept or decline policy options within the federal policy that can influence benefit amounts, benefit lengths and eligibility for benefits. For example, states can set their own income and/or deduction definitions to increase (or decrease) the benefit amount their residents receive. States can also manipulate benefit lengths by selecting longer or shorter recertification periods and can also apply for employment waivers which remove the restriction that participants must be working in order to receive benefits.

at the state level and all 50 states currently deliver benefits according to a monthly distribution cycle.[10]

SNAP is an in-kind transfer program. That is, households can only use SNAP benefits to purchase food that is meant to be prepared and consumed at home and (or) for seeds that can be used to plant a garden. Practically this means that households can purchase any form of food (e.g., baby formula, vegetables, frozen pizza, candy) so long as it is not heated or intended for in-store consumption (e.g., heated deli sandwiches, heated deli soups, heated rotisserie chicken are not SNAP eligible). Additionally, SNAP benefits cannot be used for alcohol or tobacco products.

Although there are no time restrictions on the amount of time a household can participate in the SNAP program, it is not unusual for households to cycle on and off of the SNAP program over time. In many states, households are required to recertify their eligibility after a period of six months; previous research has shown that SNAP participants drop off of the SNAP program around the time of recertification (Hastings and Shapiro (2018)).

3 Data

The data utilized in this paper was obtained from a supermarket retailer. The original data set provided by the retailer includes household level purchases generated from roughly twenty thousand households, that live in a particular state, between April of 2014 and September 2017. In its most granular form, the data set is provided at the universal product code (upc), date, store, household level.[11] Like many supermarket retailers, this retailer has developed algorithms that utilize loyalty card numbers and other matchable forms of payment (e.g., credit card numbers) in order to identify all purchases made by a household in the retailers stores. Additionally, the retailer sends promotional material

[10] The amount of benefits a SNAP household receives depends directly on their income and the size of their household. Specifically, the benefit amount is determined by a formula that indicates the maximum monthly allotment of benefits available to a household of a given size, then, from this maximum, 30% of the household's net monthly income is deducted and what remains is the household's benefit amount.

[11] The Universal Product Code is a 12 digit barcode that identifies each product.

to its customer base; as a result, the retailer can link the name and address of a specific household to purchases made at the retailer. The data we utilize includes an observation for each purchased item, along with its price and quantity for every household. In addition, we are able to observe the form of payment (e.g., credit card, cash, EBT-SNAP, EBT-WIC etc.), at the transaction level, which can also be linked to the items purchased by the household in that transaction.

SNAP benefits in the state of residence for these households are distributed monthly according to the first letter of the household's last name.[12] Benefit distribution days in this state begin on the fifth of each month and end on the twenty-third of each month. The exceptional detail of the retail data combined with the state's benefit disbursement schedule, allows us to determine the day and week of the calendar month that each household receives their SNAP benefits.

We also identify periods of SNAP adoption by the households in our sample. Following Hastings and Shapiro (2018), we identify SNAP adoption spells by household purchasing patterns in which there is a six month period where SNAP benefits are consecutively not used as a form of tender followed by a six month period in which SNAP benefits are consecutively used as a form of tender with the retailer.[13] Hastings and Shapiro (2018) validate this definition of SNAP adoption by analyzing government provided EBT transaction records. They find that this definition of SNAP adoption is correlated with a 96% chance of correctly identifying recent enrollment into the SNAP program for shoppers who conduct the majority of their spending with a supermarket retailer.[14] This is a relatively restrictive purchasing pattern as households must exhibit a considerable amount of loyalty to the retailer in order for a SNAP adoption spell to be identified. As such, we are able to identify adoption spells for roughly 850 households, approximately 4% of our initial household sample. One of the advantages of utilizing this subset of households is that we are able to test (and control) for the effect of the benefit cycle in the months

[12]Estimates of the frequency and percentage of households receiving SNAP benefits on a certain calendar day given they have experienced an adoption spell can be found in the appendix.

[13]In our data, we are able to distinguish SNAP EBT from WIC EBT and TANF EBT as forms of payment.

[14]They find that this supermarket transaction record is correlated with an 87% chance of correctly identifying recent enrollment in the SNAP program for all shoppers.

prior to SNAP adoption. We verify the robustness of our findings to the adoption-spell household restriction by also presenting results that utilize the full set of households.

Our primary outcome variables of interest are weekly total expenditures with the retailer.[15] In the paper we will collapse the purchasing data to the weekly level and evaluate sales outcomes over a seven day period. Our definition of the relevant seven day period will be constructed two ways: (1) weeks or seven day periods relative to the day of SNAP disbursement and (2) weeks or seven day periods relative to the first day of the calendar month. We evaluate and define the data relative to these two different measures of time so that we can generate the most relevant comparisons to the prior literature.

3.1 Summary Statistics

In the summary statistics that follow, we measure weeks as seven day periods following the disbursement of SNAP benefits (i.e., week one contains the day of SNAP receipt and the six days following that initial distribution date). Following the previous literature, we standardize a week as a seven day period and only consider the the first 28 days of the SNAP benefit cycle.

Table 1 provides mean overall spending for households over the weeks of the benefit cycle and separates these mean values prior to and following a period of SNAP adoption. Panel A reveals that the average household in our sample spends \$93 to \$97 dollars per week with the retailer of study prior to SNAP adoption. Panel B indicates that mean spending rises upon SNAP adoption to an average of \$124 per week; furthermore, in the months of a SNAP adoption spell, the first week of the benefit cycle illustrates considerably higher spending (\$142) relative to weeks two (\$124), three (\$116), and four (\$115). This simple comparison of means suggests that household spending in week one of the benefit cycle is, on average, 23% higher relative to week four of the benefit cycle.[16]

[15] The words "expenditures" and "sales" are used interchangeably.

[16] Table 8 presents mean overall spending for households over *calendar weeks* from the calendar week of SNAP disbursement and also separates these mean values prior to and following a period of SNAP adoption. Panel A reveals that the average household in our sample spends \$94 to \$95 dollars per calendar week with the retailer of study prior to SNAP adoption. Panel B indicates that mean spending rises upon SNAP adoption to an average of \$123 per week; furthermore, in the months of a SNAP adoption spell, the

To assess the representativeness of our retail data, we compare our summary statistics to the USDA's National Household Food Acquisition and Purchase Survey (FoodAPS). The FoodAPS survey is nationally representative and contains data of American households' food acquisitions. Spending on food items for at home consumption during the first week of the SNAP benefit cycle is about $122 for SNAP households in the FoodAPS; weekly spending declines to $71, $64, $63 for weeks two, three and four, respectively.[17] A comparison of the means, in the FoodAPS data, suggest that weekly spending in week one is twice as high as weekly spending in week four of the benefit cycle.

Relative to the FoodAPS data, the average SNAP household in our sample exhibits higher weekly spending with the retailer. This could, in part, be due to SNAP recipients in this particular state receiving higher SNAP benefit amounts relative to the national average. Between April 2014 and September 2017, SNAP households in this state received $270 in SNAP benefits, on average, compared to the national average of $255. Additionally, the retailer is located in ZIP Code Tabulation Areas (ZCTAs) with lower SNAP participation rates, suggesting higher levels of income in these areas.[18] From 2014 to 2017, the average SNAP participation rate across the ZCTAs where the retailer is located were between 9 and 11 percent.[19] In contrast, the national SNAP participation rate was consistently higher between 2014 and 2017, and averaged around 13 percent.[20] With respect to demographics and other household characteristics our sample aligns with national statistics. In 2018 the Census reported that on average 76 percent of the households that receive

first calendar week of the benefit cycle illustrates considerably higher spending ($135) relative to weeks two ($124), three ($118), and four ($115). This simple comparison of means suggests that household spending in week one of the benefit cycle is, on average, 17% higher relative to week four of the benefit cycle.

[17]These estimates are similar to Berning et al. (2016) that find $140 Week 1, Weeks 2-4 $67

[18]ZIP Code Tabulation Areas (ZCTAs) are generalized areal representations of United States Postal Service (USPS) ZIP Code service areas. The USPS ZIP Codes identify the individual post office or metropolitan area delivery station associated with mailing addresses. USPS ZIP Codes are not areal features but a collection of mail delivery routes. The term ZCTA was created to differentiate between this entity and true USPS ZIP Codes. ZCTA is a trademark of the U.S. Census Bureau; ZIP Code is a trademark of the U.S. Postal Service.

[19]SNAP participation estimates and benefit estimates data was taken from the Food and Nutrition Service of the USDA.

[20]Retailer location demographic data was drawn from Unites States Census Bureau. That is, statistics were estimated for all observable ZCTAs in which the retailer is located. This resulted in the exclusion of four ZCTAs among all retailer locations in the state of interest.

SNAP were white; our retailer is located in counties where on average 75 percent of the households are white. The average income among all ZCTAs that the retailer is located in and the national average over the researched time period differentiate minimally; \$57,229 (ZCTAs) vs. \$61,861 (national). Lastly, the proportion of households living in an owner-occupied housing unit is only two percentage points higher than the national average.

We compare summary statistics from our data to summary statistics from other data sources in order to shed light on the share of household spending that our retailer captures. In the six months prior to SNAP adoption the average monthly spending, with the retailer, for our household sample is approximately \$383; after SNAP adoption mean spending is approximately \$496 per month. These spending patterns are comparable to Hastings and Shapiro (2018), who utilize data very similar to our own, and find that average monthly SNAP-eligible spending with their retailer is \$355 prior to SNAP adoption and \$469 after SNAP adoption. Furthermore, upon SNAP adoption, the average household in our sample redeems \$147 in SNAP benefits per month as a form of tender with the retailer. Comparisons with the average SNAP benefit amount for a household in this particular state (\$270) suggest that our data capture 55% of the SNAP benefits awarded to these households, on average.

