SNAP AND BEYOND: THREE ESSAYS INVESTIGATING FOOD SPENDING, DIET QUALITY, AND RECIDIVISM

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ABSTRACT

The Supplemental Nutrition Assistance Program (SNAP) plays a vital role in providing food assistance to qualifying households in the United States. This dissertation comprises three interconnected essays that explore different facets of the impact of SNAP on food spending, diet quality, and its potential influence on criminal recidivism.

Chapter 1 investigates the effects of disruptions in SNAP disbursements on household grocery purchases. By analyzing detailed grocery purchase data during the 2018-2019 federal government shutdown, this study finds that more frequent SNAP payments lead to smoother grocery spending, particularly on perishable items. The results underscore the potential for improved consumption patterns and reduced food insecurity among SNAP participants. This chapter advocates for the policy recommendation of increased SNAP payment frequency, providing families with enhanced opportunities to manage their monthly food budgets effectively.

Chapter 2 delves into the relationship between SNAP work requirements on able-bodied adults without dependents (ABAWDs) and criminal recidivism. Leveraging administrative prison records of ex-offenders, this research reveals that counties implementing SNAP work requirements experience a reduction in the risk of recidivism for released individuals. Interestingly, ex-prisoners released within five years of the ABAWD age cutoff exhibit lower rates of recidivism, particularly related to property crimes, compared to those who age out of the work requirement. These findings shed light on the potential deterrent effect of SNAP work requirements on certain types of criminal behavior among ex-offenders.

Chapter 3 examines the causal effect of SNAP participation on diet quality using data from the Food Acquisition and Purchase Survey (FoodAPS). Employing robust methodologies to address self-selection and misclassification biases, this research presents a nuanced view. While SNAP participation leads to increased food consumption, it also demonstrates a negative effect on diet quality. However, under monotonicity assumptions, strict identification of a positive effect on diet quality remains elusive. These findings underscore the complexities of the relationship between SNAP benefits and dietary habits, urging policymakers to consider trade-offs between food access

and nutritional choices for SNAP beneficiaries.

The comprehensive analysis of these three essays highlights both the strengths and limitations of the SNAP program. It provides valuable insights for policymakers and advocates seeking to enhance the effectiveness of SNAP in alleviating hunger, improving nutrition, and reducing criminal recidivism among vulnerable populations. As the nation strives to achieve the dual goals of combating food insecurity and promoting healthier lifestyles, this dissertation contributes to a more informed and holistic understanding of the impact of SNAP and calls for innovative approaches to optimize the program's outcomes for the well-being of low-income households.

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CHAPTER 1

THE EFFECT OF SNAP DISBURSEMENT DISRUPTIONS ON GROCERY PURCHASES

1.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP) is the largest food assistance program in the United States, providing direct benefits to over 41 million individuals at a cost of $108 billion in fiscal year 2021 (United States Department of Agriculture, 2021). SNAP beneficiaries receive monthly benefits through an electronic benefit transfer (EBT) card to purchase food at authorized retailers. However, the monthly payment structure creates a SNAP cycle characterized by increased spending upon benefit receipt and decreased spending until the next payment (Castner et al., 2011). Decreasing spending over the month is accompanied by adverse consequences such as higher food insecurity (Shapiro, 2005), worse health outcomes (Cotti et al., 2020; Seligman et al., 2014), higher crime (Carr and Packham, 2019; Foley, 2011), and lower test scores (Bond et al., 2022; Cotti et al., 2018) at the end of the benefit month[1]. To address these issues, experts have recommended increasing the frequency of benefit payments (Tobin, 2018). However, the current Farm Bill limits states from issuing SNAP benefits more than once a month, making it difficult to assess the effectiveness of this approach. This study aims to investigate the impact of higher SNAP payment frequency on households' grocery spending patterns by taking advantage of the disruptions caused by the 2018-2019 federal government shutdown.

The shutdown was caused by the Congress and the President not being able to agree upon a continuing resolution to fund the operations of the federal government. The U. S. Department of Agriculture (USDA), which oversees SNAP at the federal level, is one of the agencies that experienced a funding shortfall (Evich, 2019a). As a result, SNAP beneficiaries experienced disruptions in benefit payment. The February 2019 benefits were issued early in all states by January 20, 2019. The shutdown ended on January 25 and states made their own plans for the March issuance to help families cope with the extended benefit gap. While most states made lump-sum payments to all households on a single day, four states (Florida, Georgia, Indiana, and Ohio)

[1] Benefit month refers to the interval between two monthly benefit payment.

chose to split monthly benefits into two payments issued at different times in the March benefit cycle. The difference in payment frequencies provides an opportunity to investigate whether higher SNAP payment frequency can help smooth the SNAP spending cycle and reduce the cyclicality of its associated adverse effects.

In this paper, I analyze household grocery purchases using the NielsenIQ Homescan dataset obtained from the Kilts Center at the University of Chicago Booth School of Business. The dataset contains detailed information on grocery purchases made by 22,504 households across the United States in 2018 and 2019, such as food category, purchase date, quantity, and price of each purchased item. There is also demographic information of these households, including household size, income, and zip code. Importantly, I match SNAP participation status of these panelists using data from an accompanying omnibus survey administered by NielsenIQ.

Using a dynamic triple-difference design, the study estimates the causal effect of SNAP payment frequencies on food spending smoothness by exploiting the plausible exogenous changes in payment schedules. First, I limit the analysis to the first four weeks in the March 2019 benefit cycle counting from the day of first March payment. I measure the smoothness of food expenditures by the difference in daily spending between days in the first week and days in the three weeks that follow. A greater drop in spending indicates a less smooth spending pattern. Second, I compare this difference between SNAP participating and non-participating households with the aim of cancelling out any common time trends. To further control for within-household shopping habits generated by unobserved timing of income flows from other sources, I add expenditures on the same calendar dates in 2018, one year prior to the disruption, as a second control group.

The findings suggest that semi-monthly payments lead to smoother grocery spending over the March benefit month. However, there are still challenges in validating the policy recommendations from these results. The major issue pertains to the inference of the impact of payment frequency on food consumption, given that our spending data lack information on actual food consumption. It is crucial from a policy perspective that the observed smoother food spending translates into smoother food consumption under higher payment frequency. To this end, I classify foods based on their perishability and demonstrate that the smoothing effect is driven by spending responses to perishable items such as fresh fruits and vegetables. As excessive perishable foods purchased at the start of the benefit month are unlikely to be consumed later, the results indicate impulsive food consumption under monthly payments, which refers to purchases driven by immediate gratification rather than rational decision making. In comparison, semi-monthly payments effectively spread out perishable food consumption.

By leveraging the unexpected disruptions as natural experiments, the paper yields novel insights into the SNAP spending cycle and the impact of increased payment frequency on shopping behaviors in this specific context. The study sheds light on the potential benefits of increasing the frequency of SNAP payments, which is a policy proposal that has been debated for some time. The research also emphasizes further examination of the effect of payment frequency on secondary outcomes. It calls for future pilot studies to examine the policy recommendation in a context with higher external validity by design. Lastly, this research highlights a constant provision of public benefits by gauging the changes in food purchasing behaviors during SNAP disbursement disruptions. Given that government shutdowns may disproportionately affect low-income households, this analysis sheds light on their vulnerability.

The paper proceeds as follows. I describe the institutional details of SNAP disruptions during the government shutdown are introduced in section 1.2. Prior literature on expenditure cycles is reviewed in section 1.3. Section 1.4 lays out the conceptual framework. I describe the data on food spending in section 1.5. Section 1.6 describes the empirical approach and presents the results. Section 1.7 concludes.

1.2 Background

SNAP, formerly known as the Food Stamp Program, is the largest food assistance program in the United States in terms of spending and caseload. The benefits are provided monthly through an EBT card to purchase food for at-home consumption at authorized retailers.

1.2.1 Eligibility

SNAP is a means tested program. In order to qualify for food assistance, all members of a household sharing food must have a combined gross income below or equal to 130% of the federal poverty level. The household net income must fall below the poverty line after deductions for working, housing, and other expenses have been made. Depending on age, disability status, and state of residence, there is also an asset limit that must be met for household eligibility. A typical asset limit requires the assets to be no more than $2,750 for households with a member older than 60 years old or disabled, and no more than $4,250 for households without such a member. Beyond income and asset eligibility, households can automatically gain eligibility through categorical eligibility rules based on being eligible for benefits from other low-income assistance programs such as the cash assistance from Supplemental Security Income (SSI) and Temporary Assistance for Needy Families (TANF) benefits Center on Budget and Policy Priorities (2022). However, the Congressional Research Service estimates that only a monthly average of 4.8% of all households without an elderly or disabled member had incomes above 130% of the poverty line in fiscal year 2019 Center on Budget and Policy Priorities (2022). This indicates that the income levels among most SNAP participants are below the income eligibility cutoff even though they can skip the income test through categorical eligibility.

1.2.2 Benefit Levels

Household income and the cost of an adequate monthly diet determine benefits under the SNAP program: families with very low incomes receive higher benefits than families closer to poverty, as the poor are more likely to require assistance purchasing an adequate diet. It is assumed that families will spend 30 percent of their net income on food; SNAP contributes to the difference between that 30 percent contribution and the cost of the Thrifty Food Plan (TFP), a low-cost, nutritionally adequate diet program established by the USDA at the federal level. The TFP cost is updated every year in October to account for increasing food prices. The benefits are also temporarily adjusted under special economic conditions. For example, Congress temporarily increased benefits by boosting every household to 15% above the maximum benefit in response to the COVID-19 pandemic during 2020 and 2021. As of fiscal year 2023, a typical family of four is estimated to receive $684 in monthly benefit (Center on Budget and Policy Priorities, 2022).

1.2.3 Payment Schedules

The benefits are loaded onto the EBT card once in a month. Although the level of benefits is determined by a federal standard, states determine their own disbursement schedules across individuals and issue benefits at different times. Table 1.1 lists the disbursement schedules in 2019 for the fifty states and the District of Columbia. Column 2 shows the normal disbursement outside the disruption period. Only seven states disburse their monthly benefits to all beneficiaries on the same day.[2] Most states use a staggered payment schedule to issue benefits to different households on different dates. Individuals in these states are assigned a receipt day during the month based on their case number, birth date, Social Security number, or last name. For example, Michigan distributes benefits to households whose case number ends in 0 to 9 every other day between the third and twenty-first of every month sequentially as of fiscal year 2019.

Table 1.1 State SNAP Disbursement Dates in 2019

State	Normal Issuance	February Issuance	March Issuance
Alabama	4 – 23	1/20/19	3/4/19
Alaska	1	1/20/19	3/4/19
Arizona	1 – 13	1/17/19 – 1/20/2019	3/1/19 – 3/6/19
Arkansas	4 – 13	1/17/19	3/4/19
California	1 – 10	1/16/19	3/1/19
Colorado	1 – 10	1/16/19 – 1/20/2019	3/1/19 – 3/10/19
Connecticut	1 – 3	1/20/19	3/1/19 – 3/3/19
Delaware	2 – 23	1/17/19	3/4/19
District of Columbia	1 – 10	1/16/19	2/26/19
Florida*	1 – 28	1/20/19	Half 3/1/19; Half normal
Georgia*	5, 7, 9, 11, 13, 15, 17, 19, 21, 23	1/14/19	Half 3/2/19; Half normal
Hawaii	1, 5	1/20/19	3/1/19 – 3/5/19
Idaho	1 – 10	1/20/19	3/1/19 – 3/10/19

[2]These states include Alaska, Nevada, New Hampshire, North Dakota, Rhode Island, South Dakota, and Vermont. Beneficiaries in these states receive payment on the first day of the month, except that New Hampshire pays on the 5th and South Dakota pays on the 10th of every month.

Table 1.1 (cont'd)

State	Normal Issuance	February Issuance	March Issuance
Illinois	1, 3, 4, 5, 6, 7, 8, 9, 10, 13, 17, 20	1/20/19	3/1/19
Indiana*	5, 7, 9, 11, 13, 15, 17, 19, 21, 23	1/16/19	Half 2/22/19; Half normal
Iowa	1 – 10	1/17/19	3/1/19 – 3/10/19
Kansas	1 – 10	1/20/19	3/1/19
Kentucky	1, 3, 5, 7, 9, 11, 13, 15, 17, 19	1/20/19	3/1/19
Louisiana	1 – 14	1/16/19	3/1/19 – 3/2/19
Maine	10 – 14	1/17/19	3/3/19
Maryland	4 – 23	1/20/19	3/6/19
Massachusetts	1, 2, 4, 5, 7, 8, 10, 11, 13, 14	1/17/19 – 1/20/2019	3/1/19 – 3/4/19
Michigan	3, 5, 7, 9, 11, 13, 15, 17, 19, 21	1/19/19	3/3/19 – 3/5/19
Minnesota	4 – 13	1/20/19	3/4/19 – 3/6/19
Mississippi	4 – 21	1/20/19	3/4/19
Missouri	1 – 22	1/20/19	3/1/19 – 3/22/19
Montana	2 – 6	1/17/19	3/2/19
Nebraska	1 – 5	1/20/19	3/1/19
Nevada	1	1/20/19	3/1/19
New Hampshire	5	1/20/19	3/5/19
New Jersey	1 – 5	1/17/19	3/1/19
New Mexico	1 – 20	1/20/19	3/1/19
New York	1 – 9	1/17/19	3/1/19
North Carolina	3, 5, 7, 9, 11, 13, 15, 17, 19, 21	1/20/19	3/3/19
North Dakota	1	1/20/19	3/1/19
Ohio*	2, 4, 6, 8, 10, 12, 14, 16, 18, 20	1/16/19	Half 2/22/19; Half normal
Oklahoma	1, 5, 10	1/20/19	3/1/19
Oregon	1 – 9	1/18/19	3/1/19
Pennsylvania	First 10 weekdays	1/18/19	3/1/19 – 3/14/19
Rhode Island	1	1/20/19	3/1/19
South Carolina	1, 3, 5, 7, 9, 11, 13, 15, 17, 19	1/17/19	3/5/19
South Dakota	10	1/20/19	3/10/19
Tennessee	1 – 20	1/20/19	3/2/19; 3/6/19
Texas	1, 3, 5, 6, 7, 9, 11, 12, 13, 15	1/20/19	3/1/19 – 3/10/19
Utah	5, 11, 15	1/20/19	3/5/19

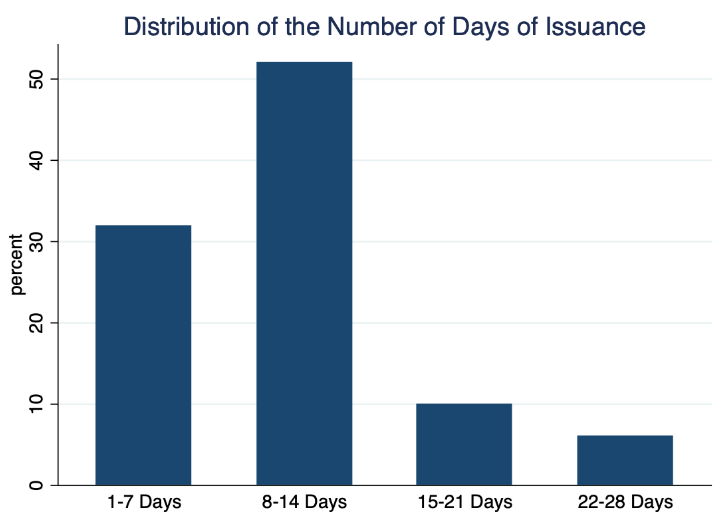
Table 1.1 (cont'd)

State	Normal Issuance	February Issuance	March Issuance
Vermont	1	1/20/19	3/1/19
Virginia	1, 4, 7	1/17/19	3/1/19
Washington	1 – 10	1/20/19	3/2/19 – 3/11/19
West Virginia	1 – 9	1/20/19	3/1/19
Wisconsin	2, 3, 5, 6, 8, 9, 11, 12, 14, 15	1/20/19	3/1/19
Wyoming	1 – 4	1/16/19 – 1/19/2019	3/1/19 – 3/4/19

[a] SNAP issuance schedules are obtained from state government websites.
[b] * indicates states that paid March benefit in two installments.

Figure 1.1 plots the distribution of the number of days of payment across these states. Over 80 percent of all states disburse benefits within 14 days every month, with more than half of the states stagger payment over 8-14 days. Figure 1A.1 in the appendix shows the disbursement patterns at the state level. Those states that issue benefits within a week's time tend to be in the regions of Rocky Mountain, Great Plains, and New England.

Figure 1.1 Number of SNAP Payment Days During Normal Times



Note: The figure plots the share of states by the number of SNAP payment days among the 50 states. SNAP issuance schedules are obtained from state government websites.

1.2.4 Disruptions During the Shutdown

On January 8, 2019, the USDA announced plans requiring states to provide full February benefits by January 20, 2019 (United States Government Accountability Office, 2019). Column 3 of Table 1.1 lists the early payment dates states chose. Most states disbursed the early payment on January 20, though some chose earlier payment dates beginning on January 14. Local SNAP agencies informed the SNAP clients that the payments were early February benefits rather than extra payment by posting messages on government websites, posters, telephone systems, and social media (Evich, 2019c). The shutdown ended on January 25, ensuring a continuing provision of SNAP benefits in March and the rest of the fiscal year.

Without modifications in March issuance, approximately 15 million households could have experienced a gap between benefit receipt for February and March of more than 40 days, while more than 4 million households could have experienced a gap of more than 50 days (Rosenbaum, 2019). Although the SNAP households received the same level of benefits from January to March that they would have received without the disruption, it could cause hardships for some families if they did not budget the early distribution over an extended period.

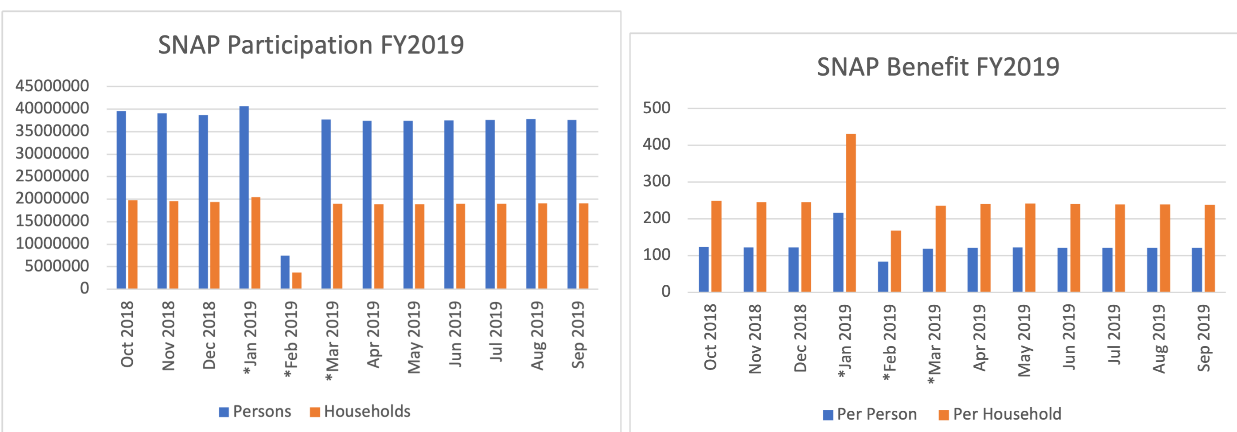
To address the long February SNAP gap, states developed their own plans for reducing the time between the issuance of February and March benefits (Evich, 2019b). Column 4 of Table 1.1 presents the March payment dates. Most states made the payment as early as March 1st to ensure that SNAP recipients would not have to go more than 40 days before their March benefits. For example, California, the state with the largest SNAP caseload, issued March benefits to all recipients on March 1st, where the benefits were usually staggered between the first and ninth every month (Department of Human Services, 2019). Other states chose to split monthly payment into two installments. For example, Florida, another state with large number of SNAP recipients, issued half of the March benefit on March 1, and sent out the second half on its normal payment schedule. Georgia, Indiana, and Ohio also split the March payment and issued half of the benefits early.[3] This creates variation in the frequency that household receive their March SNAP payment. SNAP

[3] Details from the USDA Food and Nutrition Service can be found at https://1thr423ga6tg2pem7p36a4sa-wpengine.netdna-ssl.com/wp-content/uploads/2019/02/snap-march-issuance.pdf.

agencies distributed the announcements for March issuance plans by posting on the government website, local newspapers, and social media as early as February 13.

Figure 1.2 describes the monthly caseloads and payments in all states during FY2019 using the administrative data from the USDA. The disrupted periods are marked with stars. The left panel shows the number of households and persons receiving SNAP in each month during FY2019. The number of SNAP recipients dropped sharply in February 2019 indicating most of the beneficiaries did not receive any benefit due to the early disbursement. The number does not drop to zero because households in the state of Ohio and Indiana issued half the March benefits early in February and the District of Columbia paid full March benefits in February. The right panel depicts that the amount of SNAP benefits disbursed in January nearly doubled the amount disbursed in months outside the disrupted period.

Figure 1.2 The Disruption in SNAP Disbursement



Note: The data is obtained from the monthly SNAP data table maintained by the Food and Nutrition Service of USDA (https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap). Due to the partial Federal government shutdown, most of the February 2019 SNAP benefits were issued early in the month of January 2019. February's data reflects the participation and issuance that was early March issuance in Ohio and Indiana and not part of the early February issuance. As a result, March 2019's participation and benefits show a significant increase over the previous month.

1.3 Literature Review

The literature of SNAP and food expenditures consists of three strands. The first strand assesses the effectiveness of SNAP on improving food security, which is measured by the quantity, quality, variety, and desirableness of diets. Using variations in SNAP administration, researchers find that more generous SNAP policies increase food spending and reduce food insecurity (Mykerezi and Mills, 2010; Ratcliffe et al., 2011; Shaefer and Gutierrez, 2013; Yen et al., 2008). The effect of SNAP on the healthfulness of food purchases is less clear. In a systematic review, Andreyeva et al. (2015) show that SNAP participation is associated with lower diet quality. Recent studies examine how SNAP affects the healthfulness of food purchases by linking grocery store purchases to nutrients in foods. Garasky et al. (2016) find that SNAP households and non-SNAP households purchase similar foods at the grocery store. Franckle et al. (2017) find that grocery items purchased with SNAP benefits tend to be less healthful than grocery items not purchased with SNAP benefits. Grummon and Taillie (2017) find that, along several dimensions, the grocery purchases of households participating in SNAP are less healthful than the grocery purchases of income-eligible non-participating households. Harris-Lagoudakis (2020) uses supermarket retailer data to find SNAP adoption increases spending over meats, oils and prepared products at a higher rate over other grocery product categories.

Another strand of research studies the impact of SNAP by testing the fungibility of SNAP benefits. Research using the early rollout of the food stamp program in 1980s shows the marginal propensity to consume SNAP-eligible food (MPCF) out of food stamp benefits is 0.16 to 0.32 whereas the MPCF out of cash is 0.09 to 0.10 (Hoynes and Schanzenbach, 2009). The results of their study do not reject the hypothesis that the MPCF out of food stamps is equal to the MPCF out of cash income. Newer estimates range from 0.3 to 0.6 based on more recent variations in the program administration (Beatty and Tuttle, 2015; Bruich, 2014; Hastings and Shapiro, 2018). Hastings and Shapiro (2018) rejects fungibility and attributes the difference in MPCF out of SNAP benefits and cash income to mental accounting where people mentally separate income sources according to different spending intentions. Their results generate policy-relevant insights – SNAP can work as a tool to help low-income households set aside budget to purchase more food for at-home consumption.

The third strand investigates the SNAP monthly benefit spending patterns. This literature focus on the SNAP cycle, which refers to the phenomenon that SNAP beneficiaries tend to deplete their benefits rapidly after receiving SNAP. Research finds that food expenditures of SNAP participants spike on the day of benefit receipt (Goldin et al., 2022; Harris-Lagoudakis and Wich, 2021; Hastings and Washington, 2010; Wilde and Ranney, 2000). Castner et al. (2011) study the redemption pattern and estimate that 59 percent of the monthly benefits are spent on average in the first week and a quarter of households exhaust benefits within the first week following issuance. Decreased spending over the month is accompanied by a series of adverse consequences. Foley (2011) shows that crime increases over the benefit month in areas with highly time-concentrated disbursements of welfare (including SNAP), and Carr and Packham (2019) link the SNAP cycle directly to grocery store theft rates. Seligman et al. (2014) find that hospital admissions for hypoglycemia are more common at the end of the month in low-income communities, and Bond et al. (2022) show that low-income high school students get lower scores on college admissions exam if they take the exam in the last two weeks of the SNAP benefit cycle relative to student taking the exam at other times of the cycle. Cotti et al. (2018) show that standardized test scores decrease for children in SNAP households at the end of the benefit month. Although researchers suggest that increasing payment frequencies can be beneficial, the opportunities to test this proposal are limited.

Prior research on monthly spending patterns has several limitations. First, studies using administrative SNAP redemption data do not track cash expenditures on food purchases. The spike in SNAP redemption that characterized the SNAP cycle may not indicate a decrease in food spending over the month. Second, studies using survey data on daily food expenditures and intakes do not observe spending or consumption over an entire benefit cycle. To interpret results as the average spending pattern among the survey population, researchers assume interview dates were randomly assigned and that the survey households were randomly observed in their benefit cycle (Beatty et al., 2019). Third, researchers using scanner data from a single retailer to explore the monthly pattern do not track food expenditures made in other stores. Although a method of payment indicator can identify SNAP participation, most of these data do not include information regarding the date on which each household received benefits. To circumvent this problem, previous studies focus on states that issue benefits on the first day of a month. There is, however, a possibility that the so-called first-of-the-month effect may also be driven by other income streams that arrive on the first day of the month, such as paychecks, TANF, or other social welfare benefits. Lastly, prior studies using the NielsenIQ Homescan Panel rely on the reported income to define a SNAP-eligible sample because SNAP participation is available in the annual survey (Cotti et al., 2020). Hence, the estimation of the SNAP cycle is potentially biased due to large measurement errors.

This paper uses the NielsenIQ Homescan Panel to investigate the effect of the timing of SNAP on food purchases. The dataset is matched with a separate survey of the panel households to identify SNAP participation status during the sample period. This matched data overcomes the above limitations because I observe grocery expenditures from all retail channels during the entire shutdown period among all panel households, though the self-reported SNAP participation status is still subject to measurement error due to under-reporting (Meyer et al., 2015). Second, the research context allows for the identification of the SNAP payment days during the shutdown, restricting the measurement error with regards to the start and end of the SNAP benefit cycle at the household level.

1.4 Conceptual Framework

Because expenditure and consumption cycles are correlated at the household level among SNAP participants (Kuhn, 2018), I derive expected impact on food spending patterns based on consumption models. The permanent income hypothesis suggests that a rational consumer would make consumption decisions based on their lifetime permanent income rather than their temporary income. As a result, the model anticipates a consistent consumption pattern, with temporary changes in income having little impact on consumption decisions. However, modern behavioral economics theories challenge this hypothesis by incorporating behavioral biases into the neoclassical consumption savings model. Past literature provides ample evidence that SNAP beneficiaries tend to exhaust their benefits quickly after receipt, which is a rejection of the permanent income hypothesis (Castner et al., 2011). Short-run impatience (Shapiro, 2005), preference for variety Hastings and Washington (2010), intramonth retail pricing (Goldin et al., 2022) are proposed to explain the mechanism of the observed spending dynamics. Empirical evidence suggests present bias as the main driver of the spending spikes in the beginning of the month (Hastings and Washington, 2010; Shapiro, 2005). I build the conceptual model to simulate consumption behaviors incorporating present bias under different payment schedules in the following sections.

