THREE ESSAYS ON THE U.S. FOOD AND NUTRITION ASSISTANCE PROGRAMS

by

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(Under the Direction of Travis A. Smith)

ABSTRACT

The goal of the federal food and nutrition assistance programs in the U.S. is to improve the nutritional well-being and health of low-income households. This dissertation explores the extent to which these programs have accomplished this goal.

The first essay examines how the implementation and the subsequent expiration of the American Recovery and Reinvestment Act (ARRA) affected the material well-being of the Supplemental Nutrition Assistance Program (SNAP) participants. I find that ARRA implementation on average increased the overall material well-being of SNAP participants, as measured by their total nondurable spending, whereas the ARRA expiration reduced their well-being. Furthermore, using a fixed-effect quantile estimator, I find that ARRA implementation led to a first-order improvement in the distributions of both total nondurable and food spending. I also find that low-food and high-food spending households were the most responsive to increase in benefits. ARRA expiration, however, affected households with the lowest total nondurable and food expenditures.

The second essay estimates the welfare effects of the SNAP benefit cycle – the observation that food spending of SNAP households spikes upon benefits arrival and declines over the remainder of the benefit month. I first show that the price component of food expenditure is also sensitive to the benefit arrival. I then estimate welfare changes due to the changes in prices paid. I find that by the end of the third week of the benefit month, households are paying 22% less on food bundles, implying a change in money-metric welfare of \$4.94 per day or 6.6% of the average amount spent on the first two days of the month.

The final essay estimates the effects of aging out of the Supplemental Nutrition Program for Women, Infants, and Children (WIC) on quality of children's diets and rates of food insecurity. Using a regression discontinuity design, I find a fairly large decrease in overall diet quality of children as they become age-ineligible for WIC. Moreover, by investigating the entire diet quality distribution, I find that children prone to lower-quality diets experience larger decreases in nutrition. I find no significant effect on rates of food insecurity.

INDEX WORDS: SNAP, American Recovery and Reinvestment Act, Material well-being, Fixed-effect quantile estimation, SNAP benefit cycle, Unit price, Consumer welfare, WIC, Diet Quality, Food insecurity, Regression discontinuity design, Instrumental variable quantile regression

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To my loving wife, Mona.

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Chapter 1

Introduction

A major concern in discussions of health and well-being of low-income individuals and households in the United States is about their adequate nutrition (Hoynes and Schanzenbach, 2015). In general, poor nutrition is highly correlated with a number of diseases and chronic conditions including cardiovascular disease, type 2 diabetes, and some types of cancer (Jemal et al., 2008). In addition, inadequate dietary intake during pregnancy and early childhood can impair growth, increase the developmental risk, and affect dietary behaviors in adulthood (Carlson et al., 2003; Beydoun and Wang, 2009; Black et al., 2013).

To address concerns about inadequate dietary intake, a range of food and nutrition assistance programs (e.g., the Supplemental Nutrition Assistance Program [SNAP], the National School Lunch Program [NSLP], and the Special Supplemental Nutrition Program for Women, Infants, and Children [WIC]) are provided by the U.S. Department of Agriculture (USDA). There are many features that are common to these programs. They are all means-tested; that is, to become eligible for participation individuals must live in households with limited income. Importantly, these programs share the goal of assuring adequate nutrition and are all federally administered. Thus, they provide a basic floor to protect low-income individuals and households that is similar across states.

There are, however, important ways in which the programs differ. First, the programs vary in terms of their targeted populations, from near-universal eligibility for SNAP to strictly defined age-eligible groups for WIC. Second, the income thresholds for eligibility vary between the programs, with 185% of the federal poverty guidelines (FPL) for WIC to 130% of the FPL for SNAP. Third, the degree to which benefits are provided "in-kind" varies across the programs, from largely unrestricted vouchers in SNAP to extremely targeted vouchers in WIC. Lastly, the programs differ according to whether they phase out "gradually" (i.e., SNAP) or "abruptly" with increases in income (i.e., other programs).

Despite the patchwork of nutrition assistance programs available, many program participants are still food insecure, consume low-quality diets, or both. In 2014, 15.4% of individuals and 20.9% of children lived in food-insecure households (Coleman-Jensen et al., 2015). At the same time, diet quality of Americans, despite its modest improvements, has failed to meet the federal dietary recommendations (Wang et al., 2014; Beatty et al., 2014). Thus, a pressing concern for policymakers is whether these programs are doing an adequate job enhancing and protecting the nutrition status of disadvantaged Americans. Accordingly, a large body of literature has evaluated the efficacy of these programs. In general, these studies find that nutrition assistance programs have a positive impact on well-being (see, Fox et al., 2014; Colman et al., 2012; Hoynes and Schanzenbach, 2015). Yet, there are many unresolved questions within this literature finding compelling answers to which could give policymakers guidance on ways to potentially enhance the effectiveness of these programs. This dissertation will look at 1) the material well-being effects of changes in the amount of SNAP benefits, 2) the welfare implications of the cyclical food spending pattern over the SNAP benefit month, and 3) the effects of aging of out of the WIC program on child's nutrition.

Chapter 2

How Did the American Recovery and Reinvestment Act Impact the Material Well-being of SNAP Participants? A Distributional Approach

2.1 Introduction

In April 2009, as part of the American Recovery and Reinvestment Act (ARRA), the Supplemental Nutrition Assistance Program (SNAP) benefits were temporarily increased at a constant-dollar amount equal to 13.6% of the maximum benefit for each household size (e.g., \$80 for a household of four). This unprecedented increase in benefits was intended to provide SNAP beneficiaries with adequate resources to purchase food. In November 2013, however, ARRA expired and benefits were reduced by 5.4% of the maximum benefit for each household size (e.g., \$36 for a household of four). For the first time in history, nearly all participants' benefits were cut.[1]

[1] Participants in Hawaii did not experience a reduction in SNAP benefits because their regularly calculated maximum allotments for the fiscal year 2014 had exceeded those provided by ARRA (Dean and Rosenbaum, 2013).

Food is clearly an important budgetary consideration for low-income households—an observation dating back at least to Ernst Engel who suggested it as a measure of well-being. Since SNAP benefits account for approximately 50% of at-home food spending of low-income households (Wilde, 2013), the ARRA-induced SNAP benefit changes are expected to have important implications for their overall well-being. The 2013 SNAP benefit cuts, for instance, were expected to have adverse impacts on households' abilities to meet their food needs and cause hardship for them (see, Dean and Rosenbaum, 2013; Bruich, 2014 and citations within). Indeed, the implementation of ARRA improved food security (Nord and Prell, 2011), whereas the subsequent expiration increased food insecurity (Katare and Kim, 2017).

This study takes a more holistic view of welfare and examines the extent to which the implementation and the expiration of ARRA affected the material (or money-metric) well-being of SNAP households. We focus on nondurable consumption as a measure of material well-being because for both theoretical and empirical reasons it provides a more reliable measure than income (see, Meyer and Sullivan, 2004). Specifically, we examine three forms of nondurable consumption: 1) total nondurable spending as a measure of overall material well-being, 2) food spending, and 3) nondurable nonfood spending.

We choose food spending as a more refined measure of well-being in the sense of Engel because it is important for judging the effectiveness of the food policy. Since the income effect of the ARRA SNAP benefit changes is expected to affect other nondurable expenditures, we also examine nondurable nonfood spending. Theoretically, the in-kind nature of SNAP only distorts spending of extramarginal households (i.e., those whose SNAP benefits exceed their desired food-at-home spending). According to Southworth's (1945) theory, SNAP benefits will increase the food spending of extramarginal households by more than an equivalent cash transfer would, whereas inframarginal participants (i.e., those whose at-home food spending are at least as much as their SNAP allotment) are predicted to treat SNAP benefits no differently than an equivalent cash income.[2] This implies that for inframarginal households, who constitute the vast majority of SNAP participants (Hoynes et al., 2014), a change in SNAP benefits can be considered as a pure income effect that will affect both food and nonfood spending.[3]

Previous studies examining the impacts of ARRA have focused on the effects of *either* the 2009 SNAP benefit enhancements (Nord and Prell, 2011; Beatty and Tuttle, 2015; Kim, 2016) *or* the 2013 benefit cuts (Bruich, 2014; Katare and Kim, 2017) on food spending and/or food security. To the best of our knowledge, there is no study that examines and compares the expenditure decisions of households in response to *both* an increase and a decrease in SNAP benefits due to ARRA—an issue relevant to contemporary food assistance policymaking. Specifically, we investigate whether SNAP households responded to benefit changes by modifying both their food and nonfood spending. For instance, it is possible that households reacted to benefit enhancements

[2]Previous literature, however, finds that inframarginal participants exhibit higher marginal propensity to spend (MPS) on food out of SNAP benefits than MPS on food out of non-SNAP income (e.g., Fraker, 1990; Fraker et al., 1995; Levedahl, 1995; Breunig and Dasgupta, 2002 and 2005; Smith et al., 2016). For instance, Fraker (1990) in a review of 17 studies finds that estimates of the MPS out of SNAP range from between two to ten times the MPS out of cash income.

[3]Although empirical evidence indicates that inframarginal participants do not treat SNAP income in the same manner as cash income, the estimated MPS from SNAP is less than one (e.g., Moffitt, 1989; Fraker, 1990; Levedahl, 1995; Breunig and Dasgupta, 2005; Beatty and Tuttle, 2015; Hastings and Shapiro, 2017). For instance, Hastings and Shapiro (2017) estimate that MPS on at-home food out of SNAP benefit is 0.5 to 0.6. Therefore, every \$100 increase in SNAP benefits displaces about \$40 to \$50 in cash income to be allocated to nonfood goods.

by increasing both their food and nonfood expenditures, whereas after the benefit cuts they might have preferred to maintain their food spending level at the expense of their nonfood expenditure. Findings from earlier studies may not be strictly comparable with each other due to differences in data, identification strategies, and outcomes of interest.

More importantly, existing studies have only estimated the *average* treatment effect of ARRA. Although mean impacts provide useful information for many policy decisions, they may not represent the impact of ARRA at other parts of the outcome distribution. Clearly, differences in household characteristics (e.g., food preferences and the propensity to spend SNAP benefits) may result in heterogenous impacts throughout the distribution of outcome not entirely captured by the mean effect. For example, we may expect the benefit changes to have larger impacts on the lower tail of the food spending distribution, which is more likely to be made up of extramarginal households than the remainder of the distribution. Thus, it is important to account for the heterogeneity in spending responses to exogenous changes in SNAP benefits.

We allow for the possibility of heterogeneous outcomes by estimating the impacts of benefit changes at various points in the distribution of our material well-being measures. This makes the quantile regression an attractive candidate. That is, we estimate the quantile treatment effects of changes in SNAP benefits on spending as we move from low expenditures towards high expenditures to find the answer to the policy question at hand—how did ARRA impact the material well-being of households prone to low spending separately from those inclined to high spending? The answer to this question can assist policymakers in identifying the SNAP subpopulations that are the most sensitive to variations in SNAP benefits and tell them more about for whom ARRA implementation/expiration *did or did not* work.