3.2 SNAP Disbursement & Daily Sales

In order to shed light on the effect of SNAP benefit disbursement on intramonthly expenditure patterns with the retailer, we evaluate changes in daily overall expenditure levels over the course of the benefit cycle. Explicitly we plot estimates from the following regression equation:

$$y_{ht} = \alpha + \sum_{i=0}^{30} \beta_i 1\{DaysSince = i\} x 1\{AdoptionSpell\} + \beta_{spell} 1\{AdoptionSpell\} + \gamma_{ym} + \gamma_{calendarday} + \gamma_{dow} + \gamma_h + \epsilon_{ht} \quad (1)$$

where y_{ht} represents the expenditure for household h on date t, $1\{DaysSince = i\}$ is a dummy variable that turns to one when the household is i days from benefit receipt, $1\{AdoptionSpell\}$ is an indicator that turns to one when the household is experiencing an adoption spell, γ_{ym}, $\gamma_{calendarday}$, γ_{dow}, and γ_h represent year-month, calendar day, day-of-week and household fixed effects respectively.[21] The dummy variable for 28 days from benefit receipt is omitted from Equation 1 and serves as the reference point for daily spending over the benefit cycle. Standard errors are clustered at the household level.

Figure 1 presents the β_i coefficients. Visual inspection of Figure 1 indicates that the effect of SNAP disbursement on intramonthly spending patterns has a statistically significant and positive impact for 10 to 12 days after benefit receipt. Notably, on the day that SNAP benefits are disbursed (i.e., β_0) spending is \$13 higher relative to 28 days after benefit receipt. The effect of disbursement on spending is roughly \$2 to \$4 one to three days after benefit receipt and \$1 to \$2 four to twelve days after benefit receipt.[22] Additional visual representations of the β_i coefficients for alternative specifications are available in the appendix.[23]

4 Comparison of Estimates

In order to compare the likelihood of benefit receipt estimate with the benefit receipt estimate, we structure the purchasing data around the four calendar weeks of the month. This data structure is comparable to Goldin et al. (2020) as well as Hastings and Washington (2010). We keep the first 28 days of each calendar month and define week one of the calendar month as the first seven calendar days of the month, so on and so forth.[24] Following Goldin et al. (2020), the likelihood that a household receives benefits in a given

[21] Zeros are imputed for zero purchasing days.

[22] The estimated coefficient on the dummy variable for experiencing an adoption spell (i.e., β_{spell}) is 15.66. This indicates that SNAP adoption produces a \$16 increase in daily spending across all days from SNAP benefit receipt.

[23] We present three additional figures in the appendix. First we remove the calendar day fixed effects. We then present results that do not include the adoption spell variable or the interaction with it and consequently remove the calendar day fixed effect. We then remove the data restriction and include all households (regardless of experiencing an adoption spell or not).

[24] Zeros are imputed for zero purchasing weeks.

week is defined by the number of days in the calendar week on which benefits are disbursed, divided by the number of days in the month for which benefits are scheduled to disburse. For example, in the first seven days of the calendar month, there are two days that benefits are scheduled to disburse out of ten days total in the month on which benefits are disbursed. Hence the estimated likelihood of receiving benefits in week one is 0.20.

We study the effect of benefit issuance on spending in the following regression framework:

$$Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} + \gamma_m + \gamma_h + \epsilon_{ht} \tag{2}$$

where $Sales_{ht}$ represents total expenditure for household h in calendar-week t, γ_m represents a calendar month-year fixed effect and γ_h represents a household fixed effect. We compare two estimates of $\widehat{\beta}_1$ by replacing $Issuance_{ht}$ with the following variables: (1) $\widehat{SNAPIssuance}_{ht}$, which is equal to the likelihood that a household receives benefits (i.e., $\widehat{SNAPIssuance}_{ht}$ is equal to 0.20, 0.30, 0.40 and 0.10 in weeks one, two, three and four (respectively) of the calendar month) and (2) $1\{SNAPIssuance_{ht}\}$, a dummy variable equal to one in the calendar week that the household is assigned to receive SNAP benefits according to the first letter of their last name. To help clarify the difference between the likelihood of benefit receipt and the benefit receipt variables (i.e., $\widehat{SNAPIssuance}_{ht}$ and $1\{SNAPIssuance_{ht}\}$), Figure 2 illustrates the values that the likelihood of benefit receipt and benefit receipt take for households who receive benefits in weeks one, two, three and four of the calendar month in the state of interest.

To control for income shocks that may consistently occur over the course of a calendar month, we interact $Issuance_{ht}$ with an indicator for whether or not the households is experiencing an adoption spell, while also controlling for whether or not the household is experiencing an adoption spell. This allows us to incorporate calendar-week fixed effects which control for intramonthly income shocks that are common to the household prior to and following SNAP adoption (e.g., recurring monthly payments, paycheck receipt, etc.). Regressions incorporating these additional controls take the following form:

$$Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} x 1\{AdoptionSpell\} + \beta_2 1\{AdoptionSpell\} + \gamma_{CW} + \gamma_m + \gamma_h + \epsilon_{ht} \quad (3)$$

where $1\{AdoptionSpell\}$ is an indicator for whether or not the household is in an adoption spell and γ_{CW} is a calendar-week fixed effect to control for intramonthly income shocks that are common for the households prior to and following SNAP adoption. All other variables are defined as above.

Table 2 compares the estimates of $\widehat{\beta_1}$ for the likelihood of benefit receipt estimate (Panel A) and the benefit receipt estimate (Panel B). Columns (1) and (2) of Table 2 provide estimates from Equation 3, while Columns (3) and (4) provide estimates from Equation 2 utilizing the sample of adoption spell households during a period of SNAP adoption and the full sample of households, respectively. Comparisons of these two estimates reveal that the likelihood of benefit receipt estimate is consistently larger than the benefit receipt estimate. Specifically, the likelihood of benefit receipt estimate indicates that households level expenditure increases by \$34 to \$45 in a week in which all benefits are issued compared with the benefit receipt estimates which indicates sales increases of \$15.72 to \$15.87 in the week in which benefits are issued, conditional on experiencing a SNAP adoption spell. Estimates of Equation 2, utilizing the full sample of households, reveal a similar upward bias: the likelihood of benefit receipt estimate indicates that household level sales increase by \$8.76 in a week in which benefits are issued compared with the benefit receipt estimate which indicates a sales increase of \$3.05 in the week on which benefits are issued. The likelihood of benefit receipt estimates are 2.1 to 2.8 times larger than the benefit receipt estimates across all four specifications. Additionally, the null hypothesis of equality for the likelihood of benefit receipt estimate and the benefit receipt estimate is rejected at the one percent significance level for all four specifications.[25]

[25] Panel C of Table 2 presents the p-values for the tests of equality. These tests were conducted utilizing the suest command in Stata.

We perform the same comparison of estimates at the daily level. For this comparison we collapse the data to the household-day level and impute zeros for zero purchasing days. Following Castellari et al. (2017), the likelihood that a household receives benefits on a given day is defined by whether or not benefits are disbursed on that calendar day, divided by the number of days in the month for which benefits are scheduled to disburse. Specifically, the estimated likelihood of receiving benefits on any day for which benefits are disbursed is 0.10. Our equation of interest for the daily level is defined as follows:

$$Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} x 1\{AdoptionSpell\} + \beta_2 1\{AdoptionSpell\} + \gamma_{CD} + \gamma_{DOW} + \gamma_m + \gamma_h + \epsilon_{ht} \quad (4)$$

where $Sales_{ht}$ represents total expenditure for household h on date t, $1\{AdoptionSpell\}$ is an indicator for whether or not the household is experiencing an adoption spell, γ_{CD} represents a calendar-day fixed effect, γ_{DOW} represents a day-of-week fixed effect, γ_m represents a calendar month-year fixed effect and γ_h represents a household fixed effect. We compare two estimates of $\widehat{\beta}_1$ by replacing $Issuance_{ht}$ with the following variables: (1) $\widehat{SNAPIssuance}_{ht}$, which is equal to the estimated probability that a household receives benefits on that day (i.e., $\widehat{SNAPIssuance}_{ht}$ is equal to 0.10 on days that SNAP benefits are disbursed in the state) and (2) $1\{SNAPIssuance_{ht}\}$, a dummy variable equal to one on the calendar day that the household is assigned to receive SNAP benefits according to the first letter of their last name.

Table 3 presents a comparison of the two estimates and reveals that the likelihood of benefit receipt estimate is consistently larger than the benefit receipt estimate. Specifically, the likelihood of benefit receipt estimate indicates that household level sales increase by \$23 on the day in which benefits are issued compared with the benefit receipt estimate which indicates sales increases of \$11.53 to \$11.91 on the day for which benefits are issued, conditional on experiencing a SNAP adoption spell. Estimates that utilize the full sample of households and that do not include the adoption spell interactions reveal a similar upward bias: the likelihood of benefit receipt estimate indicates that household level expenditures increase by $5.62 on the day in which benefits are issued compared with the benefit receipt estimate which indicates a sales increase of $1.97 on the day on which benefits are issued. The likelihood of benefit receipt estimate is 2.0 to 2.9 times larger than the benefit receipt estimate across all four specifications. Additionally, the null hypothesis of equality for the likelihood of benefit receipt estimate and the benefit receipt estimate is rejected at the one percent significance level for all specifications.

These findings suggest that the likelihood of benefit receipt estimate is biased upwards. In order to better understand the sources of bias and their magnitudes, we decompose the difference between the two estimates into two sources: (1) bias due to non-classical measurement error and (2) bias that arises from endogeneity.