1.4.1 Behavioral Bias and the SNAP Cycle

Consider a SNAP participating consumer who is planning for food purchases over the month. The consumer displays quasi-hyperbolic discounting and maximizes the following discounted utility at date t by choosing the optimal daily consumption level c_t (Harris and Laibson, 2003):

$$U_t = E_t \left[u\left(c_t\right) + \beta \sum_{\tau=1}^{T-t} \delta^{\tau} u\left(c_{t+\tau}\right) \right]$$

The instantaneous utility function takes the form of isoelastic utility,

$$u_c = \frac{c^{1-\rho}}{1-\rho}$$

which means the consumer has a constant relative risk aversion of ρ. The consumer at date t discounts future utility at $t+j$ by the factor $\beta\delta^{j}$. This indicates the consumer values the consumption tomorrow only $\beta\delta$ times the utility of the same level of their consumption today. And the consumption two days later is only $\beta\delta^2$ of the value of the same level of consumption today. β stands for the discount rate between the current period and any future period, showing the preference for instant consumption. A low β means the consumer is impatient in the short-run choices between current utility and future utility. At $\beta = 1$, the case is reduced to exponential discounting. δ represents the time-consistent discount rate between any two adjacent days in the long run. A low δ stands for impatience over long term choices

For simplicity, I assume the food budget is determined by the SNAP payment flows W_t. The budget constraint is given by:

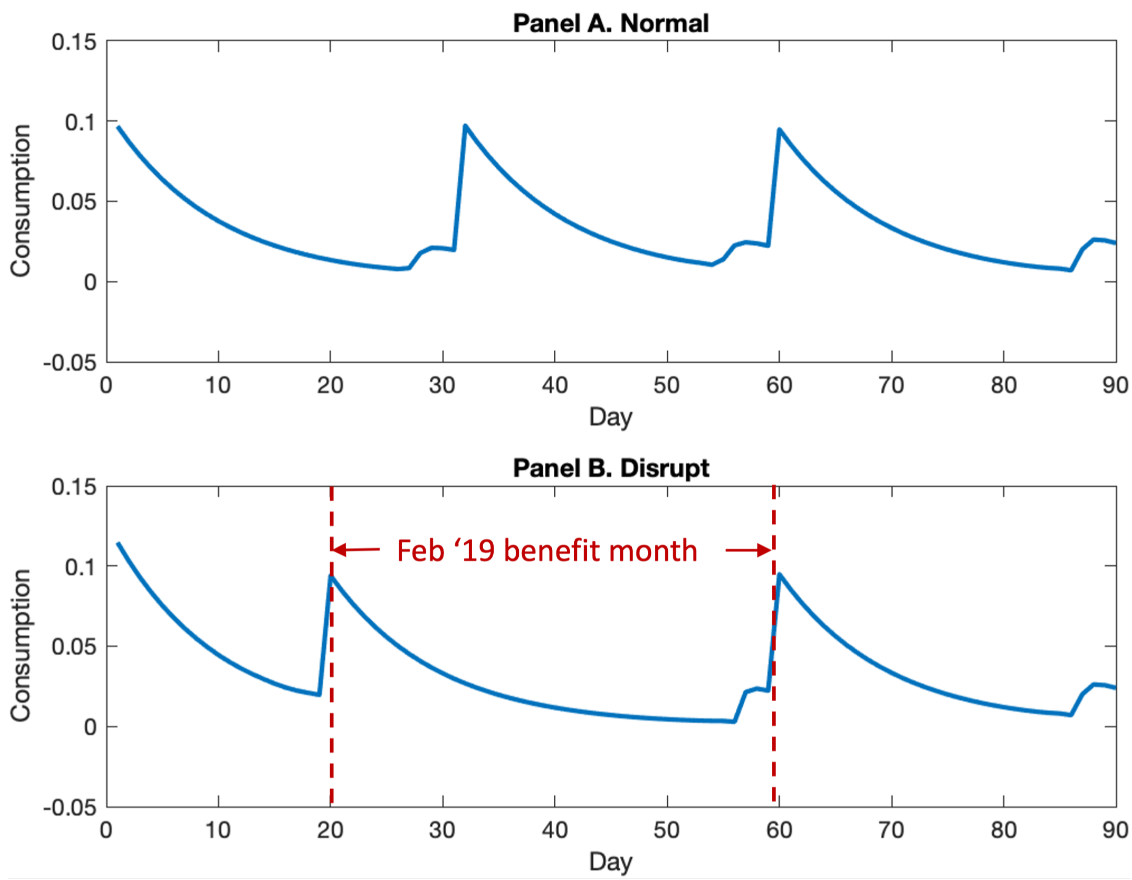
$$\sum_{t=0}^{T} W_t = \sum_{t=0}^{T} P_t \cdot C_t$$

where P_t is the relative price over time, and C_t is the consumption level on date t. Following Harris and Laibson (2003), the consumption path can be derived from the Euler equation below:

$$C_t^{-\rho} = \left[C'(W_{t+1})\beta\delta + (1 - C'(W_{t+1}))\delta\right] C_{t+1}^{-\rho}$$

In the simulation study, I assume the daily discount rate $\delta = 1$ for simplicity. To be consistent with the estimate of their baseline model in Laibson et al. (2003), the measure of the short-run impatience takes the value $\beta = 0.7$. Price levels are assumed to be constant during the study period. The relative risk aversion is set at $\rho = 3.4$ to fit the estimated calorie decline in Shapiro (2005). The income stream mimics the SNAP disbursement schedules. The monthly benefit amount is normalized to one. I use the simple setting where the benefits are issued to all households on the first day of the month to illustrate the impact of disruptions in issuance.

Figure 1.3 Simulated Impact of Early Payment



Note: The figures depict the simulated consumption paths under hyperbolic discounting, assuming the agent has perfect information about future payment schedules. The horizontal axis represents the days elapsed from the first day of 2019. The vertical axis shows the daily share of consumption during the benefit month. Panel A shows the case of the hypothetical normal disbursement at the first of each month. Panel B plots the hypothetical case where the February disbursement occurs early in the latter half of January, and disbursement returns to normal in March.

Using a parametrization consistent with results from past studies on quasi-hyperbolic discounting, Panel A of Figure 1.3 shows the simulated optimal consumption path of a representative agent receiving SNAP on the first day of month in the first quarter of the year. The x-axis shows the days since the first day of the year. And the level of consumption is depicted on the y-axis. Consumption increases upon receipt of the payment and then drops over the rest of the month until next benefit payment, which is consistent with the observed SNAP spending cycle in past studies. The simulated consumption patterns under different payment structures will be informative for inference on spending patterns when they are correlated.

1.4.2 Consumption Path under Disruption

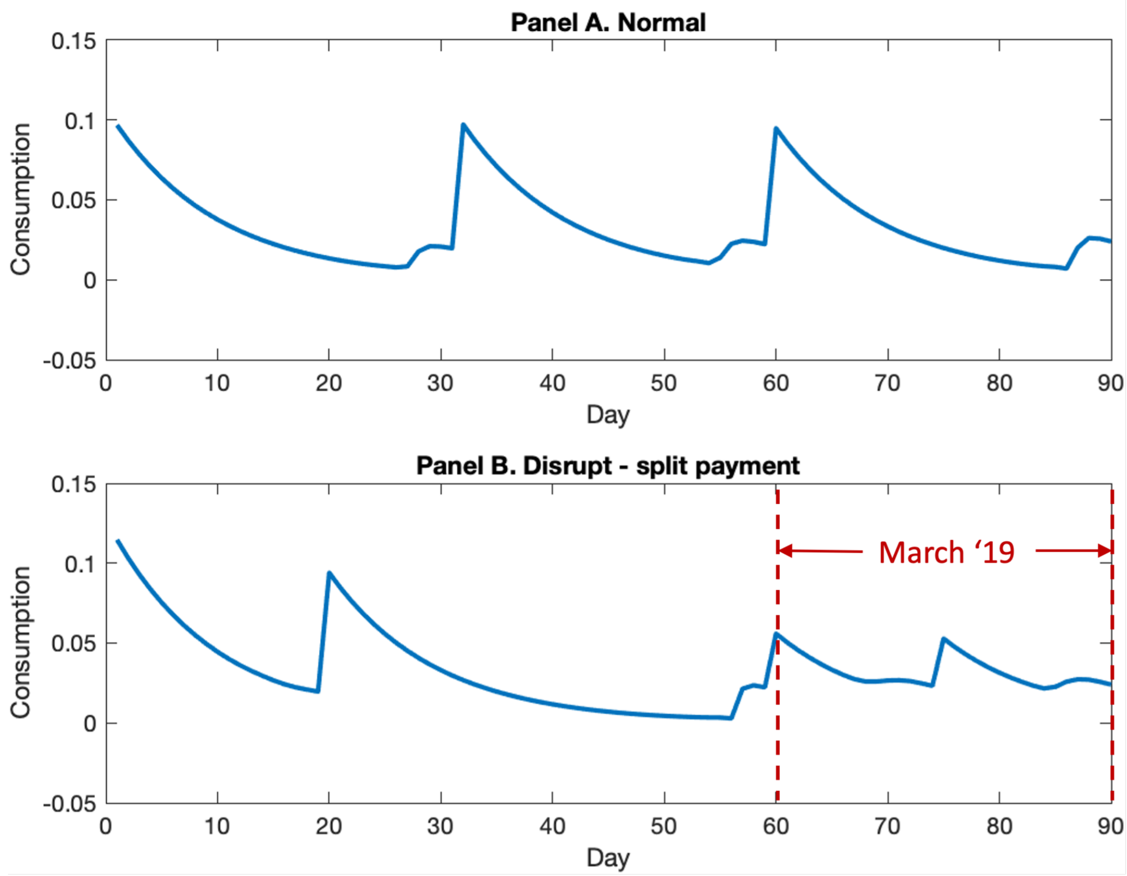
The government shutdown generates an exogenous shock to the schedule. In 2019, February payment arrived early on January 20th. If the agent resides in a state that kept the monthly lump-sum payment, Panel B of Figure 1.3 shows their consumption path during the first quarter of 2019, where the consumption spike in February is pulled forward to the early payment in January and the spike upon March payment stays unaffected.

Figure 1.4 plots the case when the agent lives in a state that chose to split March benefits into two equal payments. Panel A shows the consumption during the three months when monthly benefits are paid on the first day of month. Panel B shows the consumption pattern when the February payment was made early and March benefit was issued at different times in equal amount. Comparing the consumption paths in Panel B of Figure 1.3 and Figure 1.4, twice-monthly benefit payment is expected to generate smoother consumption pattern than a lump sum monthly payment.

1.4.3 Inferring Expenditures from Consumption Path

The key caveat of the simulation study is that the model predicts consumption instead of expenditures. Factors such as the fixed costs of shopping would cause the expenditure path to deviate from the predicted consumption path. I take two approaches to address this potential issue. First, I categorize food by shelf life, as perishable foods, such as milk, produce, etc., cannot be stored for an extended period and are typically consumed and purchased around the same time. The anticipated impact on expenditures should follow closely to the predicted impact on consumption for perishable items. Second, I examine the frequency of shopping trips over the benefit month. If the fixed travel cost associated with grocery shopping trips plays a significant role in consumers'

Figure 1.4 Simulated Impact of Semi-Monthly Payment



Note: The figures depict the simulated consumption paths under hyperbolic discounting, assuming the agent has perfect information about future payment schedules. The horizontal axis represents the days elapsed from the first day of 2019. The vertical axis shows the daily share of consumption during the benefit month. Panel A shows the case of the hypothetical normal disbursement at the first of each month. Panel B plots the hypothetical case where the February disbursement occurs early in the latter half of January, and March benefit is split into two payments on the 1st and the 15th of the month.

decision-making processes, it is expected that they will time their shopping trips to coincide with the arrival of benefit and purchase groceries for the entire month to reduce the frequency of shopping trips and avoid incurring fixed costs. Consequently, we may observe a decline in expenditures over the course of the benefit month, even if consumption remains relatively constant. However, if weekly shopping frequencies remain constant over the course of the month, it is unlikely that households are purchasing groceries for the entire month in a single trip at the time of benefit arrival. Rather, households may be stocking up on groceries for a shorter interval. In this case, the observed spending pattern should mirror the consumption pattern, rather than being generated by the fixed travel cost of grocery shopping trips.

1.5 Food Expenditure Data and Summary Statistics

1.5.1 Household Characteristics

I use the 2018 and 2019 NielsenIQ Homescan Panel data from the Kilts Center for Marketing Data at the University of Chicago Booth School of Business in this study.[4] This dataset comprises purchase records for a nationally representative panel of households. The dataset contains demographic details of the household head including gender, age, race, and education. Additional household features, including the number of residents, their composition, income, as well as the number of adults and children, along with the household's zip code of residence, are also included and updated annually. NielsenIQ aims at obtaining a nationally representative sample based on a range of demographic characteristics when recruiting panel households. Lusk and Brooks (2011) discovered that, in comparison to a random digit dial sample, Homescan panelists are typically older, more highly educated, and more likely to be white. Due to the nature of the selection process for the Homescan panel, there are two reasonable methods for interpreting the estimates in this study. First, we can view the estimates as being internally valid for the sample of Homescan panelists or the population they represent. Second, we can assume that the estimates are valid for the entire population, on the condition that the impact of the timing of SNAP payment on food expenditures is uniform across different populations.

To identify the policy relevant population, this dataset is merged with a supplemental survey administered by NielsenIQ, which provides additional information on household participation in SNAP. Specifically, the supplemental survey asks households in the Homescan Panel every other quarter:

Are you or anyone in your household currently using or have you ever used food stamps, which includes food stamp card or voucher or cash grant from the state for food (also known as Supplemental Nutrition Assistance Program (SNAP), Electronic Debit Card (EBT card))?

1) Currently using food stamps

[4]The Kilts Center for Marketing at the University of Chicago Booth School of Business (n.d.). Consumer Panel Data Overview. NIELSEN AND NIELSENIQ MARKETING DATA. Retrieved August 26, 2021, from https://www.chicagobooth.edu/research/kilts/datasets/nielseniq-nielsen

2) Have used food stamps, but not currently using them

3) Have never used food stamps

Table 1.2 Summary Statistics of Household Characteristics

Variable	Non-Participants		Participants					
			Both years		2018 only		2019 only	
Household size	2.11	(1.09)	2.05*	(1.34)	2.43*	(1.47)	2.35*	(1.37)
Income below $40k	0.27	(0.44)	0.86*	(0.35)	0.68*	(0.47)	0.68*	(0.47)
Married	0.6	(0.49)	0.27*	(0.44)	0.42*	(0.49)	0.36*	(0.48)
Presence of children	0.14	(0.34)	0.18*	(0.38)	0.24*	(0.43)	0.21*	(0.41)
Head age > 50	0.82	(0.39)	0.78*	(0.41)	0.70*	(0.46)	0.69*	(0.46)
Head Employed	0.64	(0.48)	0.35*	(0.48)	0.64	(0.48)	0.48*	(0.50)
College Degree	0.56	(0.50)	0.29*	(0.46)	0.40*	(0.49)	0.32*	(0.47)
White	0.85	(0.36)	0.79*	(0.41)	0.72*	(0.45)	0.77*	(0.42)
Black	0.08	(0.27)	0.14*	(0.35)	0.21*	(0.41)	0.14*	(0.35)
Asian	0.03	(0.18)	0.02*	(0.13)	0.02*	(0.13)	0.02*	(0.13)
Hispanic Origin	0.05	(0.22)	0.06	(0.23)	0.06	(0.25)	0.08*	(0.28)
Observations	20,696		1,166		278		364	

[a] This table summarizes demographic characteristics of the households in the NielsenIQ Homescan Panel with matched self-reported SNAP participation status. Households are categorized into groups by their participation in SNAP in the first quarter of 2018 and 2019. Marital status, age, employment, education, and race are based on the information of the household head. The first column under each category reports the mean values and the second column reports the standard deviation.

[b] * indicates significant difference from column (1) at the 5% level.

I obtain the survey information on SNAP participation for the first quarter of 2018 and 2019. Based on the SNAP participation status in both years, panelists can be divided into four groups – participants in both years, non-participants in both years, participants in 2019 only, and participants in 2018 only. Table 1.2 shows the summary statistics of these households. Of the 22,504 panelists in this sample, 20,696 do not report current or past SNAP participation in both years, and 1,166 report participation in both years. There is also a small portion of the respondents who changed SNAP participation status between the two waves of survey. Specifically, 278 panelists only received SNAP in 2018, and 364 panelists received benefits only in 2019. Compared to non-participants, SNAP participants have lower income, are less likely to be married, less likely to be employed, less educated, more likely to be Black, and less likely to be Asian than non-participants. Those who participated in only one year are younger, more likely to be married, more educated, more likely to be employed, and have higher income than two-year participants.

1.5.2 Food Purchases

In the NielsenIQ Homescan Panel data, households recruited by Nielsen Consumer LLC receive a scanner to scan the barcodes of products purchased in many different retail channels, such as grocery stores, dollar stores, convenience stores, as well as online shopping platforms, etc. For each product purchased, I observe the universal product classification (UPC) code, purchase date, transaction price, quantity purchased, total expenditure, as well as the coupon savings applied to the item.

Table 1.3 Summary Statistics of Household Characteristics

Weekly Spend	Non-Participants		Participants					
			Both years		2018 only		2019 only	
Total	114.55	(70.43)	101.73*	(74.34)	110.55	(70.56)	107.05*	(89.12)
SNAP Eligible	57.79	(34.81)	52.96*	(34.71)	57.07	(37.76)	54.93	(44.66)
Fresh Produce	6.13	(6.87)	3.85*	(5.00)	4.60*	(5.44)	4.09*	(5.01)
Meat	3.4	(3.60)	3.12*	(3.32)	3.92*	(5.38)	3.45*	(3.86)
Dairy	7.41	(5.69)	6.22*	(5.17)	6.90*	(5.97)	6.54*	(5.61)
Frozen foods	11.66	(9.56)	11.03*	(9.70)	12.06	(10.74)	11.4	(11.02)
Shelf-stable	9.1	(8.71)	9.64*	(7.12)	9.41	(8.54)	9.78	(7.32)
Shopping days	2.18	(1.09)	2.13*	(1.16)	2.28*	(1.19)	2.13	(1.22)
Observations	20,696		1,166		278		364	

[a] This table summarizes demographic characteristics of the households in the NielsenIQ Homescan Panel with matched self-reported SNAP participation status. Households are categorized into groups by their participation in SNAP in the first quarter of 2018 and 2019. Marital status, age, employment, education, and race are based on the information of the household head. The first column under each category reports the mean values and the second column reports the standard deviation.

[b] * indicates significant difference from column (1) at the 5% level.

I categorize the purchased food into different groups. Based on the USDA guidance, SNAP benefits are mainly intended for staple foods that make up a significant portion of a person's diet and are usually prepared at home. Households cannot use their benefits to buy non-food items, alcoholic beverages, cigarettes, vitamins, and foods that are hot at the point of sale. I flag items as

SNAP eligible or not according to the food eligibility rules. Then these SNAP eligible items are further categorized into four major food groups – fresh fruits and vegetables, meat, dairy products, and frozen foods. Table 1.3 displays the mean weekly shopping frequencies and food category expenditures for the four participation groups for expenditures during the first quarter of 2019.

A typical two-year SNAP participating household spends approximately \$102 in grocery purchase in an average week in the first quarter of 2019. Nearly half of these expenditures were spent on SNAP eligible foods. Other expenditures were spent on non-food grocery items or ready-to-eat foods which are not eligible for SNAP benefits. For the specific food categories, a household spends \$3.85 on fresh fruits and vegetables, \$3.15 on meat, \$6.27 on dairy products, and \$11.20 on frozen items. Their expenditures are significantly lower than those of non-participants in each category.

Expenditure data shows that compared to both-year participants, households who reported participation in the program for one year generally spend more on groceries and most food categories. However, when compared to non-participants, they tend to spend less on fresh produce, meat, and dairy. This suggests that one-year participation is a sign of temporary hardships for these households. Although they are in a better condition than both-year participants, they are still at a disadvantage compared to non-participants.

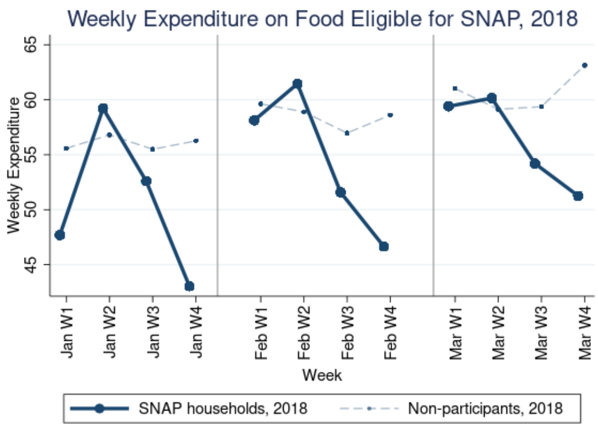
To explore how the SNAP payment disruption affects food expenditures trajectory over calendar months, Figure 1.5 plots the weekly purchase patterns by SNAP participation during the first quarter of 2019 and the same dates in 2018.[5] The outcome displayed on the y-axis is the weekly expenditure on food eligible for SNAP benefits. Because the number of days in a month differs, I only include the first four weeks of January and March in the graph. In Panel A, the thick blue line shows the expenditures among SNAP participants in 2018.[6] In the first three months of 2018, there is a clear decreasing pattern in the purchases of SNAP eligible food from the beginning of every month except January where expenditures in the first week are likely pulled forward by the holidays. This pattern confirms the so-called "first-of-the-month" effect, where SNAP recipients decrease their

[5]Because not all households receive benefits on the first of the month during this period, the graph shows expenditure trajectory over calendar months rather than actual SNAP benefit month defined as the interval between two monthly payments.

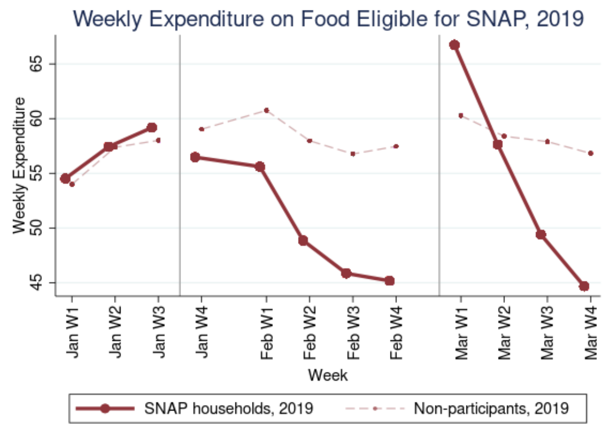
[6]This includes those who report participation in 2018 and 2019, and those who report participation only in 2018.

Figure 1.5 Weekly Expenditures on SNAP Eligible Items

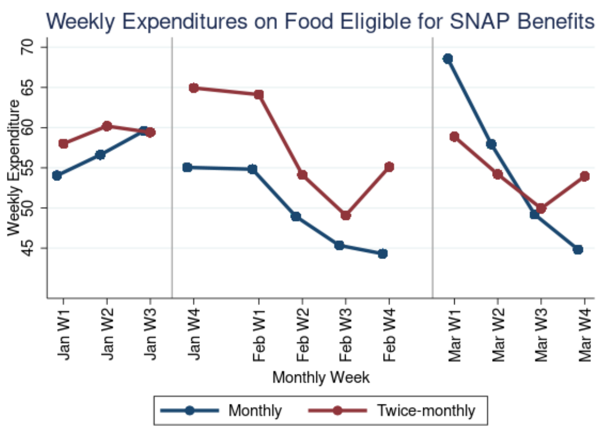
Panel A



Panel B



Panel C



Note: These figures plot the average weekly expenditures on food eligible for SNAP benefits. Weeks are the seven-day periods counting from the 1st to the 28th day of each month. SNAP households are participants in the first quarter of 2018 and 2019. Non-participants are households that did not receive SNAP in the first quarter of both years. Panel A and Panel B plot average weekly spending by SNAP participation status in 2018 and 2019 respectively. Panel C shows the weekly expenditures pattern for SNAP households in 2019 by payment frequency.

expenditures throughout the calendar month. The dashed line indicates weekly expenditures among non-participants during the same period. Their intra-month expenditures do not follow the same pattern as SNAP participants. In Panel B, the solid red line presents increased expenditures upon receipt of the early payment at the end of the third week of January in 2019. The expenditure decreases from the early payment until the beginning of March when spending increases, followed by declining spending throughout March. The spending path in 2019 is much different when SNAP payments were disrupted, whereas non-participants' spending remained relatively stable. Panel C presents the weekly expenditures pattern for SNAP households in 2019 by payment frequency.

In March 2019, there is a steeper drop in spending among households receiving monthly SNAP payments compared to those receiving twice-monthly payments, providing graphical confirmation of the smoothing effect on spending over the calendar month of twice-monthly payment. However, because the first date of March issuance varies across states, grouping daily spending by calendar dates may not accurately reflect the smoothing effect on expenditures over the benefit month.

1.6 Empirical Strategy and Results

To further explore the impact of disbursement timing on expenditures, relying solely on graphical intuition is insufficient for several reasons. First, other factors that may affect expenditures by shifting supply or demand need to be controlled for to isolate the effect of disbursement timing. For example, access to food retailers can affect shopping behaviors significantly. A household could frequently shop for food if living nearby the retailers. The differing travel costs, and consequently shopping frequencies, could systematically alter the timing and composition of food purchases. If the access to food retailers correlates with state SNAP payment frequencies during March 2019 among the survey respondents, omitting this measure could lead to biased estimates. Second, unobserved confounding trends such as the trajectory of food supply or idiosyncratic shopping pattern generated by other income flows must also be accounted for. For example, if a participant receives both the SNAP payment and the monthly paycheck on the first of the month, the SNAP cycle is exacerbated by the increased spending caused by the arrival of paychecks. In contrast, if the participant receives the SNAP payment on the first of the month and the paycheck arrives in halves on the 1st and the 15th of the month, the observed spending trajectory is smoothed out due to the later payment of labor income. Not accounting for these unobserved income streams when a state that split March SNAP payment also tends to require more frequent paychecks would bias the estimates.

I address these concerns by estimating an event study model showing the weekly change in expenditures incorporating observable factors that affect the level of food expenditures such as demographic characteristics and local food prices, as well as factors that affect shopping frequencies such as the number of retailers within the zip code of their residency. I also control for time-invariant unobserved confounding factors such as individual taste preferences over food items by including individual level fixed effects. I then combine the event study with the difference-in-differences framework to account for the time varying unobserved confounding factors such as their other income streams.

1.6.1 The Effect of SNAP Timing on Spending Trajectories

I first group daily expenditures into benefit weeks defined as the 7-day intervals counting from the day of SNAP March payment. I estimate the contribution of SNAP payment to the observed spending path by controlling for unobserved confounding trends. To control for unobserved seasonality that homogeneously affects all consumers, I use expenditures of non-participants as a control group. To account for the effect of idiosyncratic monthly spending pattern within households generated by unobserved income flows and fixed shopping habits, the expenditures of the same households in 2018 serve as a second control group. I limit the analysis to households that participated in the SNAP program in 2019 but not in 2018, which allows for the identification of the effect of SNAP payment rather than a change in the payment across years if they received benefits in both years.[7] The estimation equation is shown in equation (1.1).

$$
\begin{aligned}
y_{it} =& \beta_0 + \sum_{k=-3, k\neq -1}^{5} \beta_{1k} \cdot 1\left[\text{ Week }_{it} = k\right] + \beta_2 \cdot SNAP_i + \beta_3 \cdot Y2019_t \\
&+ \sum_{k=-3, k\neq -1}^{5} \beta_{4k} \cdot 1\left[W_{eek_{it}} = k\right] \cdot SNAP_i + \beta_5 \cdot SNAP_i \cdot Y2019_t \\
&+ \sum_{k=-3, k\neq -1}^{5} \beta_{6k} \cdot 1\left[\text{ Week }_{it} = k\right] \cdot Y2019_t \\
&+ \sum_{k=-3, k\neq -1}^{5} \beta_{7k} \cdot 1\left[\text{ Week }_{it} = k\right] \cdot SNAP_i \cdot Y2019_t + \delta \cdot X_{it} + \pi_t + c_i + \varepsilon_{it}
\end{aligned}
\tag{1.1}
$$

where y_{it} is the logged daily expenditure. $1\left[\text{ Week }_{it} = k\right]$ is the indicator of date t falling into the

[7]Analyzing the cross-year change in expenditures among households that participated in both 2018 and 2019 is an alternative approach to estimate the effect of a change in SNAP payment frequency. However, it is not feasible in this case because the SNAP payment dates for individual households in 2018 are not observed. The states that issued twice-monthly payments in 2019 used staggered issuance schedules during normal times, making it impossible to identify the start dates of the same benefit cycle in 2018. Although the start dates of the March benefit cycle in 2019 are identifiable because the first half was paid on the same day for all households, the lack of information to match payment dates with these households hinders the analysis.

kth week from the date of March payment in 2019 or the number of weeks counting from the same date in 2018 if t falls in year 2018. $SNAP_i$ indicates households that only participated in SNAP in 2019 but not 2018. The remaining households did not participate in SNAP in either year. $Y2019_t$ equals one if date t falls into 2019. I control for factors affecting shopping patterns in X_{it}. These include household characteristics such as age, marital status, education, race of the household head, presence of children in the household, the number of SNAP retailers at the same zip code and local TFP costs. I include daily average temperature and precipitation to address factors that affect shopping trips. π_t includes federal holidays and day-of-week fixed effects. c_i is the unobserved household fixed effect. ε_{it} is an error term. I use robust standard errors throughout the analysis.

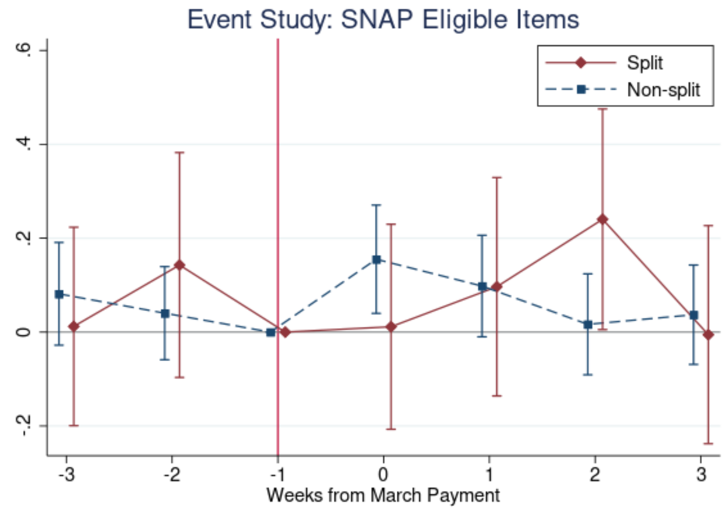
The parameter of interest is β_{7k}, which traces out the percentage change in spending by participation across years in the weeks before and after the benefit payment. Under the assumption that the difference in household purchasing pattern by SNAP participation would have evolved the same as in 2018 without the SNAP payment in 2019, β_{7k} represents the causal effect of the March SNAP payment on the spending in the same week across years.