Estimating the well-being effects of ARRA, however, is made difficult by the fact that SNAP households self-select into the program for reasons that are not easily observed (Kreider et al., 2012; Hoynes et al., 2014; Bitler, 2014). These selective processes may make SNAP participants different from non-participants in systematic ways. Thus, a simple comparison of SNAP participants to non-participants may not identify the true well-being impacts of SNAP benefit changes. Drawing on the longitudinal data from the Consumer Expenditure Survey (CEX), we use within-household variation to control for fixed observed and unobserved household characteristics associated with non-random selection into SNAP, as done elsewhere (e.g., Wilde and Nord, 2005; Beatty and Tuttle, 2015). Specifically, to estimate the full distributional impacts of ARRA, we employ a new fixed-effects quantile estimator (termed quantile regression for panel data [QRPD]) following Powell (2016), which allows coefficient estimates to be a function of fixed unobservable household characteristics.

We find that the ARRA implementation on average increased the quarterly total nondurable expenditure of SNAP households by about 6%, primarily driven by an increase in the food expenditure. Conversely, we find that the ARRA expiration reduced total nondurable spending of an average SNAP household by about 3.5%, mainly caused by a reduction in food spending. In terms of the distributional effects, we find that the ARRA implementation led to a first-order improvement (i.e., positive impacts throughout the distribution) in the distribution total nondurable expenditure. Furthermore, our results suggest that the lowest and the highest food spending households were the most responsive to the 2009 SNAP benefit enhancements. With respect to the 2013 benefit cuts, we find that the mean effects do not describe the full distributional effects in that the benefit cuts had their largest adverse effects on the most disadvantaged SNAP subpopulations.

The remainder of this chapter is organized as follows. Section 2.2 provides a brief overview of SNAP, ARRA, and related studies. Section 2.3 describes the data and our measurement of the material well-being. Section 2.4 outlines the empirical methodologies. Section 2.5 presents the results. Section 2.6 provides concluding remarks and derives policy implications.

2.2 Background

2.2.1 SNAP and ARRA

The Supplemental Nutrition Assistance Program (SNAP) is the largest federal food and nutrition assistance program in the United States. SNAP aims to accomplish its dual mandate – to “alleviate hunger” and to “permit low-income households to obtain a more nutritious diet” – by “increasing the food purchasing power for all eligible households” through in-kind transfers (Food, Conservation, and Energy Act of 2008). To become eligible for SNAP, households must meet three criteria: 1) their monthly gross income must be less than 130% of the federal poverty line (FPL), 2) their monthly net income should not exceed 100% of the FPL, and 3) their countable assets should not exceed some certain levels. In addition, nonworking, able-bodied childless adults aged 18 to 49 years (known as able-bodied adults without dependents [ABAWD]) are limited to three months of benefits within a three-year period.

In February 2009, in direct response to the "Great Recession" of 2007-2008, Congress passed the American Recovery and Reinvestment Act (ARRA) to stabilize the U.S. economy. At roughly $800 billion ARRA was the largest fiscal stimulus program in the U.S. history. ARRA injected $224 billion into entitlement programs from which SNAP received an increase in funding of around $20 billion. This rise in funding allowed the program to increase administrative funding, temporarily eliminate time limits for ABAWD individuals, and increase the monthly maximum benefits of SNAP participants. The SNAP benefit increase by ARRA, effective on April 1, 2009, was the largest since the initiation of the program.[4] The amounts of increase in maximum benefit for households of one to four were $24, $44, $63, and $80 per month, respectively.

Figure 2.1 plots the trend of maximum monthly SNAP benefit for a household of four from the fiscal year 2002 to 2015.[5] As can be seen, maximum benefit level remained constant at the new higher amount until November 2013 when ARRA expired and the maximum SNAP benefit was reduced the first time in history. The amounts of the benefit cuts for one- to four-person households were $11, $20, $29, and $36 per month, respectively.[6] Because ARRA SNAP benefit changes were implemented as a constant dollar amount of the maximum benefit for each household size, the percentage

[4]ARRA had other provisions for low-income households such as the expanded earned income tax credit, expansion of child tax credit, and other aids to low-income workers, unemployed and retirees, that could also affect their well-being. These provisions are assumed to affect SNAP participants and low-income non-participants similarly.

[5]The maximum benefit is calculated based on the cost of the USDA's Thrifty Food Plan (TFP). At the beginning of each fiscal year (i.e., in October of each year) SNAP benefits are adjusted to reflect the increase in food prices based on the cost of the TFP in the June of the prior fiscal year.

[6]As can be seen, benefit cuts are smaller than their corresponding nominal increases in 2009, reflecting the decline in the real value of benefits due to food price inflation. For example, a $36 dollar decrease for a household of four implies that inflation had already reduced about $44 of the 2009 benefit increase with the major decline (about half) happening from 2009 to 2011 (see, Nord, 2013).

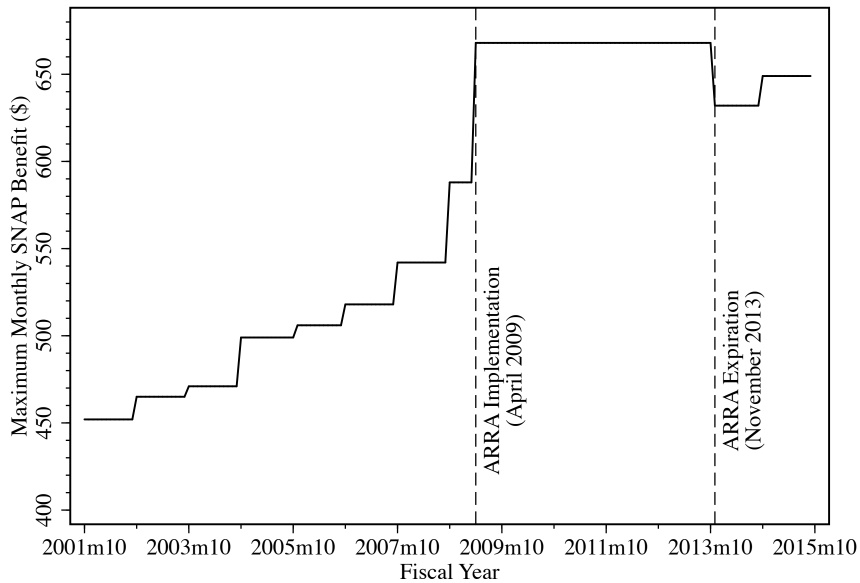

Figure 2.1: Maximum monthly SNAP benefit for a household of four, fiscal years 2002-2015

increase and decrease were greater for households that had some net income and were thus eligible for less than the maximum benefit. As we can see in figure 2.2 on average SNAP households experienced an increase of about 17% ($42) and a decrease of about 6.5% ($18) in their monthly SNAP benefits due to ARRA.

2.2.2 Literature Review

Several studies have taken advantage of the "natural experiment" of ARRA to unveil the effects of SNAP benefit changes on different households' outcomes. Using data from the Current Population Survey Food Security Supplement (CPS-FSS), Nord and Prell (2011) employ a difference-in-differences (DID) approach to estimate the impacts

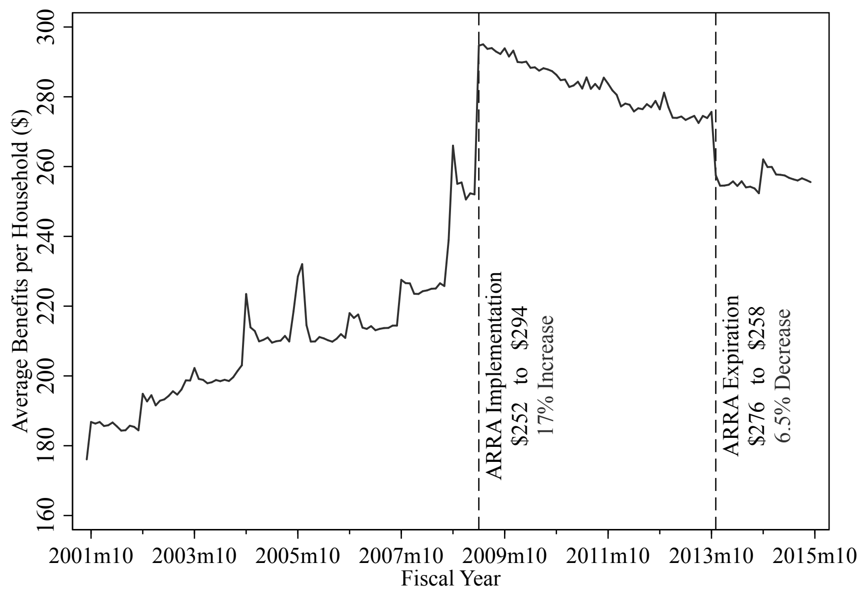

Figure 2.2: Average monthly SNAP benefit, fiscal years 2002-2015

of ARRA implementation on food expenditure and food security of low-income households. They make use of gross income limit to define their treatment group as those with gross income less than 130% of the FPL and control group as those with gross income between 150% and 250% of the FPL. They find that the ARRA implementation increased food expenditure by 5.4% and reduced food insecurity by 2.2 percentage points from 2008 to 2009.

Beatty and Tuttle (2015) use several changes in SNAP benefits over 2007 to 2010, including the large increase due to ARRA, to re-examine the Southworth's (1945) theory. Drawing on the panel data from the CEX, they use within-household variation to control for fixed household characteristics associated with self-selection into SNAP. They find that SNAP benefit enhancements led to a 0.72% increase in food-at-home's share of total expenditure, implying a marginal propensity to spend (MPS) out of the increase in SNAP of 0.48. Similarly, Kim (2016) uses data from the 2007 to 2011 CEX and employs a DID approach to investigate the impact of ARRA implementation on food and several nonfood expenditure categories. Findings from this study show that ARRA increased not only food spending but also some nonfood spending categories such as housing, transportation, and education.

Two studies have examined the impacts of the 2013 SNAP benefit cuts. Using scanner data from 400 grocery stores and the purchases of over 2.5 million households enrolled in SNAP, Bruich (2014) finds that ARRA expiration on average reduced households' monthly grocery spending by $5.91. Katare and Kim (2017), employ data from the CPS-FSS and examine the effect of the benefit cuts on food security. They find that benefit cuts increased the prevalence of food insecurity. To the extent of our knowledge, there is no study that uses survey data to examine the impact of ARRA expiration on the expenditures of SNAP households.

While this study is closest in spirit to Beatty and Tuttle (2015) and Kim (2016), several points distinguish our analysis from these studies. First, as mentioned earlier, both Beatty and Tuttle (2015) and Kim (2016) only examine the ARRA implementation and estimate its average treatment effect on spending. Our study, however, examines both the ARRA implementation and expiration and goes beyond the estimation of average treatment effects by investigating the full distributional effects of ARRA. Second, Beatty and Tuttle (2015) restrict their sample to inframarginal households, whereas we investigate the entire SNAP population (i.e., inframarginal and extramarginal households). Third, Beatty and Tuttle (2015) investigate at-home food spending of SNAP households which is considered only as a rough proxy of household's total consumption (see, Attanasio and Weber, 1995; Lusardi, 1996). To more accurately measure the well-being effects of ARRA, we examine total nondurable consumption of households as well as its food and nonfood components.

Although Kim (2016) also examines both food and nonfood expenditure categories, the identification strategy employed in her study does not lead to causal inferences of the expenditure effects of ARRA implementation. In the CEX SNAP participation is reported only for the second and fifth interviews and is imputed for the third and fourth interviews using data from the second interview. Kim (2016) argues that to avoid "SNAP imputation problem," she uses only the first quarterly interview data for each household. Thus, she does not utilize the longitudinal dimension of the CEX to deal with non-random selection into SNAP. Instead, she restricts the sample to SNAP participants and low-income non-participants and uses a multivariate regression to control for observable differences between SNAP participants and non-participants. However, selection into SNAP may also occur due to unobservable household characteristics. Therefore, Kim (2016) analysis may suffer from omitted variable bias.