4.1 Decomposition of the Difference Between the Estimates

The differences between the two estimates can be decomposed into two parts: (1) bias due to non-classical measurement error and (2) differences in endogeneity bias. For ease of exposition, we present our decomposition in a simple regression model framework. Under the assumption that the measurement error is uncorrelated with each of the k control variables, the multiple regression model analog to this decomposition produces, effectively, the same result.[26] We begin our decomposition by building a general measurement error framework and define the likelihood of benefit receipt ($\widehat{Issuance}$) as a function of benefit receipt ($1\{Issuance\}$) and measurement error (u). Specifically, we define $\widehat{Issuance}$ as follows:

$$\widehat{Issuance} = 1\{Issuance\} + u \tag{5}$$

Note that when measurement error is present in an indicator variable, it will always violate the assumptions of classical measurement error. The measurement error will by definition be correlated with the true variable of interest. In this specific context, $\widehat{Issuance}$

[26]The multiple regression decomposition is available in the appendix. Naturally, each of the k control variables are also assumed to be exogenous to the outcome. This decomposition results is the exact same analog of what is derived here with the effect of the additional controls partialled out.

takes the values 0.20, 0.30, 0.40 and 0.10 in weeks one, two, three and four (respectively) of the calendar month. As a result, $u < 0$ when $1\{Issuance\} = 1$ and $u > 0$ when $1\{Issuance\} = 0$ and the $cov(1\{Issuance\}, u) < 0$.

Our true model is of the following form:

$$Sales = \beta 1\{Issuance\} + \epsilon \tag{6}$$

We plug Equation5 in Equation 6 to obtain the estimated model below:

$$Sales = \beta 1\{Issuance\} + \epsilon = \beta(\widehat{Issuance} - u) + \epsilon = \beta \widehat{Issuance} + \epsilon - \beta u \tag{7}$$

Note that the $\widehat{\beta}$ from our estimated model is defined as follows (Bound et al. (2001)):

$$\begin{aligned} plim(\widehat{\beta}_{\widehat{Issuance}}) = \frac{cov(\widehat{Issuance}, Sales)}{var(\widehat{Issuance})} &= \frac{cov(1\{Issuance\} + u, \beta 1\{Issuance\} + \epsilon)}{var(1\{Issuance\} + u)} \\ &= \frac{\beta\sigma_I^2 + \sigma_{I,\epsilon} + \beta\sigma_{u,I} + \sigma_{u,\epsilon}}{\sigma_I^2 + \sigma_u^2 + 2\sigma_{u,I}} \end{aligned} \tag{8}$$

where σ_I^2 represents the variance of $1\{Issuance\}$, $\sigma_{I,\epsilon}$ represents the covariance between $1\{Issuance\}$ and the error term (ϵ), $\sigma_{u,I}$ represents the covariance between the measurement error (u) and $1\{Issuance\}$, $\sigma_{u,\epsilon}$ is the covariance between the measurement error (u) and the error term (ϵ) and σ_u^2 is the variance of the measurement error.

We re-write the Equation 8 as follows:

$$plim(\widehat{\beta}_{\widehat{Issuance}}) = \beta(1 - b_{u,\widehat{Issuance}}) + \frac{\sigma_{u,\epsilon}}{\sigma_I^2 + \sigma_u^2 + 2\sigma_{u,I}} + \frac{\sigma_{I,\epsilon}}{\sigma_I^2 + \sigma_u^2 + 2\sigma_{u,I}} \tag{9}$$

where $b_{u,\widehat{Issuance}}$ is equal to $\frac{\sigma_u^2 + \sigma_{u,I}}{\sigma_I^2 + \sigma_u^2 + 2\sigma_{u,I}}$. It can similarly be shown that the $plim(\widehat{\beta}_{1\{Issuance\}})$ from equation 6 is equal to $\frac{\beta\sigma_I^2 + \sigma_{I,\epsilon}}{\sigma_I^2}$. Given the probability limits of the two estimates, we define the probability limit of the difference between the two estimates as $\Delta = plim(\widehat{\beta}_{\widehat{Issuance}}) - plim(\widehat{\beta}_{1\{Issuance\}})$. We define Δ below:

$$\Delta = \sigma_{I,\epsilon}\left[\frac{1}{\sigma_I^2+\sigma_u^2+2\sigma_{u,I}} - \frac{1}{\sigma_I^2}\right] - \beta b_{u,\widehat{Issuance}} + \frac{\sigma_{u,\epsilon}}{\sigma_I^2+\sigma_u^2+2\sigma_{u,I}} \tag{10}$$

Given the structure of the SNAP benefit cycle and the definition of $\widehat{Issuance}$, we are able to assign numerical values to the variance of $Issuance$ (σ_I^2) and the variance of $\widehat{Issuance}$ ($\sigma_I^2+\sigma_u^2+2\sigma_{u,I}$). Explicitly, we impose that $\widehat{Issuance}$ takes the values 0.20, 0.30, 0.40 and 0.10 with a probability of 0.25 and that $1\{Issuance\}$ takes the value one with a probability of 0.25 and the value zero with a probability of 0.75. Given this, $\sigma_I^2 = 0.1875$ and $\sigma_I^2+\sigma_u^2+2\sigma_{u,I} = 0.0125$. Hence, Δ can be re-written as follows:

$$\Delta = 74.67\sigma_{I,\epsilon} + 80\sigma_{u,\epsilon} - \beta b_{u,\widehat{Issuance}} \tag{11}$$

The difference between the two estimates can be decomposed into two parts. First, $74.67\sigma_{I,\epsilon} + 80\sigma_{u,\epsilon}$ captures the difference between the two estimates due to differences in endogeneity bias. In this scenario, $74.67\sigma_{I,\epsilon}$, represents the difference between the two estimates attributable to bias that stems from the endogeneity of benefit issuance. If benefit issuance is endogenous, the degree of bias will be roughly 75 times larger in the likelihood of benefit receipt estimate. This is due to the fact that the variance of the likelihood of benefit receipt is smaller than the variance of benefit receipt. Furthermore, $80\sigma_{u,\epsilon}$, represents additional endogeneity bias that the likelihood of benefit receipt estimate is subject to because the likelihood of benefit receipt is measured with error. The second component contributing to differences between these two estimates is $\beta b_{u,\widehat{Issuance}}$; this part of the difference between these two estimates is attributable to bias stemming from measurement error.

Given that the day of benefit receipt is known in our data (i.e., $1\{Issuance\}$ is known), we are able to define u as the difference between $\widehat{Issuance}$ and $1\{Issuance\}$. With this information, $b_{u,\widehat{Issuance}}$ can be estimated via ordinary least squares by defining the following regression equation:

$$u = b\widehat{Issuance} + v \tag{12}$$

where the b is equal to $b_{u,\widehat{Issuance}}$ (Bound et al. (2001)). Table 4 presents the estimates of

$b_{u,\widehat{Issuance}}$ and illustrates that $b_{u,\widehat{Issuance}}$ is generally not statistically nor economically significant; the estimates range from -0.01 to 0.09. The estimate of $b_{u,\widehat{Issuance}}$, when utilizing the full sample of households, is statistically significant at the five percent significance level, but is not economically significant. If the benefit receipt estimate is a consistent estimator of the effect of SNAP issuance on weekly sales, then bias from non-classical measurement error would only explain two percent of the difference between the likelihood of benefit receipt estimate and the benefit receipt estimate when utilizing the full set of households.[27] Our estimates are aligned with econometric theory as the probability limit of $b_{u,\widehat{Issuance}}$ can be shown to be zero.[28] As a result, $\widehat{Issuance}$ falls into the optimal predictor error model framework developed in Hyslop and Imbens (2001).

4.2 Discussion of Results

Comparisons of the likelihood of benefit receipt estimate to the benefit receipt estimate, indicate that the likelihood of benefit receipt estimate is generally 2.0 to 2.9 times larger than the benefit receipt estimate. The decomposition of the differences between these two estimates provides compelling evidence to suggest that all of the difference between these two estimates is attributable to endogeneity bias. There are two forms of endogeneity bias that contribute to these differences. First, endogeneity bias attributable to the endogeneity of true issuance will be 75 times more amplified in the likelihood of benefit receipt estimate relative to the benefit receipt estimate. Second, if measurement error is endogenous, the bias stemming from this source of endogeneity will only be present in the likelihood of benefit receipt estimate.

Given the semi-random assignment of dates for which a household receives SNAP

[27]The difference between the two estimates that is due to non-classical measurement error is given by $-\beta b_{u,\widehat{Issuance}}$. If the benefit receipt estimate is a consistent estimator of β then $-\beta b_{u,\widehat{Issuance}} = -3.053x0.0345 = -0.1053$ The difference between the likelihood of benefit receipt estimate and the benefit receipt estimate for the full sample of households is \$4.702. The percentage of this difference accounted for by non-classical measurement error bias is therefore $\frac{0.1053}{4.702}x100 = 2.24\%$. Also note that the bias from non-classical measurement error is pulling these two estimates closer rather than farther apart.

[28]The fact that the probability limit of $b_{u,\widehat{Issuance}} = 0$ can be shown if one imposes that $\widehat{Issuance}$ takes on the values 0.20, 0.30, 0.40 and 0.10 with a probability of 0.25 and that true issuance takes the value one with a probability of 0.25 and the value zero with a probability of 0.75.

benefits, the SNAP benefit cycle literature has generally assumed that the date of issuance is random and is therefore uncorrelated with other factors that may influence purchasing patterns throughout the month (i.e., $\sigma_{1\{Issuance\},\epsilon} = 0$). Imposing this assumption implies that the only source of differences between these two estimates is endogenous measurement error. The measurement error present in this setting only takes positive values in weeks in which the household does not receive SNAP benefits. Additionally, Figure 1 indicates that the effect of SNAP benefits on spending patterns are not exclusively contained to the first day nor the first week of benefit receipt. As a result, weeks with higher expenditure due to treatment in the prior day(s) or week(s) also have positive measurement error, which may lead to a positive correlation between the measurement error, u and the error term ϵ. In other words, we suspect that the likelihood of benefit receipt estimate captures both the contemporaneous effect of benefit receipt and the lagged effect of benefit receipt.