In Figure 1.6, the point estimates of the weekly change in expenditures on SNAP eligible items compared to the week prior to the March payment, along with their 95% confidence intervals, are presented by payment frequency. For SNAP households under monthly payments, the spending in the first week after payment remained significantly higher than pre-payment levels and gradually decreased throughout the month.

Figure 1.6 also plots the case with households receiving twice-monthly payment. Prior to the March payment, I do not detect any significant changes in expenditures compared to the last week before the payment. While the arrival of the first half of the March benefit did not induce a significant increase in expenditures, there was a significant increase two weeks after the payment of the first half benefit. These results suggest that twice-monthly SNAP payment contributed to a smoother spending trajectory by increasing expenditures relative to SNAP participants under monthly payment in the latter half of the benefit month.

Figure 1.7 shows the estimation results by food category. For households receiving lump-sum

Figure 1.6 Dynamic Triple Difference: Change in Spending on SNAP Eligible Items



Note: The figure plots the estimates of the dynamic triple difference model evaluating the effect of March SNAP payment by payment frequency. The sample consists of households that participated in SNAP in 2019 but not 2018, as well as households that did not participate in 2018 or 2019. The coefficients are estimated from random effect regressions of daily food expenditures on the three-way interactions of week indicators, an indicator of year 2019, and an indicator of SNAP participation. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

payment, the March payment led to a significantly increase in expenditures on fresh fruits and vegetables, as well as shelf stable foods in the week of payment, suggesting that the monthly lump-sum payment of SNAP benefits drives an intra-monthly spending cycle characterized by increased purchases of fresh and shelf-stable foods upon benefit receipt. Given the limited shelf life of fresh produce, this also suggests an increase in consumption at the beginning of the month. The higher expenditures on shelf-stable foods suggest that stocking up was also a driver of the observed spending pattern.

For households under twice-monthly payments, the higher payment frequency caused expenditures on fresh fruits and vegetables to increase two weeks after receipt. However, the effect on spending over shelf-stable foods is not significantly different from zero throughout the March benefit cycle. These findings indicate the split payments smooth spending on fresh fruits and vegetables and meat later in the month. These results have important implications for policymakers seeking to optimize the timing and frequency of SNAP benefits to improve food security for low-income households, as they provide evidence of smoother consumption under higher payment frequency.

1.6.2 Testing the Smoothing Effect of Higher SNAP Payment Frequencies

I measure the smoothness of the spending trajectory by the difference between the average daily expenditures in the first week after payment and three weeks that follow. Specifically, I estimate the following regression by ordinary least squares:

$$\begin{aligned} y_{it} =& \alpha_0 + \alpha_1 \cdot \text{Week1}_{it} + \alpha_2 \cdot \text{Y2019}_t + \alpha_3 \cdot \text{SNAP}_i + \alpha_4 \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \\ &+ \alpha_5 \cdot \text{Week1}_{it} \cdot \text{SNAP}_i + \alpha_6 \cdot \text{Y2019}_t \cdot \text{SNAP}_i + \alpha_7 \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \cdot \text{SNAP}_i \\ &+ \delta \cdot X_{it} + \pi_t + c_i + \varepsilon_{it} \end{aligned} \tag{1.2}$$

where Week1_{it} is the indicator of the first seven days following the date of March payment in 2019 and the same seven calendar dates in 2018. Other notations are consistent with previous definitions. I use robust standard errors throughout the analysis. The sample period is restricted to the first four weeks following March benefit payment. The estimate of α_7, which is a measure of the smoothness of the spending path in March, accounting for the unobserved within-household time effect and across-household seasonal trend. A significantly positive $\hat{\alpha}_7$ indicates a declining spending path caused by the SNAP payment.

Column (1) of Table 1.4 reports the estimation results with households residing in states that used a twice monthly payment. Column (2) presents the results for households in states that issued monthly payment in March 2019. Under a monthly lump-sum payment, the spending on average day in the first week following benefit payment was 12% higher than that of an average day in the subsequent weeks, while the difference across weeks was not significant under a twice-monthly payment scheme.

A closely related study, Beatty et al. (2019), finds an average household spends 8% more if the day of requisition is in the first week of the benefit cycle. Shapiro (2005) estimates the daily decline in the value of food consumption to be 7% using food intake survey data. This is equivalent to a 7.3% decline in food value if we group dates into weeks. Despite the difference in the study population and identifying assumptions, the estimate with the population under monthly payment during the disruption is consistent with these earlier findings.

Table 1.4 The Smoothness of the SNAP Spending Cycle

	ln(Daily Expenditures on SNAP Items)	
	(1)	(2)
	Twice-monthly	Monthly
Week1*SNAP*Y2019	-0.109	0.124*
	(0.112)	(0.060)
Time Fixed Effect	✓	✓
Household Characteristics	✓	✓
Retailers & Weather	✓	✓
Avg. Exp. In Ref. Weeks	60.71	51.81
N	249,049	1,061,039

[a] This table presents estimation results of equation (3) where the dependent variables are logged expenditures on items eligible for SNAP benefits and the main independent variables are the interactions of a dummy for the year of disruption, a dummy for the first week in the benefit month, and a dummy for 2019-only SNAP participant. The sample consists of purchases during the four weeks after the March payment among households that only participated in 2019 and households that never participated in 2018 and 2019. All specifications are estimated using random effect GLS accounting for unobserved household fixed effect. All regressions control for the full set of control variables.

[b] Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

To formally test whether there was a smoothing effect of higher payment frequencies, I add an interaction of an indicator of split benefits to the estimation equation 1.2. The coefficient on the four-way interaction term shows the effect of splitting benefits into twice-monthly payments.

$$\begin{aligned}
y_{it} =& \gamma_0 + \gamma_1 \cdot \text{Week1}_{it} + \gamma_2 \cdot \text{Y2019}_t + \gamma_3 \cdot \text{SNAP}_i + \gamma_4 \cdot \text{Split}_i + \gamma_5 \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \\
&+ \gamma_6 \cdot \text{Week1}_{it} \cdot \text{SNAP}_i + \gamma_7 \cdot \text{Week1}_{it} \cdot \text{Split}_i + \gamma_8 \cdot \text{Y2019}_t \cdot \text{SNAP}_i \\
&+ \gamma_9 \cdot \text{Y2019}_t \cdot \text{Split}_i + \gamma_{10} \cdot \text{SNAP}_i \cdot \text{Split}_i + \gamma_{11} \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \cdot \text{SNAP}_i \\
&+ \gamma_{12} \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \cdot \text{Split}_i + \gamma_{13} \cdot \text{Week1}_{it} \cdot \text{SNAP}_i \cdot \text{Split}_i \\
&+ \gamma_{14} \cdot \text{Y2019}_t \cdot \text{SNAP}_i \cdot \text{Split}_i + \gamma_{15} \cdot \text{Week1}_{it} \cdot \text{Y2019}_t \cdot \text{SNAP}_i \cdot \text{Split}_i \\
&+ \delta \cdot X_{it} + \pi_t + c_i + \varepsilon_{it}
\end{aligned} \tag{1.3}$$

Table 1.5 The Effect of Twice-Monthly SNAP Payment

	(1) ln(SNAP exp)	(2) ln(SNAP exp)	(3) ln(SNAP exp)	(4) ln(SNAP exp)
Split vs. No Split	-0.198*	-0.195*	-0.195*	-0.196*
	(0.104)	(0.103)	(0.104)	(0.104)
Time Fixed Effect		✓	✓	✓
Household Characteristics			✓	✓
Retailers & Weather				✓
Avg. Weekly Exp. in Week 1	54.88	54.88	54.88	54.88
Avg. Exp. Other weeks	51.66	51.66	51.66	51.66
N	1,320,416	1,320,416	1,320,416	1,310,088

[a] This table contains the results obtained when the dependent variables are logged expenditures on items eligible for SNAP benefits and the main independent variables are the interactions of a dummy for the year of disruption, a dummy for the first week in the benefit month, a dummy for 2019-only SNAP participant, and a dummy for twice-monthly payment. The sample consists of purchases during the four weeks after the March payment among households that only participated in 2019 and households that never participated in 2018 and 2019. All specifications are estimated using random effect GLS accounting for unobserved household fixed effect. Columns (2)-(4) control for day-of-week fixed effects and holidays. Columns (3) and (4) additionally control the presence of children, marital status, age, education, and race of the household head, along with local retailers and food cost. Daily precipitation and temperature are added in the last column.

[b] Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.5 shows the results of the four-way interaction regression model. The coefficient on the four-way interaction term captures the difference in the smoothness of the spending cycle caused by the higher frequency of payment. In all specifications, the first week spending spike is 20% lower for households that received March benefits in two payments than those received one-time monthly payment. Under the assumption that the SNAP cycle would have been the same as in monthly-payment states for households receiving split disbursement if it were monthly payment, this estimate has a causal interpretation.

Table 1.6 presents the estimation results by food categories. Decomposing the effect of receipt frequencies into food categories, for fresh fruit and vegetables, the first-week spending spike was 10% lower under twice-monthly payment than the lump-sum payment. These findings provide supporting evidence that higher payment frequency caused smoother spending over the benefit month. Furthermore, there are indications of smoother expenditures translating into smoother consumption of fresh fruit and vegetables under the twice-monthly payment, providing a policy-relevant insight that higher SNAP payment frequency could improve food security for SNAP beneficiaries.

Table 1.6 The Effect of Twice-Monthly SNAP Payment, Decomposition by Food Categories

	(1) ln(Fresh)	(2) ln(Dairy)	(3) ln(Meat)	(4) ln(Frozen)	(5) ln(Stable)
Split vs. No Split	-0.103*	-0.043	-0.04	-0.01	-0.087
	(0.057)	(0.059)	(0.047)	(0.080)	(0.068)
Avg. Weekly Exp. Week 0 ($)	4.34	6.65	3.29	12.04	9.24
Avg. Exp. Other weeks ($)	4.13	6.3	3.08	10.78	8.82
N	1,310,088	1,310,088	1,310,088	1,310,088	1,310,088

[a] This table contains the results obtained when the dependent variables are logged expenditures on food categories eligible for SNAP benefits and the main independent variable is the interaction of a dummy for the year of disruption, a dummy for the first week of the benefit month, a dummy for 2019-only SNAP participants, and a dummy for twice-monthly payment. The sample consists of purchases during the four weeks after the March payment among households that only participated in 2019 and households that never participated in 2018 and 2019. All specifications are estimated using random effect GLS accounting for unobserved household fixed effect. All columns control for day-of-week fixed effects and holidays, and household characteristics including presence of children, marital status, age, education, and race of the household head, number of local SNAP retailers, the local food cost, as well as daily precipitation and temperature.

[b] Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

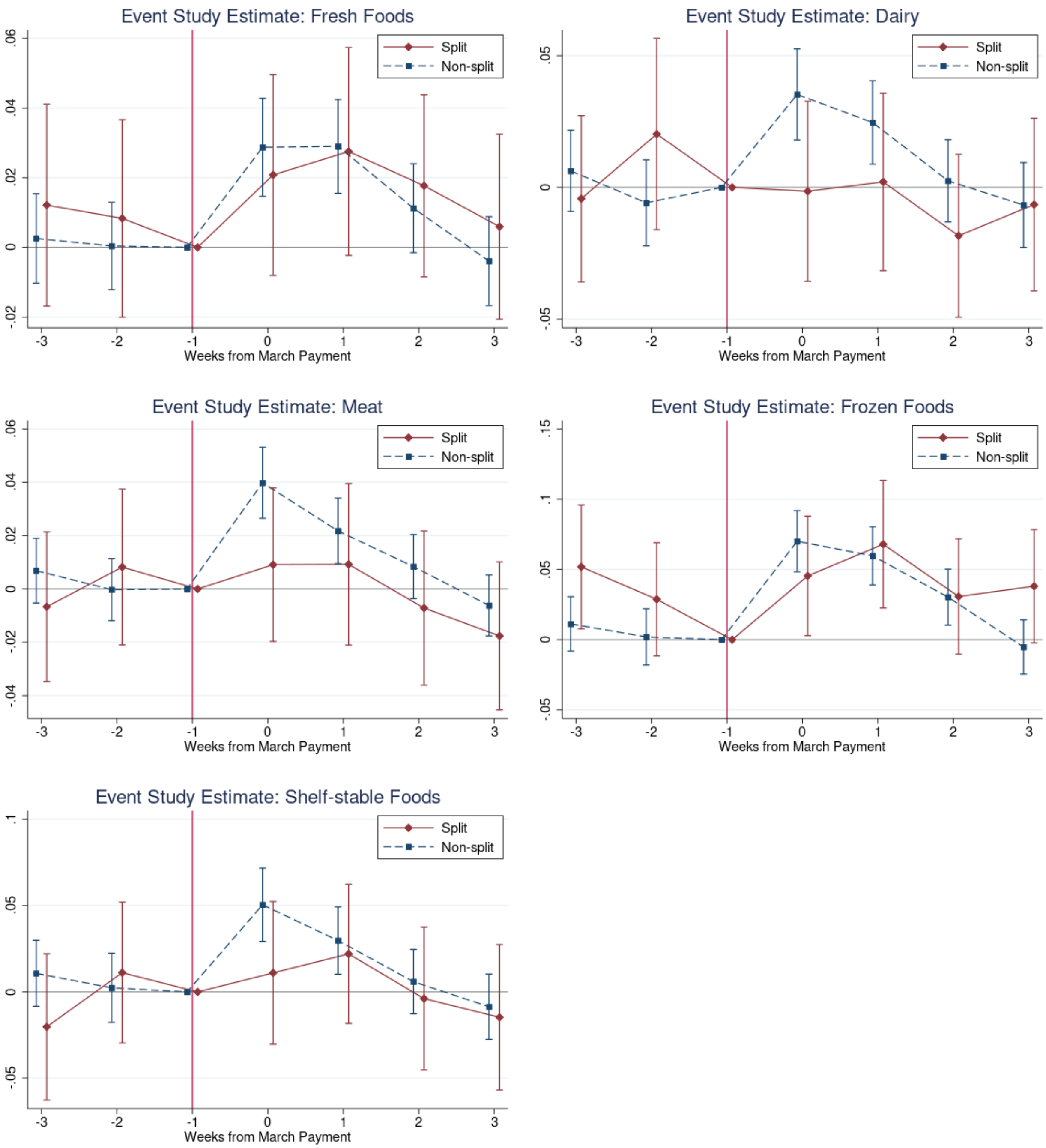
1.7 Conclusion

This study investigates the impact of higher SNAP payment frequency on households' grocery spending patterns, taking advantage of the disruptions caused by the 2018-2019 federal government shutdown. The findings suggest that semi-monthly payments lead to smoother grocery spending over the benefit month, particularly in perishable food items such as fresh fruits and vegetables, indicating potential smoother consumption out of this category. These results provide policy-relevant insights where higher payment frequency could be a cost-effective way to help SNAP beneficiaries stretch food budget over the month.

While the findings offer support for the policy proposal of increasing the frequency of SNAP payments, this research also highlights the need for future pilot studies to examine the policy recommendation in a context with higher external validity by design, for example, a longer-term policy experiments not in the wake of a government shutdown. Additionally, the study is limited in the inference of the impact on food consumption because of the lack of consumption data. The study stresses the significance of delving deeper into the direct impact of payment frequency on food consumption, along with any correlated secondary outcomes.

Finally, this analysis sheds light on the vulnerability of low-income households during government shutdowns and emphasizes the need for government stability. The results show significant changes in food purchases during a short-lived disruption in SNAP benefit payment. Due to the fact that families who rely on public assistance are disproportionately affected by shutdowns, it is crucial to ensure adequate funding for the social safety net in order to protect the health and well-being of the country's most vulnerable population.

Figure 1.7 Dynamic Triple Difference: Change in Daily Spending by Food Category



Note: The figure plots the estimates of the dynamic triple difference model evaluating the effect of March SNAP payment by payment frequency. The sample consists of households that participated in SNAP in 2019 but not 2018, as well as households that did not participate in 2018 or 2019. The coefficients are estimated from random effect regressions of daily food expenditures on the three-way interactions of week indicators, an indicator of year 2019, and an indicator of SNAP participation. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

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APPENDIX A

POINT ESTIMATES UNDERLYING THE PLOTS

Table 1A.1 Event Study: Daily Spending on SNAP Eligible Items (Figure 1.6)

	(1) Split	(2) Non-Split
Year=2019; SNAP=1; Week=-3	0.012	0.081
	(0.108)	(0.056)
Year=2019; SNAP=1; Week=-2	0.143	0.040
	(0.122)	(0.051)
Year=2019; SNAP=1; Week=0	0.011	0.155***
	(0.111)	(0.059)
Year=2019; SNAP=1; Week=1	0.097	0.098*
	(0.119)	(0.055)
Year=2019; SNAP=1; Week=2	0.240**	0.017
	(0.120)	(0.055)
Year=2019; SNAP=1; Week=3	0.200	-0.004
	(0.124)	(0.072)
N	676,130	3,202,732

[a] The table presents the estimates of the triple difference model evaluating the effect of March SNAP payment during the government shutdown. The sample consists of 2018 and 2019 expenditures among households participating in SNAP only in 2019 and households not participating in SNAP in either year. The coefficients are estimated from random effect regressions of logged daily food expenditures on the three-way interactions of each week indicator, the year indicator, and the SNAP indicator, as well as the two-way interactions and the main effects of the three variables. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. Column (1) shows the results with households in states that paid March benefits in two payments. Column (2) shows the results with other households that received a monthly lump-sum payment in March.

[b] Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

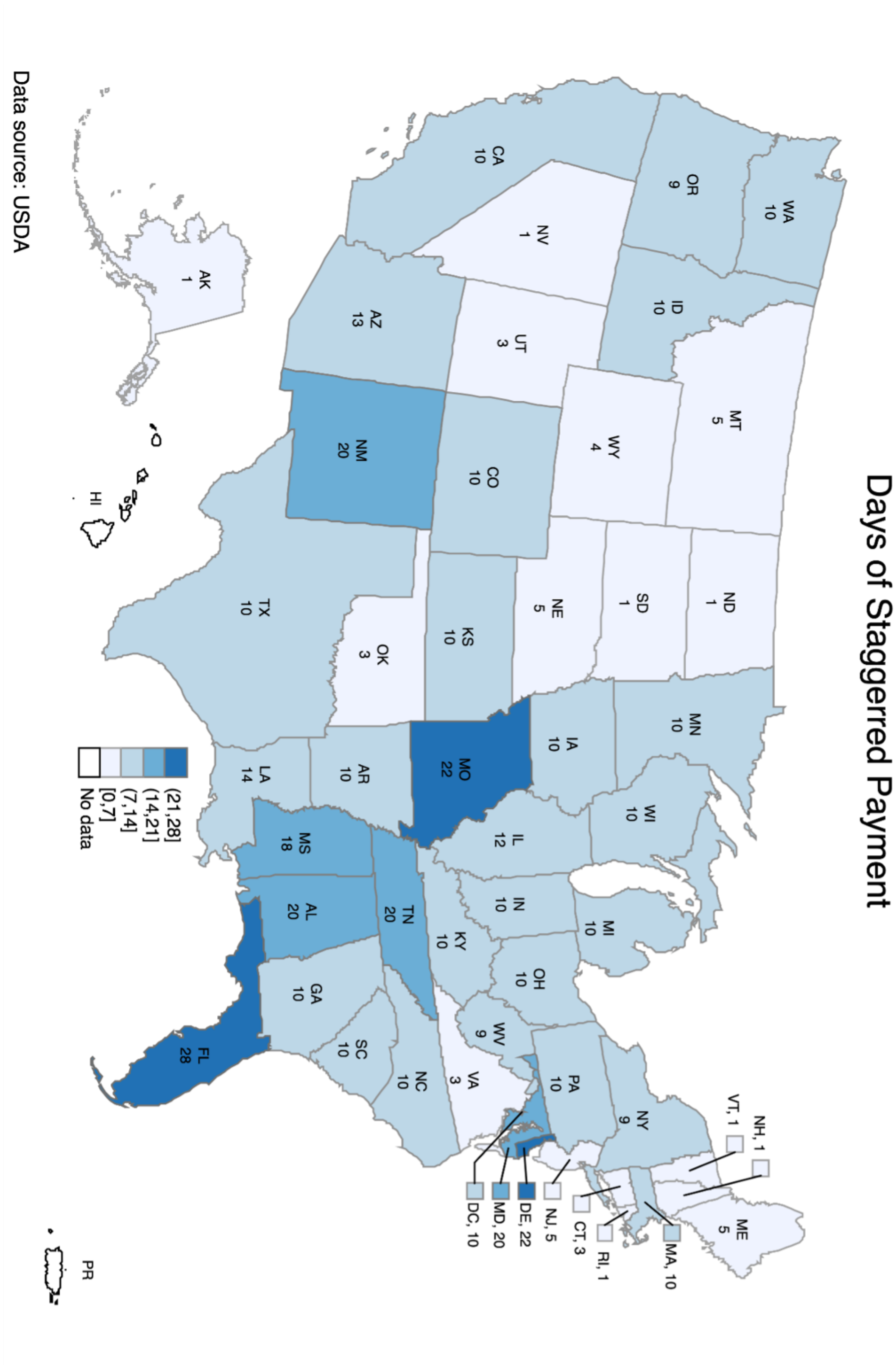
Table 1A.2 Event Study: Daily Spending on SNAP Eligible Items (Figure 1.7)

	(1) Fresh Fruits and Vegetables	(2) Dairy	(3) Meat	(4) Frozen Foods	(5) Shelf-Stable Foods
	Panel A. SNAP Participants under Split Payment				
Week=-3	-0.016	0.016	-0.025	-0.019	-0.120*
	(0.050)	(0.059)	(0.050)	(0.075)	(0.065)
Week=-2	0.006	0.092	0.058	0.121	-0.012
	(0.055)	(0.057)	(0.060)	(0.075)	(0.064)
Week=0	-0.021	0.042	0.012	0.037	-0.063
	(0.050)	(0.057)	(0.051)	(0.080)	(0.077)
Week=1	0.054	0.066	0.034	0.029	0.008
	(0.042)	(0.074)	(0.052)	(0.083)	(0.061)
Week=2	0.100**	0.083	0.110**	0.092	0.022
	(0.044)	(0.051)	(0.052)	(0.083)	(0.062)
Week=3	-0.050	0.042	0.014	-0.015	0.020
	(0.043)	(0.056)	(0.048)	(0.068)	(0.062)
Observations	676,130	676,130	676,130	676,130	676,130
	Panel B. SNAP Participants under Monthly Lump-sum Payment				
Week=-3	0.016	0.033	0.024	-0.002	0.052
	(0.023)	(0.028)	(0.023)	(0.036)	(0.033)
Week=-2	0.000	0.027	0.001	-0.017	0.030
	(0.023)	(0.027)	(0.023)	(0.032)	(0.033)
Week=0	0.056**	0.044	0.003	0.050	0.086**
	(0.024)	(0.027)	(0.023)	(0.036)	(0.037)
Week=1	0.026	0.048*	0.018	0.043	0.072*
	(0.022)	(0.028)	(0.021)	(0.036)	(0.034)
Week=2	0.015	0.019	-0.007	-0.014	0.021
	(0.021)	(0.028)	(0.022)	(0.033)	(0.035)
Week=3	0.025	0.031	0.007	0.022	0.035
	(0.021)	(0.028)	(0.019)	(0.033)	(0.034)
Observations	3,202,732	3,202,732	3,202,732	3,202,732	3,202,732

[a] The table presents the estimates of the event study model using the sample consists of 2019 expenditures among households participating in SNAP in 2019. The coefficients are estimated from random effect regressions of logged daily food expenditures on the week indicators and a full set of control variables. Panel A shows the results with households in states that paid March benefits in two payments. Panel B shows the results with other households that received a monthly lump-sum payment in March.

[b] Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Figure 1A.1 State SNAP Disbursement Days during Normal Times

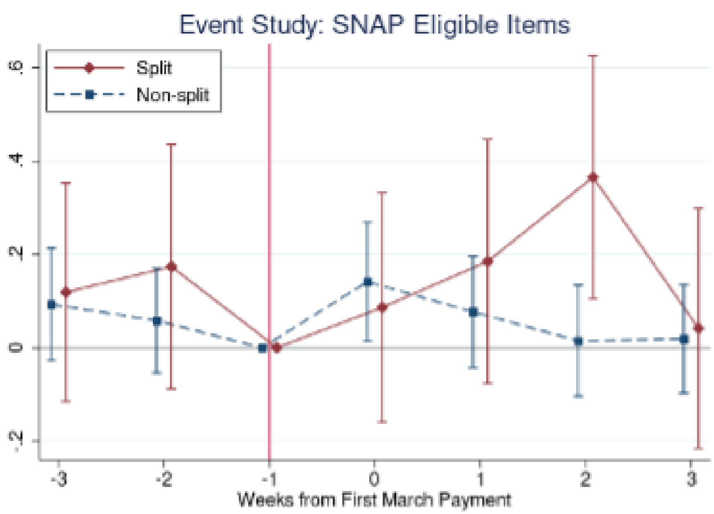


APPENDIX B

USING ELIGIBLE NON-PARTICIPANTS AS THE COMPARISON GROUP

In the main analysis, I use expenditures from the past year and expenditures of all non-participants to control for unobserved trends. In this section, I assess the robustness of the main results by restricting the comparison group to a more comparable group of households – eligible non-participants. I define households as eligible if their reported household income falls below the income threshold of the corresponding family size for SNAP eligibility. Because the incomes are reported with a two-year lag and I ignore the asset requirement, this eligibility measure is subject to measurement error. The following figures and tables present the replication results of the regressions described before.

Figure 1B.1 Dynamic Triple Difference: Change in Spending on SNAP Eligible Items (Using past-year and eligible non-participants as the control groups)

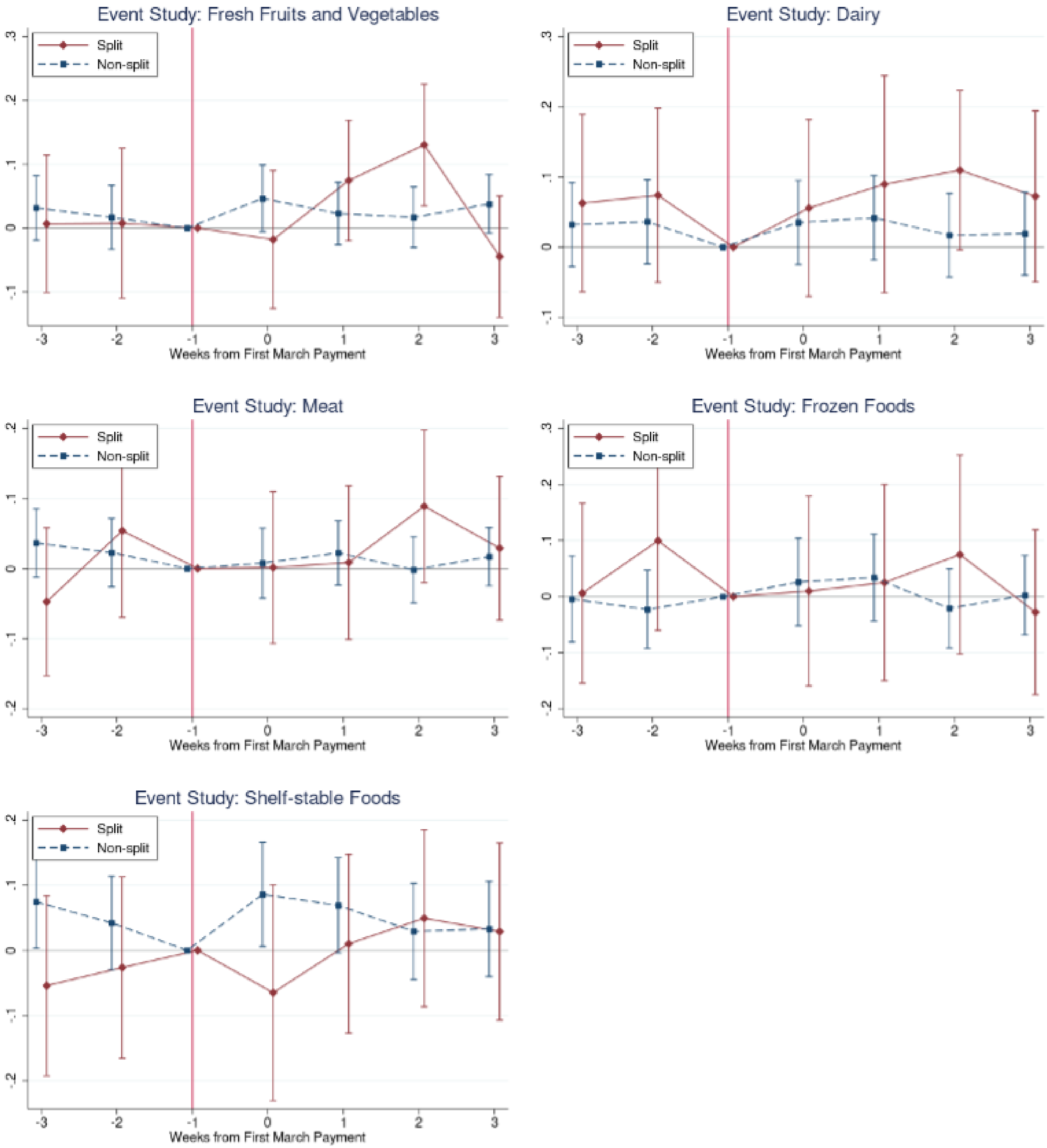


Note: The figure plots the estimates of the triple difference model evaluating the effect of March SNAP payment during the government shutdown. The sample consists of 2018 and 2019 expenditures among households participating in SNAP only in 2019 and households not participating in SNAP in either year. The coefficients are estimated from random effect regressions of logged daily food expenditures on the three-way interactions of each week indicator, the year indicator, and the SNAP indicator, as well as the two-way interactions and the main effects of the three variables. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

Figure 1B.1 presents the results of the regression of equation (2). The results with expenditures on all food items eligible for SNAP benefits are similar with the main results. However, the magnitude is bigger when limiting the sample to participants and eligible non-participants. Under the March lump-sum payment, the spendings on SNAP eligible items increased by 14 percent in the week after payment compared to the week before. The difference gradually decreased and became insignificant over the next three weeks. The magnitude of the effects among households in states that paid two installments are also bigger using this alternative comparison group. Compared with eligible non-participants, participants in such states did not increase spending significantly right after the first payment. However, they spent 38 percent more than eligible non-participants in the third week after the first payment. Because the second payments were spread out according to the normal disbursement schedules in the four states, this spike is not an expected effect of SNAP payments. However, due to the limited sample size of participants in these four states, non-random normal payment days among these households can explain the spike if most get paid in the same calendar week absent the disruption during the shutdown.