To avoid any likely issues due to SNAP imputation, we include in our sample only households that were participating in SNAP on both the second and fifth interviews and those that were never participating in SNAP. It is unrealistic for a household's SNAP participation status to change twice within a six-month period (i.e., from the second to fifth interview) as is assumed in Kim (2016).[7] Put differently, it is less likely that a household reporting to be SNAP participant on the second and fifth interviews to be

[7]Although after initial SNAP eligibility households must be re-certified, for most households the recertification period is about twelve months.

a non-participant on the third or fourth interview.[8] Therefore, similar to Beatty and Tuttle (2015), we take advantage of the longitudinal dimension of the CEX and use within-household variation to control for fixed observable and unobservable household characteristics associated with self-selection into SNAP.

2.3 Data

We draw our sample from the Consumer Expenditure Survey (CEX) which is a nationally representative survey administered by the Bureau of Labor Statistics (BLS). The CEX consists of two separate components, an interview survey and a diary survey. Our analysis utilizes data from the interview survey which is a rotating panel survey administered quarterly. Each interview quarter includes approximately 7,000 households and with the rotating panel design of the survey, 20% of the respondents are replaced each quarter. Within each interview quarter, interviews are conducted monthly and about one-third of the sample is surveyed every month. In each month households provide information about their expenditures for the past three months. Therefore, there is a distinction between calendar and interview quarters.

The CEX follows participating households up to five consecutive quarters and reports the quarterly expenditure measures at the household level from the second to fifth interviews. One potential issue with the CEX, however, is that it does not follow households who relocate. This is particularly problematic when we use the longitudinal property of the CEX to observe the same household under two different benefit regimes (i.e.,

[8] However, to test the plausibility of this assumption, we excluded the third and fourth interviews from the sample and estimated our regression models again. Results were robust to the exclusion of these observations.

before and after ARRA benefit changes) and to control for their observable and unobservable household characteristics. To address this issue, we exclude households whose demographic characteristics are inconsistent over different interviews, as done in Beatty and Tuttle (2015). We drop households from the sample if the age of the household head changes by more than one year or a negative amount or if the household size changes by more than three in absolute magnitude from one quarterly interview to another.

2.3.1 Measurement of Well-Being

To measure the material well-being of SNAP households, we use consumption-based methods rather than the income-based approaches. According to the Permanent Income Hypothesis (PIH), income is comprised of permanent and transitory components and consumption is based on the permanent component. Therefore, consumption is less susceptible to positive and negative income shocks, which do not necessarily reflect changes in well-being, as households can smooth consumption and maintain their welfare status through saving and dissaving. Moreover, income is substantially underreported in national surveys and this problem is aggravated at the bottom of the income distribution due to the prevalence of transfers and off-the-books income (Meyer and Sullivan, 2004). In addition, income data do not capture in-kind transfers, whereas expenditure data reflect them. For these reasons, it is preferable to focus on consumption measures to assess the well-being. This requires constructing a measure of consumption using household expenditure data because in practice actual consumption cannot be estimated.

The CEX collects expenditure data on durables such as housing and vehicles and nondurables such as food and utilities. Utilizing spending on nondurables, we construct our consumption measures. Following Lusardi (1996), we create a nondurable expenditure measure by summing the quarterly spending on food at home and away from home, alcoholic beverages, tobacco, utilities, personal care, household operations, public transportation, gas and motor oil, apparel and services, health care, education, and miscellaneous expenses. This nondurable expenditure measure is generally referred to as "nondurable consumption" (see, Lusardi, 1996). We follow previous work on well-being by excluding health care and education expenses from our nondurable consumption measure as they can be inferred as an investment (e.g., Attanasio and Weber, 1995; Meyer and Sullivan, 2004).[9] Hereafter, we refer to this measure as total nondurable expenditure.

Moreover, we construct our food consumption measure by summing spending on food at home and food away from home.[10] By subtracting food spending from total nondurable expenditure, we construct our nondurable nonfood spending measure. Finally, we construct a measure of total expenditure by summing spending on durables and nondurables which will be used in the empirical section as a representation of household's total resources in our Engel curve specification. These expenditure measures are then expressed in real (2009) dollars using their corresponding Consumer Price Indices (CPIs).[11] By deflating the expenditure measures, we adjust for the annual cost of living adjustments in SNAP benefits. Thus, we examine the effects of real changes in SNAP

[9] See table A.1 in the appendix for more details on each expenditure group.

[10] Lusardi's (1996) definition of food consumption includes alcoholic beverages. However, since we are examining the well-being effects of changes in SNAP benefit we do not include alcoholic beverages into our food consumption measure.

[11] Using monthly CPI data, we calculated the quarterly CPI corresponding to the CEX interview quarters.

benefits that are due to the implementation and the expiration of the ARRA, rather than the impacts of several changes in SNAP benefits on expenditures.

2.3.2 Summary Measures

The analysis samples are drawn from the 2007 to 2015 CEX. To investigate the impacts of ARRA implementation, we consider the period from April 2007 to June 2011. To examine the effects of benefit cuts, we choose the period from December 2011 to December 2015.[12,13] Although the ARRA-induced SNAP benefit changes were exogenous to SNAP households, one challenge to identification is separating the effects of these exogenous changes in benefits from all other confounding factors such as seasonality and macroeconomic conditions. To address this issue, we use a difference-in-differences (DID) research design which compares changes in the expenditures of SNAP households due to ARRA implementation/expiration with changes in the spending of non-SNAP households.

Since SNAP participants self-select into the program, a comparison between SNAP participants and the *full* population of nonparticipants would be misleading. To make the treatment and control groups more comparable, we limit our analysis to SNAP participants and low-income nonparticipants households. We define SNAP participants as households who reported receiving any positive amount of SNAP benefits during the previous 12 months. Low-income nonparticipants are defined as those who did not re-

[12]Our results are robust to alternative study periods.

[13]For each study period, due to the implausibly small and implausibly large expenditures, we drop the bottom and top percentiles of the real total nondurable expenditure distribution.

port receiving any SNAP benefits and had annual income[14] less than 185% of the FPL.[15]

A few points are worth noting here. First, as is also mentioned in Beatty and Tuttle (2015) and Kim (2016), in the CEX similar to other nationally representative survey data SNAP participation is underreported. Thus, our control group erroneously contains SNAP participants. As a result, the estimates from this study will underestimate the true effects of the ARRA. Second, households that were eligible for SNAP but did not enroll in the program before the ARRA implementation may have been induced by the large increase in benefits to participate. Similarly, due to temporary elimination of time limits for ABAWD individuals, some households may have participated in SNAP after ARRA implementation that were not participants before it. However, since our sample includes only households that were participating in SNAP before and after the ARRA and households that were never participating in SNAP, our estimates will not be confounded by the likely changes in the participant population.

Table 2.1 shows the summary statistics for household demographic characteristics. As can be seen, demographics are different between SNAP participants and income-eligible nonparticipants within each study period. Similarly, in each period, SNAP participants are less likely to be: married, headed by a male, employed, of a smaller household size, and white. Since our identification strategy relies on the changes in maximum benefit levels by the ARRA that are exogenous to individual households, demographic differences between SNAP and non-SNAP households would have been less problematic if program participation was also exogenous. Households, however, select

[14]Income is defined as financial income before tax minus the value of SNAP benefits.

[15]Although the federal gross income cutoff for SNAP eligibility is 130% of the FPL, due to the categorical eligibility policy adopted by many states, households with higher gross income may become eligible for SNAP.

into the program due to observable and unobservable factors which could make them different from nonparticipants in systematic ways. If these factors do not change over the study period, using panel data and conditioning on household fixed effects will help identification. Put differently, by conditioning on household fixed effects and assuming conditional exogeneity, (time-invariant) unobservable and observable household characteristics associated with program participation are no longer confounding.

Table 2.1: Household Characteristics

	ARRA Implementation		ARRA Expiration	
	Participants	Nonparticipants	Participants	Nonparticipants
Married	0.26	0.44	0.27	0.43
	(0.44)	(0.50)	(0.44)	(0.50)
Female	0.74	0.56	0.68	0.55
	(0.44)	(0.50)	(0.47)	(0.50)
Employed	0.44	0.58	0.44	0.57
	(0.50)	(0.49)	(0.50)	(0.50)
Household Size	3.02	2.32	2.93	2.31
	(1.83)	(1.51)	(1.82)	(1.52)
White	0.68	0.82	0.67	0.80
	(0.47)	(0.38)	(0.47)	(0.40)
Black	0.28	0.14	0.27	0.14
	(0.45)	(0.35)	(0.44)	(0.35)
Other Races	0.04	0.04	0.06	0.06
	(0.20)	(0.19)	(0.24)	(0.24)
Age	43.69	51.21	46.20	52.27
	(15.61)	(19.46)	(16.28)	(19.35)
Households	2,553	16,795	3,755	16,348

Notes: All calculations use survey weights. Standard deviations are in parentheses. All differences between SNAP participants and SNAP-eligible non-participants (except for Other Races) are statistically significant at 1% significance level.

Table 2.2 presents summary statistics for quarterly household expenditures. Like-

Table 2.2: Quarterly Expenditures

	Mean	SE	p5	p25	p50	p75	p95
			Panel A: ARRA Implementation				
Total Expenditure							
Participants	3539.94	(61.20)	725.57	1562.50	2654.08	4561.35	9198.45
Nonparticipants	5221.15	(34.64)	1001.72	2248.50	3952.82	6788.35	13946.89
Nondurable Expenditure							
Participants	1707.71	(27.66)	349.65	773.43	1324.68	2325.49	4264.23
Nonparticipants	2081.60	(12.86)	425.98	956.90	1656.72	2780.91	5234.12
Food Expenditure							
Participants	815.10	(13.34)	130.55	349.13	628.05	1112.82	2090.17
Nonparticipants	980.47	(6.42)	180.25	431.68	758.71	1303.31	2504.08
Nondurable Nonfood Expenditure							
Participants	886.35	(16.61)	115.52	332.13	655.80	1192.15	2456.11
Nonparticipants	1091.38	(7.47)	158.49	445.01	828.23	1455.70	2918.25
			Panel B: ARRA Expiration				
Total Expenditure							
Participants	3786.74	(55.71)	782.75	1676.36	2858.50	4884.31	9663.29
Nonparticipants	5457.82	(44.91)	905.26	2139.76	3777.86	6664.68	15079.57
Nondurable Expenditure							
Participants	1715.51	(23.12)	356.23	788.58	1326.44	2278.45	4364.00
Nonparticipants	2026.90	(14.42)	367.15	879.52	1547.95	2633.40	5132.48
Food Expenditure							
Participants	815.93	(11.31)	150.27	362.52	617.31	1071.56	2143.04
Nonparticipants	941.22	(6.30)	154.35	398.15	720.88	1243.76	2414.67
Nondurable Nonfood Expenditure							
Participants	894.18	(13.74)	118.96	346.45	663.80	1209.67	2441.86
Nonparticipants	1075.18	(9.29)	127.90	406.64	768.50	1378.82	2903.29

Notes: All calculations use survey weights. Standard errors (SE) for mean expenditures are clustered at the household level. Columns labeled p5-p95 refer to percentiles. All expenditure figures are expressed in 2009 dollars using their corresponding Consumer Price Indices (CPI). All differences between SNAP participants and low-income non-participants are statistically significant at 1% significance level.

wise, in both periods (panels A and B) we observe that mean spending are different between SNAP participants and nonparticipants with the latter having higher expenditures than the former. Further, the results of a two-sample Kolmogorov-Smirnov test indicate that in both periods distributions of spending of nonparticipants stochastically dominate the corresponding spending distributions of SNAP participants at the first order.[16] Since our empirical approach compares the changes in the spending of SNAP households due to ARRA, with changes in the expenditures of non-SNAP households, these differences are not problematic.