A natural solution for endogeneity bias is an instrumental variables approach. However, given the source of endogeneity, it is unclear what variables might serve as good instruments in the case where SNAP distribution days are unknown to the researcher. Another solution might be to focus on states where SNAP benefits are distributed on a single day of the month or on a single week of the month. Most of the states that distribute SNAP benefits on a single day of the month do so on the first of each month. A potential disadvantage of this strategy is that the first of the month may be correlated with other income shocks such as cash welfare payments, rent payments and(or) other recurring monthly payments. In the next section, we gauge the extent to which the first of the month may lead to biased estimates by replicating Hastings and Washinton (2010) who study the effect of the SNAP benefit cycle in a state where all benefits are distributed on the first of the month.

5 Replication

In this section we replicate Hastings and Washington (2010) who evaluate the spending patterns of SNAP households in a state where all benefits are distributed on the first of the calendar month. This exercise is performed to evaluate the extent to which the first of the month may be endogenous due to recurring monthly payments that also occur at the beginning of the month. Given that SNAP distribution dates are semi-randomly assigned and distributed throughout the first four calendar weeks of the month in our data, we believe that our estimates are less likely to be confounded with other intra-monthly shocks to disposable income (e.g., cash welfare payments, paychecks and rent or utility payments). Furthermore, we are able to control for intramonthly shocks to disposable income that remain constant prior to and following SNAP adoption.

In order to perform the most comparable replication, we define weeks of spending relative to the day of SNAP disbursement (i.e., week one contains the day of SNAP disbursement and the six days that follow). Our equation of interest is defined as follows:

$$y_{ht} = \alpha + \sum_{i=2}^{4} \beta_i 1\{week = i\} + \sum_{i=1}^{4} \beta_{i,spell} 1\{week = i\} x 1\{AdoptionSpell\} + \gamma_{year-month} + \gamma_h + \epsilon_{ht} \quad (13)$$

where y_{ht} is the outcome of interest for household, h, in week, t. The variable $1\{week = i\}$ is an indicator for which week of the benefit cycle household h is in, $1\{AdoptionSpell\}$ is an indicator for whether or not the household is experiencing an adoption spell, $\gamma_{year-month}$ is a year-month fixed effect for the year-month at the start of the given week (i.e., if the week of the benefit cycle starts on 1/15/20 then the year-month fixed effect is 1/20) and γ_h is a household fixed effect. Standard errors are clustered by the first letter of the households last name as this is what determines the day that SNAP benefits are received throughout the month.

We evaluate log(sales), level sales, log(food sales), level food sales and the decision to shop (i.e., $1\{Sales > 0\}$). Log sales outcomes are naturally going to drop zeros on the left hand side. Level sales outcomes retain weeks of zero sales, which have been imputed by the researchers. Table 5 presents the results.

Table 5 supports the identifying assumption that the assigned date of distribution is uncorrelated with other shocks to household spending patterns. Prior to a household adopting SNAP benefits, estimates for the week of the benefit cycle do not indicate significant nor sizable changes in households expenditure overall, expenditures on food items nor in the probability of shopping with the retailer.[29] In contrast, when a household is experiencing a SNAP adoption spell, our estimates indicate that mean total spending in week four of the benefit cycle is 23.6 percent ($28) lower than spending in week one of the benefit cycle. Our estimates also indicate that mean food spending is 27.0 percent ($25) lower and that the household is four percentage points less likely to shop at the retailer in week four of the benefit cycle, relative to week one of the benefit cycle. Our results for total sales and food sales are moderately smaller in magnitude than Hastings and Washington (2010) who estimate a 29.9 percent decline in sales between weeks one and four.

Following Hastings and Washington (2010) we also perform a McDonald Moffit decomposition in order to decompose the expected sales outcome, unconditional on positive purchases, by the intensive and extensive margins. For this exercise, we condition on the household experiencing an adoption spell (i.e., $1\{AdoptionSpell\} = 1$). The expected sales outcome, unconditional on the decision to shop, can be written as:

$$E(Sales) = \left(\Delta 1\{\widehat{Shop_{ht}}\}\right) x \left(\overline{Sales}|Sales > 0\right) + \left(\Delta\widehat{Sales}|Sales > 0\right) x \left(\overline{1\{Shop_{ht}\}}\right) \quad (14)$$

where $\left(\Delta 1\{\widehat{Shop_{ht}}\}\right) x \left(\overline{Sales}|Sales > 0\right)$ explains changes in sales due to changes in the extensive margin (i.e., whether or not to shop) and $\left(\Delta\widehat{Sales}|Sales > 0\right) x \left(\overline{1\{Shop_{ht}\}}\right)$ represents changes due to changes in the intensive margin (i.e., how much to buy). We will present the change in sales between weeks one and four; hence Δ refers to the difference in predicted outcomes between weeks one and four. We present the means and standard errors of 100 estimates produced from 10 percent random samples (Hastings and Washington (2010)).

The estimated unconditional change in total expenditure (-$27.62), presented in Table

[29] For level sales and level food spending, the results suggest that spending increases by roughly $2 in week two of the benefit cycle. However, these coefficients are only significant at the 10% significance level and represent a fairly small (2%) change relative to mean pre-adoption spending.

6, is strikingly similar to the ordinary least squares estimates presented in Table 5 for level sales outcomes (-$27.96). Relative to Hastings and Washington (2010), the magnitudes of our estimates are a bit larger (overall change of $27.62 vs. $9.60) and the ratio of the change along the intensive margin to the total change is also slightly larger in our setting (0.74 vs. 0.60). The differences in magnitudes could simply be due to the fact that we condition on SNAP adoption, while Hastings and Washington (2010) condition on EBT use within the past year. Similar to Hastings and Washington (2010), we conclude that the majority of the change in total spending is attributable to changes in spending behavior along the intensive margin.

There are moderate differences between the estimates presented in this paper and Hastings and Washington (2010). Perhaps the most notable difference is the comparison of the effect on log total sales. Our estimates indicate a 23.6 percent decline in spending between weeks one and four while Hastings and Washington estimate a 29.9 percent decline. It is well known that the first of the month is correlated with other income shocks–specifically, cash welfare payments, in the state of study for Hastings and Washington (2010). This confounder might create an upward bias; hence, the fact that our estimates are 10% smaller when evaluating changes in food expenditure and 21% smaller when evaluating total expenditure is perhaps expected. Regardless, the estimates in Hastings and Washington (2010) are quite comparable to our own and appear to be subject to less endogeneity relative to the likelihood of benefit receipt estimates.

6 Robustness

We evaluate the robustness of our findings to single-person households and to an alternative estimation strategy that models the lagged effect of SNAP issuance on sales outcomes. In both cases we continue to find large discrepancies between the likelihood of benefit receipt estimates and the benefit receipt estimates. For single-person households we find that the likelihood of benefit receipt estimates are 2.7 to 12.7 times larger than the benefit receipt estimates when evaluating the effect of issuance on total weekly expenditure. Additionally, the null hypothesis of equivalence among the two estimates is rejected for all three of the specifications presented in the appendix. When the lagged effect of SNAP issuance on sales outcomes is incorporated into the estimation strategy, the likelihood of benefit receipt estimates are 1.8 to 2.7 times larger than the benefit receipt estimates. The null hypothesis of equality between the two estimates is rejected for all specifications at the ten percent significance level and is rejected at the one percent significance level when the full sample of households are utilized. Additional details on the different subset of households, the alternative estimation strategy and the results are available in the appendix.

7 Discussion & Conclusion

This paper compares and contrasts estimation strategies that utilize variation in SNAP benefit receipt (benefit receipt estimates) to estimation strategies that utilize variation in the likelihood of SNAP benefit receipt (likelihood of benefit receipt estimates) to understand the effect of SNAP issuance on intramonthly expenditures for SNAP beneficiaries. We find that the likelihood of benefit receipt estimates are 2.0 to 2.9 times larger than the benefit receipt estimates. We decompose the differences between these two estimation strategies and find that all of the difference between the estimates is attributable to endogeneity bias. Explicitly, there are two forms of endogeneity bias that contribute to the differences. First, endogeneity bias attributable to the endogeneity of the timing of benefit receipt will be 75 times more amplified in the likelihood of benefit receipt estimates relative to the benefit receipt estimates. Second, bias attributable to endogenous measurement error, which will only be present in the likelihood of benefit receipt estimates.

The SNAP benefit cycle literature generally assumes the timing of benefit receipt to be exogenous due to the semi-random assignment of distribution dates determined at the state level. If the exogeneity of the timing of issuance is assumed to be true, the only source of differences between the likelihood of benefit receipt estimates and the benefit receipt estimates is endogenous measurement error. We have reason to believe that measurement error is endogenous in this setting due to the fact that the effect of SNAP benefit receipt on expenditures is not entirely concentrated on the first day or within the first week of benefit receipt and that measurement error only takes positive values on the days or weeks of no benefit receipt. As a result, weeks with higher expenditure due to benefit receipt in the prior day(s) or week(s) also have positive measurement error, which may lead to a positive correlation between measurement error and the error term. In other words, we believe that the likelihood of benefit receipt estimate captures both the contemporaneous effect of benefit receipt as well as the lagged effect of benefit receipt, leading to overestimates. Our findings illustrate that empirical approaches in which group or time averages are utilized as a proxy for individual stimuli may be subject to endogeneity bias when the effect of treatment or exposure has a dynamic effect on the outcome of interest.