Figure 1B.2 plots the results by food category. The spending patterns are similar with the main results with expenditures on fresh fruits and vegetables and shelf-stable foods as the driver of the spike observed upon benefit payment in lump-sum payment states. The first payment in the two payments states did not lead to a significant increase in spendings in all food categories in the first week. And the spike two weeks later was driven by higher spending on fresh fruits and vegetables.

Figure 1B.2 Dynamic Triple Difference: Change in Daily Spending by Food Category



Note: The figure plots the estimates of the triple difference model evaluating the effect of March SNAP payment during the government shutdown. The sample consists of 2018 and 2019 expenditures among households participating in SNAP only in 2019 and households not participating in SNAP in either year. The coefficients are estimated from random effect regressions of logged daily food expenditures on the three-way interactions of each week indicator, the year indicator, and the SNAP indicator, as well as the two-way interactions and the main effects of the three variables. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

APPENDIX C

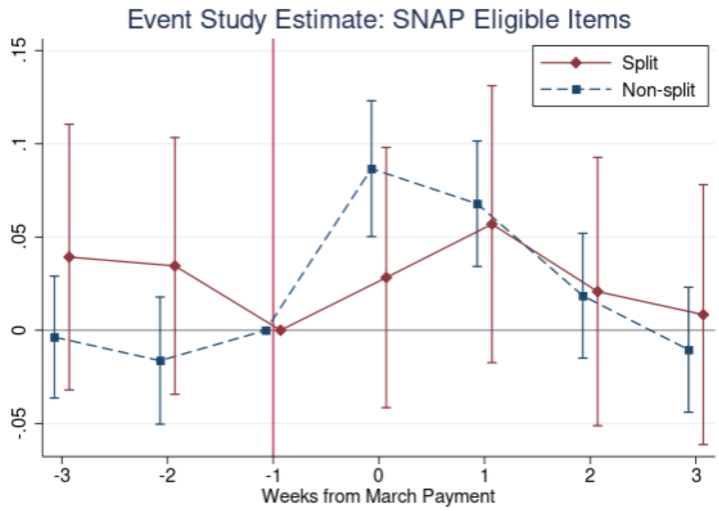
ANALYSIS WITHOUT COMPARISON GROUPS

The following section presents the analysis with 2019 expenditures data among SNAP participants only. I employ an event study approach to estimate the change of daily expenditures in the weeks before and after the March payment for states with one payment and two payments separately. For states that issued semi-monthly March benefits, the weeks are defined as the number of weeks from the first March payment. I estimate the event study model as follows with 2019 expenditures data among SNAP participants:

$$y_{it} = \beta_0 + \sum_{k=-3, k\neq -1}^{5} \beta_{1k} \cdot 1\left[W_{eek_{it}} = k\right] + \delta \cdot X_{it} + \pi_t + c_i + \varepsilon_{it}$$

Figure 1C.1 illustrates the dynamic effects of the March payment(s) on household spending on SNAP eligible foods, separately for SNAP participants under monthly payment and SNAP participants under twice-monthly payment. For households receiving March benefits as a lump sum, the spending path aligns with the predicted consumption pattern in the conceptual model. Specifically, spending levels remained constant in the three weeks prior to the March payment, which is reasonable because households may have depleted their February benefits due to the early payment in January. After the arrival of March benefits, there was a statistically significant 9 percent increase in expenditures on SNAP eligible items, compared with the baseline spending level in the week before the payment. This increase is followed by a gradual decline in subsequent weeks. During the second week following payment, the food spending is still 7 percent higher than the week before payment. Spending in the third and fourth week returned to a similar level as in the pre-payment week, with the third week 2 percent higher and the fourth week 1 percent lower than the baseline level. And these differences are statistically insignificant at standard levels. While the dynamic effects are similar to the pattern estimated in section 1.6 accounting for unobserved trends, the magnitude of the increase in spending on SNAP eligible items is lower. This indicates that the spending patterns generated by SNAP disbursement were partly mitigated by seasonality and idiosyncratic shopping patterns over the month.

Figure 1C.1 Event Study: Daily Spending on SNAP Eligible Items



Note: The figure plots the estimates of the event study model evaluating the effect of March SNAP payment during the government shutdown. The sample consists of 2019 expenditures data among households participating in SNAP in 2019. The coefficients are estimated from a random effect regression of daily food expenditures on the week indicators as well as a full set of control variables. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

On the other hand, for households receiving twice-monthly payments, because the benefits were half of the monthly amount in each payment, the spending path following the first March payment did not show a significant increase upon receipt. The increase in the first week is approximately 3 percent compared to the week before. The magnitude of the changes in expenditures over SNAP eligible items are similar to those among SNAP households under a monthly payment. The milder change indicates a smoothing effect on spending towards SNAP eligible food items.

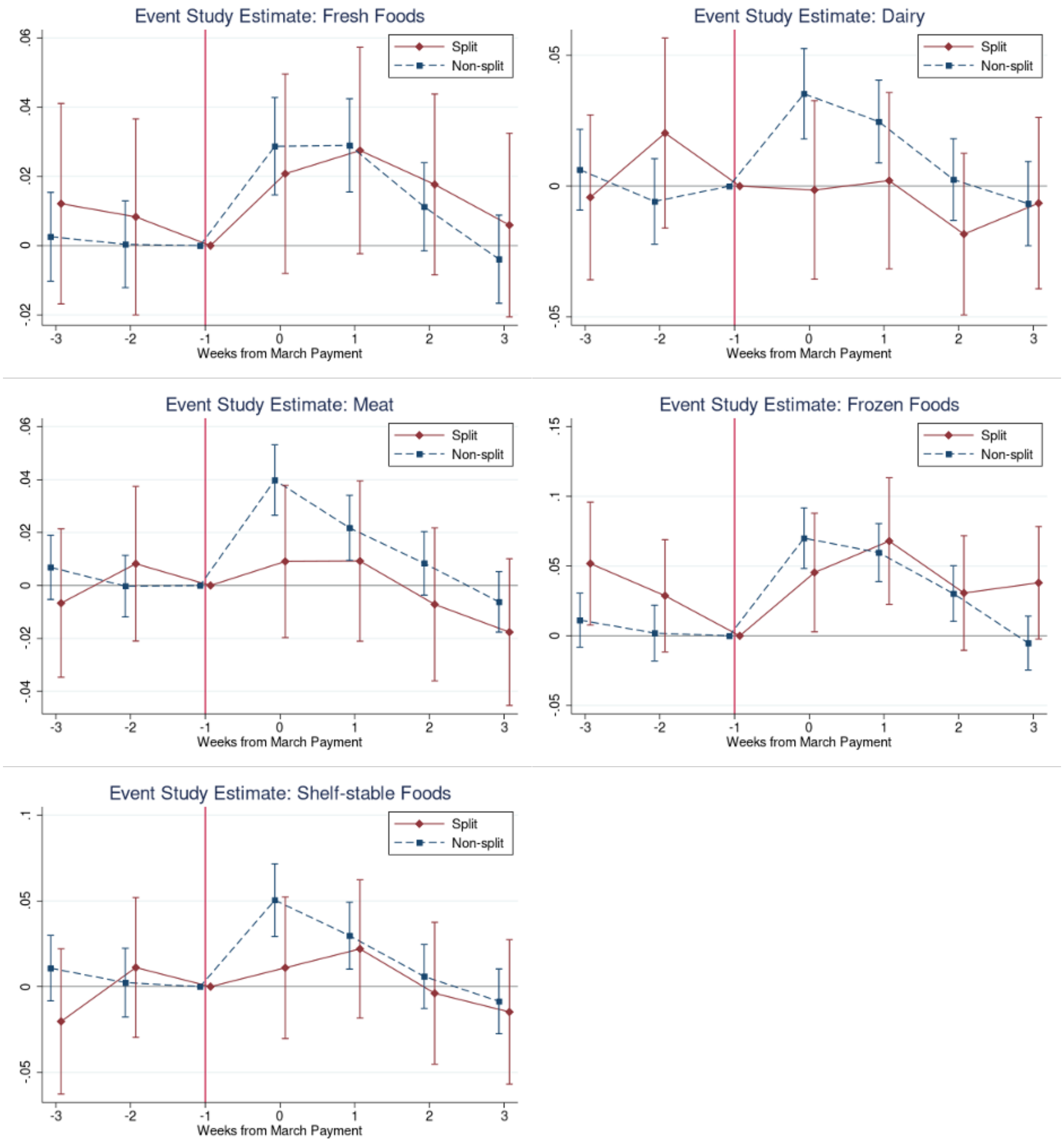
To investigate the potential smoothing effect of SNAP payments on household food consumption, I estimate the same equation using expenditures by food categories, including fresh fruits and vegetables, dairy products, meat, frozen foods, and shelf-stable foods. Since perishable foods have a limited shelf life, consumption of these items should more closely follow expenditures compared to shelf-stable foods. Figure 1C.2 presents the results of the regression analyses by food categories.

The findings suggest that households receiving monthly SNAP payments experienced a significant increase in spending on all food categories upon receipt of the March payment – expenditures over fresh fruits and vegetables, and dairy products increased by 3 percent, spending on meat and shelf-stable foods increased by 5 percent, and spending over frozen foods raised by 7 percent compared to the levels one week before. These increases were followed by a gradual decline over the subsequent weeks, with spending returning to pre-payment levels in the fourth week after payment.

In contrast, the changes in expenditures post-payment were smaller for households receiving twice-monthly payments, indicating a smoothing effect on spending across all food categories. Notably, the results for dairy show constant level of consumption of these most perishable items, despite fluctuations in overall spending.

The assumption for a causal interpretation of the event study estimates described above lies in there being no systematic changes over time except for SNAP payment. Under this assumption, the above estimates the causal effect of March SNAP payment on expenditures among SNAP participants in 2019. There are potential threats to this assumption. First, promotions or food stock at local retailers could shift food demand systematically within the month. Second, unobserved timing of other incomes could contribute to certain intramonth shopping patterns. For example, pay frequency laws and the disbursement schedules of other benefit programs vary by states. The different timing of paychecks could cause the spending pattern across states to diverge if the timing is correlated with grocery trips.

Figure 1C.2 Event Study: Daily Spending by Food Category



Note: The figure plots the estimates of the event study model evaluating the effect of March SNAP payment during the government shutdown. The sample consists of 2019 expenditures data among households participating in SNAP in 2019. The coefficients are estimated from a random effect regression of daily food expenditures on the week indicators as well as a full set of control variables. The regressions control for household characteristics (age, marital status, education, race of the household head, and presence of children under age 18), number of SNAP authorized retailers, local TFP cost, weather effect (daily highest temperature and precipitation), time effect (day-of-week fixed effect, holiday effect), and unobserved household fixed effect. The figure also plots corresponding 95% confidence intervals using robust standard errors.

CHAPTER 2

SNAP WORK REQUIREMENT AND CRIMINAL RECIDIVISM

2.1 Introduction

In 2019, the number of incarcerated individuals in the United States exceeded two million, which was a 500 percent increase from four decades earlier. As a result, the country now has one of the largest incarcerated populations and highest incarceration rates globally (Sawyer and Wagner, 2020). Each year, more than 600,000 offenders are released from US prisons (Carson, 2021). The reentry into their communities is coupled with a series of vulnerabilities including chronic diseases (Mallik-Kane and Visher, 2008), food insecurity (Testa and Jackson, 2019), homelessness (Fontaine, 2013), and limited job prospects (Travis et al., 2014). These obstacles to successful reentry are associated with recidivism rates of two-thirds within three years, with half of ex-offenders returning to prison (Durose et al., 2015). Consequently, ex-offenders are contributing a growing share to overall crime rates (Rosenfeld et al., 2005).

Public benefits play an essential role in the transition among released prisoners. Prisoners may be eligible for certain public assistance programs such as Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Medicaid upon their release, like other low-income individuals and families depending on various factors such as their individual circumstances and the specific program requirements.[1] The Personal Responsibility and Work Opportunity Act (PRWORA), also known as the 1996 welfare reform, mandated work requirements on welfare recipients to reduce dependency and encourage work. Specifically, the reform imposed a time limit for ABAWDs (able bodied adults aged 18-49 without dependents) to receive food stamp benefits, limiting them to three months in a 36-month period unless they work or participate in a work program at least 80 hours per month. Ex-offenders are disproportionately impacted than the general population by this policy because childless adults make up half of the prison population (Maruschak et al., 2021).

[1] In certain states, there are restrictions that prevent individuals with felony drug convictions from accessing SNAP and TANF benefits. See Thompson and Burnside (2022) for the state ban status as of April 2022 at https://www.clasp.org/wp-content/uploads/2022/04/2022Apr_No-More-Double-Punishments.pdf

The work requirement can have mixed effects on recidivism among ex-prisoners. On the one hand, according to the canonical crime model following Becker (1968), which assumes crimes are rational behavior, individuals commit crimes when the expected utility from criminal behaviors exceeds that of non-criminal activities. The work requirement raises the opportunity cost of obtaining public assistance, so ex-prisoners may resort to crimes to achieve a higher utility level. On the other hand, the requirement may reduce the time devoted to illegal activities among ex-prisoners who strive to remain eligible for transfer benefits, resulting in lower recidivism rates among released ABAWDs. Whether work requirements promote or discourage recidivism remains an empirical question.

This paper studies how work requirements linked with the Supplemental Nutrition Assistance Program (SNAP) affect recidivism. SNAP is the largest food assistance program in the United States, providing direct benefits to over 41 million individuals at a cost of $108 billion in fiscal year 2021 (United States Department of Agriculture, 2021). It plays a vital role in supporting post-prison reentry by providing basic food assistance and supplementing inadequate income (Wolkomir, 2018). Additionally, the SNAP Employment and Training (E&T) program offers robust and targeted interventions that could help alleviate the significant employment barriers faced by some formerly incarcerated individuals (Wolkomir, 2018). PRWORA provides states with flexibility to apply for a SNAP time limit waiver for areas with a high unemployment rate or inadequate job opportunities. Moreover, states can exempt up to 15% of non-waived ABAWDs who have depleted their eligibility. During the great recession, a blanket waiver was implemented through September 2010 via the American Recovery and Reinvestment Act (ARRA). States began reinstating their work requirements as employment began to recover. This variation provides a source of identification for estimating the effect of the SNAP work requirement on recidivism of ex-offenders by utilizing administrative data of federal and state prison records from 2011 to 2017, exploiting the temporal and geographical variation in work requirement status and the ABAWD upper age cutoff in a triple difference approach.

I find that work requirements reduce recidivism. Specifically, while ex-prisoners released to areas with work requirements have higher probability of recidivism regardless of age, compared with those who aged out of ABAWD upon release, ex-prisoners who are below the upper age cutoff are less likely to be reincarcerated within one year of release when released to a county with SNAP work requirement. Given that certain types of crimes may signify a greater level of financial need and, consequently, a stronger connection to public assistance, I conduct a decomposition of the results based on the types of crimes committed. Decomposing by the type of returning offense, I find the ABAWD time limit reduces the risk of committing property crimes after release. These results provide evidence in support of the time allocation effect of SNAP work requirements, where ex-offenders allocate more time towards job-related activities under the requirements instead of criminal activities.

This paper is relevant to two strands of research. First, it contributes to research about the impact of social welfare programs on criminal behaviors. Prior work finds that access to social safety net lowers crime and recidivism (Agan and Makowsky, 2018; Beach and Lopresti, 2019; Berk et al., 1980; Berk and Rauma, 1983; Carr and Packham, 2019; Foley, 2011; He and Barkowski, 2020; Mallar and Thornton, 1978; Palmer et al., 2019; Wen et al., 2017; Yang, 2017a). This paper builds upon these findings and further investigates the work requirement component of the social safety net. Second, it complements recent literature on the debate over work disincentives of SNAP. Prior work shows that access to SNAP discourages labor supply using early rollout of the program and changes in SNAP administration (East, 2018; Hoynes and Schanzenbach, 2012; Tuttle, 2019). While research finds work requirements discourage SNAP participation (Ganong and Liebman, 2018; Gray et al., 2023; Mulligan, 2012; Ribar et al., 2010; Wilde, 2000; Ziliak et al., 2003), its impact on employment and work effort among the general population is less clear (Cuffey et al., 2022; Han, 2022; Harris, 2021; Ritter, 2018). By examining the impact of work requirements on ex-prisoners, who are potential SNAP participants, this study provides insight into the effects of work requirements that may not be observed in analyses of the general population. Additionally, given the policy aim of reducing dependency on benefit programs, it is important to investigate whether work requirements promote work or impose harm on public safety by screening out vulnerable populations.

The rest of the paper proceeds as follows. Section 2.2 introduces the background of SNAP work requirements. Section 2.3 discusses previous literature. Section 2.4 describes data used in analysis. Section 2.5 lays out the empirical strategy and section 2.6 presents results. Section 2.7 concludes.

2.2 Background

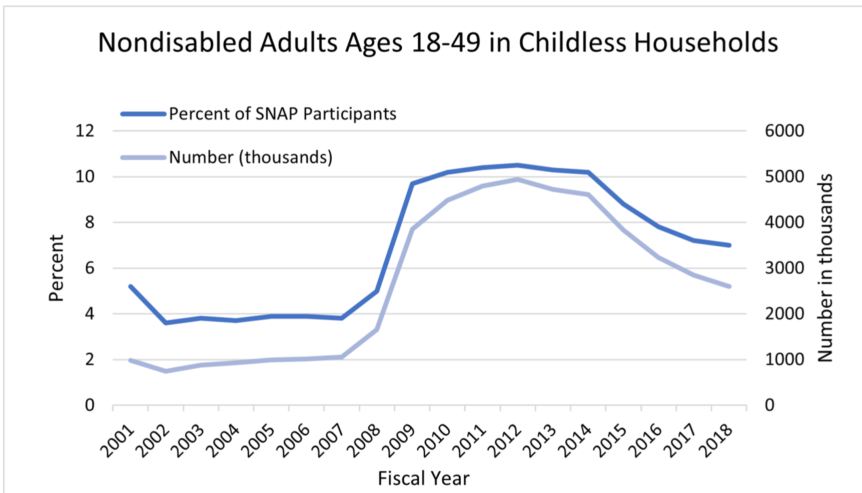
2.2.1 The ABAWD Time Limit

SNAP is a crucial food assistance program in the United States, providing supports to over 35 million low-income individuals as of 2019, including working and non-working families, children, and those who are unable to work due to age or disability. To qualify for SNAP, individuals aged 16-59 must meet general work requirements, unless they are exempt for reasons such as disability. These requirements include registering for employment, accepting job offers, participating in E&T when assigned by the State, and not quitting a job or reducing work hours below 30 hours per week without good reason.

Moreover, the ABAWD time limit, established by the PRWORA in 1996, only applies to a subset of SNAP recipients who are between the ages of 18 and 49, live in households without children, are not pregnant, and are physically or mentally capable to work. These individuals must meet an additional work requirement by working or participating in a qualifying work program for at least 20 hours a week or by participating in a workfare program. Participants subject to the ABAWD work requirement will lose eligibility for SNAP if they receive benefits for three months within a 36-month period and fail to meet the work requirement.

The ABAWD time limit applies to a relatively small percentage of individuals who receive SNAP benefits. According to Stavrianos and Nixon (1998), prior to the implementation of the time limit in 1996, able-bodied adults without children accounted for approximately 3.8% of SNAP participants. Although the number of such adults receiving benefits had already been declining along with the overall SNAP caseload before the implementation of the time limit, the number fell by about 40% within the first year of the ABAWD provisions taking effect, with most of the decrease occurring within the first two to three months. Participation continued to decline at a slower pace through 1998 and 1999 (Czajka et al., 2001). As is shown in Figure 2.1, the share of ABAWDs among SNAP participants remained at 4 percent for years before the Great Recession in 2008. Although the ABAWD time limit affects only a small portion of the general SNAP recipients, it is relevant to criminal recidivism because the ABAWD SNAP population consists mostly of homeless individuals, ex-offenders, or those with drug or alcohol dependency issues (Wheaton et al., 2021).

Figure 2.1 The Trend in Adults Ages 18-49 without Disabilities in Childless Households



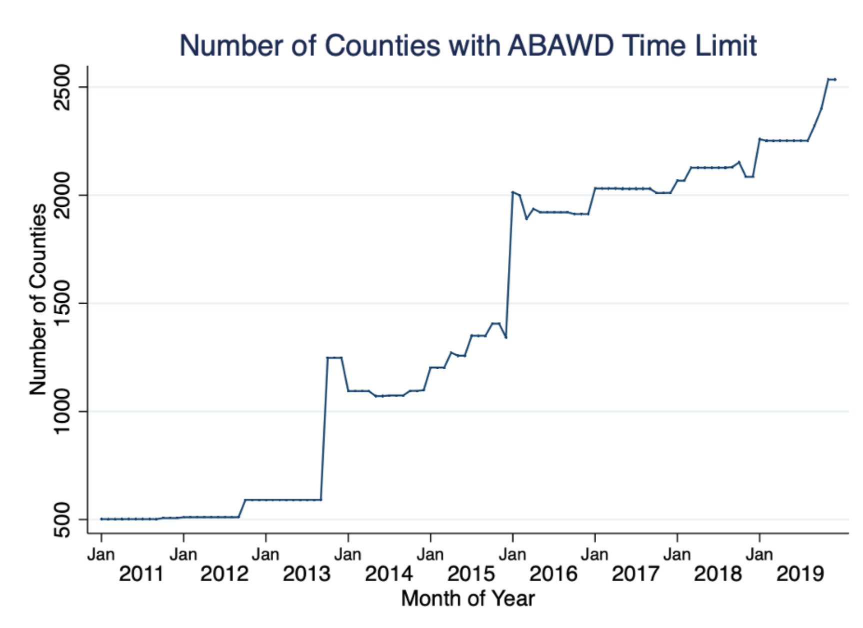
Note: The figure plots the size of population that are nondisabled, childless and aged between 18-49. Data obtained from the Food and Nutrition Service's report series of Characteristics of SNAP Households during fiscal year 2001 and 2018. The reports are available on http://www.fns.usda.gov/ops/research-and-analysis.

2.2.2 Waivers and Reinstatement of the Time Limit

The ABAWD time limit can be waived in areas with an unemployment rate of over 10 percent or limited job opportunities, as permitted by the PRWORA. These areas can be the entire state, county, or a combination of counties and towns, as long as their combined unemployment rates satisfy the required threshold. However, the ABAWD waiver only waives the work requirement specific to ABAWDs but not the general work requirement. Additionally, states have the discretion to cover a limited number of participants who may lose eligibility due to the work requirement.

During the Great Recession, the ARRA temporarily suspended the time limit nationwide from April 1, 2009, to September 30, 2010, with the option for states to extend the waiver to September 30, 2011. Due to the persistently high unemployment rates, many states continued to waive the time limit in subsequent years. Figure 2.2 presents the number of counties with ABAWD time limit reinstated after the expiration of the national waiver. From 2011 to 2019, the number of counties subject to the ABAWD time limit increased over time. While some counties obtained waivers intermittently, the majority of counties did not receive a waiver after the limit was reinstated.

Figure 2.2 Monthly Trend of Counties Under ABAWD Time Limit, 2011-2019



Note: The figure plots the number of counties with reinstated ABAWD time limit post-recession. Time limit status are calculated from waiver records obtained through a Freedom of Information Act request to the U.S. Department of Agriculture, Food and Nutrition Service. Counties with part of the area waived from the ABAWD time limit are coded as waived in this figure.

The increasing share of the ABAWD population among SNAP recipients shown in Figure 2.1 is correlated with the national time limit waiver. However, as states started to reinstate the ABAWD time limit, the share gradually dropped in subsequent years, indicating the correlation of the work requirement with SNAP participation among ABAWDs.

2.3 Literature Review

Existing research shows that access to SNAP benefits is correlated with lower crime. Tuttle (2019) studies the SNAP drug ban, which excludes drug felons from receiving benefits since the PRWORA. The enactment of the drug ban increases recidivism among drug traffickers, as ex-prisoners may turn to criminal activities to supplement lost welfare income. Yang (2017a) uses more recent variation in opting out of the SNAP drug ban to find access to SNAP lowers recidivism among drug felons. In addition, SNAP payment timing affects local crime rates. Carr and Packham (2019) show that theft increases during the last week of the benefit cycle, and staggering payments to different households reduces monthly theft at grocery stores in Illinois. Using data from multiple cities, Foley (2011) also finds cyclicality in financially motivated crimes that corresponds to SNAP payment schedules.

Other welfare programs also affect crime rates. Studies using experimental data show that immediate access to transitional aid such as unemployment benefits upon release from prison reduces property crimes (Berk et al., 1980; Mallar and Thornton, 1978). Similarly, Beach and Lopresti (2019) find that more generous access to unemployment benefits leads to smaller increases in trade-related property crimes during periods of increased import competition. Access to medical benefits also has the positive externality of reducing crime. He and Barkowski (2020) find the Medicaid expansion under the Affordable Care Act is negatively associated with burglary, vehicle theft, homicide, robbery, and assault among childless adults. Focusing on the Health Insurance Flexibility and Accountability (HIFA) waivers component of the Medicaid expansion, Wen et al. (2017) find offering substance use disorder treatment reduces robbery, aggravated assault, and larceny theft.

In addition, research explores the impact of labor market on recidivism among ex-offenders. Agan and Makowsky (2018) use administrative prison release records to demonstrate that an increase in the minimum wage reduces one-year prison reentry rates associated with property and drug crimes. Moreover, the availability of state Earned Income Tax Credits reduces recidivism among women. Using the same dataset, Yang (2017b) finds that improved labor market conditions, characterized by higher wages for low-skilled workers, significantly reduce the risk of recidivism.

This paper also relates to the ongoing debate surrounding the work disincentives of SNAP. Previous research examining the early rollout of the food stamp program shows that access to food stamps reduces employment and hours worked (Hoynes and Schanzenbach, 2012). Focusing on the subpopulation affected by major changes in SNAP rules during the 1996 welfare reform, East (2018) strong evidence of labor supply disincentives among immigrants. Existing research shows that the ABAWD work requirements discourage SNAP participation. For example, in a descriptive analysis of participation patterns during 1994 to 1998, Genser (1999) finds SNAP participation among nonimmigrant childless unemployed adults fell by 59 percent. Additionally, the work requirement reduces program participation by 53 percent among Virginia residents subject to work requirements (Gray et al., 2023). Ribar et al. (2010) also find that participation spells among ABAWDs are also shorter and participation rates are lower. Ganong and Liebman (2018) find the ABAWDs time limit waivers explain part of the increase in enrollment from 2007 to 2011.

The effect of ABAWD time limit on labor supply remains unclear. Several studies find no evidence that the ABAWD time limit affects employment or labor force participation (Cuffey et al., 2022; Han, 2022; Harris, 2021; Ritter, 2018; Stacy et al., 2018).[2] However, Harris (2021) uses American Community Survey (ACS) data from 2010 to 2017 and finds that the ABAWD time limit increases employment among ABAWDs by 1.3 percentage points. In addition, Lippold and Levin (2021) use a regression discontinuity design to compare counties just below and above the unemployment threshold required for the ABAWD waiver and find that removing the ABAWD work requirement reduced hours of work.