Table 2.3 provides a simple DID analysis using the sample means. In panel A, we see that the change in real total nondurable spending of SNAP participants due to ARRA implementation is very small, whereas spending of nonparticipants drops significantly, resulting in a DID estimate of about \$114 increase in total nondurable expenditure of SNAP households. DID estimates for food and nonfood expenditures suggest that, as is predicted by economic theory, the April 2009 increase in SNAP benefits raised spending on both food and nonfood items. In panel B, we observe a decrease in total nondurable spending of SNAP participants and a slight increase in the spending of nonparticipants, leading to a DID estimate of about a \$51 decrease in total nondurable spending of SNAP households. Similarly, we see that households responded to benefit cuts by reducing both their food and nonfood expenditures. Additionally, these descriptive results suggest that ARRA benefit changes had a larger impact on food spending in both periods.

Figure 2.3 compares differences between the distributions of total nondurable spend-

[16] For two distributions A and B, characterized by cumulative distribution functions (CDFs) F_A and F_B, distribution B stochastically dominates distribution A at first order if $F_A(y) \geq F_B(y)$ for all y, with strict inequality at some y (see, Davidson and Duclos, 2000).

Table 2.3: Quarterly Expenditures by SNAP Participation Before and After the ARRA

	Before		After		
Expenditures:	Participants	Nonparticipants	Participants	Nonparticipants	DID
		Panel A: ARRA Implementation			
Total Nondurable	1709.35	2138.60	1706.61	2022.18	113.67
	(43.20)	(18.15)	(34.84)	(17.65)	(59.83)
Food	824.48	1020.17	808.79	939.07	65.41
	(21.51)	(9.19)	(16.51)	(8.69)	(29.46)
Nonfood	880.74	1110.40	890.12	1071.55	48.23
	(25.15)	(10.48)	(21.39)	(10.31)	(35.46)
		Panel B: ARRA Expiration			
Total Nondurable	1751.64	1949.67	1690.38	1939.83	-51.41
	(37.99)	(16.73)	(27.14)	(16.34)	(50.58)
Food	845.82	926.98	795.14	908.10	-31.80
	(18.77)	(8.28)	(13.28)	(7.96)	(25.05)
Nonfood	899.18	1012.72	890.62	1025.65	-21.39
	(22.26)	(9.74)	(16.26)	(9.82)	(29.80)

Notes: All calculations use survey weights. Standard errors in parentheses are clustered at the household level. All expenditure figures are expressed in 2009 dollars using their corresponding Consumer Price Indices (CPI).

ing of SNAP participants and non-participants before and after ARRA benefit changes. Panel A presents the empirical CDFs of spending for SNAP participants and non-participants (denoted by F_P and F_{NP}, respectively) and the difference between them before the ARRA implementation. Panel B likewise for after the ARRA implementation. In comparing the differences in subpanels A and B, we see a smaller gap between the distributions of total nondurable spending for participants and non-participants following the ARRA implementation.[17] This is formally shown in panel C by taking the difference in the differences (i.e., subpanels A and B). Similarly, panel D shows that non-participants spend more on nondurables than SNAP households at all points in the distribution prior to ARRA expiration. In panel E we see that the spending gap becomes

[17]The area under the difference curve in each subpanel equals the area between the distributions.

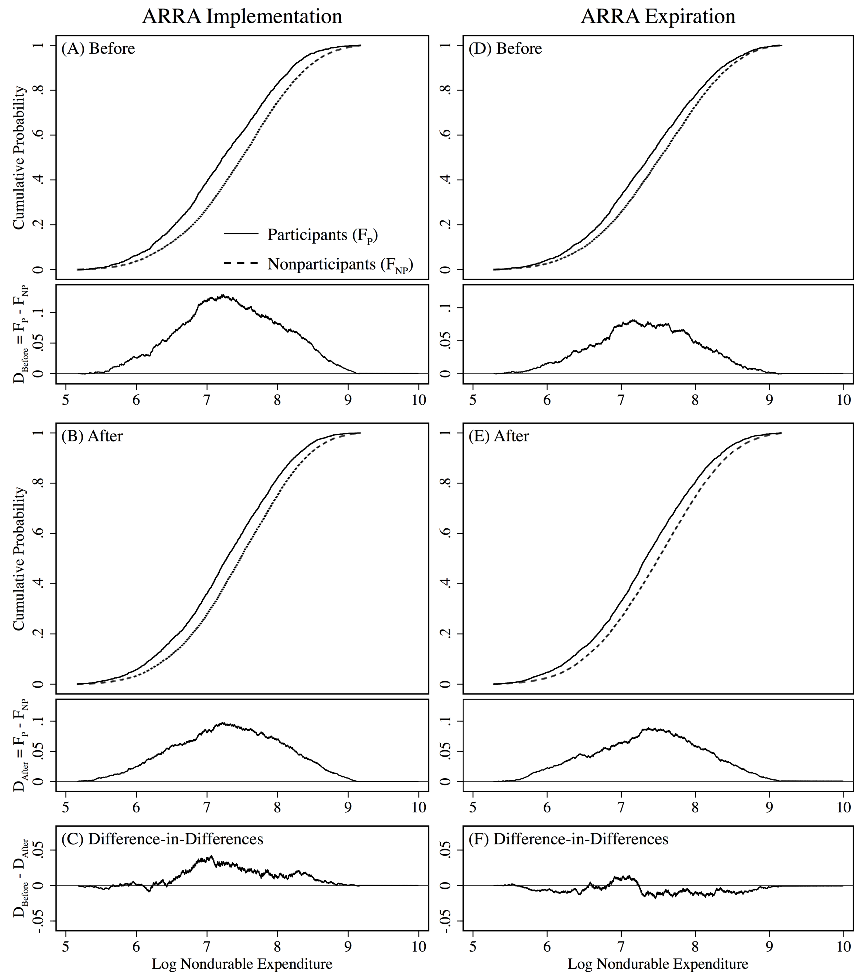

Figure 2.3: Unconditional cumulative distribution function (CDF) of the total nondurable expenditure by SNAP participation before and after the ARRA

even larger after the 2013 benefit cuts. This can be seen more easily using the simple DID in panel F, revealing that the ARRA expiration reduced total nondurable spending almost at all points in the distribution. Moreover, as can be seen in both panels C and F, there is good evidence that the impacts of the ARRA benefit changes are not uniform. For instance, we see larger effects towards the top of the distribution. Similar patterns are observed for food and nondurable nonfood expenditures.[18]

None of the aforementioned descriptive findings, however, control for factors known to impact spending such as total household resources, household size, and seasonality. Further, the presence of unobservable characteristics further casts doubt on drawing causal inferences from table 2.3 and figure 2.3. In the following section, we employ regression methods to better isolate the impacts of the ARRA on the material well-being of SNAP households.

2.4 Empirical Methods

2.4.1 Average Impacts

We first discuss the regression model for estimating the average treatment effects (ATE). Let $SNAP_i = 1$ if household i is a SNAP participant. We divide each study period into pre- and post-ARRA implementation/expiration periods. We consider the period from April 2007 to April 2009 as the pre-implementation period and the period from May 2009 to June 2011 as the post-implementation.[19] Likewise, we consider the period from December 2011 to November 2013 as the pre-expiration period and the period from December 2013 to December 2015 as the post-expiration.[20] Accordingly, we define a dummy variable, $post_t$, which takes on the value of zero in the pre-ARRA implementa-

[18] Figures for food and nondurable nonfood spending are available upon request from the authors.

[19] Households who were interviewed in April 2009 reported expenditures for January, February, and March 2009. Therefore, April 2009 belongs to the pre-ARRA implementation period.

[20] Households who were interviewed in November 2013 reported expenditures for August, September, and October 2013. Thus, November 2013 belongs to the pre-ARRA expiration period.

tion/expiration period and one in the post-policy periods.[21] Then, the OLS fixed-effects (OLS-FE) model for estimating the ATEs of the ARRA implementation and expiration is:

$$\begin{aligned} log(Y_{it}) = \beta_1 SNAP_i + \beta_2 post_t + \beta_3 SNAP_i \times post_t + \beta_4 log(X_{it}) \\ + \alpha_i + \lambda_t + \gamma_m + \eta_y + \epsilon_{it}, \end{aligned} \tag{2.1}$$

where Y_{it} is either total nondurable, food, or nondurable nonfood spending for household i in interview quarter $t = \{2, 3, 4, 5\}$.[22] X_{it} is total expenditure to control for household's total resources in the sense of an Engel curve specification. Including the total expenditure in the regression model could also control for household "need," which is considered as a common source of self-selection bias (see, Fox et al., 2004). α_i is the household fixed effect, λ_t is the interview quarter fixed effect, and γ_m and η_y are calendar month and year fixed effects. Finally, ϵ_{it} is assumed to be an idiosyncratic error. The coefficient of interest is β_3 on the interaction term, which can be directly interpreted as the ATE of the ARRA implementation/expiration on household expenditures.

2.4.2 Distributional Impacts

The mean regression in equation 2.1 provides the average change in household quarterly spending in response to SNAP benefit changes. We aim to provide a more comprehensive picture of the extent of the ARRA's impacts by looking at different points of the

[21]Households interviewed in May 2009 reported expenditures for February, March, and April. Since only April's expenditures reflect the new level of SNAP benefits, $post_t$ takes on the value of 0.33 in May 2009 and with a similar argument, it takes on the value of 0.66 in June 2009. Similarly, $post_t$ takes on values of 0.33 and 0.66 in December 2013 and January 2014, respectively.

[22]Quarterly expenditures are only available from the second interview onward.

distribution of our outcome variables. Quantile regression (QR) is an appropriate candidate for building such a picture.

A unique feature of QR is that coefficients vary according to a nonseparable error term, $U_{it} = f(\alpha_i, \epsilon_{it})$, which is also called the rank variable and defines the conditional quantiles over which estimation occurs (see, Chernozhukov and Hansen, 2013 for details). For example, consider a linear-in-parameter quantile specification corresponding to equation 2.1:

$$\begin{aligned} log(Y_{it}) = \beta_1(U_{it})SNAP_i + \beta_2(U_{it})post_t + \beta_3(U_{it})SNAP_i \times post_t \\ + \beta_4(U_{it})log(X_{it}) + \delta_{htm}(U_{it}). \end{aligned} \tag{2.2}$$

The general idea within the present context is that high quantiles (i.e., a high value of U_{it}) are defined by a relatively high preference for the outcome (e.g., food spending). Part of this preference is fixed (i.e., α_i), while the other is idiosyncratic (i.e., ϵ_{it}). No functional form is placed on this relationship. Therefore, the model tells us how the ARRA impacted well-being at different points in the distribution, as defined by U_{it}. These impacts (i.e., the quantile treatment effects [QTEs]) are again captured by β_3. Also note that in this equation δ_{htm} is a full interaction term based on household size $h = \{1, 2, 3, 4, 5+\}$, interview quarter t, and calendar month m.[23] This interaction term allows the distribution of spending to shift based on time and household size. Without this adjustment, higher quantiles of expenditures would primarily refer to larger households as household size is directly linked to the level of expenditure.