A natural solution to endogeneity bias is an instrumental variables approach. However, given the source of endogeneity, it is unclear what variables might serve as good instruments when more information regarding the day of SNAP disbursement is unavailable.[30] Another solution would be to focus on states where SNAP benefits are all disbursed on a specific day or within a specific week of the calendar month.[31] To asses the validity of this solution, we replicate Hastings and Washington (2010) who evaluate the effect of SNAP disbursement in a state where all benefits are distributed on the first of the month. We find a 23.6 (27.0) percent decline in overall (food) spending between weeks one and four of the SNAP benefit cycle. Our findings are moderately smaller in magnitude relative to Hastings and Washington (2010) who find a 29.9 percent decline in spending between weeks one and four of the SNAP benefit cycle. Given that the estimates in Hastings and Washington (2010) are quite comparable to our own, we believe that estimates generated by states where SNAP benefits are distributed on a single day or within a single week of the month are less likely to be subject to the same degree of endogeneity bias that the likelihood of benefit receipt estimates suffer.[32]

[30]Haider and Stephens (2020) show that the assumption about invariant missclassification rates across all values of the instrument, when instrumental variable are utilized to correct for misclassification in a binary regressor, is invalid.

[31]First of the month: Alaska, Nevada, New Hampshire, North Dakota, Rhode Island, South Dakota, Vermont, Virgin Islands. Single week: Connecticut, Hawaii, Maine, Montana, New Jersey, Wyoming (USDA).

[32]While we cannot explicitly estimate whether endogeneity bias due distribution on the first of the month is present in Hastings and Washington (2010), we interpret the similarity of our result to theirs as evidence to suggest that the magnitude of endogeneity bias due to the first of the month is either relatively small or null.

In conclusion, researchers utilizing variation in a group or time averages, in place of an otherwise unknown treatment variable, should exercise extreme caution. Our research suggests that group or time averages are likely to produce biased estimates when the effect of treatment is not exclusively concentrated in the time period of treatment. In the context of the SNAP benefit cycle, our research suggests that for the outcome of sales the likelihood of benefit receipt estimate is positively biased and roughly 2.0 to 2.9 times larger than it should be.[33] We strongly encourage researchers utilizing the likelihood of benefit receipt estimate to perform robustness checks by specifically looking at states that distribute all benefits on the first day of the calendar month and with states that distribute all benefits during a specific week of the calendar month. Furthermore, differences in the magnitudes and signs between these robustness checks and the main results should be taken very seriously when interpreting results and when making policy recommendations.

[33]The two other papers that utilize the likelihood of benefit receipt estimate to evaluate the impact of SNAP issuances on sales are Goldin et al. (2020) and Castellari et al. (2017). Goldin et al. (2020) utilize cross-state variation and, in some specifications, within state variation in the likelihood of benefit receipt in order the estimate the effect of SNAP issuance on store level retail sales. Castellari et al. (2017) utilize within state variation in the likelihood of benefit receipt to estimate the effect of SNAP issuance on household level purchases. While the identification assumptions when making use of cross-state variation are slightly different, we are not convinced that cross-state variation in the likelihood of benefit receipt would adequately address the source of endogenity we discuss in this paper.

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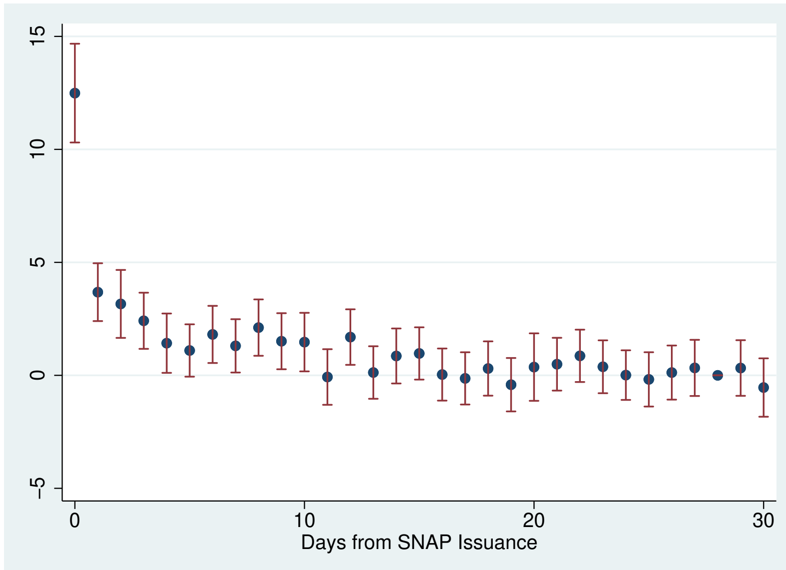
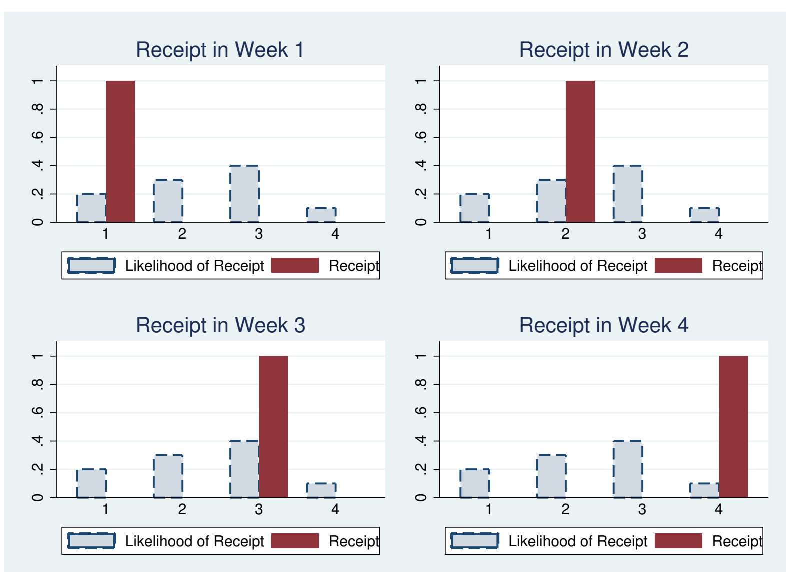
8 Figures

Figure 1: Mean Spending Over the Benefit Cycle | SNAP Adoption



Note: The estimated coefficient on the dummy variable for experiencing an adoption spell (i.e., β_{spell}) is 15.66. This indicates that SNAP adoption produces a $16 increase in daily spending across all days from SNAP benefit receipt.

Figure 2: Likelihood of Benefit Receipt vs. Benefit Receipt



9 Tables

Table 1: Summary Statistics Over the Benefit Cycle

Weeks Measured Relative to Day of SNAP Receipt

Panel A: Prior to SNAP Adoption	Overall	Week 1	Week 2	Week 3	Week 4
Sales ($)	95.69	95.18	97.68	93.61	96.27
	(118.19)	(112.37)	(122.32)	(111.45)	(126.05)
HH-Week Observations	35,932	9,066	9,045	8,957	8,864

Panel B: Post SNAP Adoption	Overall	Week 1	Week 2	Week 3	Week 4
Sales ($)	123.97	141.86	123.69	115.6	114.86
	(133.12)	(139.38)	(129.06)	(134.14)	(127.87)
HH-Week Observations	38,886	9,681	9,697	9,735	9,773

847 Adoption Households
Standard errors in parenthesis

Note: Following Hastings and Shapiro (2018), we identify SNAP adoption spells by household purchasing patterns in which there is a six month period where SNAP benefits are consecutively not used as a form of tender followed by a six month period in which SNAP benefits are consecutively used as a form of tender with the retailer. Panel A presents average spending over weeks from the assigned SNAP disbursement date in the six months prior to SNAP adoption and Panel B presents average spending over weeks from the assigned SNAP disbursement date in the months following SNAP adoption. Week 1 is defined as the day of assigned benefit receipt plus the 6 days following. Week 2 is defined as the seven days following Week 1, so on and so forth.

Table 2: The Effect of Issuance on Weekly Sales: Comparison of Estimates

Panel A: Likelihood of Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$\widehat{Issuance}_{ht}$ x 1{Adoption Spell}	44.67***	33.56***		
	(7.097)	(5.879)		
1{AdoptionSpell}	20.18***	22.96***		
	(2.578)	(2.360)		
$\widehat{Issuance}_{ht}$			33.56***	7.755***
			(5.911)	(0.608)
Calendar Week f.e.	X			
Observations	75,848	75,848	39,264	2,758,148
R-Squared	0.363	0.362	0.3922	0.387

Panel B: Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$1\{Issuance_{ht}\}$ x 1{Adoption Spell}	15.87***	15.72***		
	(1.780)	(1.771)		
1{AdoptionSpell}	27.38***	27.42***		
	(1.969)	(1.968)		
$1\{Issuance_{ht}\}$			15.72***	3.053***
			(1.781)	(0.174)
Calendar Week f.e.	X			
Observations	75,848	75,848	39,264	2,758,148
R-Squared	0.364	0.363	0.394	0.387

Panel C: Comparison of Estimates

	(1)	(2)	(3)	(4)
$\frac{\beta_{\widehat{Issuance}}}{\beta_{1\{Issuance\}}}$	2.81	2.13	2.13	2.54
p-value for null of equality	0.000	0.002	0.002	0.000
Household Sample	Adoption Spell Households	Adoption Spell Households	Adoption Spell Households	All Households
Sample ∣ 1{Adoption Spell} = 1	No	No	Yes	No

All regressions include household and year-month fixed effects
Standard errors in parenthesis and clustered at the household level
Tests for equality were generated using the suest command in Stata
For tests of equality, household fixed effects were incorporated by de-meaning all variables.