Two prior studies use linked SNAP administrative data and Unemployment Insurance (UI) wage records to investigate the impact of the ABAWD time limit. Gray et al. (2023) find no effect of the reinstatement of the ABAWD time limit in Virginia in 2013 on employment on average, but do find some evidence of increased earnings among participants working close to the required level to maintain eligibility. Ribar et al. (2010) discover that the ABAWD time limit increases exits into employment as well as exits into nonemployment, but did not estimate the impact on the probability of being employed.

[2] Cuffey et al. (2022) examine the Current Population Survey Food Security Supplement data from 2004 to 2009 and find that the ABAWD time limit has no significant impact on employment. Similarly, Stacy et al. (2018) uses linked American Community Survey (ACS) data and administrative data from nine states between 2005 and 2015 to show that the ABAWD time limit does not affect labor force participation or hours worked. Han (2022) conducts a similar analysis using ACS data from 2005 to 2017 and finds that suspending work requirements have no impact on employment. Ritter (2018) investigates the effects of the ABAWD time limit on the employment of older ABAWDs using basic monthly Current Population Survey data from 2000 to 2016 and SNAP Quality Control data from 2003 to 2017, and finds no evidence that the time limit affects employment.

Overall, the evidence on the impact of ABAWD time limit on labor supply remains inconclusive. This paper extends previous research and provides empirical evidence of the effect of ABAWD time limits on recidivism among ex-offenders.

2.4 Data

2.4.1 Administrative Prison Records

I use administrative prison records from the National Corrections Reporting Program (NCRP)[3], which consists of offender-level data on admissions and releases from state prisons as well as yearend prison custody records voluntarily reported by state departments of correction. This paper uses the NCRP data from 2011 to 2017 when 43 states provided data at some point in this period.

The data provide information on each offender's prison spell, including the date of admission and release, the state and county of prison sentence imposed, and the state and county of last known address prior to incarceration. In order to approximate the state and county of release, I utilize the sentencing state and county as a proxy, as the vast majority of offenders (over 90 percent) tend to reside in the county where they were convicted following their release (Raphael and Weiman, 2007). Each term record within the data corresponds to a distinct period of incarceration for an individual offender. By leveraging the sentencing time and location information, I can match each term record with the ABAWD time limit status at the time of release. Furthermore, the dataset includes additional specifics regarding the prison spell, such as the offense leading to conviction, the number of counts convicted, the total sentence length, the type of prison, as well as the reasons for entering prison and subsequent release.

The data include additional information on offender characteristics such as month and year of birth, race, sex, and highest education. This truncated information is helpful for approximating the offender's age at release and therefore determining their eligibility status based on the age cutoff.[4] A crucial element of this study is the unique identifier assigned to offenders within a state.

[3]United States. Bureau of Justice Statistics. National Corrections Reporting Program, [United States], 2000-2017. ICPSR37608-v1. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2020-11-19. http://doi.org/10.3886/ICPSR37608.v1

[4]One major caveat of this sample is I do not observe disability status, marital status, or dependent child at release. These are not reported in the NCRP data.

The identification number allows for linking term records of the same offender if their convictions occurred in the same state. This enables the tracking of whether an ex-offender released between 2011 to 2017 returned to incarceration, defined as recidivism in this paper, in the same state within this timeframe, as well as the ability to track the time of recidivism using the admission and release time data.

Table 2.1 Summary Statistics: Characteristics of Released Prisoners

	All	Age 45-49	Age 50-54
Male	0.878	0.871*	0.890*
White	0.469	0.502*	0.499*
Black	0.359	0.36	0.382*
Hispanic	0.167	0.136*	0.116*
High School Degree	0.371	0.437*	0.435*
Age	33.786	46.928*	51.839*
Admit – Property	0.292	0.291	0.282*
Admit – Violent	0.268	0.249*	0.259*
Admit – Drug	0.256	0.255	0.246*
Time Served (days)	709.825	908.903*	1027.335*
Observations	3,003,346	342,830	263,560

[a] The first column reports characteristics among all prisoners released between 2011 to 2017 after sample restriction. The second and third column report the characteristics among those who aged 45-49 and 50-54 at release respectively. Violent, Property, Drug are indicators for the most serious offense for which the offender initially went to prison.

[b] * represents statistically significant difference from the values in the first column at the 5% level.

The analysis sample used in this study comprises all releases between 2011 and 2017 in the NCRP dataset. I restrict the sample by excluding records with missing information on county of sentencing and age, which are necessary for matching ABAWD time limit status and eligibility. In addition, I also exclude the 2011 California data because of a major policy change that sentenced more convicts to county jails instead of state prison (Agan and Makowsky, 2018). Table 2.1 presents the characteristics of the analysis sample, as well as ex-prisoners who returned to prison within one year and three years, respectively. Of the 3.6 million ex-prisoners, 88 percent are male, with non-Hispanic whites representing a smaller proportion of the released offenders than in the general

U.S. population. Nearly 40 percent earned at least a high school degree and the average age at release is in the mid- thirties. Decomposing by the offense that leads to their longest sentence, similar numbers of ex-offenders committed property, violent and drug crimes. An average released prisoner served two years in prison by the time of release.

By narrowing down the sample to individuals released near the upper ABAWD age cutoff, I examine the summary statistics for two age groups: those released between the ages of 45 and 49 (column 2), and those released between the ages of 50 and 54 (column 3). When compared to the average released prisoner, ex-prisoners within these age ranges exhibit certain distinctions. Specifically, they are more likely to be White, have attained a high school degree, served longer prison sentences, and less likely to be of Hispanic ethnicity or have been convicted of violent crimes.

Furthermore, when comparing ex-prisoners above and below the ABAWD age cutoff, I observe additional differences. Those below the cutoff are less likely to be male or Black and have served shorter prison sentences. Conversely, they are more likely to be White or Hispanic, have been convicted of property crimes or drug crimes, and less likely to have been convicted of violent crimes.

Table 2.2 presents the rates of recidivism across various prisoner characteristics by age group. I adjusted the sample used in Table 2.1 to include a full year post-release for all released prisoners when calculating the one-year recidivism rates. Similarly, for the calculation of three-year recidivism rates, I restricted the sample to prisoners released between 2011 and 2014, thus allowing for a three-year post-release observation period. For the 45-49 age group, the recidivism rate within one year of release is nearly 15 percent. Similarly, the one-year recidivism rate for the 50-54 age group is close to 14 percent. Looking at the three-year post-release period, over 30 percent of ex-prisoners released between the ages of 45 and 49 are reincarcerated, while the recidivism rate for the 50-54 age group is 27 percent. It is worth noting that male offenders and those convicted of property crimes exhibit higher recidivism rates compared to their respective counterparts.

Table 2.2 Summary Statistics: Recidivism Rates by Characteristics, Age 45-54

	Age 45-49	Age 50-54	Age 45-49	Age 50-54
Overall	0.149	0.137	0.302	0.271
Male	0.155	0.143	0.314	0.283
Female	0.107	0.087	0.217	0.173
White	0.141	0.127	0.284	0.247
Black	0.166	0.156	0.334	0.31
Hispanic	0.127	0.115	0.263	0.241
High School Degree	0.166	0.152	0.332	0.298
Admit – Property	0.18	0.173	0.362	0.339
Admit – Violent	0.139	0.123	0.273	0.24
Admit – Drug	0.135	0.122	0.281	0.248

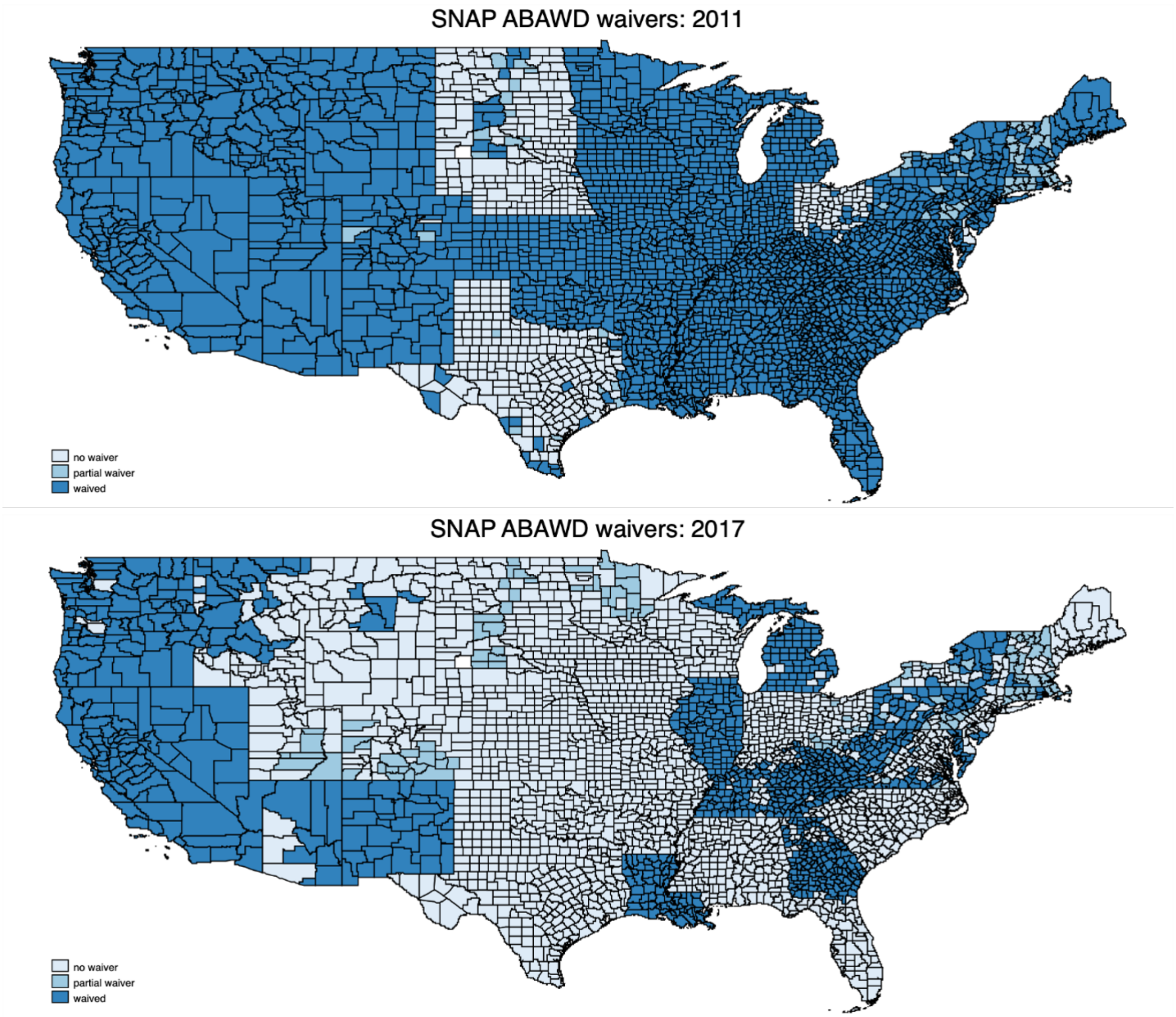
[a] The table reports the recidivism rates among ex-prisoners by demographic groups and age. "Recidivate 1 Year" indicates returning to prison within 1 year of their release. The recidivism rates are calculated using prisoners in each category released from 2011 to 2016 so that I observe a full year after release. Similarly, 3-year recidivism rates are calculated using prisoners released during 2011-2014 so that I observe three years post-release. Violent, Property, and Drug are indicators for the offense for which the offender initially went to prison.

2.4.2 SNAP Work Requirement Waiver Records

I obtained the SNAP ABAWD time limit waivers records from the Food and Nutrition Service (FNS) through a Freedom of Information Act request. The FNS provided the approval letters sent to all states requesting a ABAWD time limit waiver during 2000 - 2017. The letters provide information on the approved areas as well as the expiration date of each waiver. To meet the unemployment rate cutoff for waiver eligibility, states can combine smaller areas that may be below the county level. Since the location information regarding the released prisoners is restricted to the county level, I code counties that have part of their area approved for the waiver as waived. This enables me to compile a policy database of the ABAWD time limit status at the county level.

There are two caveats regarding these policy data that may result in measurement error. First, I do not observe whether states used or declined the waiver after receiving approvals. Second, states are also granted exemption to waive 15 percent of the ABAWD participants at its own discretion. I do not have information on how states use their exemptions. Therefore, if a released prisoner is sent to an area with a lenient ABAWD exemption policy, there is a greater chance they will be exempt from the time limit even if the county does not have an AWAWD time limit waiver in place.

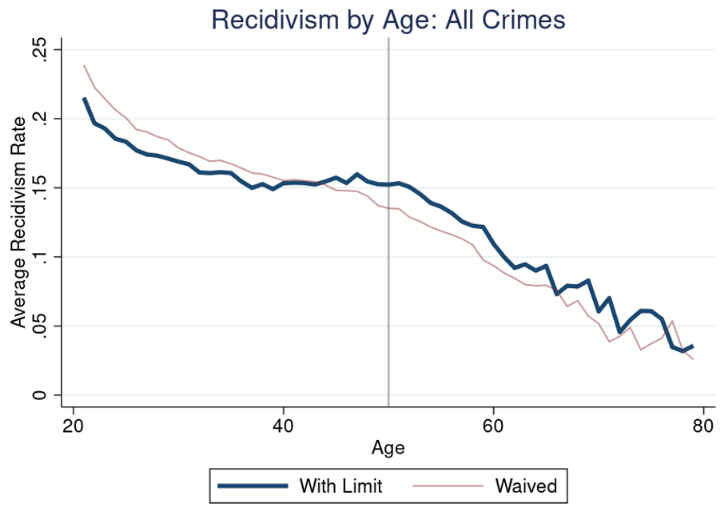
Figure 2.3 ABAWD Time Limit Waivers between 2011 and 2017



Note: The maps plot the waiver status at the county level in 2011 and 2017. The dark blue areas are fully covered by ABAWD time limit waivers. The lighter blue areas have part of the county waived. The lightest colored areas do not have any waivers in place. Waiver status is calculated from waiver records obtained through a Freedom of Information Act request to the U.S. Department of Agriculture, Food and Nutrition Service.

Figure 2.3 illustrates the waiver status in 2011 and 2017, categorized into fully waived, partially waived, and non-waived counties. In 2011, following the expiration of the nationwide ABAWD time limit waiver, only a handful of states reintroduced the limit due to the slow recovery of employment after the recession. Nonetheless, in these states, there were still several counties waived or partially waived from the limit due to high unemployment rates. As the economy began to recover, more and more areas saw the implementation of the time limit. By 2017, most states had either all or part of the state subject to the ABAWD time limit. The pattern corresponds with the yearly trend shown in Figure 2.2.

Figure 2.4 One-year Recidivism Rates by Age and ABAWD Limit Status



Note: The figure plots the one-year recidivism rates at different age by the limit status at the county level pooling together all sample periods. One-year recidivism is defined as returning to prison within one year after release. The vertical line in the figure represents the upper age cutoff for ABAWDs at age 50.

Figure 2.4 shows the one-year recidivism rates by age and local work requirement status among prisoners released between the ages of 20 to 80. The recidivism rates decrease with age regardless of the work requirement status in the county they are released to. In areas with the ABAWD time limit, the recidivism rates among prisoners over 40 years old are generally higher than those released to areas with time limit waivers, indicating a possible association between the ABAWD time limit and recidivism behaviors among older prisoners. The vertical line in the figure represents the upper age cutoff for ABAWDs at age 50. In areas without the time limit, there is a consistent decreasing trend in recidivism for prisoners around the age cutoff. However, in areas with the time limit, the recidivism rates remain constant for prisoners aged under the cutoff and start to decrease for prisoners aged out of the upper age cutoff. The difference in recidivism rates among prisoners subject to the time limit by local work requirement status reveals the potential impact of the ABAWD work requirement on reducing recidivism compared to older released prisoners.

2.4.3 Local Labor Market Variables

I control for other labor market variables that affect recidivism rates throughout the study. Firstly, I use annual county-level unemployment data from the Bureau of Labor Statistics, which is used by the FNS to determine eligibility for the time limit waiver.[5] Secondly, I obtain quarterly data on the average monthly earnings of non-college educated men at the county level from the Quarterly Workforce Indicators data because ex-offenders mostly have less than a college degree.[6] This wage data give a good representation of the potential wage for employment among ex-prisoners. Thirdly, I incorporate state-level and sub-state minimum wage data from Vaghul and Zipperer (2016) to reflect general labor market regulations in the areas the prisoners were released. I additionally include policy data on the status of SNAP drug bans to isolate the impact of the ABAWD work requirement using data reported in Yang (2017a).

2.5 Method

To identify the effect of ABAWD time limit on a potentially eligible population of ex-offenders, I leverage the temporal and geographical change in the ABAWD work requirement as well as the upper age cutoff for ABAWDs in a triple-difference framework. Specifically, I estimate the following equation with pooled ordinary least squares (OLS) using the sample of prisoners released at ages between 45 and 54

$$
\begin{aligned}
y_{ict} =& \beta_0 + \beta_1 \cdot \text{Limit}_{ct} + \beta_2 \cdot \text{Below50}_{it} + \beta_3 \cdot \text{Limit}_{ct} \cdot \text{Below50}_{it} + \beta_4 \cdot X_{ct} + \beta_5 \cdot Z_i \\
& + \alpha_c + \gamma_t + \varepsilon_{ict}
\end{aligned}
\tag{2.1}
$$

where the outcome of interest, Y_{ict}, is an indicator of recidivism of individual i released to county c on year and month t within one year or three years of release. Limit_{ct} stands for the ABAWD time limit reinstatement in county c and year-month t. Below50_{it} is an indicator of individual i being under age 50 at the time of release on year-month t, and therefore subject to existing ABAWD time limits. X_{ct} is a vector that contains a series of local labor market and policy variables, such

[5] Bureau of Labor Statistics (BLS). (n.d.). Local Area Unemployment Statistics. Retrieved from https://www.bls.gov/lau/

[6] U.S. Census Bureau, Center for Economic Studies, Longitudinal Employer-Household Dynamics Program (2019). Quarterly Workforce Indicators [dataset]. Retrieved from https://lehd.ces.census.gov/data/

as county unemployment rates, minimum and low-skilled wages, EITC policy indicators, housing price indexes, density of policy officers, and SNAP drug ban status. Z_i is a vector of individual characteristics including gender, race, and an indicator of a high school degree. α_c and γ_t are county and year-month fixed effects respectively. Standard errors are clustered at the state level to account for correlations within a state caused by the unobserved state court system differences.

The validity of the research design depends on the exogeneity of the reinstatement of the ABAWD time limit across counties where the prisoners are released to, as well as the exogeneity in their age eligibility for ABAWDs at prison release. The exogeneity of age eligibility is trivial because the ABAWD age cutoff is predetermined and the age at release is not self-selected. The concern about endogeneity in time limit reinstatement stems from the fact that the eligibility for the time limit waiver is determined by local unemployment rate. When the limit status is colinear with local unemployment rates, the effect of SNAP work requirement is confounded with the effect of local labor market conditions on criminal recidivism. Further, state SNAP agencies may manipulate county grouping to meet the unemployment rate cutoff. This creates variation in limit status regardless of county unemployment for better identification. However, it also introduces confounders that affect the state's waiver application decision. To address these concerns, I control for county unemployment rates and other state-level policy variables in the regression. In addition, using prisoners released at an age above the ABAWD age cutoff as a control group also controls for common trend caused by the unobserved confounders.

2.6 Results

2.6.1 Main Results

Table 2.3 presents the main results of the effect of the work requirement. The sample consists of ex-prisoners released during 2011 to 2017 aged between 45 to 54 at the time of release. Column 1 shows the results for one-year recidivism, defined as returning to prison within one year after release. The first row presents the main effect of age, which indicates that being below the ABAWD age cutoff increases the risk of returning to prison within one year by 0.6 percentage points. This is consistent with the decreasing trend in recidivism by age demonstrated in Figure 2.4. The time limit increases the one-year recidivism rate among all ex-prisoners in the sample by 1.5 percentage points. This implies when released to areas with limited access to SNAP, ex-prisoners are more likely to return to prison even if the local labor market conditions and demographic characteristics are similar to those in areas with work requirement waivers.

Table 2.3 The Effect of ABAWD Time Limit on Recidivism Among Released Prisoners, Age 45-54

	Recidivate 1 Year	Recidivate 3 Years
Below50	0.009***	0.028***
	-0.002	-0.003
Limit	0.015***	0.015***
	-0.006	-0.006
Limit*Below50	-0.005**	-0.005
	-0.002	-0.004
Mean Outcome		
All	0.144	0.289
Below50	0.149	0.302
Above50	0.137	0.271
Observations	539,740	365,120

[a] The table reports regression results of equation (1) with a sample of prisoners released at the age between 45 and 54. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. All specifications include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

The focus of the analysis is on the interaction effect of being below the ABAWD age cutoff and the time limit. The results show a significant negative impact on one-year recidivism. Among individuals released at an age younger than 50, being released to areas with the ABAWD time limit reduces the probability of returning to prison within one year by 0.7 percentage points. Given an average recidivism rate of 14 percent, this represents a 5 percent decrease in the one-year recidivism rate. This indicates the recidivism reducing effect of the work requirement among those subject to the age requirement.

Column 2 shows the result with three-year recidivism. I restrict the sample to ex-prisoners released during 2011 to 2014 to ensure a sufficient post-release follow-up period. The findings are consistent with those for one-year recidivism rates, but with larger effects sizes. The work requirement reduces the risk of returning to prison within three years of release by 1 percentage point among those below the ABAWD age cutoff compared to those aged out of the age requirement. The difference is statistically significant at the 1% significance level.

2.6.2 Robustness

One concern of the main analysis is the inclusion of offenders released multiple times during the sample period in the pooled OLS. Individual level fixed behavioral patterns could bias the results if not accounted for. For example, if waived areas have a higher share of repeat offenders among all released prisoners and these repeat offenders tend to be below the ABAWD age cutoff, the estimated recidivism reduction effect of the SNAP time limit may be biased upward. I use an alternative sample consisting of the first observation of prison release among all prisoners during 2011 to 2017. The results are presented in Column 2 of Table 2.4 and Column 1 replicates the findings from Table 2.3 for reference. For one-year recidivism, excluding records of repeat offenders leads to a similar estimate of the effect of SNAP time limit. The point estimate of a 0.5 percentage point decrease translates into a 6 percent lower one-year recidivism rate across first-time release records. However, the effect on three-year recidivism does not hold after excluding multi-time release records during the sample period. This may indicate that the longer-term effect of the time limit is at a higher risk of bias due to not accounting for individual level correlations when including all prisoners in the analysis.

Table 2.4 The Effect of ABAWD Time Limit on Recidivism, Alternative Samples

	Full Sample	First-time Release	Steady Age Group
		Panel A. Recidivate 1 Year	
Below50	0.009***	0.006***	0.011***
	-0.002	-0.001	-0.003
Limit	0.015***	0.011***	0.014**
	-0.006	-0.005	-0.006

Table 2.4 (cont'd)

	Full Sample	First-time Release	Steady Age Group
Limit*Below50	-0.005**	-0.005*	-0.006**
	-0.002	-0.003	-0.003
Mean Outcome			
All	0.144	0.079	0.144
Below50	0.149	0.083	0.15
Above50	0.137	0.075	0.137
Observations	539,740	205,966	447,193
		Panel B. Recidivate 3 Years	
Below50	0.028***	0.025***	0.039***
	-0.003	-0.007	-0.005
Limit	0.015***	0.008	0.014
	-0.006	-0.006	-0.009
Limit*Below50	-0.005	-0.002	-0.006
	-0.004	-0.007	-0.005
Mean Outcome			
All	0.289	0.173	0.285
Below50	0.302	0.183	0.309
Above50	0.271	0.16	0.271
Observations	365,120	146,559	228,714

[a] The table reports regression results of equation (1) with different samples of prisoners released at the age between 45 and 54. Column 1 reports the results obtained with the full sample. It is a duplicate of the results in Table 3 listed for reference purpose. Column 2 presents the results with a sample of the first term record among all prisoners observed during 2011 – 2017. Column 3 shows the results with a sample excluding those who changed age eligibility status within one year / three years after release for the analysis of one-year / three-year recidivism. The average recidivism rates among all prisoners and subgroups by age eligibility are listed below the estimation results.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

[c] See full tables of columns (2) and (3) in Table 2B.2 and Table 2B.3.

Another concern is that the released prisoner may age out of the ABAWD age cutoff during the post-release observation period. For example, if a prisoner is released at the age of 49 years and 6 months old, he/she is classified as below the age cutoff in the analysis. In six months after release, the prisoner turns 50 and is not subject to the time limit anymore. Since the outcome measure is whether returning to prison in one year or longer observation period, the change in age eligibility status will cause bias. To mitigate the concern, I exclude prisoners released at the age of 49 for the analysis of one-year recidivism, and those released at an age between 47 and 49 for the analysis of three-year recidivism. The results are presented in Column 3 of Table 2.4 Using the remaining prisoners who do not change age eligibility in the post-release analysis period, I find similar results with the main analysis for one-year recidivism. The time limit lowers the probability of returning to prison within one year by 0.6 percentage points. With a 14-percentage point average risk of one-year recidivism, this is a 4 percent decrease in the recidivism rate. While there is also a 0.6 percentage point decrease in the risk of returning to prison within three years when released to counties with a SNAP time limit, the difference is not statistically significant.

2.6.3 Effects by Offense Type

I decompose recidivism by the types of returning offense and present the results in Table 2.5. Column 1 presents results for returning to prison due to property offense. The SNAP time limit lowers the rate of recidivism of property crime by 0.3 percentage points in one year and 0.4 percentage points in three years among released prisoners below the age cutoff. In terms of percentage change with regards to the average rate of recidivism of property crimes, this translates into a 6 percent decrease in one year and 4 percent decrease in three years, respectively. Column 2 shows the results for returning due to violent crimes. The SNAP time limit has no significant impact on future violent offenses in one year or in three years post-release. Column 3 indicates that the SNAP time limit reduces the risk of future drug crimes three years after release but has no significant impact within one year of release.

Table 2.5 The Effect of ABAWD Time Limit on Recidivism, By Returning Offense

	Property Crime	Violent Crime	Drug Crime
	Panel A. Recidivate 1 Year		
Below50	0.002**	0.001	0.002*
	-0.001	-0.001	-0.001
Limit	0.003	0	0.010**
	-0.003	-0.002	-0.004
Limit*Below50	-0.003**	-0.001	-0.002
	-0.001	-0.001	-0.001
Mean Outcome			
All	0.05	0.033	0.032
Below50	0.051	0.034	0.034
Above50	0.048	0.031	0.03

Table 2.5 (cont'd)

	Property Crime	Violent Crime	Drug Crime
Observations	499,587	499,587	499,587
	Panel B. Recidivate 3 Years		
Below50	0.003***	0.001	0.002
	-0.001	-0.001	-0.001
Limit	0.006	0.002	0.012**
	-0.005	-0.003	-0.006
Limit*Below50	-0.004***	-0.002	-0.003**
	-0.001	-0.001	-0.001
Mean Outcome			
All	0.101	0.062	0.068
Below50	0.105	0.065	0.071
Above50	0.095	0.059	0.062
Observations	343,933	343,933	343,933

[a] The table reports regression results of equation (1) with a sample of prisoners released at the age between 45 and 54 by returning offense. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. All specifications include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

The observation that individuals below the upper ABAWD age cutoff for SNAP work requirement are less likely to commit future property crimes has two possible implications. First, SNAP ABAWD work requirement reduces financially motivated criminal behaviors by encouraging employment or employment training programs. Individuals aged out of the ABAWD age limit are less incentivized to do so because they are not subject to this work requirement. Steady employment alleviates financial hardship and reduces the time available for criminal activities, making property crimes less accessible than individuals without employment. Second, younger ex-offenders get higher benefits from employment and job training than older ex-offenders. The potential benefit of adhering to the work requirement may exceed its opportunity cost – the expected gains from criminal activities that makes a living. The decreased benefit of abiding by the work requirement by age also explains the age effect of recidivism of property crimes.

2.6.4 Effects by Demographic Group

In Table 2.6 I present the results by gender of the released prisoners. Column 1 presents the results for one-year recidivism among male prisoners under the ABAWD age cutoff. The recidivism rate among male prisoners released to counties with SNAP time limit is lower than older male prisoners released to the same counties. However, the result is not statistically significant. Column 2 indicates a 2-percentage point lower probability of returning to prison within one year of release among female offenders under the ABAWD age cutoff and released to counties with a SNAP time limit. This is a dramatic 20 percent decrease relative to an average of 10 percent risk of one-year recidivism among all female offenders. The effect is statistically significant at the 1 percent level. Columns 3 and 4 present the results for three-year recidivism among male and female prisoners respectively. The results are similar with to the one-year recidivism results. While there is a slightly lower probability of recidivism among younger male offenders below the SNAP time limit, the effect is not significant. The drop in three-year recidivism is significant among female offenders with a magnitude of 1.7 percentage point or a 9 percent decrease compared to all female offenders below ABAWD age cutoff released to locations with work requirements.[7]

Table 2.6 The Effect of ABAWD Time Limit on Recidivism, By Gender

	Recidivate 1 Year		Recidivate 3 Years	
	Male	Female	Male	Female
Below50	0.010***	0.007*	0.030***	0.032***
	-0.003	-0.004	-0.004	-0.004
Limit	0.013*	0.027***	0.020**	0.028**
	-0.007	-0.009	-0.01	-0.011
Limit*Below50	-0.004	-0.020***	-0.005	-0.017**
	-0.003	-0.006	-0.005	-0.008
Mean Outcome				
All	0.15	0.099	0.301	0.2
Below50	0.155	0.107	0.314	0.217

[7] However, I use caution when interpreting this result as an effect of SNAP ABAWD requirement because females are less likely to be ABAWDs than males.