As with the mean regression, equation 2.2 yields endogenous results when attributes

[23]Results are robust to the inclusion of year fixed effects.

in α_i are correlated with both right-hand and left-hand side variables. One approach is to linearize the functional form of U_{it} and directly condition on α_i in an additive manner (e.g., Koenker, 2004; Canay, 2011). The main shortcoming of this additive approach, however, is that it alters the interpretation of the coefficients of interest because rank is now defined by the idiosyncratic part ϵ_{it} (see, Powell, 2016 for details). Intuitively, the logic falters here because to be at the top of the idiosyncratic distribution has no meaningful interpretation in the present study. We therefore choose to maintain the ranking structure based on $U_{it} = f(\alpha_i, \epsilon_{it})$, which will populate and rank the conditional distribution according to fixed preferences for the outcome, and use a demeaning-type approach (i.e., a within transformation) for identification.[24]

The specific estimation approach taken in this study is to utilize the quantile regression estimator for panel data (QRPD) with nonadditive fixed effects proposed by Powell (2016).[25] For identification purposes, this estimator *conditions* on household fixed effects but does not directly estimate parameter values for each α_i, similar to a demeaning approach in OLS. Consequently, the resulting estimates are directly comparable to the standard QR estimator because coefficient estimates in QRPD and QR vary by U_{it}. Powell (2016) provides estimation details. In short, we follow Chernozhukov and Hong (2003) and use a Markov chain Monte Carlo (MCMC) algorithm to derive

[24]We remind the reader that several OLS specifications lead to the same fixed-effects coefficient estimate β_{FE}: a differencing approach, a time demeaning (i.e., the within transformation) approach, or directly include N dummies for each household (i.e., the dummy variable regression). One should not extend the logic of OLS to quantile regression. Indeed, Wooldridge (2010, p. 309) notes, "Generally, we should view the fact that the dummy variable regression produces β_{FE} as the coefficient vector ... as a coincidence."

[25]Powell's (2016) method has been used to investigate an exporter premium (Powell and Wagner, 2014), the effects of the economic stimulus payments of 2008 on household labor earning (Powell, 2015), the effect of maternal depression on children's cognitive development (Yu and Wilcox-Gök, 2015), and the impact of school food programs on the distribution of child dietary quality (Smith, 2017).

QRPD estimates.[26] Inferences are then drawn from the posterior distribution.

2.5 Results

2.5.1 Average Treatment Effects (ATEs)

Table 2.4 presents the estimation results from equation 2.1. Each column presents results for a different expenditure category. In both panels A and B, the coefficients on the interaction terms show the ATEs of ARRA benefit changes on households' quarterly spending categories. All coefficient estimates are multiplied by 100 such that they can be interpreted as the percentage change in the expenditures.

As can be seen in panel A, column (1), coefficient estimates indicate that the ARRA implementation, on average, increased the total nondurable spending of SNAP households by about 6%. Moving to column (2), we see that this increase is mainly driven by an around 11% increase in food expenditure. To better understand the magnitudes of these impacts, we use the conditional means of total nondurable expenditures and food spending which are estimated to be \$1625 and \$747, respectively. Therefore, our results indicate that the ARRA implementation, on average, increased nondurable and food spending by about \$95 and \$84.6, respectively. This finding suggests that ARRA implementation increased nondurable nonfood spending of SNAP participants by about \$10. However, the results in column (3) do not imply a significant impact on nondurable nonfood expenditure. One plausible explanation is that due to underreporting of SNAP

[26]We use an adaptive MCMC algorithm (see, Baker, 2014; Powell et al., 2014) applied to equation 2.2 in conjunction with a two-step procedure suggested by Yin (2009). The first step uses a Metropolis-within-Gibbs (MWG) sampling with 400 draws. Coefficient estimates from this step are then used as the initial values of the second step which uses a global sampling approach with 3000 draws.

Table 2.4: Average Treatment Effects of the ARRA on Expenditures

Dependent variable:	OLS-FE			IV-FE		
	Log Total Nondurable	Log Food	Log Nonfood	Log Total Nondurable	Log Food	Log Nonfood
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ARRA Implementation						
$post_t$	-0.13	-1.71	1.95	-0.09	-1.53	1.90
	(1.15)	(1.69)	(1.58)	(1.16)	(1.74)	(1.58)
$SNAP_i \times post_t$	5.86^{**}	11.33^{***}	-0.07	5.78^{**}	10.84^{**}	0.08
	(2.57)	(4.18)	(3.29)	(2.67)	(4.34)	(3.27)
$log(X_{it})$	45.02^{***}	39.97^{***}	46.40^{***}	33.18^{***}	13.05	60.94^{***}
	(1.03)	(1.21)	(1.28)	(11.38)	(19.01)	(16.31)
Panel B: ARRA Expiration						
$post_t$	7.35^{**}	8.01	6.33	7.36^{**}	7.90	6.29
	(3.26)	(5.00)	(4.46)	(3.26)	(5.25)	(4.81)
$SNAP_i \times post_t$	-3.50^{*}	-3.72	-2.35	-3.30	-2.47	-3.51
	(2.00)	(2.93)	(2.70)	(2.03)	(3.16)	(2.81)
$log(X_{it})$	44.89^{***}	38.15^{***}	46.23^{***}	38.98^{***}	1.18	81.46^{***}
	(1.03)	(1.17)	(1.23)	(14.57)	(21.76)	(19.68)
N (Panel A):						
Observations	42,644	42,402	42,543	42,644	42,402	42,543
Households	19,348	19,286	19,309	19,348	19,286	19,309
N (Panel B):						
Observations	42,153	41,935	42,032	42,153	41,935	42,032
Households	20,103	20,036	20,043	20,103	20,036	20,043

Notes: $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions use survey weights. Standard errors in parentheses are clustered at the household level. Coefficient estimates are multiplied by 100 to represent the percentage change.

participation our estimations are underestimated, and thus such a small treatment effect may not be detected. Additionally, columns (4) to (6) present the estimation results from instrumental variable fixed-effects (IV-FE) model to see if/how the endogeneity of total expenditure[27] in equation 2.1 impacts the ATE estimates. Using household income as the instrument for total expenditure, we find similar results which suggest that endogeneity of total expenditure does not have serious implications for our coefficients of interest estimates.

Turning to panel B, column (1), results indicate that the ARRA expiration decreased the total nondurable spending of an average SNAP household by about 3.5%. Using the estimated conditional mean of total nondurable expenditure of $1725, this effect translates into a $60 decrease. Given that the magnitudes of the 2013 benefit cuts were smaller than the 2009 increase in benefits, the estimated ATE of $60 is comparable to the ATE of $95. Further, in columns (2) and (3), we observe that the impacts of ARRA on food and nonfood spendings are not precisely estimated. Directions of the impacts, however, suggest that SNAP households, as is predicted by economic theory, may have responded to the benefit cuts by reducing both their food and nonfood expenditures. Similar results are obtained from IV-FE model. In interpreting these findings, one needs to note that our results are underestimates of the true effects of the ARRA SNAP benefit changes.

To check the robustness of our results with respect to the choice of functional form and to make them more comparable with other studies (e.g., Beatty and Tuttle, 2015), we re-estimate equation 2.1 using expenditures' shares of total expenditure as the outcome variables (i.e., a Working-Leser Engel curve specification). Results are reported in table 2.5. As can be seen, comparable results are obtained from both OLS-FE and IV-FE models. Estimates in panel A show that ARRA implementation increased both

[27]Expenditure measures appear on both the left-hand-side as the outcome variables and the right-hand-side of equation 2.1 as a part of the total expenditure.

total nondurable and food spending share of total expenditures with no significant effect on nonfood's share. In panel B, we see a decrease in expenditures' shares of total expenditures. These results, however, are not precisely estimated.

Table 2.5: Average Treatment Effects of the ARRA on Expenditures' Shares of Total Expenditure

	OLS-FE			IV-FE		
Dependent variable:	Nondurable's Share	Food's Share	Nonfood's Share	Nondurable's Share	Food's Share	Nonfood's Share
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ARRA Implementation						
$post_t$	-0.39	-0.55^{*}	0.28	-0.35	-0.49	0.29
	(0.46)	(0.32)	(0.37)	(0.50)	(0.35)	(0.37)
$SNAP_i \times post_t$	3.30^{***}	2.77^{***}	-0.10	3.22^{***}	2.61^{***}	-0.14
	(1.08)	(0.87)	(0.83)	(1.19)	(0.90)	(0.84)
$log(X_{it})$	-17.79^{***}	-8.86^{***}	-9.39^{***}	-30.54^{***}	-17.24^{***}	-13.06^{***}
	(0.35)	(0.22)	(0.27)	(5.13)	(3.93)	(4.02)
Panel B: ARRA Expiration						
$post_t$	2.51^{*}	0.68	1.73	2.55^{*}	0.64	1.73
	(1.36)	(1.01)	(1.09)	(1.48)	(1.20)	(1.09)
$SNAP_i \times post_t$	-0.82	-0.16	-0.52	-0.31	0.35	-0.46
	(0.87)	(0.62)	(0.70)	(0.96)	(0.71)	(0.71)
$log(X_{it})$	-17.60^{***}	-9.10^{***}	-9.08^{***}	-32.27^{***}	-24.09^{***}	-10.89^{**}
	(0.34)	(0.22)	(0.26)	(6.99)	(4.67)	(4.87)
N (Panel A):						
Observations	42,644	42,402	42,543	42,644	42,402	42,543
Households	19,348	19,286	19,309	19,348	19,286	19,309
N (Panel B):						
Observations	42,153	41,935	42,032	42,153	41,935	42,032
Households	20,103	20,036	20,043	20,103	20,036	20,043

Notes: $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions use survey weights. Standard errors in parentheses are clustered at the household level.

Overall, findings from the mean regression models indicate that ARRA-induced SNAP benefit changes imposed comparable effects on the overall material well-being of an average SNAP participant, as measured by the total nondurable spending. Analysis of the food and nonfood components of total nondurable spending suggests that households reacted to benefit changes by primarily modifying their food expenditure. With respect to nonfood spending, results from our quantile regression model could be more informative. Although it is possible that the effects of benefit changes on nonfood spending were simply too small to have a significant effect on nonfood household expenditures, an alternative explanation might be that mean regression model masks the impacts of the benefit changes at different points of the distribution of nonfood expenditure. For instance, ATEs could average together positive and negative expenditure responses, and thus obscure the extent of the ARRA's effects.

2.5.2 Quantile Treatment Effects (QTEs)

The QTE estimates of the ARRA on the log of quarterly expenditure measures are presented in figure 2.4. The left column plots the QTEs of ARRA implementation (panels A to C). Likewise, the right column for the ARRA expiration (panels D to F). In each panel, the solid line represents the QRPD point estimates, the horizontal dashed line represents the OLS-FE estimates and the shaded area represent 90% confidence intervals (CI) which is calculated pointwise from the posterior of MCMC draws. In all panels coefficient estimates are reported for quantiles 1 to 95 in 1-unit increments. The quantiles on the x-axis refer to the counterfactual (or untreated) expenditure distribution, which gives the QTE estimates a *ceteris paribus* interpretation. Lastly, all coefficients estimates are multiplied by 100, giving them a percentage change interpretation.