Table 3: The Effect of Issuance on Daily Sales: Comparison of Estimators

Panel A: Likelihood of Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$\widehat{SNAPIssuance}_{ht}$ x 1{Adoption Spell}	23.11***	23.36***		
	(2.434)	(2.094)		
1{AdoptionSpell}	3.667***	3.659***		
	(0.272)	(0.270)		
$\widehat{SNAPIssuance}_{ht}$			23.36***	5.620***
			(2.094)	(0.164)
Calendar Day Fixed Effect	X			
Observations	576,173	576,173	296,300	26,394,723
R-Squared	0.069	0.069	0.0721	0.093

Panel B: Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$1\{SNAPIssuance_{ht}\}x1\{AdoptionSpell\}$	11.53***	11.91***		
	(1.013)	(1.000)		
1{AdoptionSpell}	4.048***	4.035***		
	(0.265)	(0.265)		
$1\{SNAPIssuance_{ht}\}$			11.91***	1.970***
			(1.001)	(0.0691)
Calendar Day Fixed Effect	X			
Observations	576,173	576,173	296,300	26,394,723
R-Squared	0.070	0.070	0.0737	0.093

Panel C: Comparison of Estimates

	(1)	(2)	(3)	(4)
$\frac{\beta_{\widehat{Issuance}}}{\beta_{1\{Issuance\}}}$	2.00	1.96	1.96	2.85
p-value for null of equality	0.000	0.000	0.000	0.000
Household Sample	Adoption Spell Households	Adoption Spell Households	Adoption Spell Households	All Households
Sample | 1{Adoption Spell} = 1	No	No	Yes	No

All regressions include household, year-month and day-of-week fixed effects
Standard errors in parenthesis and clustered at the household level
Tests for equality were generated using the suest command in Stata
For tests of equality, household fixed effects were incorporated by de-meaning all variables.

Table 4: Estimates of $b_{u,\widehat{Issuance}}$

	(1)	(2)	(3)	(4)
Bias from Measurement Error	-0.0134	0.0839	0.0839	0.0345**
	(0.0144)	(0.0684)	(0.0688)	(0.0141)
Year-Month f.e.	X	X	X	X
Household f.e.	X	X	X	X
Calendar Week f.e.	X			
1{Adoption Spell}	X	X		
Observations	75,848	75,848	39,264	2,758,148
R-squared	0.006	0.000	0.001	0.000
Household Sample	Adoption Spell Households	Adoption Spell Households	Adoption Spell Households	All Households
Sample | 1{Adoption Spell} = 1	No	No	Yes	No

Standard errors in parenthesis
Standard errors clustered at the household level

Table 5: Benefit Cycle Purchasing Patterns by Adoption Spells

	Log(Sales)	Sales	Log(Food)	Food	1{Shop}
1{week=2}	0.0144	2.552*	0.00915	1.818*	0.00311
	(0.0143)	(1.379)	(0.0146)	(1.053)	(0.00673)
1{week=3}	-0.00908	-1.607	-0.00127	-0.322	-0.000825
	(0.0175)	(2.037)	(0.0126)	(1.159)	(0.00707)
1{week=4}	0.0127	1.069	0.00982	0.789	-0.00480
	(0.0156)	(1.703)	(0.0158)	(1.138)	(0.00582)
1{AdoptionSpell}	0.419***	51.14***	0.484***	45.36***	0.111***
	(0.0160)	(2.150)	(0.0217)	(1.803)	(0.00768)
1{week=2} x1{AdoptionSpell}	-0.138***	-20.75***	-0.170***	-19.82***	-0.0297***
	(0.0233)	(2.328)	(0.0311)	(1.942)	(0.00781)
1{week=3} x 1{AdoptionSpell}	-0.196***	-24.63***	-0.242***	-23.72***	-0.0392***
	(0.0144)	(1.685)	(0.0126)	(1.522)	(0.00727)
1{week=4}x 1{AdoptionSpell}	-0.236***	-27.96***	-0.270***	-24.73***	-0.0407***
	(0.0161)	(2.192)	(0.0214)	(1.711)	(0.00769)
Year-Month f.e.	X	X	X	X	X
Household f.e.	X	X	X	X	X
Observations	61,504	74,818	59,712	74,818	74,818
R-squared	0.337	0.363	0.306	0.363	0.159

Standard errors in parenthesis
Standard clustered by first letter of households last name
847 households for which we can identify an adoption spell

Table 6: Extensive and Intensive Margins - Conditional on Adoption

	Δ in Unconditional Mean Expenditure	Δ in Expenditure on Extensive Margin	Δ in Expenditure on Intensive Margin	$\frac{\Delta Intensive}{\Delta Total}$
Overall	-27.62	-7.28	-20.34	0.74
	(6.62)	(2.45)	(6.01)	

Standard errors in parenthesis

10 Appendix

10.1 SNAP Disbursement Days

Table 7: Calendar Disbursement Days of SNAP Benefits

Disbursement Days	Frequency	Percentage
5	105	12.4
7	109	12.87
9	95	11.22
11	71	8.38
13	76	8.97
15	90	10.63
17	100	11.81
19	93	10.98
21	38	4.49
23	70	8.26
Total	847	100

Note: This table depicts the frequency and percentage of households receiving SNAP benefits on a certain calendar day given they experienced an adoption spell.

10.2 Summary Statistics for Calendar Weeks

Table 8: Summary Statistics Over the Benefit Cycle

Weeks Measured Relative to Calendar Week of SNAP Receipt

Panel A: Prior to SNAP Adoption	Overall	Week 1	Week 2	Week 3	Week 4
Sales ($)	94.70	94.60	95.10	94.10	95.01
	(119.48)	(120.59)	(120.47)	(111.30)	(125.16)
HH-Week Observations	36,584	9,146	9,146	9,146	9,146

Panel B: Post SNAP Adoption	Overall	Week 1	Week 2	Week 3	Week 4
Sales ($)	122.80	134.59	123.93	118.11	114.57
	(131.99)	(138.49)	(128.22)	(133.34)	(126.74)
HH-Week Observations	39,264	9,816	9,816	9,816	9,816

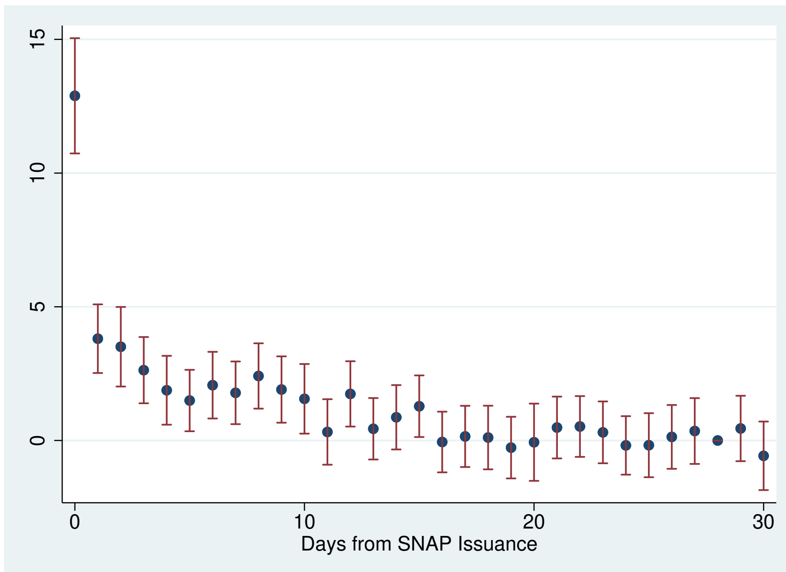
847 Adoption Households
Standard errors in parenthesis

Note: Following Hastings and Shapiro (2018), we identify SNAP adoption spells by household purchasing patterns in which there is a six month period where SNAP benefits are consecutively not used as a form of tender followed by a six month period in which SNAP benefits are consecutively used as a form of tender with the retailer. Panel A presents average spending over weeks from the assigned SNAP disbursement date in the six months prior to SNAP adoption and Panel B presents average spending over weeks from the assigned SNAP disbursement date in the months following SNAP adoption. Week 1 is defined as the calendar week on which benefits are assigned to be issued. Week 2 is the following calendar week, so on and so forth.

10.3 Mean Spending Over the Benefit Cycle

This subsection presents the results of three alternative specifications to Figure 1. Figure 3 presents the estimates that are produced when calendar day fixed effects are not included.

Figure 3: Mean Spending Over the Benefit Cycle | SNAP Adoption

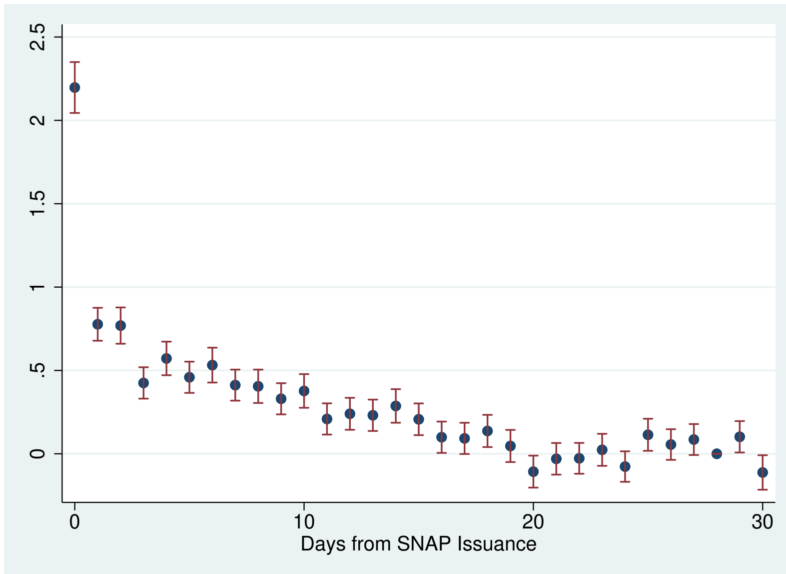


Estimates from Figure 3 are produced by estimating Equation 1 without calendar day fixed effects.

Figure 4 present estimates of the effect of SNAP distribution on spending that do not condition on the household experiencing an adoption spell. Figure 4 utilizes all of the households in the sample. The estimating equation is also detailed below.

$$y_{ht} = \alpha + \sum_{i=0}^{30} \beta_i 1\{DaysSince = i\} + \gamma_{ym} + \gamma_{dow} + \gamma_h + \epsilon_{ht} \tag{15}$$

Figure 4: Mean Spending Over the Benefit Cycle



Estimates from Figure 4 are produced by estimating Equation 15 with all of the household data provided by the retailer.