Table 2.6 (cont'd)

	Recidivate 1 Year		Recidivate 3 Years	
	Male	Female	Male	Female
Above50	0.143	0.087	0.283	0.173
Observations	439,111	60,477	302,303	41,631

[a] The table reports regression results of equation (1) with a sample of prisoners released at the age between 45 and 54 by gender. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. All specifications include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

I also break down the effect by race and present the results in Table 2.7 Columns 1 to 3 show the results for one-year recidivism among white, black, and Hispanic offenders, respectively. For prisoners released between 2011 and 2016, the effect of the work limit is not significantly different from zero in all subgroups. Columns 4 to 6 present the results for three-year recidivism. The time limit has large and significant impact for black offenders among the three racial subgroups, consistently suggesting its effect among socially disadvantaged groups. Higher potential benefits of meeting the work requirement by attending job training programs and keeping a stable job because of existing labor market discrimination can help explain the impact of SNAP ABAWD work requirement for black ex-offenders.

Table 2.7 The Effect of ABAWD Time Limit on Recidivism, By Race

	Recidivate 1 Year			Recidivate 3 Years		
	White	Black	Hispanic	White	Black	Hispanic
Below50	0.011**	0.007***	0.013***	0.042***	0.021***	0.023***
	-0.004	-0.002	-0.003	-0.005	-0.002	-0.005
Limit	0.018**	0.008	0.005	0.028***	-0.002	0.012
	-0.007	-0.009	-0.007	-0.009	-0.013	-0.015
Limit*Below50	-0.007	-0.005	0.001	0.001	-0.011**	-0.001
	-0.005	-0.003	-0.006	-0.006	-0.004	-0.01

Table 2.7 (cont'd)

	Recidivate 1 Year			Recidivate 3 Years		
	White	Black	Hispanic	White	Black	Hispanic
Mean Outcome						
All	0.135	0.161	0.122	0.268	0.324	0.254
Below50	0.141	0.166	0.127	0.284	0.334	0.263
Above50	0.127	0.156	0.115	0.247	0.31	0.241
Observations	251,564	187,896	59,721	171,004	133,840	39,820

[a] The table reports regression results of equation (1) with a sample of prisoners released at the age between 45 and 54 by race. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. All specifications include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

2.7 Conclusion

This study investigates the impact of the reinstatement of the ABAWD time limit during 2011-2017 on recidivism. With the effect of time limits being theoretically ambiguous, I find that time limit reduced recidivism among ex-prisoners who are below the ABAWD age cutoff compared to those who aged out of the cutoff at the time of release. Although the ABAWD time limit was temporarily suspended because of the economic downturn caused by the COVID-19 pandemic, the findings are still relevant as states begin to reintroduce the time limit in light of the economic recovery and the 2023 lifting of the COVID-19 Public Health Emergency. While the economic context may differ post-pandemic, the results still provide valuable insights on the SNAP program. More importantly, this research emphasizes the importance of social safety net for the reentry of ex-prisoners. Instead of providing unconditional transfers to beneficiaries, adding a component that promotes work and self-dependence also tends to be beneficial to the participants.

An important area for future research would be to explore the possible channels through which the estimated effect of the SNAP ABAWD time limit reduces recidivism among ex-prisoners. One possible explanation is that the SNAP ABAWD time limit encourages ex-offenders to allocate their time towards job-seeking and job-training, thereby reducing the likelihood of committing crimes. To validate this channel, researchers can evaluate time use data among released prisoners. Moreover, it would be beneficial to study the heterogenous effects by the characteristics of state SNAP E&T programs. If ex-offenders have better access to E&T programs to meet the ABAWD work requirement, they may be more likely to respond to SNAP time limit by spending time on job related activities. The investigation of these mechanisms can inform policy considerations regarding the administration of SNAP E&T programs as well as other safety net programs.

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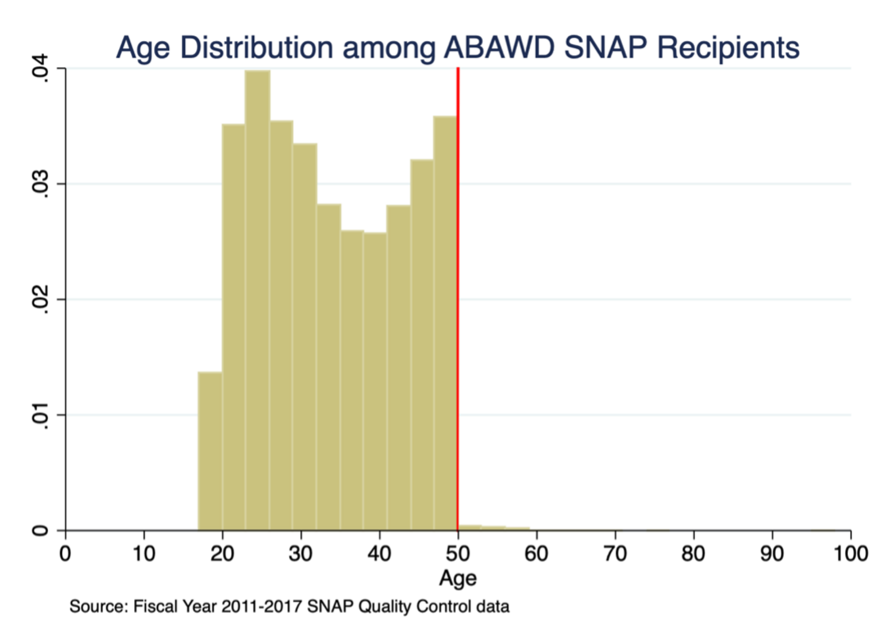
APPENDIX A

THE AGE DISTRIBUTION AMONG SNAP RECIPIENTS

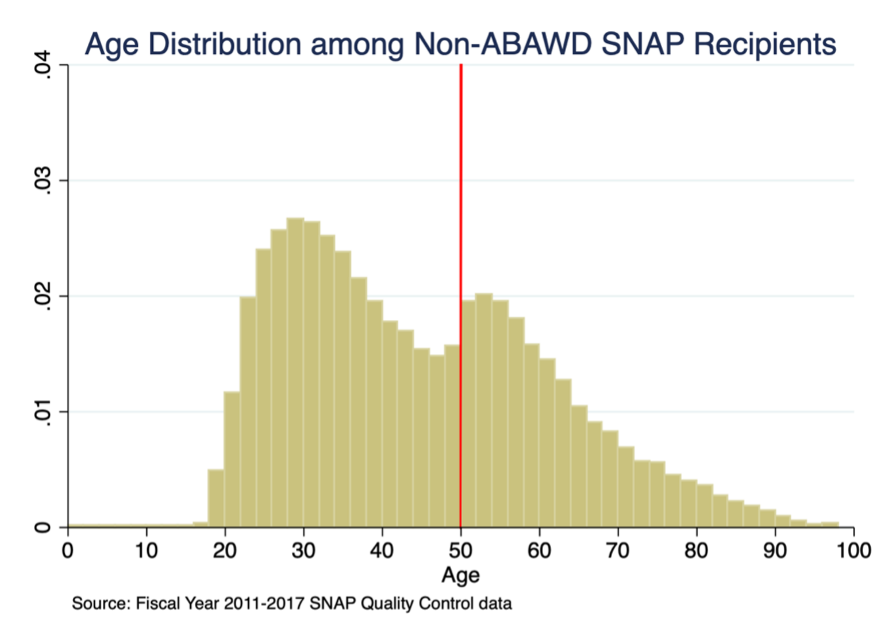
I utilize the SNAP Quality Control data from fiscal year 2011 to 2017 to visualize the age distribution of individuals classified as ABAWD. This dataset is a crucial source utilized by the Food and Nutrition Service (FNS) for monitoring changes in the demographics of SNAP participants and households, as well as the overall program caseload. It comprises an edited version of the original datafile intended for monthly case reviews conducted by state agencies to evaluate the accuracy of eligibility determinations and benefit calculations for SNAP recipients in each state. The dataset includes information on individual age, gender, disability, employment, income, assets, and household composition that derive determine ABAWD status of the recipient. There are also sampling weights to ensure the weighted totals match administrative caseload and program spending records at the state-year level.

Figure 2A.1 Age Distribution among ABAWD SNAP Recipients



The figures display the weighted distribution of recipient ages categorized by ABAWD status during fiscal year 2011-2017. Figure 2A.1 illustrates the age distribution among ABAWDs, revealing that all individuals classified as ABAWD fall within the age range of 18 to 49, with only a few exceptions beyond the age of 49. Among ABAWDs, the majority are concentrated in their mid-20s and close to 50 years old. Figure 2A.2 depicts the distribution for non-ABAWD SNAP recipients. While the ages are more spread-out for SNAP participants not identified as ABAWD, there is noticeable clustering at the upper age cutoff because of the ABAWD age requirement.

Figure 2A.2 Age Distribution among Non-ABAWD SNAP Recipients



APPENDIX B

FULL REGRESSION TABLES

Table 2B.1 The Effect of ABAWD Time Limit on Recidivism (Full Table of Table 2.3)

	Recidivate 1 Year		Recidivate 3 Years	
	(1)	(2)	(3)	(4)
Below50	0.007***	0.009***	0.023***	0.028***
	-0.002	-0.002	-0.003	-0.003
Limit	0.016***	0.015***	0.014**	0.015***
	-0.002	-0.006	-0.006	-0.006
Limit*Below50	-0.005***	-0.005**	-0.007**	-0.005
	-0.002	-0.002	-0.004	-0.004
Male		0.043***		0.091***
		-0.001		-0.002
White		-0.011***		-0.016***
		-0.002		-0.004
Black		0.008***		0.029***
		-0.003		-0.004
Hispanic		-0.041***		-0.070***
		-0.003		-0.004
High school		-0.006***		-0.003*
		-0.001		-0.002
Ln(low-skill wage)		-0.028*		-0.037
		-0.015		-0.027
Unemployment Rate		0.001		-0.003**
		-0.001		-0.002
SNAP Drug Ban		-0.002		0
		-0.003		-0.014
TANF Drug Ban		0.003		-0.111***
		-0.003		-0.026
Observations	539,740	539,740	343,933	343,933

[a] The table reports regression results of equation (2.1) with a sample of prisoners released at the age between 45 and 54. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. Column (1) and column (3) display regression results from the short regression without including control variables. Results in column (2) and column (4) include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

Table 2B.2 The Effect of ABAWD Time Limit on Recidivism, First-time Offenders (Full Table of Table 2.4 Column (2))

	Recidivate 1 Year		Recidivate 3 Years	
	(1)	(2)	(3)	(4)
Below50	0.005***	0.006***	0.021***	0.025***
	-0.001	-0.001	-0.007	-0.007
Limit	0.012**	0.011***	0.007	0.008
	-0.005	-0.005	-0.012	-0.006
Limit*Below50	-0.005*	-0.005*	-0.003	-0.002
	-0.003	-0.003	-0.008	-0.007
Male		0.022***		0.059***
		-0.002		-0.003
White		0.002		0.011
		-0.004		-0.008
Black		0.011***		0.032***
		-0.004		-0.008
Hispanic		-0.025***		-0.048***
		-0.005		-0.008
High school		-0.005**		-0.009***
		-0.002		-0.003
Ln(low-skill wage)		-0.025		-0.041
		-0.019		-0.047
Unemployment Rate		0.003		0
		-0.002		-0.004
SNAP Drug Ban		-0.001		0.008*
		-0.004		-0.005
TANF Drug Ban		0.002		-0.076***
		-0.004		-0.007
Observations	205,966	205,966	146,559	146,559

[a] The table reports regression results of equation (2.1) with a sample of first-time offenders released at the age between 45 and 54. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. Column (1) and column (3) display regression results from the short regression without including control variables. Results in column (2) and column (4) include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

Table 2B.3 The Effect of ABAWD Time Limit on Recidivism, Steady Age Group (Full Table of Table 2.4 Column (3))

	Recidivate 1 Year		Recidivate 3 Years	
	(1)	(2)	(3)	(4)
Below50	0.008**	0.011***	0.021***	0.025***
	-0.001	-0.003	-0.007	-0.007
Limit	0.016**	0.014**	0.007	0.008
	-0.005	-0.006	-0.012	-0.006
Limit*Below50	-0.007**	-0.006**	-0.003	-0.002
	-0.003	-0.003	-0.008	-0.007
Male		0.052***		0.103***
		-0.004		-0.004
White		-0.011**		-0.018*
		-0.005		-0.009
Black		0.009*		0.027***
		-0.004		-0.01
Hispanic		-0.042***		-0.072***
		-0.008		-0.014
High school		-0.005		-0.001
		-0.005		-0.008
Ln(low-skill wage)		-0.03		-0.04
		-0.025		-0.05
Unemployment Rate		0.001		-0.005
		-0.003		-0.004
SNAP Drug Ban		-0.003		0.016**
		-0.007		-0.006
TANF Drug Ban		-0.002		-0.108***
		-0.005		-0.007
Observations	447,193	447,193	228,714	228,714

[a] The table reports regression results of equation (2.1) with a subsample of prisoners released at the age between 45 and 54, who did not age out of ABAWD age cutoff during the post release observation period. Below50 is an indicator of being under age 50 at the time of release. The dependent variable is recidivism measured by returning to state prison within one year or three years. ABAWD time limit is at the county level. Counties with part of its area covered by a time limit waiver are classified as no limit. Column (1) and column (3) display regression results from the short regression without including control variables. Results in column (2) and column (4) include the full set of control variables – demographic characteristics of the released prisoner, county fixed effects, month-of-year fixed effects, and local labor market characteristics.

[b] Robust standard errors clustered at the state level in parentheses. * p<0.01, ** p<0.05, *** p<0.10.

CHAPTER 3

THE EFFECT OF SNAP ON DIETARY QUALITY: EVIDENCE FROM FOODAPS

3.1 Introduction

Rising economic inequality has become a central issue in the United States. The wealth gap extends beyond income inequalities to nutritional inequalities, where the poor eat less healthfully than the wealthy. To address this issue, the Supplemental Nutrition Assistance Program (SNAP) has been established as the largest federal food assistance program in the United States. SNAP serves as a safety net against food insecurity and provides income support to alleviate poverty (Tiehen et al., 2012). According to the Food and Nutrition Act of 2008, the goal of SNAP is to allow low-income households to obtain a more nutritious diet by increasing their food purchasing power. While prior research shows that SNAP meets the goal of reducing hunger and food insecurity among SNAP participants (Mabli et al., 2013; Yen et al., 2008; Nord, 2009; Gundersen et al., 2017), its impact on dietary quality and nutrition intake is less clear. Although research demonstrates a positive relationship between grocery spending and diet quality (Mabli et al., 2013), SNAP participants often have lower dietary quality despite consuming adequate calories, in comparison to nonparticipants (Andreyeva et al., 2015). A more recent study suggests that narrowing the disparity in food expenditure between households with high and low socioeconomic status would not lead to a reduction in the disparity observed in summary indicators of food healthfulness (Hastings et al., 2021).

This paper builds on existing research by investigating the causal relationship between SNAP participation and dietary quality using unique data and an innovative methodology. This paper adds value by leveraging the Food Acquisition and Purchase Survey (FoodAPS) data, which provides detailed information on food acquisition, SNAP participation, and local food environments. This allows me to more accurately measure participation and to account for the availability of healthy foods in the local food environment. Moreover, I employ the partial identification design developed by Kreider et al. (2012) to address issues of endogenous selection into the program, which has not been applied in the context of food quality. The combination of these unique data and innovative methodology sheds new light on the effect of SNAP on dietary quality.

Under innocuous assumptions concerning the nature of the selection process and the type of misclassification, I find that participation in SNAP fails to have a strictly positive average treatment effect (ATE). This is consistent with alternative estimators that point identify the ATE under stringent assumptions (Hastings and Shapiro, 2018; Gregory et al., 2013).

Some data limitations are worth noting. First, the data only measures food purchases and not actual consumption. Therefore, the findings may not accurately reflect the true food consumption patterns. I focus on intensive measures of food healthfulness such as ratios of nutrients to kilocalories purchased so that the inferences are valid under the assumption that wastage affects all inputs to these measures in equal proportion.

Second, the partial identification bounding method adopted in this paper addresses endogenous selection and underreporting of participation status with relatively weak nonparametric assumptions, but it only applies to binary outcomes. Therefore, I proceed by designating a diet with a Healthy Eating Index (HEI) above the national average as "healthy" although the raw HEI score is continuous. I conduct robustness checks using various cutoffs. However, to narrow the bounds of the average treatment effect of SNAP participation, an extension of the method to incorporate continuous outcome variable is needed.

The paper proceeds in the following way. Section 3.2 introduces institutional details of the SNAP program. Section 3.3 describes theoretical predictions of SNAP participation. Section 3.4 provides a review of related literature. Section 3.5 gives an overview of data and summary statistics. Section 3.6 presents results from control strategies that requires conditional independence. Section 3.7 explains the methodology of partial identification and presents the estimation results. Section 3.8 concludes.

3.2 The Supplemental Nutrition Assistance Program

SNAP, formerly known as the Food Stamp Program, is a vital program in the United States aimed at mitigating hunger. It is not only the most substantial program in terms of spending, which totaled approximately $9.5 billion in the average month of fiscal year (FY) 2022, but also in terms of caseload, with 41 million participants receiving $230 in monthly food benefits on average (United States Department of Agriculture, 2023). The program disburses benefits through an electronic benefit transfer (EBT) card to purchase food for at-home consumption at participating retailers.

SNAP is a means-tested program. To be eligible for SNAP benefits, the food assistance unit, which includes all members of a household who share food, must have a gross income that does not exceed 130% of the federal poverty guideline. Additionally, net income must be under the poverty line after deductions have been made for working, housing expenses, and other adjustments. There is also an asset limit - $4,250 in countable resources if elderly or disabled and $2,750 in countable resources if not, as of FY 2023.[1] The rules regarding what counts as an asset vary by state. Beyond income and asset eligibility, households qualify for SNAP if they are "categorically eligible"[2] — that is, they are automatically eligible for those benefits based on their participation in other Federal or State programs.

SNAP benefits are related to income: poorer households receive larger benefits than households closer to the poverty line since they need more help affording an adequate diet. The benefit formula assumes that families will spend 30 percent of their net income for food, with SNAP making up the difference between that contribution and the cost of the Thrifty Food Plan, a diet plan the USDA establishes that is designed to be nutritionally adequate at a very low cost. Benefit amounts are adjusted every year in October to account for increasing food prices. Currently, the program is operated with an EBT card, and the benefits can be redeemed for unprepared foods at authorized retailers. Benefits cannot be used to purchase alcoholic beverages, cigarettes, vitamin supplements, non-food grocery items such as household supplies, or prepared foods.

3.3 Expected Effects of SNAP Participation on Food Consumption

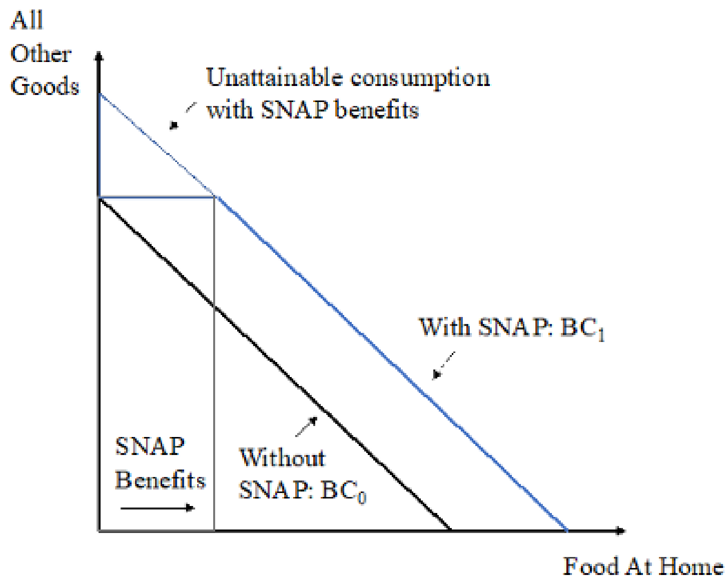
The traditional economic theory of consumer choice regarding SNAP benefits is first introduced by Southworth (1945). In this theory, the total income of participants consists of both cash income and SNAP benefits, while the total income of non-participants consists solely of cash income.

[1]Information retrieved on July 11, 2023, from https://www.cbpp.org/sites/default/files/11-18-08fa.pdf

[2]SNAP units are categorically eligible if all SNAP-unit members receive SSI, cash welfare, or general assistance benefits (Pure Public Assistance), or meet the broad-based categorically eligible (BBCE) rules in their State.

Subject to resource constraints, consumers optimize their spending decisions for an at-home food good and a non-food good, with the latter representing a combination of all goods that cannot be purchased with SNAP benefits. The idea is illustrated in Figure 3.1.

Figure 3.1 Budget Constraint with and without SNAP



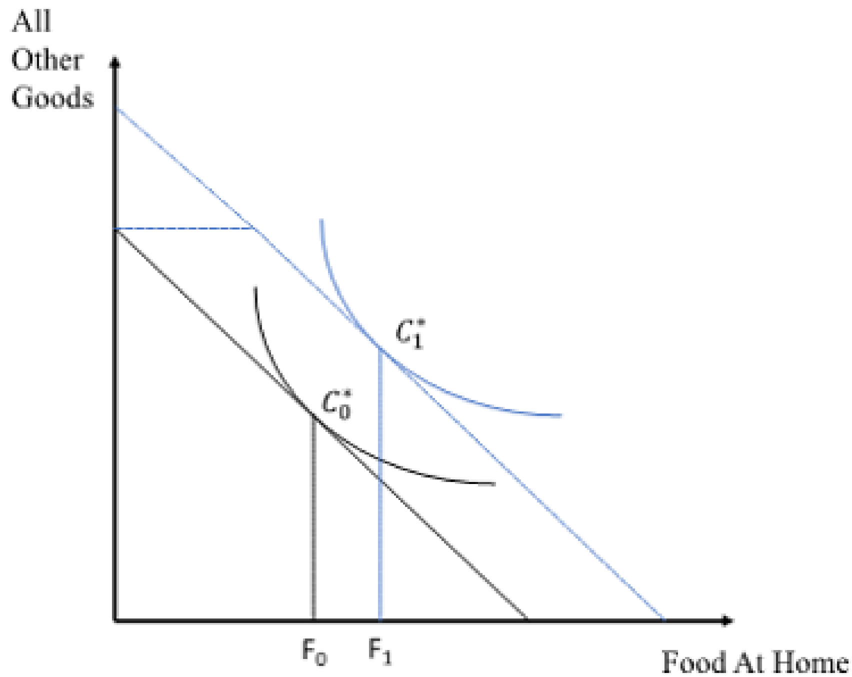
Note: This figure illustrates the impact of SNAP benefits on the budget constraint of recipients. The horizontal axis represents expenditures on food consumed at home, while the vertical axis represents expenditures on all other items. Since SNAP benefits can only be used for food purchases intended for at-home consumption, the budget constraint is shifted horizontally to the right by the amount of the benefit, leaving an unattainable portion of the budget curve.

The original budget line BC_0 reflects the tradeoff between food and all other goods and is shifted out horizontally to BC_1 by the amount of SNAP benefits received. Unlike a cash subsidy, SNAP is an in-kind benefit that constrains families' choice of food. Since the SNAP benefits can only be used for purchasing food for at-home consumption, part of the region under the new budget line is not attainable under this in-kind transfer scheme.

If the food households could buy with a SNAP EBT card were identical to that they would choose to buy with cash, then economic theory predicts that, if food is a normal good, SNAP benefits would increase at-home food consumption.

Families who would have spent at least as much on qualifying foods as the SNAP benefits should have their choices of at-home foods remain relatively unaffected by SNAP (so-called infra-marginal

Figure 3.2 The Effect of SNAP as an In-Kind Transfer: Infra-marginal Households



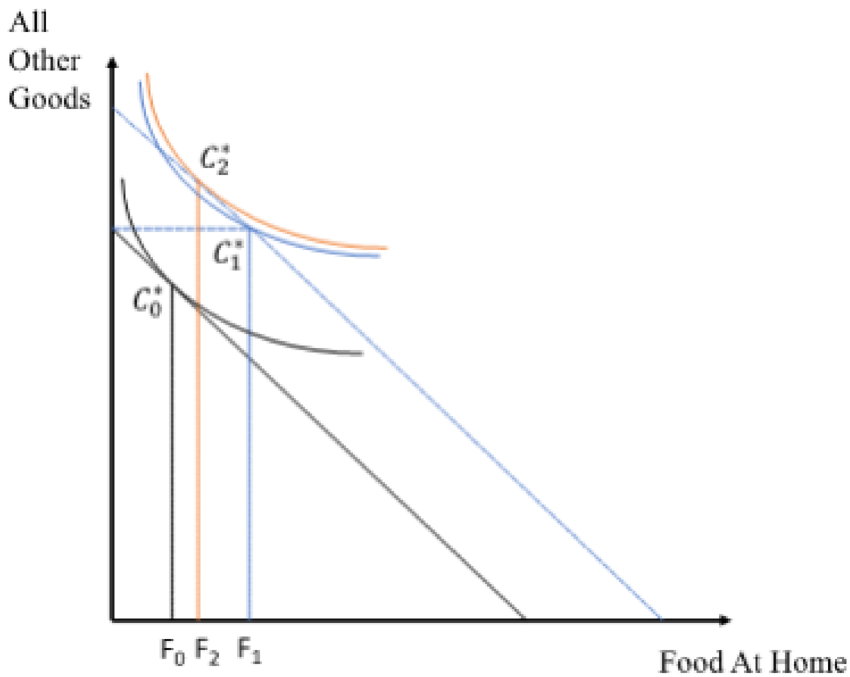
Note: The figures show how SNAP benefits shift optimal food consumption by household types. For infra-marginal households, the shift of the budget constraint causes a change in consumption not restricted by the in-kind nature of SNAP. SNAP is equal to disbursing cash to these households.

or unconstrained participants), compared with a cash transfer. As shown in Figure 3.2, the increase in spending on at-home food consumption $F_1 - F_0$ should be less than the amount of SNAP benefit.

The only families influenced by the in-kind nature of the program are those who would have spent less than their SNAP allotment on qualifying foods (so-called extra-marginal or constrained participants). As is shown in Figure 3.3, households with high demand for non-food items or meals out relative to at-home food consumption are constrained at the kink. If the transfer were in the form of cash, optimal consumption for these households would have moved from pointC_0^* to C_2^*. Because of the in-kind nature of SNAP transfer, they can only consume at the kink C_1^*. For these families, food stamps would lead to a larger increase in food consumption than an equivalent transfer in cash.

Economic theory predicts that a marginal increase in targeted food assistance benefit will strongly affect at-home food spending for extra-marginal participants, and the increase will only weakly affect at-home food spending for infra-marginal participants. Regardless of the magnitude

Figure 3.3 The Effect of SNAP as an In-Kind Transfer: Extra-marginal Households



Note: The figures show how SNAP benefits shift optimal food consumption by household types. For extra-marginal households, the consumption bundle under SNAP benefits is constrained by the in-kind nature.

of the impact, participation in SNAP would lead to an increase in at-home food consumption. However, the effect on away-from-home food consumption is less clear. The income effect from the SNAP benefits would predict an increase in food consumed away-from-home. The substitution effect from lower out-of-pocket price of at-home foods suggests a decrease in consumption of food-away-from-home. Since SNAP participating households fall in the low-income class, I assume the substitution effect dominates and expect the consumption of away-from-home foods to decrease.

The impact of SNAP on nutrition is not as straightforward as other food assistance programs, such as the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).[3] SNAP can have an impact on nutrition through several mechanisms. First, SNAP expands participants' budget constraint to allow for higher consumption of foods, either nutritious or non-nutritious. Second, the nutritional education component may provide incentives for pursuing a more healthful

[3]WIC improves the nutritional health of low-income Americans by providing vouchers for specific nutritious foods. There are two key differences between WIC and SNAP. First, WIC is a quantity voucher instead of a value voucher. Second, the WIC voucher can only be redeemed for WIC qualified goods. WIC is expected to improve nutrition among participants. However, since SNAP does not specify what kind of unprepared foods is eligible its effect on diet quality is ambiguous.

diet. Third, the monthly disbursement affects the timing and composition of food purchased and consumed. Behavioral bias such as the present bias could cause the SNAP cycle phenomenon, which reveals that recipients tend to spend a significant portion of their monthly benefits early in the month, potentially leading to nutritional deficiencies later in the benefit period. Families who plan their grocery shopping for the entire month may allocate their expenditures towards less healthful, shelf-stable foods, while reducing purchases of perishable items like fresh fruits and vegetables. The above channels exert mixed influence on nutrient intake and hence confound the effect SNAP has on diet quality.