Results in panel A show that ARRA implementation had similar positive impacts

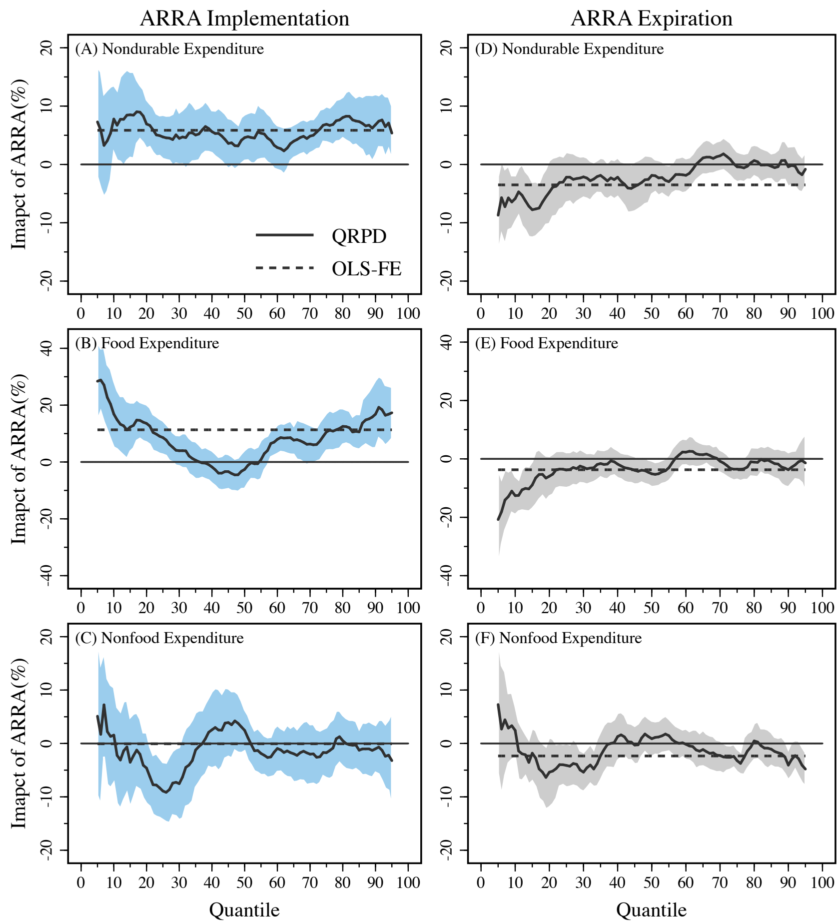

Figure 2.4: Impacts of the ARRA on the distributions of quarterly expenditures

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 5 to 95 in 1-unit increments. All calculations use survey weights. Coefficient estimates are multiplied by 100 to represent the percentage change.

at all points in the distribution of total nondurable expenditure (i.e., a first-order improvement) of SNAP participants. Considering that ARRA increased the maximum SNAP benefits at a constant-dollar amount for all households and given that our quantile regression model controls for total expenditure and household size, this pattern is expected. Moving down the left column to panel B, we observe that increase in benefits had larger effects on the lower food spending quantiles. These larger impacts on the bottom tail of the food spending distribution conform to theory as this part of the distribution has a relatively higher probability of containing extramarginal households. While we do not observe significant impacts at middle quantiles, throughout the remainder of the distribution we see significant effects similar to to the average impact. Overall, results in panel B could be interpreted as a first-order improvement in a sense that ARRA increased food expenditure at most parts of the distribution, without any significant negative effects.

Panel C shows that benefit enhancements did not have significant impacts on the distribution of nondurable nonfood expenditure, excepts for negative effects at a few quantiles around the first quartile of the distribution. One explanation could be that some households (e.g., with higher preferences for food) might have increased their food spending at the cost of their nonfood expenditure.[28]

Panel D plots the effects of ARRA expiration on the distribution of total nondurable spending. We observe large significant negative effects at low total nondurable spending quantiles. However, as we move across the distribution, the negative impacts become smaller and statistically insignificant. Given that these effects are underestimated due to underreporting of SNAP, one might be inclined to interpret these findings as a first-order disimprovement in the overall material well-being of SNAP households. We, however, take the conservative stance that ARRA expiration reduced the well-being of households at the bottom of nondurable spending distribution.

[28]One, however, needs to be cautious while interpreting these findings as they are underestimated due to the underreporting of the SNAP participation.

In panel E, we see a similar pattern for the distribution of food expenditure. The large significant negative effects at the lower quantiles of the distribution are expected because, as mentioned before, this part of the distribution is relatively more likely to include extramarginal households. Finally, panel F shows the results for nondurable nonfood spending distribution. Comparing the results in this panel with the panel C, we observe a similar pattern. This finding suggests that the impact on nonfood spending due to income effect of SNAP benefit changes was trivial and households reacted to benefit changes mainly by changing their food spending.

Figure 2.5 shows the results from the estimation of equation 2.2 using expenditures' budget shares as the outcome variables. As can be seen, similar patterns are observed. For instance, in panel A we observe a first-order improvement in the distribution of total nondurable's budget share and in panel B we see larger effects on tails of the food's budget share distribution. Overall, our distributional analysis expands our understanding of the impacts of the ARRA. In other words, the distributional results presented in this section provide evidence that mean estimates are not necessarily representative of the full distributional effects of SNAP benefit changes on household spending.[29]

2.6 Conclusions and Discussion

This study investigates the effects of the largest increase as well as the first-time decrease in SNAP benefits, due to the American Recovery and Reinvestment Act (ARRA), on the material well-being of SNAP recipients. We use nondurable consumption, represented

[29] Similar results are obtained from the IV-QRPD model, indicating that endogeneity of total expenditure does not have any implications for the estimated quantile treatment effects.

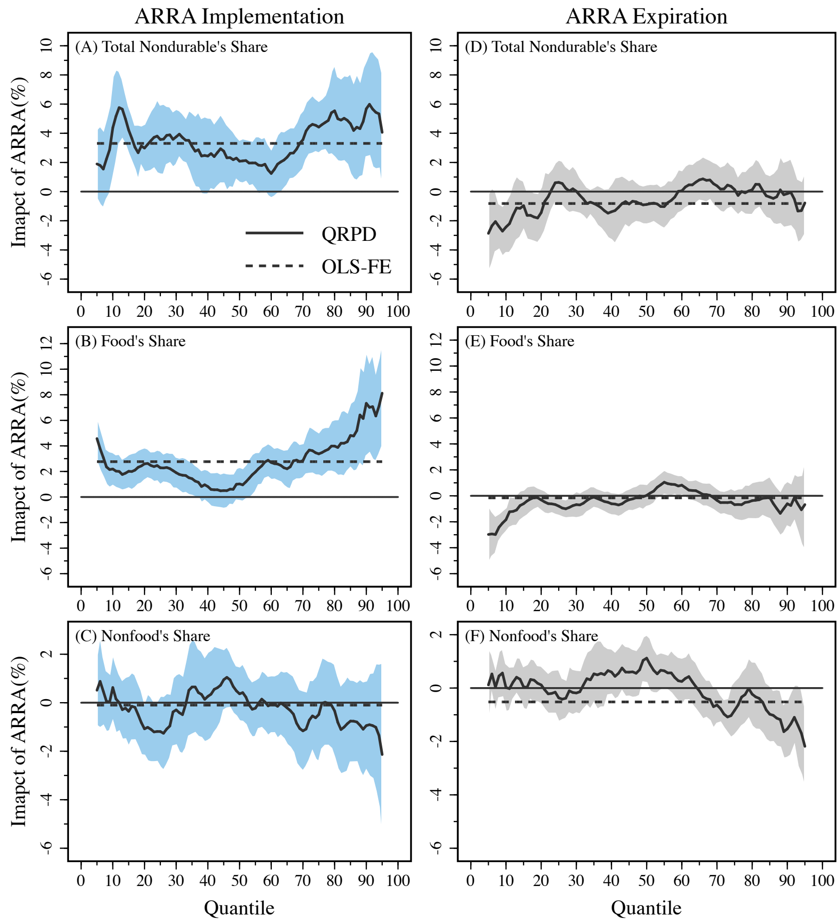

Figure 2.5: Impacts of the ARRA on the distributions of expenditures' shares of total expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 5 to 95 in 1-unit increments. All calculations use survey weights.

by total nondurable expenditure and its food and nonfood components, as our measure of the material well-being of SNAP participants. Although household expenditures do not capture several important aspects of the material well-being such as physical and mental health, neighborhood and school quality, they are arguably close approximates of a household's material well-being (Meyer and Sullivan, 2004). We believe this to be especially true in the present context given that the policy is directed at food.

Clearly, it is desirable to understand how the ARRA implementation or its subsequent expiration separately impacted the material well-being of SNAP participants in the context of policy applications. However, it is important to be able to compare spending decisions of SNAP households when facing an increase as compared to a decrease in SNAP benefits. Due to differences in the data, identification strategies, and the outcome variables, results from earlier studies may not be compared with each other to provide such insights for the current food policy. Moreover, it is critical to go beyond estimating the average treatment effects of such policy changes by examining the effects at all points of the spending distribution to identify to which households it is the most critical to provide an increase in benefits or to avoid a decrease.

Drawing on the panel data from the CEX, we use within-household variation to control for time-invariant household characteristics associated with self-selection into SNAP. By employing a newly developed fixed-effect quantile estimator, we simultaneously account for the heterogeneity in the effects of SNAP benefit changes and endogeneity due to non-random selection into SNAP. Consistent with Nord and Prell (2011), Beatty and Tuttle (2015), and Kim 2016, we find that the ARRA implementation increased average quarterly food spending of SNAP households. The magnitude of our estimates, however, are larger than Nord and Prell (2011) and Beatty and Tuttle (2015). One reason is that Nord and Prell (2011) use income cutoffs to identify SNAP and non-SNAP households. Considering that all income-eligible households do not necessarily participate in SNAP, results from their study are probably more underestimated than the results in our study. Beatty and Tuttle (2015) examine food-at-home spending of only inframarginal households. The estimated larger effects at the bottom of the distribution of food spending in this study, which is likely due to the presence of extramarginal households in our sample could be one plausible explanation for the observed difference.

Unlike Kim (2016), we do not find a significant effect on nonfood expenditures following the ARRA implementation. This is mainly because Kim (2016) does not account for self-selection into the SNAP program which leads to finding highly overestimated effects. The average household size in Kim (2016) analysis sample is about three. Given the maximum increase in SNAP benefits of $63 per month for a household of this size, an average treatment effect of about $408 on total quarterly expenditure seems to be a highly overestimated effect. As well, our nonfood expenditure measure excludes some expenditure categories such as shelter, entertainment, and education which are found to be significantly affected in Kim (2016).