10.4 Multivariate Decomposition

The differences between the two estimates can be decomposed into two parts: (1) bias due to non-classical measurement error and (2) differences in endogeneity bias. We begin by building a general measurement error framework and define the likelihood of benefit receipt ($\widehat{Issuance}$) as a function of benefit receipt ($1\{Issuance\}$) and measurement error (u) (Wooldridge, 2012). Specifically, $\widehat{Issuance}$ is defined as follows:

$$\widehat{Issuance} = 1\{Issuance\} + u \tag{16}$$

Note that when measurement error is present in an indicator variable, it will always violate the assumptions of classical measurement error. The measurement error will by definition be correlated with the true variable of interest. In this specific context, $\widehat{Issuance}$ takes the values 0.20, 0.30, 0.40 and 0.10 in weeks one, two, three and four (respectively) of the calendar month. As a result, $u < 0$ when $1\{Issuance\} = 1$ and $u > 0$ when $1\{Issuance\} = 0$ and the $cov(1\{Issuance\}, u) < 0$.

Our true model is of the following form:

$$Sales_{ht} = \beta_1 1\{Issuance_{ht}\} + \sum_{k=2}^{K} \beta_k x_k + \gamma_{ht} \tag{17}$$

where the x_k represent k additional regressors, added as controls, and γ_{ht} represents the error term.

Following Wooldridge (2012), we apply the Frisch-Waugh theorem to obtain the estimate of β_1 from the true model (Equation 17). First, we partial out the effects of the k control variables by estimating r_1 from the following equation:

$$\begin{aligned} 1\{Issuance\} &= \alpha_0 + \sum_{k=2}^{K} \alpha_k x_k + r_1 \\ \Rightarrow 1\{Issuance\} - \alpha_0 - \sum_{k=2}^{K} \alpha_k x_k &= r_1 \end{aligned} \tag{18}$$

We then evaluate the effect of $1\{Issuance_{ht}\}$ on $Sales_{ht}$ by regressing Sales on the estimate of r_1 (e.g., $\hat{r}_1$); note that $\hat{r}_1$, the part of $1\{Issuance_{ht}\}$, is uncorrelated with the k control variables. In other words, β_1 can be recovered by estimating the following:

$$Sales_{ht} = \beta_1 \hat{r}_1 + \epsilon_{ht} \tag{19}$$

The estimate of β_1 from the true model (Equation 17), is given as follows:

$$plim(\widehat{\beta}_{1,1\{Issuance\}}) = \frac{cov(\hat{r}_1, Sales)}{var(\hat{r}_1)} = \beta_1 + \frac{cov(\hat{r}_1, \epsilon)}{var(\hat{r}_1)} \tag{20}$$

In order to obtain the estimate of $\widehat{\beta}_{1,\widehat{Issuance}}$, we plug Equation 16 in Equation 17 to obtain the estimated model below:

$$Sales = \beta_1(\widehat{Issuance} - u) + \sum_{k=2}^{K} \beta_k x_k + \gamma = \beta_1 \widehat{Issuance} + \sum_{k} \beta_k x_k + \gamma - \beta_1 u \tag{21}$$

Following Wooldridge (2012), we apply the Frisch-Waugh theorem to obtain the estimate of β_1 from the estimated model (Equation 21). First, we partial out the effects of the k control variables by estimating $\tilde{r}_1$ from the following equation:

$$\begin{aligned} \widehat{Issuance} = 1\{Issuance\} + u = \alpha_0 + \sum_{k=2}^{K} \alpha_k + \tilde{r}_1 \\ \Rightarrow 1\{Issuance\} - \alpha_0 - \sum_{k=2}^{K} \alpha_k + u = \tilde{r}_1 \end{aligned} \tag{22}$$

Note that under the assumption u is uncorrelated with each of the k covariates, $\hat{\hat{r}}_1 = \hat{r}_1 + u$. We then evaluate the effect of $\widehat{Issuance}$ on $Sales_{ht}$ by regressing Sales on the estimate of $\hat{\hat{r}}_1$; note that $\hat{\hat{r}}_1$ the part of $\widehat{Issuance}$ that is uncorrelated with the k control variables. In other words, $\beta_{1,\widehat{Issuance}}$ can be recovered by estimating the following:

$$Sales_{ht} = \beta_1 \hat{\hat{r}}_1 + \rho_{ht} \tag{23}$$

where ρ_{ht} represents the error term.

The $\widehat{\beta_1}$ from our estimated model is defined as follows:

$$\begin{aligned} plim(\widehat{\beta}_{1,\widehat{Issuance}}) &= \frac{cov(\hat{\hat{r}}_1, Sales)}{var(\hat{\hat{r}}_1)} = \frac{cov(\hat{r}_1 + u, Sales)}{var(\hat{r}_1 + u)} \\ &= \beta_1 \frac{var(\hat{r}_1)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} + \beta_1 \frac{cov(u, \hat{r}_1)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} + \frac{cov(u, \epsilon)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} \\ &+ \frac{cov(\hat{r}_1, \epsilon)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} \\ &= \beta_1 \left[1 - b_{u,\hat{\hat{r}}_1}\right] + \frac{cov(u, \epsilon)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} + \frac{cov(\hat{r}_1, \epsilon)}{var(\hat{r}_1) + var(u) + 2cov(\hat{r}_1, u)} \end{aligned} \tag{24}$$

where $b_{u,\hat{\hat{r}}_1} = \frac{var(u)+cov(u,\hat{r}_1)}{var(\hat{r}_1)+var(u)+2cov(\hat{r}_1,u)} = \frac{cov(u,\hat{\hat{r}}_1)}{var(\hat{r}_1)+var(u)+2cov(\hat{r}_1,u)}$.

Given the probability limits of the two estimates, we define the probability limit of the difference between the two estimates as $\Delta = plim(\widehat{\beta}_{1,\widehat{Issuance}}) - plim(\widehat{\beta}_{1,1\{Issuance\}})$. We define Δ below:

We define Δ below:

$$\Delta = cov(\hat{r}_1, \epsilon) \left[\frac{1}{var(\hat{r}_1 + u)} - \frac{1}{var(\hat{r}_1)}\right] - \beta_1 b_{u,\hat{\hat{r}}_1} + \frac{cov(u, \epsilon)}{var(\hat{r}_1 + u)} \tag{25}$$

This is the multivariate analog of Equation 10 that appears in the main body of the paper. Similar to the main body of the paper, $b_{u,\hat{\hat{r}}_1}$ can be estimated by OLS as follows:

$$u = b_{u,\hat{\hat{r}}_1} \widehat{Issuance} + \sum_{k=2}^{K} b_k x_k + \nu \tag{26}$$

10.5 Single Person Households

10.5.1 Summary Statistics

In the summary statistics that follow, we measure weeks as seven day periods following the disbursement of SNAP benefits (i.e., week one contains the day of SNAP receipt and the six days following that initial distribution date). Following the previous literature, we standardize a week as a seven day period and only consider the the first 28 days of the SNAP benefit cycle.

Table 9 provides mean spending for single-person households over the weeks of the benefit cycle and separates these mean values by whether or not the household was experiencing a SNAP adoption spell. Panel A reveals that the average single person household in our sample spends \$85 to \$90 dollars per week with the retailer of study prior to SNAP adoption. Panel B indicates that mean spending rises upon SNAP adoption to an average of \$114 per week; furthermore, in the months of a SNAP adoption spell, the first week of the benefit cycle illustrates considerably higher spending (\$124) relative to weeks 2 (\$117), 3 (\$107), and 4 (\$111). This simple comparison of means suggests that single-person household spending in week one of the benefit cycle is, on average, 12% higher relative to week four of the benefit cycle. The spike in week one spending for single-person households is about half as large, in percentage terms, as the spike in week one spending for all households. Comparing these results to our non-single-person household results, we find that expenditures between week one and week four decline more for non-single-person households compared to single-person households. That is, expenditures for a single-person household decline by approximately 11%, compared to a 20% decline for non-single-person households. These results align with findings from Kuhn (2018), who shows that an additional member of a household is correlated with a larger decline in expenditures on a daily basis over the benefit month (i.e., 5%).

Table 9: Summary Statistics Over the Benefit Cycle | Single Person Household

Panel A: No Adoption Spell	Overall	Week 1	Week 2	Week 3	Week 4
Sales (\$)	87.16	87.75	89.84	86.04	84.92
	(107.35)	(113.72)	(111.87)	(102.62)	(100.37)
HH-Week Observations	5,924	1,498	1,492	1,479	1,455

Panel B: Adoption Spell	Overall	Week 1	Week 2	Week 3	Week 4
Sales (\$)	114.84	124.57	117.25	106.67	110.91
	(128.67)	(130.00)	(126.99)	(122.41)	(134.37)
HH-Week Observations	6,571	1,638	1,642	1,641	1,650
143 Single Person Adoption Households Standard errors in parenthesis					

10.5.2 Comparison of Estimates for Single Person Households

In order to compare the likelihood of benefits receipt estimate with the benefit receipt estimate, we structure the purchasing data around the four calendar weeks of the month. This data structure is comparable to Goldin et al. (2020) as well as Hastings et al. (2010). We keep the first 28 days of each calendar month and define week one of the calendar month as the first seven calendar days of the month, so on and so forth. The liklihood that a household receives benefits in a given month is defined by the number of days in the calendar week on which benefits are disbursed, divided by the number of days in the month for which benefits are scheduled to disburse. For example, in the first seven days of the calendar month, there are two days that benefits are scheduled to disburse out of ten days total in the month on which benefits are disbursed. Hence the liklihood of receiving benefits in week one is 0.20.