3.4 Literature Review

This paper contributes to a large body of literature studying nutritional inequality. The inequality is affected by factors from both the supply side and the demand side.

In terms of the supply side, previous research highlights that low-income neighborhoods often face limited access to affordable and nutritious foods, referred to as food deserts. However, the evidence linking food deserts to unhealthy eating habits among low-income individuals is not conclusive (Bitler and Haider, 2011) and it remains uncertain whether improving access to healthy food will effectively alleviate dietary problems (Kyureghian et al., 2013). Policies aimed at increasing the supply of healthy foods, such as the Healthy Food Financing Initiative, have very limited effect in reducing nutritional inequality (Allcott et al., 2019).

On the demand side, food assistance programs aim at enhancing food security by providing low-income households with the resources to purchase food. SNAP is the largest among these programs in the United States in terms of spending and caseload. However, evaluation of the effectiveness of SNAP faces many challenges. Outcomes of interest, such as diet quality, are not easily measured and they are affected by endogenous and uncontrolled factors, thus leading to the continuing investigation of the efficacy of SNAP in alleviating hunger and improving diets. I review two strands of relevant studies on the effects of SNAP or the Food Stamp Program on diet quality.

The first strand of research in this area focuses on comparing the diets and food purchases of SNAP participants with other subgroups using survey data. In a systematic review, Andreyeva et al.

(2015) show that SNAP participation is associated with lower diet quality. Several studies analyze grocery store purchases linked to nutrient information to investigate whether SNAP affects the healthfulness of food purchases. Garasky et al. (2016) find that SNAP households and non-SNAP households make similar food purchases at the grocery stores. Franckle et al. (2017) discover that grocery items purchased with SNAP benefits tend to be less healthful than those purchased without SNAP benefits. Grummon and Taillie (2017) use scanner data from Nielsen's Homescan Panel to find that, along several dimensions, the grocery purchases of households participating in SNAP are less healthful compared to those of income-eligible non-participating households. Harris-Lagoudakis (2020) uses supermarket retailer data and find that post-SNAP adoption increases spending on meats, oils and prepared products at a higher rate over other grocery product categories.

The second strand employs various methodologies to identify the causal effect of SNAP where the endogeneity of SNAP participation is taken into account. Hoynes and Schanzenbach (2009) utilize the food stamp rollout as an identification strategy to show that Food Stamp participation changed where people acquired their food, increased consumption of food at home and decreased out-of-pocket spending. Yen (2010) uses the 1994-1996 Continuing Survey of Food Intakes by Individuals (CSFII) data and a dual treatment effect model addressing endogenous participation in SNAP and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). He finds that SNAP has minimal additional effects on children's nutrient intake compared to WIC. Kreider et al. (2012) use data from the 2001-2006 National Health and Nutrition Examination Survey (NHANES) and partial identification bounding methods. Their findings indicate that SNAP participation leads to modest reductions in undesirable outcomes such as food insecurity. Gregory et al. (2013) and Todd and Ver Ploeg (2014) treat state-level SNAP policy variables as excluded instruments for SNAP participation. Gregory et al. (2013) analyze data from the 2001-2008 HNANES data and find that SNAP participation increases the consumption of whole fruits but decreases the consumption of dark-green vegetables, resulting in an overall decrease in diet quality compared to nonparticipants from survey data when local food environment is not considered. Todd and Ver Ploeg (2014) find that SNAP participation reduces caloric intake from sugar-sweetened beverages based on data from the 2005-2008 HNANES. Bronchetti et al. (2017) use variation in local food prices to isolate plausibly exogenous variation in the real value of SNAP benefits and find that increased SNAP purchasing power slightly improves HEI-2010 scores among children but has no detectable effect among adults. Using data from the Survey of Income and Program Participation (SIPP) and partial identification bounding methods, Gundersen et al. (2017) find that SNAP participation reduces food insecurity for households with children. Hastings et al. (2021) estimate the causal effect using a panel event-study design and a quasi-experimental design based on program entry and exit timing, and they find only a small impact on food healthfulness using scanner data from a single retailer.

The evaluation of means-tested assistance programs in existing literature often relies on linear response models and the assumption that some observed instrumental variable (IV), typically based on cross-state and time variation in program rules and regulations, affects program participation but otherwise has no effect on the potential outcomes. However, as is now widely recognized, the classical linear response model assumption is difficult to justify when considering programs that are believed to have heterogeneous effects (Moffitt, 2005).

While researchers employ various methodologies to account for endogenous participation and estimate the impact of SNAP, credible solutions for addressing underreporting of participation status in household surveys remain elusive. Estimates of the association between diet-related outcomes and SNAP participation are sensitive to the measurement of SNAP participation (Courtemanche et al., 2019). A nonrandom measurement error problem arises because a large fraction of food stamp recipients fail to report their program participation in household surveys, confounding identification of the causal effect. Meyer et al. (2008) provide evidence of extensive underreporting of food stamps by comparing administrative micro data for the Food Stamp Program with nationally representative survey data.

Studies using scanner data can help overcome this problem, but the food purchases tracked from scanner data only include food consumed at home and do not come from a representative sample of households, as noted by Hastings et al. (2021). SNAP may be linked to health outcomes through its influence on Food-Away-From-Home (FAFH) consumption (Fox et al., 2004). Household FAFH consumption in the United States has steadily increased in recent years. Between the years 2004 and 2014, total FAFH expenditure on all eating and drinking places rose by about 63% (USDA ERS, 2016). Although SNAP benefits are restricted to be spent on Food-At-Home (FAH) only, as predicted in Section 3.3, the fungibility of SNAP benefits allows inframarginal households to utilize benefits for purchases of SNAP-ineligible items such as FAFH. While this income effect positively influences FAFH consumption, the decrease in the out-of-pocket price of at-home foods induces a substitution effect that leads to a decrease in FAFH consumption. A significant body of empirical literature documents the decreases in FAFH and its share of total food spending (Hoynes and Schanzenbach, 2009; Beatty and Tuttle, 2015; Liu et al., 2013; Pan and Jensen, 2008; Burney, 2018). A broader measure of nutrition intake that combines both FAH and FAFH will capture the spillover effect of SNAP participation on nutrition.

The existing literature incorporating measures of food supply to examine SNAP participation is limited, even though economic theory predicts nutrient supply affects nutrient intake. Household food choices are constantly evolving, influenced by the food landscape and program policy design. Most previous studies rely on broad area-based measures of food access, such as supermarket density within Metropolitan Statistical Areas or Census Blocks, rather than individual-level measures (Ver Ploeg et al., 2015). Kyureghian et al. (2013) use county business pattern data on the number of establishments within a 100-square-mile area. Panel studies mostly include food environment in state or county fixed effects, assuming away the variations in supply-side factors. With the availability of data, including individual-level supply measures would uncover the details about how food choices are affected by local food supply.

This paper extends existing research on the effect of SNAP on dietary quality by addressing endogenous participation, incomplete measurement of food consumption, and supply-side variation. Using the FoodAPS data, this paper tackles these issues comprehensively. Using partial identification strategy, this paper estimates the causal effect under weak and innocuous assumptions.

3.5 Data

3.5.1 FoodAPS

I utilized the publicly available USDA's National Household Food Acquisition and Purchase Survey (FoodAPS) for my analysis. This survey comprehensively gathered information about all food items acquired or purchased by household members from various sources. The survey was conducted with 4,826 households over a period of seven days between April 2012 and January 2013. During the seven days, the primary respondent (PR) for each household — the main food shopper or meal planner — provided information about the household and individuals in the household through two in-person interviews. These interviews collected household demographics and information related to food purchases, diet and health.

The survey was designed to be representative of households participating in SNAP as well as households not participating in SNAP in three income groups - households with incomes below 100% of the Federal Poverty Line (FPL), households with incomes between 100% and 185% of the FPL, and households with incomes above 185% of the FPL. In the survey, the PR provided income information for each member of the household. Gross income across six categories – earnings from work; unemployment compensation; welfare, child support and alimony; retirement and disability; investment income; and other income – are recorded on a continuous scale. To assess the income status of each household, an income ratio was calculated. This ratio involved dividing the sum of individual incomes by the 2012 poverty guideline that accounts for household size. This calculation helps in determining the relative income level of each household. The SNAP and low-income non-participant groups were oversampled to allow analysis of food spending and shopping patterns specifically for these groups, which might not have been possible with other surveys or data collection methods in the past.

In addition to the in-person interviews, the survey households recorded food acquisitions in separate "food books" according to whether the acquisition was FAH or FAFH. Each entry in the food book corresponded to a specific event, such as a visit to the grocery store or a meal at a restaurant. For FAH events, households were instructed to record the total amount spent and scan all the items acquired using a handheld scanner. Similarly, for FAFH events, households are asked to record total spending and write down all items acquired. These recorded entries amounted to 15,999 FAH acquisition events, encompassing 143,050 food items, and 38,869 FAFH acquisition events, comprising 116,074 food items. For each food acquisition, I observe the quantity obtained, the amount spent, and the food group, USDA Food Pattern, and nutritional information associated with each item acquired. In cases where the quantity of a particular food item was missing, FoodAPS employed imputed quantities sourced from the USDA Economic Research Service, as outlined in Mancino et al. (2018).[4] However, it is important to note that 286 FAFH items with missing quantities were excluded from the sample as they lacked the necessary food codes required for quantity imputation.

The FoodAPS datasets incorporate a geographic component that captures information related to the proximity of food outlets to each household. After conducting the interviews, data regarding the distances between the households' residences (or the center of their census block group) and nearby food establishments were collected and processed. The geographic component encompasses not only the distance measurements for the food outlets that were actually visited by the households during the survey week (thus providing a distance-from-home measure for each recorded food event), but also includes distance measurements for the food outlets that were accessible to each household within their Primary Sampling Unit (PSU) or within adjacent PSUs. Having information on stores in adjacent PSUs means that access to food outlets is measured without border constraints for all households. In particular, for six FAH outlet categories and two FAFH outlet categories, we have the distance from each household's residence to the closest outlet of each category, as well as the number of outlets of each category within a one-mile radius. These data allow us to construct comprehensive pictures of the local food environments surrounding the surveyed households, shedding light on the accessibility and availability of various food outlets for the respondents.

Crucial to this study, the dataset contains details of SNAP participation for each household derived from both self-reported data and matched administrative SNAP records. Among the total

[4]Of all 254,587 food items with non-missing food codes, 36,388 do not have information on quantity, which takes up 14.3% of all items acquired by households in the sample.

4,826 households included in the study, only 122 did not provide consent for data matching, which reduces the potential for misreporting concerns in the analysis. Current participation status is confirmed for 1,838 households. And 3,252 were identifies as non-participants.[5] Within the FoodAPS dataset, details regarding the date and amount of SNAP benefits last received are also reported for households that are currently participating or have recently participated in the program. Additionally, FoodAPS employs the Microanalysis of Transfers to Households (MATH) SIPP+ Microsimulation Model to identify eligible SNAP units.[6] This model applies federal and state eligibility requirements to the survey data from FoodAPS enabling the identification of households that meet the eligibility criteria for SNAP. In this paper, SNAP participation status is primarily determined by administrative records, with self-reported status being used as a replacement when there is no match or consent for data matching. The paper also utilizes the simulated eligibility information from FoodAPS to determine the SNAP eligibility of households.

3.5.2 Healthy Eating Index 2010

The Healthy Eating Index 2010 (HEI-2010) is a widely used measure of diet quality, designed to evaluate the extent to which a set of foods aligns with the recommendations outlined in the 2010 Dietary Guidelines for Americans and the accompanying USDA Food Patterns (Guenther et al., 2014). It serves as a tool to assess the adherence to key dietary guidelines. The HEI-2010, along with its predecessor, the HEI-2005, is frequently employed in studies examining the determinants and factors influencing diet quality (Mabli et al., 2013; Gregory et al., 2013; Wang et al., 2014; Condon et al., 2015; Drewnowski et al., 2016).

The HEI-2010 ranges from 0 to 100 and is the sum of 12 component scores, each representing conformance to a different aspect of the 2010 DGAs. Nine of these twelve component scores, such as whole fruit, total vegetables, and whole grains, assess the adequacy of the diet, while the remaining three component scores, including empty calories, sodium, and refined grains, evaluate

[5]Participation is derived from matches with two administrative datasets – Caseload data and ALERT data. Non-participation and inconsistent information between the two matches would result in no match. And household report of participation status was considered determinative if no match in the FoodAPS.

[6]A microsimulation model is composed of an underlying database, a set of parameters, and simulation techniques. For MATH SIPP+, the set of parameters and simulation techniques apply the rules of SNAP to each household to determine its eligibility for, participation in, and benefit amount for that program.

the moderation of the diet. All component scores are computed such that higher scores represent better diet quality.[7]

In this study, for each household and food item with nonzero kilocalories purchased[8], I calculate the HEI-2010. To establish a benchmark for healthy diets, I refer to the Scientific Report of the 2015 Dietary Guidelines Advisory Committee, which states that the average HEI-2010 score for Americans aged 2 years and older was 57 during the period from 2009 to 2012. Therefore, I define a diet as healthy if the household's HEI-2010 score exceeds the national average level. Conversely, a diet is classified as not healthy if the HEI-2010 score falls below 57. Additionally, alternative cutoff values may also be considered to ensure robustness in the analysis.

3.5.3 Descriptive Statistics

The analysis sample consists of 4,722 households that provided information on both their SNAP participation status and food acquisition events.[9] I use self-reported SNAP participation for 120 households that did not consent to administrative data matching. The main analysis focuses on 2,183 households who are eligible to receive SNAP benefits.

Table 3.1 presents a summary of measures related to weekly food expenditures and dietary quality for both the entire sample of respondents and mutually exclusive subgroups categorized based on their SNAP participation and eligibility status. In terms of food spending, SNAP participating households spend significantly less than nonparticipants. Decomposing food expenditures into FAH spending and FAFH spending, I find SNAP households spend similar amount on at-home food consumption compared to eligible nonparticipants but less than noneligible households. Moreover, spending on FAFH is less for SNAP households than the other two groups.

[7]Further details on the definition of each of the 12 component scores can be found in Guenther et al. (2014).

[8]There are 254,587 food items recorded from food acquisition events in the sample. 4,537 items with missing energy level are not matched to nutrient databases. And 6,098 items with zero energy are excluded from the analysis because the calculation of HEI score requires nonzero energy.

[9]Among the 104 excluded respondents - 2 respondents reported they do not know if anyone in the household is receiving SNAP benefits and did not provide consent for data matching, 102 households from FoodAPS did not report any food acquisition event.

Table 3.1 Summary Statistics: Weekly Food Consumption and Dietary Quality Measure

Variable	Overall (1)	SNAP (2)	Non-SNAP Eligible (3)	Non-SNAP Ineligible (4)
		Panel A. Weekly Food Expenditures		
Food Spending	52.08 (0.65)	41.91 (0.95)	**48.23** (1.33)	**60.84** (1.02)
FAH expenditures	34.49 (0.51)	31.31 (0.86)	31.69 (1.01)	**38.01** (0.79)
FAFH expenditures	17.59 (0.34)	10.61 (0.35)	**16.54** (0.71)	**22.83** (0.57)
Energy Intake	17.86 (0.40)	17.64 (0.42)	16.93 (0.47)	18.46 (0.79)
Energy from Home	14.22 (0.40)	14.41 (0.41)	13.52 (0.46)	14.44 (0.79)
Energy Away from Home	3.64 (0.05)	3.23 (0.08)	3.41 (0.12)	**4.03** (0.08)
		Panel B. Diet quality measure		
HEI-2010	51.56 (0.21)	48.34 (0.35)	**51.75** (0.46)	**53.65** (0.31)
Healthy (HEI-2010≥57)	0.33 (0.01)	0.23 (0.01)	**0.31** (0.02)	**0.40** (0.01)
HEI-2010 FAH	50.55 (0.24)	46.94 (0.40)	**50.91** (0.52)	**52.82** (0.36)
Healthy FAH	0.33 (0.01)	0.23 (0.01)	**0.32** (0.02)	**0.40** (0.01)
HEI-2010 FAFH	43.62 (0.18)	43.10 (0.32)	42.74 (0.38)	**44.41** (0.26)
Healthy FAFH	0.12 (0.01)	0.11 (0.01)	0.09 (0.01)	0.13 (0.01)
		Panel C. HEI-2010 Component Density (per 1000 kcal)		
Total vegetables	0.83 (0.02)	0.66 (0.02)	**0.90** (0.03)	**0.91** (0.03)
Dark green veg and beans	0.13 (0.01)	0.08 (0.00)	**0.14** (0.01)	**0.16** (0.01)
Total fruit	0.39 (0.01)	0.33 (0.01)	**0.40** (0.02)	**0.43** (0.01)
Whole fruit	0.29 (0.01)	0.23 (0.01)	**0.30** (0.01)	**0.33** (0.01)
Whole grain	0.35 (0.01)	0.29 (0.02)	**0.37** (0.03)	**0.39** (0.01)
Dairy	0.82 (0.01)	0.83 (0.02)	0.81 (0.02)	0.82 (0.01)
Total meat/protein	2.66 (0.03)	2.60 (0.04)	2.68 (0.06)	2.69 (0.04)
Seafood and plant protein	0.57 (0.01)	0.46 (0.02)	**0.57** (0.03)	**0.65** (0.02)
Fatty acid ratio	1.93 (0.01)	1.93 (0.02)	1.95 (0.03)	1.92 (0.02)
Sodium	1.75 (0.04)	1.66 (0.05)	**1.85** (0.08)	1.76 (0.08)
Refined grains	2.79 (0.02)	2.83 (0.04)	2.87 (0.05)	2.74 (0.03)
% Calories from added sugar, solid fat, and alcohol	32.23 (0.18)	33.80 (0.32)	**31.46** (0.41)	**31.53** (0.25)
N Households	4722	1470	1098	2154
Share of Households	-	0.31	0.23	0.46

[a] The table summarizes the food acquisitions in the FoodAPS of respondents who reported both SNAP participation status and food acquisition events. In each cell, I present the mean value of the category, next to which I report the standard errors in parentheses. Bold texts in columns (3) and (4) indicate the average value is statistically different from the reference group, SNAP households (column (2)), at the significance level of 5%.

In terms of energy intake per individual measured in calories, there is no notable disparity observed among all groups, suggesting that SNAP households exhibit a lower cost per calorie given lower spending on food. In other words, they are getting a higher amount of energy (calories) per unit of currency spent. This could be due to factors such as purchasing lower-cost, calorie-dense foods or being more efficient in their food shopping choices, while eating less healthfully. This discovery aligns with prior research. When scrutinizing dietary quality, it becomes evident that nonparticipants maintain a more healthful diet, reflected by a higher average HEI score and an increased adherence rate to the criterion of a healthy diet compared to SNAP participants. This trend remains consistent in terms of food-at-home purchases, with only ineligible households exhibiting healthier eating habits outside of their residences. Among various components of food purchases, SNAP households report purchasing fewer vegetables, fruits, grains, seafood, and plant protein comparing to eligible nonparticipants. Additionally, SNAP participants obtain a higher proportion of their calories from added sugar, solid fat, and alcohol relative to eligible nonparticipants.

When examining the overall HEI-2010 score as well as scores for each individual component, it becomes apparent that SNAP participation is correlated with a lower diet quality. However, it is important to note that these comparisons do not take into account any underlying household characteristics. To assign a causal interpretation to the differences in the HEI-2010 score, it would require households to participate in the SNAP program randomly. However, it is evident from Table 3.2 that significant disparities exist in the household characteristics, highlighting the potential confounding factors at play.

Table 3.2 provides an overview of household (HH) and PR characteristics. The eligibility rules for SNAP indicate that eligible households tend to face more disadvantages, as evidenced by their lower income, fewer liquid assets, and lower likelihood of owning a vehicle. Among all eligible households in the sample, those who choose not to participate in SNAP tend to have higher income, more liquid assets, and own a vehicle compared to those who do participate. On average, SNAP households are larger in size, more likely to have children, less likely to have elderly members, and less likely to report being food secure. Additionally, a higher proportion of SNAP participants are also receiving WIC benefits during the sampling period. The primary respondents of SNAP households are generally younger, more likely to be female, and less likely to have a bachelor's degree.

Table 3.2 Summary Statistics: Household and Primary Respondent Characteristics

Variable	Overall	SNAP	Non-SNAP	
	(1)	(2)	Eligible (3)	Ineligible (4)
Panel A. Household (HH) Characteristics				
Income Ratio	250.02 (3.64)	118.64 (2.72)	**172.29** (5.27)	**385.59** (6.24)
Assets > $2,000	0.34 (0.01)	0.09 (0.01)	**0.29** (0.01)	**0.55** (0.01)
Any Vehicle	0.84 (0.01)	0.72 (0.01)	**0.82** (0.01)	**0.94** (0.01)
Household Size	2.97 (0.03)	3.44 (0.05)	**2.75** (0.05)	**2.74** (0.03)
Whit	0.69 (0.01)	0.62 (0.01)	**0.68** (0.01)	**0.74** (0.01)
Black	0.14 (0.01)	0.21 (0.01)	**0.13** (0.01)	**0.10** (0.01)
Asian	0.04 (0.00)	0.01 (0.00)	**0.07** (0.01)	**0.05** (0.00)
Hispanic	0.20 (0.01)	0.25 (0.01)	0.23 (0.01)	**0.14** (0.01)
Non-US citizen	0.08 (0.00)	0.08 (0.01)	**0.14** (0.01)	**0.06** (0.01)
Children age <18	0.46 (0.01)	0.61 (0.01)	**0.37** (0.01)	**0.39** (0.01)
Elderly age >65	0.19 (0.01)	0.14 (0.01)	**0.24** (0.01)	**0.19** (0.01)
Food Secure	0.73 (0.01)	0.55 (0.01)	**0.69** (0.01)	**0.87** (0.01)
WIC	0.10 (0.00)	0.19 (0.01)	**0.08** (0.01)	**0.03** (0.00)
Panel B. Primary Respondent (PR) Characteristics				
Age	46.36 (0.25)	43.37 (0.40)	**49.00** (0.57)	**47.12** (0.36)
Female	0.74 (0.01)	0.79 (0.01)	**0.72** (0.01)	**0.71** (0.01)
Less than high school	0.17 (0.01)	0.28 (0.01)	**0.19** (0.01)	**0.09** (0.01)
High school or GED	0.28 (0.01)	0.33 (0.01)	0.31 (0.01)	**0.24** (0.01)
Some college education	0.33 (0.01)	0.31 (0.01)	0.30 (0.01)	**0.36** (0.01)
Bachelor's degree or more	0.22 (0.01)	0.08 (0.01)	**0.21** (0.01)	**0.32** (0.01)
Panel C. Reason not eating healthy food				
Costs	0.42 (0.01)	0.50 (0.01)	**0.43** (0.02)	**0.36** (0.01)
Time	0.20 (0.01)	0.16 (0.01)	**0.19** (0.01)	**0.24** (0.01)
Taste	0.13 (0.00)	0.17 (0.01)	**0.13** (0.01)	**0.09** (0.01)
N Households	4722	1470	1098	2154
Share of Households	-	0.31	0.23	0.46

[a] Note: The table provides an overview of household characteristics for respondents who reported both their SNAP participation status and food acquisition events. Each cell displays the mean value of the corresponding category, with standard errors reported in parentheses. In columns (3) and (4), I have highlighted in bold text the average values that are statistically different from the reference group, which is SNAP households (column (2)), at a significance level of 5%.

During the initial interview, primary respondents were asked about factors that may hinder their consumption of healthy foods. When it comes to reasons for not consuming enough healthy food, SNAP participants tend to agree more often that the cost of healthy foods is prohibitive. They also report being less busy to allocate time for preparing healthy foods relative to non-participants. Furthermore, a higher proportion of SNAP households express that they do not enjoy the taste of healthy foods.

Among SNAP-eligible households, those who choose to participate exhibit systematic differences compared to eligible nonparticipants. They tend to be younger, have lower levels of education, and face budget constraints. These findings suggest that SNAP participants differ from eligible nonparticipants in observable ways. Therefore, it is crucial to account for selection based on observed characteristics as well as potential unobserved factors.

SNAP participants encounter distinct retail food environments compared to households that do not participate in SNAP. Table 3.3 provides an overview of the retail food environment in which the FoodAPS households reside, focusing on seven outlet categories: 1) Superstore, 2) Supermarket, 3) Combo Retail, 4) Convenience store, 5) Grocery store, 6) Fast-food restaurant, and 7) Non-fast-food restaurant.[10] When examining the number of outlets located within a one-mile radius of each household's residence, notable differences emerge between households that choose to participate in SNAP and eligible households that do not participate. Compared with eligible households who do not participate, SNAP participants have fewer superstores, grocery stores and non-fast-food restaurants within one mile. While the distance to the closest superstore, convenience stores, and fast-food restaurant is shorter for SNAP participants than for nonparticipating eligible households, SNAP households face a less adequate food supply than eligible households in poverty in terms of the number of food retailers in the neighborhood. Therefore, it is crucial to control for heterogenous food environment when evaluating the causal effect of SNAP participation.

[10]The retail food environment measures for FAH outlets are constructed using the nationwide STARS datasets that include all retailers authorized to receive SNAP benefits as of June 2012. The locations of FAFH outlets came from InfoUSA, which is a private company that develops databases of business addresses. The InfoUSA data is from January 2012.

Table 3.3 Summary Statistics: Retail Food environment

Variable	Overall (1)	SNAP (2)	Non-SNAP Eligible (3)	Non-SNAP Ineligible (4)
Panel A. Number of stores in a 1-mile radius				
Super Store	0.81 (0.02)	0.79 (0.03)	**0.95** (0.05)	0.75 (0.03)
Supermarket	0.91 (0.02)	0.96 (0.04)	1.01 (0.05)	**0.84** (0.03)
Combo Retail	2.28 (0.04)	2.46 (0.07)	2.48 (0.10)	**2.05** (0.06)
Convenience Store	4.90 (0.12)	5.67 (0.20)	5.76 (0.29)	**3.90** (0.16)
Grocery Store	1.41 (0.07)	1.41 (0.10)	**1.94** (0.19)	**1.14** (0.09)
Fast Food Restaurant	5.81 (0.10)	5.78 (0.16)	6.17 (0.21)	5.66 (0.15)
Non-Fast-Food Restaurant	28.69 (0.83)	26.58 (1.23)	**33.93** (2.08)	27.46 (1.22)
Panel B. Distance to closest store (miles)				
Super Store	2.59 (0.05)	2.50 (0.09)	2.54 (0.11)	2.69 (0.08)
Supermarket	2.56 (0.06)	2.27 (0.10)	2.50 (0.13)	**2.80** (0.10)
Combo Retail	1.50 (0.03)	1.32 (0.05)	1.42 (0.07)	**1.67** (0.06)
Convenience Store	1.28 (0.03)	1.02 (0.04)	**1.19** (0.06)	**1.51** (0.05)
Grocery Store	4.06 (0.07)	3.83 (0.14)	3.82 (0.15)	**4.35** (0.11)
Fast Food Restaurant	1.71 (0.05)	1.57 (0.08)	1.61 (0.09)	**1.87 (0.07)**
Non-Fast-Food Restaurant	0.82 (0.02)	0.77 (0.04)	0.76 (0.04)	**0.88** (0.03)
N Households	4722	1470	1098	2154
Share of Households	-	0.31	0.23	0.46

[a] The table provides an overview of food access for respondents who reported both their SNAP participation status and food acquisition events. Each cell displays the mean value of the corresponding category, with standard errors reported in parentheses. In columns (3) and (4), I have highlighted in bold text the average values that are statistically different from the reference group, which is SNAP households (column (2)), at a significance level of 5%.

3.6 Results with Control Strategies

Challenges arise when analyzing the effect of SNAP on dietary quality due to the presence of preexisting characteristics that are likely correlated with both SNAP participation and dietary quality. For example, households with economic hardships and limited resources are more likely to participate in SNAP and have poorer dietary habits. Given the systematic differences between SNAP participants and nonparticipants, it is essential to appropriately account for these disparities in order to obtain unbiased estimates. Simply comparing the outcomes between these groups without proper adjustment can lead to biased conclusions.

This section discusses results from two common control strategies that depend on the conditional independence assumption – the potential outcomes are independent of treatment status conditional on a given set of covariates.

3.6.1 Regression Analysis

The summary statistics reveal systematic differences in demographics and the food environment between SNAP participants and eligible nonparticipants. Given the potential confounding effects of these heterogeneities, I proceed with an analysis that aims to control for various covariates and uncover the correlation between SNAP participation and a range of outcomes. The analysis focuses on 2,337 households identified as SNAP eligible. Specifically, I estimate the following equation:

$$y_i = \beta_0 + \beta_1 \cdot \text{SNAP}_i + \gamma \cdot \text{HH}_i + \delta \cdot \text{PR}_i + \theta \cdot \text{Access}_i + \varepsilon_i \tag{3.1}$$

where Y_i represents the outcome of interest of household i, including the HEi-2010 score, the indicator of the HEI-2010 score being above the healthy diet cutoff, the weekly food expenditures, and the weekly energy intake. SNAP_i is an indicator of SNAP participation. HH_i is a vector of household characteristics including their income-to-FPL ratio, liquid assets exceeding \$2,000, vehicle ownership, household size, race, and the presence of children or elderly members. PR_i represents the demographic characteristics of the primary respondent, such as age, gender, race, and an indicator of having a bachelor's degree. Access_i includes the number of SNAP authorized retailers and fast-food restaurants within one mile of the household's residence, and the distance to the closest SNAP retailer and fast-food restaurant.