With respect to ARRA expiration, we do not find an average significant negative effect on either food or nonfood spending. Given the small size of the treatment effect, estimated using grocery scanner data, in Bruich (2014) and the fact that our estimates are underestimated due to misreporting of SNAP participation in the CEX, the insignificant negative effect of benefit cuts on food spending in our study is not inconsistent with Bruich (2014). Indeed, Bruich (2014) argues that detecting the small treatment effect of the ARRA expiration using data with a higher degree of measurement error than the scanner data used in his study is questionable. Our distributional approach, however, enables us to detect larger treatment effects at the bottom of food spending distribution.

The distributional approach taken in this study allows us to better understand the extent of the well-being effects of ARRA within the SNAP population. Our findings indicate that ARRA implementation led to a first-order improvement in the material well-being of SNAP participants. This finding is of importance for policymakers as it indicates that the ARRA implementation *worked* in that it had its intended impact across the distribution. This is particularly true because the increase in total nondurable spending is mainly due to the increase in food spending. The observed heterogeneity in the impacts of ARRA implementation on the distribution of food expenditure indicates that households with lowest and highest food spending (preferences) were the most responsive to changes in SNAP benefits. The significant negative effects on households with low total nondurable and food spending suggest that benefit cuts were the most hurtful to the most disadvantaged SNAP subpopulations, a finding that could steer the contemporary food policy when modifying SNAP benefit allotments.

Overall, findings from this study suggest that while a *constant-dollar* amount increase in SNAP benefits could improve the well-being of all SNAP participants, in the case of the benefit cuts it could be more harmful to more vulnerable households. This could be because lower-income households have little liquid wealth and limited budgeting skills. Thus, they may not be able to fully accommodate reductions in their benefits. Such insights could provide important guidance to policymakers as they debate future changes, in particular reductions, in the program's budget.

Chapter 3

The Welfare Effects of Cyclical Spending among SNAP Beneficiaries

3.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP), the nation's largest food assistance program, provided over 45 million individuals with nearly $70 billion in benefits in 2015 (FNS-USDA, 2016). SNAP benefits are distributed monthly as lump-sum payments on known calendar dates and can be redeemed for food at grocery stores and other authorized retailers. While SNAP improves food security, child health, and birth weight outcomes (Bitler, 2015), the monthly benefit provision has been found to induce cyclical spending and consumption behavior: food expenditures spike markedly in the days following benefit receipt (Wilde and Ranney, 2000; Hasting and Washington, 2010; Smith et al., 2016) and there is evidence of reduced caloric intake at month's end (Shapiro, 2005; Todd, 2015).[1]

[1] Cyclical purchasing behavior is not unique to SNAP beneficiaries – similar evidence has been found among U.S. social security recipients (Stephens Jr., 2003; Mastrobuoni and Weinberg, 2009) and pay-

SNAP aims to accomplish its dual mandate – to reduce food insecurity and increase the quality of food purchases – by "increasing the food purchasing power" of low-income households (Food, Conservation, and Energy Act of 2008). Besides the pure income effect, households can additionally leverage benefits by "generating" lower prices on otherwise identical foods. For example, households can generate a lower price by taking advantage of bulk purchase discounts, shopping around, and using coupons (Griffith et al., 2009; Broda et al., 2009; Kaufman et al., 1997; Leibtag and Kaufman, 2003). While these shopping behaviors can be utilized with both SNAP and non-SNAP income, we hypothesize that the arrival of SNAP benefits, in conjunction with its in-kind nature, affects the intensity of price seeking behavior. In this case, households may generate higher prices via a reduction in shopping savviness when using SNAP income as opposed to cash income[2] or because they exhibit "impatience" (i.e., time inconsistent preferences) regardless of the type of income they use.[3] In either case, the result is a less-than-efficient usage of benefits at the beginning of the month when benefits are flush.

Alternatively, households could pay different prices for food through changing the quality of their food purchases over the benefit month (see, Kaufman et al., 1997; Drewnowski and Specter, 2004; Beatty, 2010). That is, at the beginning of the month

check recipients in the United Kingdom (Stephens Jr., 2006).

[2]Previous research is fairly decided in that beneficiaries do not budget and spend SNAP dollars as they do cash dollars. Thaler's (1985, 1999) theory of mental accounting describes this sort of behavioral response as "transaction utility," whereby households derive utility from the value of a "deal." Levendahl (1995) provides a theoretical framework whereby SNAP and non-SNAP income are allowed to have differing shadow prices.

[3]Previous studies find that SNAP households discount consumption between two far-off days at a much lower rate than they discount tomorrow's consumption (Shapiro, 2005; Mastrobuoni and Weinberg, 2009; Hastings and Washington, 2010; Smith et al., 2016). Put differently, a time inconsistent household exhibits short-run impatience, and therefore has a higher preference for today's consumption.

households might choose to buy higher quality foods (e.g., higher-quality beef) and thus pay higher prices, whereas they may purchase lower quality foods (e.g., lower-quality beef) later in the month. While this shopping behavior may not be considered as an inefficient use of SNAP benefits, it cannot be considered as a price-seeking strategy as foods with different qualities are not identical.

This study builds upon previous research that has shown cyclical food spending over the benefit month can be partially attributed to a decrease in prices paid on foods (Cheng and Beatty, 2016). To be clear, changes in prices paid over the month appear to be *endogenous* in that they are self-induced as households either change the intensity of their price-seeking behavior or their demand for food quality soon after benefits are received.[4] The former implies that less expenditure is required to reach a reference utility level as the month progresses due to a decline in prices paid on otherwise identical foods. The latter, however, indicate that more expenditure is needed to achieve a reference utility level because of a decline in food quality toward the end of the month. Therefore, in either case, changes in welfare due to changes in prices paid can be measured.

We utilize USDA's National Household Food Acquisition and Purchase Survey (FoodAPS), which collected daily food acquisitions from households over a one-week period during April 2012 to January 2013. FoodAPS is unique in that it not only records detailed information on individual food items from UPC codes such as item-level quantities and expenditures but also contains the number of days since SNAP benefit issuance. Both SNAP participation and the benefit issuance date are confirmed by ad-

[4]An exogenous change in price over the month would be due to pricing strategies by the retailer for example, as investigated in Hastings and Washington (2010). Cheng and Beatty (2016), using the same data as ours, and also Goldin et al. (2016) reject this hypothesis, and we therefore do not investigate it here.

ministrative records for the majority of SNAP participants—even a small amount of endogenous misreporting is sufficient to overturn statistical inferences (Gundersen and Kreider, 2008).

Our identification strategy involves leveraging the randomness of survey timing relative to benefit receipt in conjunction with fixed effects for households and food types. We also include a rich set of controls, namely coupon usage, in-store sales, bulk purchasing, type of income used (i.e., SNAP versus non-SNAP), store format (e.g., big box versus convenience store), and national label brands. Similar to Cheng and Beatty (2016), we find a declining pattern for prices paid. However, by utilizing a more refined grid of parameters over the benefit month, we find that by the end of the third week (i.e., days 19-21) average prices paid are 22% lower than the first two days of the month. Interestingly, prices begin to rise to beginning-of-the-month levels in the last three days of the benefit month.

We use these estimated price changes to calculate the welfare impacts associated with a decline in prices paid using an exact measure of Hicksian compensating variation (CV) following Hausman (1981).[5] To the best of our knowledge, this is the first study to estimate the welfare implications of endogenous food price variation over the benefit month. We find that the loss in household welfare due to a change in prices paid, which is more likely to be driven by within food type substitutions toward lower-quality foods, is fairly sizable with a maximum loss of $4.94 per day on days 19-21 of the benefit

[5]The exact Hicksian CV is a monetary value of welfare defined as the minimum amount a SNAP household would relinquish or preserve after a change in price to be as well off as before the price change. Thus, our estimates do not account for other changes in welfare over the benefit month, such as reduced calorie consumption (Shapiro, 2005; Todd, 2015) or increased hospital admissions (Seligman et al., 2014).

month, measured relative to the first two days. This is equivalent to 6.6% of the average amount spent on food during the first two days.

In terms of heterogeneity, our findings show that some households, such as those with income over the 100% of the federal poverty guidelines (FPL) or who report financial security, do not pay different prices throughout the month. In contrast, other households, such as those who do not report shopping for lower prices or travel less than 15 minutes to the grocery store, exhibit sharper changes in prices paid and accordingly larger changes in welfare.

The results from this research could help steer food policy decisions related to the efficiency of the program. Even small improvements in the efficiency of benefit usage could help the program to accomplish its goals of increasing the quality of food purchases and purchasing power of participants. For example, educational efforts, such as the Supplemental Nutrition Assistance Program Education program (SNAP-Ed program), aim to teach SNAP eligible and participating households how to stretch their marginal food dollars further.[6] Some SNAP-Ed programs teach participants how to shop smarter and pay lower prices by planning ahead and budgeting, instead of shifting toward lower-quality foods. For instance, households are instructed to look for coupons, sales, store specials, or to sign up for store discount cards before shopping. Likewise, households are recommended to buy store brand items and compare unit pricing to find the best deals while shopping (USDA, 2015). Therefore, understanding the degree to which SNAP households are efficient in spending their benefits could inform these

[6]SNAP-Ed is a non-mandatory extension of the SNAP for people using or eligible for SNAP to join. The goal of SNAP-Ed is to educate people how to make healthy food choices within a limited budget (Koszewski et al., 2011).

SNAP-Ed programs about where to focus efforts to achieve the program's goals.

The remainder of this chapter is organized as follows. First, we describe our data and provide some descriptive statistics. Next, we outline our identification strategy for estimating the impacts of the timing of SNAP benefit receipt on unit prices followed by findings from this analysis. In the second part, we provide a brief overview of Hausman's (1981) method for estimating the welfare effects due to price changes. The welfare analysis results are followed by a discussion of policy implications.

3.2 Data

FoodAPS is a nationally representative survey that collected daily food acquisitions over a seven-day period for 4,826 households from April 2012 to January 2013.[7] The primary respondent for each household, which is usually the main food shopper or meal planner, provided food related information as well as rich socio-demographic information through two in-person interviews. The initial interview took place before the start of the seven-day period, in most cases the day before the first-day acquisitions were tracked. Households were interviewed a second time after the completion of the seven-day period, usually the day after the end of the food-reporting week.

Respondents were instructed to record daily food acquisitions for "food at home" (FAH) which are food items purchased for the purpose of being consumed at home, and "food away from home" (FAFH) which are food items purchased to be consumed

[7]In a comparison study, estimates of household food spending overall and for FAH and FAFH from FoodAPS track fairly closely to those from the Consumer Expenditure diary survey (Clay et al, 2016). While there were some small differences across the surveys in the amount of expenditures, estimates of FAH expenditures for SNAP participants were similar for the surveys.

outside the home. The primary focus of this study is the purchase of food from SNAP authorized retailers (e.g., grocery stores). Clearly, items can be purchased from a grocery and consumed outside the home, which may be recorded as FAFH. Therefore, to avoid confusion and to be in line with previous literature, we consider items purchased from SNAP authorized stores as FAH, regardless of where the household intends to consume the food.[8] All food purchased from unauthorized SNAP retailers (e.g., fast food) is considered FAFH.

Households were asked to scan the Universal Product Codes (UPC), either on the food package or provided in the diary for loose/bulk items, as well as to provide a receipt if one was given. Therefore, FoodAPS contains detailed information on individual food item purchases such as item-level expenditure, quantities acquired, total amount saved from coupons and/or in-store savings, type of brand (national, private, or generic), and package size. Additionally, the survey also recorded information on the place of acquisition. These locations were then identified as authorized to accept SNAP benefits from administrative data. Furthermore, the survey reports whether households used their SNAP benefits via their Electronic Benefit Transfer (EBT) card to pay for their transactions in part or completely.