We study the effect of issuance on spending in the following regression framework:

$$Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} + \gamma_m + \gamma_h + \epsilon_{ht} \tag{27}$$

where $Sales_{ht}$ represents expenditure for household h in calendar-week t, γ_m represents a calendar month-year fixed effect and γ_h represents a household fixed effect. We compare two estimates of $\widehat{\beta}_1$ by replacing $Issuance_{ht}$ with the following variables: (1) $\widehat{SNAPIssuance}_{ht}$, which is equal to the liklihood that a household receives benefits (i.e., $\widehat{SNAPIssuance}_{ht}$ is equal to 0.20, 0.30, 0.40 and 0.10 in weeks one, two, three and four (respectively) of the calendar month) and (2) $1\{SNAPIssuance_{ht}\}$, a dummy variable equal to one in the calendar week that the household is assigned to receive SNAP benefits according to the first letter of their last name.

In order to control for income shocks that may consistently occur over the course of a calendar month, we interact $Issuance_{ht}$ with an indicator for whether or not the households is experiencing an adoption spell, while also controlling for whether or not the household is experiencing an adoption spell. This allows us to incorporate calendar-week fixed effects that are common to the households prior to and following SNAP adoption (e.g., recurring monthly payments, paycheck receipt, etc.). Regressions incorporating these additional controls take the following form:

$$Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} x 1\{AdoptionSpell\} + \beta_2 1\{AdoptionSpell\} + \gamma_{CW} + \gamma_m + \gamma_h + \epsilon_{ht} \tag{28}$$

where $1\{AdoptionSpell\}$ is an indicator for whether or not the household is in an adoption spell and γ_{CW} is a calendar-week fixed effect to control for intramonthly income shocks that are common for the households prior to and following SNAP adoption. All other variables are defined as above.

Table 10 compares the estimates of $\widehat{\beta_1}$ for the liklihood of benefits receipt estimate (Panel A) and the benefit receipt estimate (Panel B) for households with one individual in them. Columns (1) and (2) of Table

10 provide estimates from Equation 28 while column (3) and provides estimates from Equation 27 utilizing all households in the sample. Comparisons of these two estimates reveal that the likelihood of benefit receipt estimate is consistently larger than the benefit receipt estimate. Specifically, the liklihood of benefits receipt estimate indicates that households level sales increase by $49 to $69 in a week in which all benefits are issued compared with the benefit receipt estimates which indicates sales increases of $5 to $6 in the week in which benefits are issued, conditional on experiencing a SNAP adoption spell. Estimates of Equation 27 reveal a similar upward bias: the likelihood of benefit receipt estimate indicates that households level sales increase by $8 in a week in which all benefits are issued compared with the benefit receipt estimate which indicates a sales increase of $3 in the week on which benefits are issued. The likelihood of benefits receipt estimates are 2.7 to 12.7 times larger than the benefit receipt estimates. Additionally, the equivalence of the two estimates is rejected for all three specifications.

Table 10: The Effect of Issuance on Weekly Sales: Comparison of Estimates

Panel A: Likelihood of Benefit Receipt Estimates			
	(1)	(2)	(3)
$\widehat{SNAPIssuance_{ht}}$ x 1{Adoption Spell}	68.94***	48.82***	
	(14.84)	(10.25)	
1{AdoptionSpell}	2.893	7.922**	
	(4.248)	(3.295)	
$\widehat{SNAPIssuance_{ht}}$			7.637***
			(1.172)
Year-Month fixed Effect	X	X	X
Household Fixed Effect	X	X	X
Calendar Week Fixed Effect	X		
Observations	12,684	12,684	403,680
R-Squared	0.035	0.034	0.012

Panel B: Benefit Receipt Estimates			
	(1)	(2)	(3)
$1\{SNAPIssuance_{ht}\}x1\{AdoptionSpell\}$	5.415**	5.980**	
	(2.689)	(2.649)	
1{AdoptionSpell}	18.77***	18.63***	
	(2.177)	(2.175)	
$1\{SNAPIssuance_{ht}\}$			2.830***
			(0.303)
Year-Month fixed Effect	X	X	X
Household Fixed Effect	X	X	X
Calendar Week Fixed Effect	X		
Observations	12,684	12,684	403,680
R-Squared	0.034	0.033	0.012

Times Larger	12.73	8.16	2.70
p-value for null of equality	0.000	0.001	0.002

Standard errors in parenthesis
Standard errors clustered at the household level

10.6 Modeling the Effect of Lagged Treatment

We modify our estimation strategy from the main body of the paper to coarsely model the lagged effect of benefit issuance on weekly sales. Specifically, we modify Equations 2 and 3 as follows:

$$Sales_{h,t} = \beta_0 + \beta_1 Issuance_{h,t} + \beta_2 Issuance_{h,t-1} + \beta_3 Issuance_{h,t-2} + \gamma_m + \gamma_h + \epsilon_{h,t} \tag{29}$$

where $Sales_{ht}$ represents total expenditure for household h in calendar-week t, γ_m represents a calendar month-year fixed effect and γ_h represents a household fixed effect. We compare two estimates of $\widehat{\beta}_1$ by replacing $Issuance_{h,t}$ with the following variables: (1) $\widehat{SNAPIssuance}_{h,t}$, which is equal to the likelihood that a household receives benefits (i.e., $\widehat{SNAPIssuance}_{h,t}$ is equal to 0.20, 0.30, 0.40 and 0.10 in weeks one, two, three and four (respectively) of the calendar month) and (2) $1\{SNAPIssuance_{h,t}\}$, a dummy variable equal to one in the calendar week that the household is assigned to receive SNAP benefits according to the first letter of their last name.

In order to control for income shocks that may consistently occur over the course of a calendar month, we interact $Issuance_{h,t}$ with an indicator for whether or not the households is experiencing an adoption spell, while also controlling for whether or not the household is experiencing an adoption spell. This allows us to incorporate calendar-week fixed effects which control for intramonthly income shocks that are common to the household prior to and following SNAP adoption (e.g., recurring monthly payments, paycheck receipt, etc.). Regressions incorporating these additional controls take the following form:

$$\begin{aligned} Sales_{ht} = \beta_0 + \beta_1 Issuance_{ht} x1\{AdoptionSpell\} + \beta_2 Issuance_{h,t-1} x1\{AdoptionSpell\} \\ + \beta_3 Issuance_{h,t-2} x1\{AdoptionSpell\} + \beta_4 1\{AdoptionSpell\} + \gamma_{CW} + \gamma_m + \gamma_h + \epsilon_{ht} \end{aligned} \tag{30}$$

where $1\{AdoptionSpell\}$ is an indicator for whether or not the household is in an adoption spell and γ_{CW} is a calendar-week fixed effect to control for intramonthly income shocks that are common for the households prior to and following SNAP adoption. All other variables are defined as above.

Table 11 compares the estimates of $\widehat{\beta_1}$ for the likelihood of benefit receipt estimate (Panel A) and the benefit receipt estimate (Panel B). Columns (1) and (2) of Table 11 provide estimates from Equation 30 while columns (3) and (4) provides estimates from Equation 29 over the sample of adoption spell households and all households, respectively. Comparisons of these two estimates reveal that the likelihood of benefit receipt estimate is consistently larger than the benefit receipt estimate. Specifically, the likelihood of benefit receipt estimate indicates that household level expenditure increases by \$36.0 to \$39.8 in the week in which benefits are issued compared with the benefit receipt estimates which indicate sales increases of \$19.5 to \$20.0 in the week in which benefits are issued, conditional on experiencing a SNAP adoption spell. Estimates of Equation 29, without conditioning on a SNAP adoption spell, reveal a similar upward bias: the likelihood of benefit receipt estimate indicates that households level sales increase by \$11.3 in the week in which benefits are issued compared with the benefit receipt estimate which indicates a sales increase of \$4.1 in the week on which benefits are issued. The likelihood of benefit receipt estimates are 1.8 to 2.7 times larger than the benefit receipt estimates across all four specifications. The null hypothesis of equality for the likelihood of benefit receipt estimate and the benefit receipt estimate is rejected for all specifications at the ten percent significance level and is rejected at the one percent significance level when all households are incorporated (i.e., Column (4) estimates).

Table 11: The Effect of Issuance on Weekly Sales: Comparison of Estimators with Lagged Treatment

Panel A: Likelihood of Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$\widehat{SNAPIssuance}_{ht}$ x 1{Adoption Spell}	39.75***	36.01***		
	(9.166)	(6.367)		
1{AdoptionSpell}	14.49***	9.079**		
	(5.374)	(3.793)		
$\widehat{SNAPIssuance}_{ht}$			36.01***	11.29***
			(6.628)	(0.637)
$\widehat{SNAPIssuance}_{ht}$ Lags	X	X	X	X
Calendar Week Fixed Effect	X			
Observations	75,848	75,848	39,264	2,758,148
R-Squared	0.028	0.028	0.012	0.013

Panel B: Benefit Receipt Estimates

	(1)	(2)	(3)	(4)
$1\{SNAPIssuance_{ht}\}x1\{AdoptionSpell\}$	19.54***	20.02***		
	(1.493)	(1.469)		
1{AdoptionSpell}	16.56***	15.97***		
	(1.307)	(1.297)		
$1\{SNAPIssuance_{ht}\}$			20.02***	4.120***
			(1.529)	(0.147)
$1\{SNAPIssuance_{ht}\}$ Lags	X	X	X	X
Calendar Week Fixed Effect	X			
Observations	75,848	75,848	39,264	2,758,148
R-Squared	0.030	0.029	0.015	0.013

Panel C: Comparison of Estimates

	(1)	(2)	(3)	(4)
$\frac{\beta_{\widehat{Issuance}}}{\beta_{1\{Issuance\}}}$	2.03	1.80	1.80	2.74
p-value for null of equality	0.0562	0.0654	0.0654	0.000
Household Sample	Adoption Spell Households	Adoption Spell Households	Adoption Spell Households	All Households
Sample ∣ 1{Adoption Spell} = 1	No	No	Yes	No

All regressions include household and year-month fixed effects
Standard errors in parenthesis and clustered at the household level
Tests for equality were generated using the suest command in Stata