Table 3.4 presents the results of OLS regression estimating the effects of SNAP participation on the likelihood of a household consuming a healthy diet. Column (1) shows the regression results controlling for household characteristics. The results indicate that SNAP participation is associated with a 2.3 percent lower likelihood of consuming a healthy diet. Additionally, larger household size is linked to a reduced probability of maintaining a nutritious diet, while higher income and greater assets are associated with higher diet quality. Moreover, Asian and Hispanic families, as well as those living with elderly members, exhibit a higher rate of healthy eating.

Table 3.4 The Effects of SNAP Participation on Healthy Diet

	All Eligible Households			Income <100% FPL	Income ≥100% FPL
	(1)	(2)	(3)	(4)	(5)
SNAP Participation	-.023	-.018	-.017	-.029	-.007
	(.021)	(.021)	(.021)	(.032)	(.028)
Log Income to FPL Ratio	.024*	.021	.018	.023	.051*
	(.013)	(.013)	(.013)	(.019)	(.031)
Assets > 2000	.093***	.075**	.074**	.192***	.031
	(.029)	(.03)	(.029)	(.066)	(.033)
Own a Vehicle	.035	.034	.056***	.048*	.079**
	(.021)	(.021)	(.021)	(.028)	(.034)
Household Size	-.015**	-.014**	-.013**	-.008	-.02**
	(.006)	(.006)	(.006)	(.008)	(.009)
White	.005	.002	.021	-.016	.063**
	(.02)	(.02)	(.02)	(.028)	(.03)
Asian	.156***	.129**	.08	.001	.126*
	(.059)	(.06)	(.059)	(.098)	(.076)
Hispanic	.107***	.109***	.07***	.049	.098***
	(.023)	(.023)	(.024)	(.032)	(.037)
Children age<18	.011	.009	.012	-.008	.037
	(.026)	(.027)	(.027)	(.038)	(.037)
Elderly age>65	.045*	.019	.013	-.025	.048
	(.026)	(.032)	(.032)	(.047)	(.043)
Age		.001	.001*	.002	.001
		(.001)	(.001)	(.001)	(.001)
Female		.046**	.045**	.012	.064**
		(.022)	(.022)	(.032)	(.03)
Bachelor's Degree		.105***	.095***	.069	.11***
		(.031)	(.031)	(.052)	(.039)
Number of SNAP retailers < 1 mile			.001***	.001*	.001***
			(0.00)	(.001)	(0.00)
Number of Fast-Food Restaurant < 1 mile			.002	.005*	.000
			(.002)	(.003)	(.003)
Distance to Closest SNAP Retailer			-.002	-.006	.002
			(.008)	(.011)	(.012)
Distance to Closest Fast-Food Restaurant			-.002	.001	-.005
			(.003)	(.003)	(.005)

Table 3.4 (cont'd)

	All Eligible Households			Income <100% FPL	Income ≥100% FPL
	(1)	(2)	(3)	(4)	(5)
Observations	2204	2204	2204	1018	1186
R-squared	.034	.043	.055	.058	.061

[a] Note: This table presents the estimation results of the impact of SNAP participation on maintaining a healthy diet from the OLS regression of equation (1). A healthy diet is defined as the HEI-2010 score being higher than 57. In columns (1) – (3), the sample consists of all SNAP eligible households who reported SNAP participation and food acquisition in the FoodAPS. The three columns sequentially add household characteristics, primary respondent's demographics, and local food access in the regressions. Column (4) replicates the regression using a restricted sample of eligible households with income less than the FPL and the full set of control variables. Column (5) reports the regression results with the sample of eligible households with income above the FPL and the full set of control variables.

[b] *** p<.01, ** p<.05, * p<.1

Columns (2) and (3) report the estimates where I add the PR's demographics and measures of food access sequentially into the model. With the inclusion of these additional controls, the negative impact of SNAP participation on a healthy diet becomes insignificant, and the magnitude of the effect diminishes. This finding aligns with the notion that incorporating more controls helps alleviate potential bias stemming from selection issues. Furthermore, larger household size remains associated with 1.4 percent lower probability of maintaining a healthy diet. Notably, a higher education level of the primary shopper of the household significantly increases the probability of having a healthy diet. Specifically, households where the primary shopper holds at least a bachelor's degree exhibit a 10 percent higher probability of eating nutritiously compared to those with lower education levels. Additionally, households residing in proximity to more SNAP-authorized retailers are more likely to have a healthy diet. These results hold consistently across different income subgroups divided by the 100% FPL cutoff, suggesting that SNAP participants, regardless of their income level, do not appear to have a higher likelihood of adopting a healthier diet.

3.6.2 Propensity Score Methods

With substantial difference in characteristics across groups, multivariate regression fails to accurately quantify the similarities and dissimilarities between the treatment and control groups, also known as the common support problem. This raises a question of whether the regression results truly capture the mean difference between comparable members. To address this concern, I proceed with the propensity score matching method to match households based on their observed characteristics. By comparing the matched households in terms of diet quality, the results reflect the treatment effect if the conditional independence assumption holds. This method may be preferable to regression because it does not rely on a linear functional form to adjust for potential confounding variables. Table 3.5 shows the estimates from a doubly robust approach that incorporates propensity score to match treatment and control groups.

Table 3.5 The Effects of SNAP Participation from a Doubly-Robust Approach

	(1)	(2)	(3)
	Dichotomous outcome of healthy diet		
SNAP	-0.038	-0.022	-0.041
	(0.025)	(.037)	(.043)
	HEI-2010 Score		
SNAP	-2.403***	-1.750	-2.535**
	(0.876)	(1.099)	(1.296)
	Weekly Food Spending		
SNAP	2.018	-1.03	0.893
	(2.061)	(2.855)	(3.381)
	Energy Intake		
SNAP	2.993***	1.645*	2.364**
	(0.877)	(0.919)	(1.040)
Household Characteristics	✓	✓	✓
PR's Demographics		✓	✓
Food Access			✓
Observations	2,059	2,059	2,059

[a] Note: This table presents the estimation results of the impact of SNAP participation on various measures of food acquisitions. In all columns, the sample consists of all SNAP eligible households who reported SNAP participation and food acquisition in the FoodAPS. The three columns sequentially add household characteristics, primary respondent's demographics, and local food access in the estimation.

[b] *** p<.01, ** p<.05, * p<.1

In Table 3.5, columns (1)-(3) report estimates from models that subsequentially incorporate household characteristics, PR's demographics, and measures of food access. Consistent with the findings from OLS, the results indicate that SNAP participants have lower but statistically insignificant diet quality when measured as a dichotomous outcome across all three models. However, when measured using the continuous HEI-2010 scores, SNAP participants demonstrate significantly lower diet quality. Interestingly, despite the puzzling negative effect on food quality, SNAP benefits appear to increase the quantity of food consumed, as evidenced by higher food spending and energy intake.

There are several factors that could contribute to the perplexing results, where SNAP participants spend more on food but have lower quality. While multivariate regression or propensity matching techniques can control for observable differences between households, there may be important unobserved differences that could bias the estimation of the causal effect of SNAP participation. Furthermore, if there is a high rate of measurement error in determining true participation status, it can introduce serious issues. Traditional methods addressing selection on unobservables, such as instrumental variables, rely on exclusion assumptions that may be questionable. To address the challenges of self-selection and measurement error under a set of weaker assumptions, I adopt a partial identification approach.

3.7 Methodology for Partial Identification

To partially identify the average treatment effect (ATE) of SNAP participation on dietary quality, I use the notation from Kreider et al. (2012). Let $S^* = 1$ denote that the household receives SNAP benefits and $S^* = 0$ denote that the household does not participate in SNAP. The ATE with binary outcome is given by

$$\text{ATE}(1, 0 \mid X \in \Omega) = P[H(1) = 1 \mid X \in \Omega] - P[H(0) = 1 \mid X \in \Omega] \tag{3.2}$$

where H stands for realized dietary quality with $H = 1$ representing the household is on a healthy diet (HEI-2010 scores no smaller than 57) and $H = 0$ representing unhealthy diet. $H(1)$ denotes the potential dietary quality if the household were to participate in SNAP and $H(0)$ denotes the potential dietary quality if the household were not to receive SNAP. X denotes a set of control variables whose value lies in the set Ω. The observed outcome for a particular household is given by $H = H(1)S^* + H(0)(1 - S^*)$. And the ATE reveals the mean difference of the probability of eating a healthy diet if all households received SNAP benefit versus if all did not participate. The set of conditioning variables X is left implicit throughout the remainder of the article. In the usual regression framework, a set of observed covariates that may influence diet choice are controlled for to meet the exogenous selection. However, in this approach, covariates only serve to define subpopulation of interest since there are no regression orthogonality conditions to be met. Further, the problem is well-defined regardless of how the subpopulations are specified (Pepper, 2000).

The probabilities in equation (3.2) can be decomposed using the Law of Total Probability as

$$P[H(1) = 1] = P[H(1) = 1 \mid S^* = 1]P(S^* = 1) + P[H(1) = 1|S^* = 0]P(S^* = 0) \tag{3.3}$$

$$P[H(0) = 1] = P[H(0) = 1 \mid S^* = 1]P(S^* = 1) + P[H(0) = 1|S^* = 0]P(S^* = 0) \tag{3.4}$$

Two key identification problems arise in the identification of the ATE. First, the selection problem confounds the inference on the ATE due to the unobserved counterfactual outcome. If true participation status is observed, then the selection probability, $P(S^* = 1)$ and $P(S^* = 0)$, is identified from the sampling process. And the expectations of realized outcomes conditional on participation status are observed such that $P[H(1) = 1 \mid S^* = 1] = P[H = 1 \mid S^* = 1]$ and $P[H(0) = 1 \mid S^* = 0] = P[H = 1 \mid S^* = 0]$. But the counterfactual outcomes, $P[H(1) = 1 \mid S^* = 0]$ and $P[H(0) = 1 \mid S^* = 1]$, are not revealed from the data. The second problem stems from the measurement error of participation status. When participation is misreported, self-reported participation is different from true participation and all the probabilities on the right-hand-side are unknown.

The sample uses 2,334 eligible households with 1,236 SNAP participants and 1,098 nonparticipants. Among these households, data matching confirms the SNAP participation status for 1,035 households and nonparticipation status for 79 households. However, 1,181 households, which accounts for over half of all eligible households, are not matched with administrative data. This could be due to these households never participating in SNAP or inconsistencies in the information available in the administrative datasets. Additionally, only 39 households did not provide consent for data matching.

In this study, misreporting is less problematic if it is assumed that all households that consented to data matching accurately reported their participation status. If it not true, this assumption potentially introduces bias or inaccuracies in the analysis. I first deal with the selection problem alone and then address both the selection and measurement error.

3.7.1 Addressing the Selection Problem

Simply using the fact that the missing counterfactuals are bounded by 0 and 1 as a probability, the worst-case selection bounds are obtained by taking the two polar extremes

$$P(H = 1, S^* = 1) \leq P(H(1) = 1) \leq P(H = 1, S^* = 1) + P(S^* = 0) \tag{3.5}$$

$$P(H = 1, S^* = 0) \leq P(H(0) = 1) \leq P(H = 1, S^* = 0) + P(S^* = 1) \tag{3.6}$$

Then the upper bound on the ATE is obtained by subtracting the lower bound on $P[H(0) = 1]$ from the upper bound on $P[H(1) = 1]$. And the lower bound is obtained by subtracting the upper bound on $P[H(0) = 1]$ from the lower bound on $P[H(1) = 1]$. The bounds are sharp for accurately measured data (Manski, 2007).

Assuming away measurement error in participation status, I consider a number of different monotonicity assumptions that narrow the bounds of ATE.[11]

3.7.1.1 Monotone Treatment Selection (MTS)

I adopt the MTS assumption (Manski and Pepper, 2000) that imposes assumption on the selection mechanism underlying SNAP participation. This assumption posits that households who become SNAP participants have a lower latent dietary quality compared to nonparticipants. This means SNAP participants tend to eat less healthfully than nonparticipants would if they received SNAP benefits and they have a less healthy diet than nonparticipants if they did not participate in the program. Mathematically, the MTS assumption is expressed by

$$P[H(j) = 1 \mid S^* = 0] \leq P[H(j) = 1 \mid S^* = 1] \qquad j = 0, 1$$

[11]These assumptions regarding potential outcomes are generally not testable and their implications may not hold true.

One common explanation for the negative relationship is the endogenous selection into participation, whereby eligible households may choose to participate in SNAP due to poor health or nutrition (Bitler and Seifoddini, 2019). It is also plausible that unobserved factors related to poor diet are positively associated with the decision to participate. For instance, households may opt to participate in SNAP because they anticipate having a suboptimal diet.

A potential violation of the assumption is the idea that households choose to participate in SNAP because they prioritize food and nutrition but face financial constraints preventing them from affording a healthy diet. In this scenario, the underlying diet of SNAP participants could be more nutritious than that of eligible nonparticipants who place less emphasis on nutrition, assuming all other conditions are equal. However, empirical evidence does not support this hypothesis. Zeballos and Anekwe (2018) examined the association between nutrition information use (NIU) and diet quality also using the FoodAPS data. They constructed an NIU factor score to assess nutrition knowledge and information use based on relevant questions from the FoodAPS interview. Their analysis revealed that higher-income nonparticipant households had a mean score of 0.35, while low-income nonparticipant households and SNAP households had lower NIU factor scores with means of -0.25 and -0.24, respectively. If nutrition knowledge can be considered a valid indicator of how much families prioritize nutrition, this evidence suggests that SNAP participants do not exhibit a higher level of concern for nutrition compared to eligible nonparticipants.

3.7.1.2 Monotone Instrumental Variable (MIV)

MIV places assumption on how certain observed covariates are monotonically related to the latent probability of a healthy diet. I use income-to-FPL ratio as the monotone instrument variable, where I assume that the probability of maintaining a healthy diet weakly increases with the household's reported income-to-FPL ratio that adjusts for family composition. Let v be the income ratio, then MIV implies

$$u_1 \leq u \leq u_2 \Rightarrow P\left[H(j)=1 \mid v=u_1\right] \leq P[H(j)=1 \mid v=u] \leq P\left[H(j)=1 \mid v=u_2\right] \quad j=0,1$$

The validity of the MIV assumption is supported by the fact that higher-income households are more likely to afford a healthy diet. Additionally, these households tend to exhibit a greater concern for nutrition, leading to a higher demand for a healthy diet (Zeballos and Anekwe, 2018).

3.7.1.3 Monotone Treatment Response (MTR)

The idea of MTR is formalized by assuming SNAP participation does not lower the quality of a household's diet. Despite the observed positive correlations in the data between SNAP participation and unfavorable outcomes, I assume receiving extra money for food consumption would not lower dietary quality of the household. This is represented by

$$H(0) \leq H(1)$$

While MTR rules out the possibility of deleterious effects of SNAP on dietary quality, it leaves open the question of whether the program has strong beneficial effects, mild beneficial effects, or no effects.

3.7.2 Addressing Both the Selection and Measurement Error Problem

Denote the latent indicator Z^* that indicates whether a report of SNAP participation is accurate, where $Z^* = 1$ if true participation status $S = S^*$ and $Z^* = 0$ otherwise. Then the probability in equation (3.3) can be further decomposed as

$$\begin{aligned} P[H(1) = 1] = P(H = 1, S = 1) + \theta_1^- - \theta_1^+ + P\left[H(1) = 1 \mid S^* = 0\right] \\ \times \left[P(S = 0) + \left(\theta_1^+ + \theta_0^+\right) - \left(\theta_1^- + \theta_0^-\right)\right] \end{aligned}$$

where $\theta_k^+ \equiv P(H = k, S = 1, Z^* = 0)$ and $\theta_k^- \equiv P(H = k, S = 0, Z^* = 0)$ denote the fraction of observations that are false positive and false negative with outcome $k = 0, 1$. Without imposing assumptions on the measurement error probabilities θ, the counterfactual outcome distribution $P[H(1) = 1|S^* = 0]$ can take any value from $[0, 1]$. Kreider et al. (2012) show the ATE can be sharply bounded as

$$-P(H = 0, S = 1) - P(H = 1, S = 0) + \Theta \leq ATE \leq P(H = 1, S = 1) + P(H = 0, S = 0) + \Theta$$

where $\Theta \equiv (\theta_1^- + \theta_0^+) - (\theta_0^- + \theta_1^+)$. The range of Θ need to be restricted to address the measurement error problem since the error probabilities are not identified in data.

By leveraging the matched administrative data on SNAP participation status, I can examine the two ends of the misreporting spectrum. One scenario assumes that all households refusing data matching did not participate in the program, while the other assumes that all of them were actual SNAP participants. In the full sample, 120 out of 4,722 households declined verification of their participation status, indicating an underreporting rate within the range of 0% to 8%. To assess the sensitivity of our results, I also explore higher misreporting rates. Furthermore, robustness checks are performed on different subsamples to ensure the robustness of our findings.

3.7.3 Implementation

The bounds under different sets of assumptions are functions of various nonparametrically estimable conditional probabilities.[12] I only need to estimate the selection probabilities, $P(S^* = s)$, and the joint probabilities of diet quality outcome and treatment status, $P(H = h, S^* = s)$, where h and s are binary measures of diet quality and participation. The probabilities are estimated by the empirical distribution of these variables accounting for the survey sampling design.

For models incorporating the MIV assumption, the sample is split into 5 cells based on values of the income measure. Bounds are calculated within each cell, considering the other assumptions specific to each model. The overall bounds for the entire sample are obtained by taking a weighted average of the appropriate bound estimates across all five cells. This is achieved by including the lower and upper bounds in each cell and averaging them over the range of individual bounds across all cells, including itself. To mitigate finite-sample bias, the estimates of the overall bounds are adjusted using a bootstrapping approach. This involves resampling the sampling distribution of bounds estimated within each cell, allowing for a more accurate assessment of the bounds in consideration of the sample size. For all models, the confidence intervals (CI) are calculated following Imbens and Manski (2004), which asymptotically cover the true value of the parameter with fixed probability.

[12] Proofs are provided by Manski and Pepper (2000), Kreider and Pepper (2007), Kreider et al. (2012), and Gundersen et al. (2017)

3.7.4 Results

Tables 3.6 presents bias-corrected bounds, confidence intervals, and estimated finite-sample biases under a variety of different models for measurement error levels at 0%, 2%, 5% and 8%. The first row reports estimation results for the worst-case model where no assumption is made regarding the selection process. In the absence of measurement error, the bounds have a width of one and always include zero. This indicates that, without additional assumptions to address self-selection, I cannot determine whether SNAP has a substantial positive or negative impact on diet quality. Column (1)-(4) show results for different levels of misreporting. As the misreporting rate increases, the bounds become wider, providing less information on the treatment effect.

Table 3.6 Sharp bounds on the ATE of SNAP participation

	Arbitrary Errors				No False Positives			
	0%	2%	5%	8%	0%	2%	5%	8%
	Worst Case Selection							
p.e.	[-0.491, 0.509]	[-0.511, 0.529]	[-0.541, 0.559]	[-0.571, 0.589]	[-0.491, 0.509]	[-0.511, 0.529]	[-0.541, 0.559]	[-0.571, 0.589]
CI	[-0.510, 0.519]	[-0.530, 0.539]	[-0.560, 0.579]	[-0.590, 0.599]	[-0.510, 0.519]	[-0.530, 0.539]	[-0.560, 0.579]	[-0.590, 0.599]
	MTS Assumption							
p.e.	[-0.091, 0.509]	[-0.156, 0.529]	[-0.260, 0.559]	[-0.375, 0.589]	[-0.091, 0.509]	[-0.113, 0.529]	[-0.144, 0.559]	[-0.176, 0.589]
CI	[-0.108, 0.519]	[-0.173, 0.539]	[-0.277, 0.579]	[-0.390, 0.599]	[-0.108, 0.519]	[-0.172, 0.539]	[-0.277, 0.579]	[-0.388, 0.599]
	MTS and MTR Assumptions							
p.e.	[0.000, 0.509]	[0.000, 0.529]	[0.000, 0.559]	[0.000, 0.589]	[0.000, 0.509]	[0.000, 0.529]	[0.000, 0.559]	[0.000, 0.589]
CI	[0.000, 0.519]	[0.000, 0.539]	[0.000, 0.579]	[0.000, 0.599]	[0.000, 0.519]	[0.000, 0.539]	[0.000, 0.579]	[0.000, 0.599]
	MIV and MTS Assumptions							
p.e.	[-0.091, 0.509]	[-0.156, 0.529]	[-0.235, 0.559]	[-0.303, 0.589]	[-0.091, 0.509]	[-0.111, 0.529]	[-0.144, 0.559]	[-0.176, 0.589]
CI	[-0.108, 0.519]	[-0.173, 0.539]	[-0.264, 0.579]	[-0.327, 0.599]	[-0.108, 0.519]	[-0.173, 0.539]	[-0.178, 0.579]	[-0.193, 0.599]
Bias	+0.017 +0.000	+0.019 +0.000	+0.019 +0.000	+0.017 - 0.001	+0.017 +0.000	+0.016 +0.000	+0.017 +0.000	+0.021 - 0.000
	MIV, MTS and MTR Assumptions							

Table 3.6 (cont'd)

	Arbitrary Errors				No False Positives			
	0%	2%	5%	8%	0%	2%	5%	8%
p.e.	[0.000, 0.509]	[0.000, 0.529]	[0.000, 0.559]	[0.000, 0.589]	[0.000, 0.509]	[0.000, 0.529]	[0.000, 0.559]	[0.000, 0.589]
CI	[0.000, 0.519]	[0.000, 0.539]	[0.000, 0.579]	[0.000, 0.599]	[0.000, 0.519]	[0.000, 0.539]	[0.000, 0.579]	[0.000, 0.599]
Bias	+0.017 +0.000	+0.019 +0.000	+0.019 +0.000	+0.017 - 0.001	+0.017 +0.000	+0.016 +0.000	+0.017 +0.000	+0.021 - 0.000

[a] The table presents bias-corrected bounds, confidence intervals, and estimated finite-sample biases under a variety of different models. The left panel shows the results for the arbitrary errors case, where non-participating households can report participation, and SNAP participants can report non-participation. The right panel presents the results obtained under the assumption that no non-participants report participation. The four columns within each panel show how the estimation results change with different mis-reporting rates.

[b] p.e. stands for bias-corrected point estimates of the population bounds

[c] CI is 90% confidence intervals using methods from Imbens and Manski (2004) with 1,000 pseudo-samples.

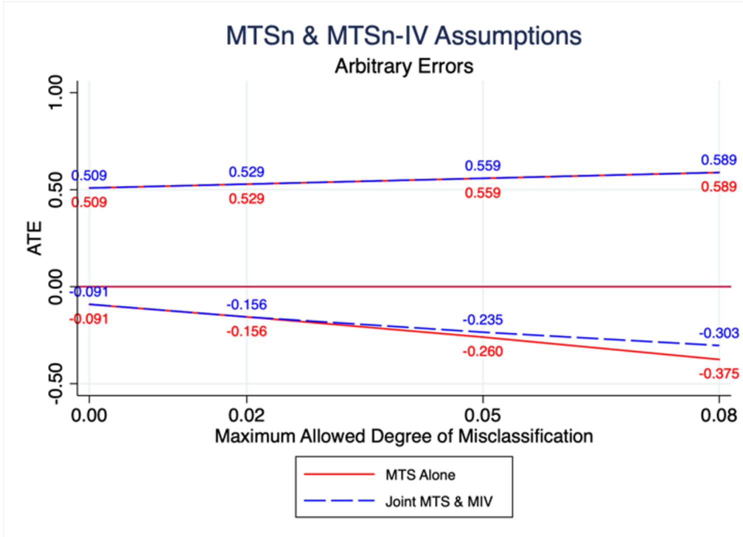
To narrow the bounds, various combinations of common monotonic assumptions are applied in subsequent models. When only the MTS assumption is imposed, which suggests that SNAP households likely have worse latent outcomes, the sign of the ATE remains unidentified despite narrower bounds in all cases. However, it is important to note that the MTS assumption alone eliminates the possibility of substantial decreases in dietary quality due to SNAP. For instance, assuming no misreporting, the lower bound of the causal effect increases from -0.491 to -0.091 when the MTS assumption is incorporated. On the other hand, SNAP participation may still lead to significant improvements in the probability of consuming a healthy diet.

In the model combining the MIV-MTS assumptions, the lower bounds are raised to a higher level but still fail to identify the sign of the treatment effect. Furthermore, the presence of measurement error expands the bounds for both patterns of misreporting. The most restrictive model, which incorporates MIV, MTS, and MTR assumptions, does not provide narrower upper bounds. In fact, the lower bounds increase to zero due to the nature of the MTR assumption, which rules out the possibility of SNAP lowering dietary quality.

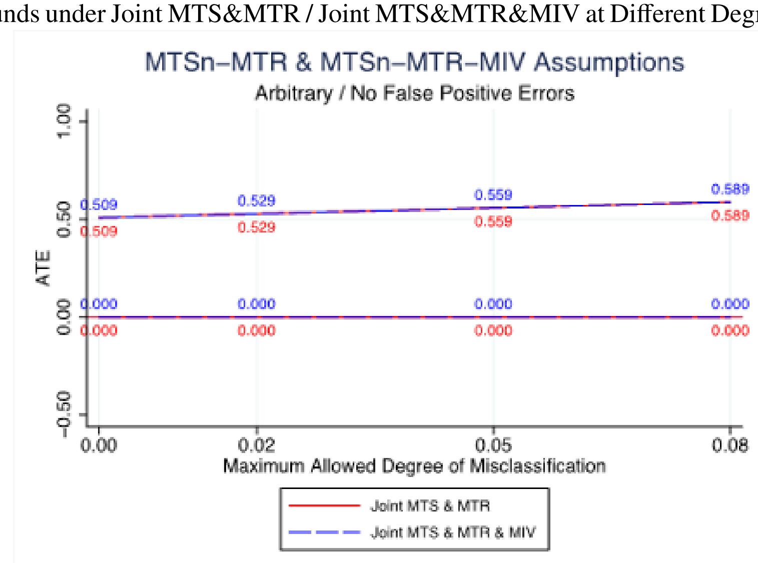
Figure 3.4 illustrates the estimated bounds of the ATE as a function of misreporting rates. When considering either the MTS assumption or the joint MTS-MIV assumptions, the upper

Figure 3.4 Estimated ATE Bounds

Panel A. The ATE Bounds under MTS / Joint MTS&MIV at Different Degrees of Misclassification



Panel B. The ATE Bounds under Joint MTS&MTR / Joint MTS&MTR&MIV at Different Degrees of Misclassification



Note: The figures plot the ATE bounds estimated under different sets of assumptions and different mis-reporting rates. Panel A shows the arbitrary case where misreporting can result from both false positives and false negatives. Because the estimation results are the same for arbitrary misreporting and only false positives under joint MTS&MTR and joint MTS&MTR&MIV cases, Panel B plots the results for both misreporting scenarios.

bounds remain consistent across all levels of misreporting. The inclusion of the MIV assumption, however, helps narrow the lower bounds as misreporting rates increase. Unfortunately, neither of these assumptions aids in identifying the sign of the treatment effect.

Under the joint MTS-MTR assumptions and the joint MTS-MTR-MIV assumptions, the two sets of assumptions yield identical estimated bounds. The MTR assumption restricts the lower bounds to nonnegative values by design. Even in the absence of measurement error, there is insufficient evidence to support the claim that participation in SNAP would strictly and positively impact the probability of consuming a healthy diet among SNAP-eligible households.

By examining various combinations of monotone assumptions and different criteria for a healthy diet, the treatment effect cannot be bounded to a strictly positive value. Assuming the credibility of these monotonicity assumptions, SNAP participation does not yield a positive effect on diet quality.

3.8 Conclusion

A considerable amount of research has been dedicated to examining the role of food programs in enhancing food access and improving the quality of diets. However, program evaluation faces methodological challenges, prompting ongoing investigations. This paper employs a partial identification method to assess the effect of SNAP participation on dietary quality among eligible households, addressing both endogenous participation and measurement error.

Using data from FoodAPS, the findings regarding whether SNAP increases or decreases overall diet quality remain inconclusive under various credible assumptions, even after controlling for demographics and measures of food access. This emphasizes the significance of understanding the underlying reasons why individuals in SNAP households struggle to maintain a healthy diet. Exploring whether this stems from households' inherent preference for taste over nutritional content would provide valuable insights, particularly in evaluations of nutrition education programs such as the Supplemental Nutrition Assistance Program Education (SNAP-Ed).

Extending the current partial identification technique to continuous outcome measures holds promise for obtaining more informative bounds on the causal effect of SNAP participation. Additionally, future research can contribute to developing theoretical frameworks that capture households' trade-offs between food quantity and quality when faced with limited resources and an increased food budget. Investigating the effect of SNAP benefit amounts is also crucial, as program outcomes may vary depending on variations in benefit levels. Furthermore, the development of techniques that address non-binary treatments under endogeneity and misreporting is essential for advancing research in this field.

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