A goal of FoodAPS was to understand the food acquisitions of SNAP and non-participating low-income households, and the sample was stratified accordingly. During the initial interview, households were asked to report their current SNAP participation status and the date they last received SNAP benefits. Respondents' reports of participation were then matched against administrative data for households who had given

[8]Over 96% of items purchased from authorized SNAP stores are recorded as FAH items. Likewise, more than 97% of reported FAH items are purchased from SNAP authorized stores.

permission for data matching. To avoid any biases associated with SNAP participation misreporting (e.g., Gundersen and Kreider, 2008), we focus on administratively confirmed SNAP participants.[9] For each daily food acquisition for each household, FoodAPS provides the days since receiving SNAP benefits with day zero indicating the day benefits are received and day 30 the last possible day.[10] When calculating days since benefit receipt, a household's own report of last benefit receipt is preferred to a date from the administrative data unless a household did not report the last receipt date or the reported date was inconsistent with being a current participant.

In household surveys including FoodAPS, item prices are usually not available. Instead, unit prices can be derived by dividing an item's total expenditure by its total quantity (in pounds). These unit prices are actual unit prices paid by households in that they are net of coupons and in-store promotional savings. Computing unit values, however, requires having information on both the expenditure and quantity of the items purchased. FoodAPS respondents reported purchasing 40,565 food items from SNAP authorized stores. Among them, there are 11,032 items (27% of all items) with either missing expenditure (87 items or 0.21% of all items), quantity (10,439 items or 25.73% of all items) or both (506 items or 1.25% of all items). These observations are excluded from our sample.[11] From here we use Information Resources, Inc. (IRI) 5-digit food

[9]In our final sample (i.e., after excluding the missing values) there were 1206 self-reported current SNAP participant households. A small portion (N=21) did not grant permission for the administrative match, and 154 households could not be linked due to administrative data. Our final sample consists of 1031 administratively confirmed households with no missing covariate information.

[10]Due to the randomization of food diaries, some households were nearing the end of their benefit cycle during the initial interview. For these households, FoodAPS assumes benefits were again received on the same calendar day as the previous month.

[11] We looked at the pattern of missing observations across IRI 2-digit food department, different store formats, and over the survey days and did not find systematic differences in missing information across these aspects of food acquisitions. See Cheng and Beatty (2016) for a more detailed analysis of missing

type codes to classify individual items purchased by each household on each acquisition day and within each item brand type (i.e., national brand vs. other brand types) into 534 food types. For each food type we calculate daily unit values (dollars per pound) paid by the household using a simple average within each food type.

3.2.1 Summary Measures

Our final sample includes 1,031 households, 2,472 positive FAH purchase days, and 18,479 aggregated food items acquired from SNAP authorized stores. Table 3.1 presents summary statistics for our sample. In addition to the standard demographic characteristics (e.g., household size, age, race), the survey also asked about the financial condition of households. We consider households to be "financially secure" if they reported "very comfortable and secure" or "able to make ends meet without much difficulty." All other households are defined to be financially insecure. As well, the survey asked about the reasons for shopping at their primary stores and also travel time to get there (in minutes). We split the sample by households who select their primary stores because of "low prices/good values." We also split the sample by those who report travel times to their primary store as less than 15 minutes, which is the 75^{th} percentile.

We make use of these demographic characteristics to demonstrate the randomness of survey timing relative to SNAP benefit receipt. To do so, we compare the characteristics of households surveyed during different weeks of the benefit month. Therefore, we split the sample into four weeks. Column (2) summarizes characteristics of "Week 1" households defined by their first diary day falling within the first seven days of the

observations.

Table 3.1: Summary Statistics for Households

	Full Sample	Week 1	Week 2	Week 3	Week 4	*p*-value
Household Size	2.72	2.69	2.66	2.66	2.86	0.81
	(0.08)	(0.15)	(0.15)	(0.19)	(0.18)	
Age	45.67	44.19	47.86	45.18	45.44	0.37
	(0.89)	(1.37)	(1.60)	(1.71)	(2.36)	
Non-Hispanic White	0.52	0.51	0.53	0.54	0.49	0.93
	(0.02)	(0.05)	(0.05)	(0.06)	(0.05)	
Hispanic	0.22	0.21	0.22	0.20	0.25	0.90
	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	
Non-Hispanic Black	0.24	0.25	0.22	0.24	0.24	0.92
	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)	
Gender (Male=1)	0.25	0.23	0.31	0.25	0.19	0.33
	(0.02)	(0.04)	(0.05)	(0.05)	(0.04)	
Under 100% Poverty	0.59	0.62	0.57	0.58	0.56	0.83
	(0.02)	(0.04)	(0.05)	(0.05)	(0.05)	
Financially Secure	0.23	0.21	0.28	0.25	0.19	0.43
	(0.02)	(0.04)	(0.05)	(0.05)	(0.04)	
Shop for Price	0.61	0.70	0.64	0.54	0.51	0.02
	(0.03)	(0.04)	(0.05)	(0.06)	(0.05)	
$\geq$15 Min Travel Time	0.32	0.36	0.31	0.26	0.31	0.47
	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)	
Households	1,031	303	281	196	251	

Notes: All calculations use survey weights. Weeks 1-4 are defined as days 0 to 6, 7 to 13, 14 to 20, and 21 to 30, respectively. P-values represent a statistical difference F-test for the joint hypothesis that all weeks are equal.

benefit cycle (i.e., days 0 to 6). The next three columns present household characteristics for "Week 2" (days 7 to 13), "Week 3" (days 14 to 20), and "Week 4" (days 21 to 30) households, respectively. The last column reports the *p*-values from the test of the joint hypothesis that household characteristics are equal between weeks of the benefit month.

If households are randomly surveyed, we expect their demographics to be similar over the weeks. As expected, all differences, except for the households shopping at their primary stores because of better prices, are statistically insignificant. We might expect the proportion of "price-seeking" households change over the month since this behavior is directly related to the outcome of interest, which is also expected to change over the month.

Table 3.2 summarizes daily expenditure patterns in terms of total food (i.e., FAH plus FAFH), FAH, FAFH, and the quantities of FAH purchased in pounds. As expected, as the month proceeds food expenditures decrease. This drop is entirely from FAH expenditures, and FAFH expenditures remain level (at about $4) per day. A natural question arises, is the drop in FAH expenditures due to decreases in quantities purchased, prices, or both? The last column of table 3.2 shows that decline in the quantities of FAH explains a large portion of variations in FAH expenditure.

Figure 3.1 uses a simple nonparametric regression model to show how unit prices (i.e., dollars per pound) change over the benefit month. As we see unit prices decline as the month progresses with the exception of the increase in the last three days of the month. This suggests that some of the excess sensitivity of FAH expenditures is due to a decline in food prices in addition to quantities purchased.

Figure 3.2 shows further graphical evidence of price-seeking behavior over the benefit month. There is no clear pattern as a single mechanism driving variation in unit prices over the month. For example, the probability of using a coupon increases at month's end, but we observe an increase in unit prices. On the other hand, bulk purchases and shopping at discount stores (i.e., club, superstore and supercenters) fall, which should

Table 3.2: Average Daily Food Spending and Quantities Conditional on a Positive FAH Purchase

	Total Food Expenditure	FAH Expenditure	FAFH Expenditure	FAH Quantities
Full Month	31.69	27.39	4.29	20.99
	(0.93)	(0.88)	(0.25)	(0.64)
Days 0-1	74.58	70.66	3.92	48.68
	(5.19)	(5.00)	(1.05)	(3.24)
Days 2-3	35.19	30.83	4.36	22.70
	(3.75)	(3.65)	(1.01)	(2.81)
Days 4-6	31.48	27.38	4.10	20.16
	(2.23)	(2.14)	(0.58)	(1.43)
Days 7-9	29.77	24.62	5.15	19.17
	(1.96)	(1.80)	(0.70)	(1.22)
Days 10-12	24.86	20.81	4.05	16.24
	(1.57)	(1.46)	(0.55)	(1.09)
Days 13-15	25.99	21.77	4.22	17.82
	(1.87)	(1.72)	(0.57)	(1.34)
Days 16-18	25.93	22.33	3.60	16.89
	(2.12)	(2.07)	(0.46)	(1.25)
Days 19-21	23.24	18.20	5.04	15.21
	(1.99)	(1.76)	(1.06)	(1.39)
Days 22-24	21.15	16.81	4.34	14.97
	(1.89)	(1.68)	(0.64)	(1.90)
Days 25-27	23.32	18.96	4.36	17.66
	(2.00)	(1.76)	(0.72)	(1.74)
Days 28-30	25.90	21.79	4.11	17.82
	(2.68)	(2.64)	(0.76)	(1.96)
Observations	2,472	2,472	2,472	2,472
Households	1,031	1,031	1,031	1,031

Notes: All calculations use survey weights. Standard errors in parentheses are clustered at the household level.

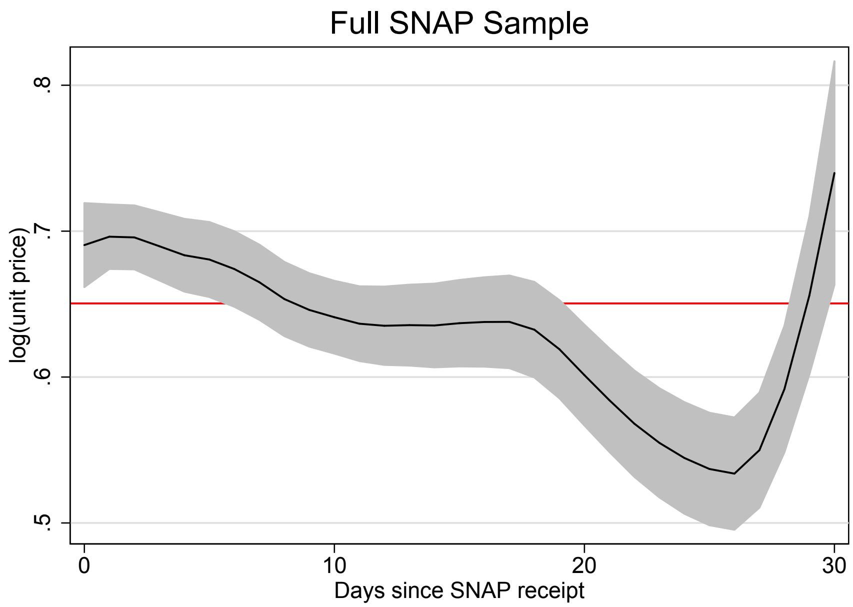

Figure 3.1: Paid unit price pattern throughout the SNAP benefit month

lead to higher unit prices. Moreover, the graphical results from figure 3.1 are unconditional with respect to household fixed characteristics and food types. Therefore, prior to estimating welfare effects, we employ regression methods to identify the impact of SNAP receipt.

3.3 Impact of SNAP Benefit Receipt on Prices Paid

Our identification strategy relies on the fact that household diary weeks are randomly distributed throughout the benefit month. The plausibility of our randomization assumption was demonstrated in table 3.1. Moreover, during the time of the survey period SNAP distribution dates were randomly assigned to households based on either the first letter of their last name (8 states) or by their social security/program identification num-

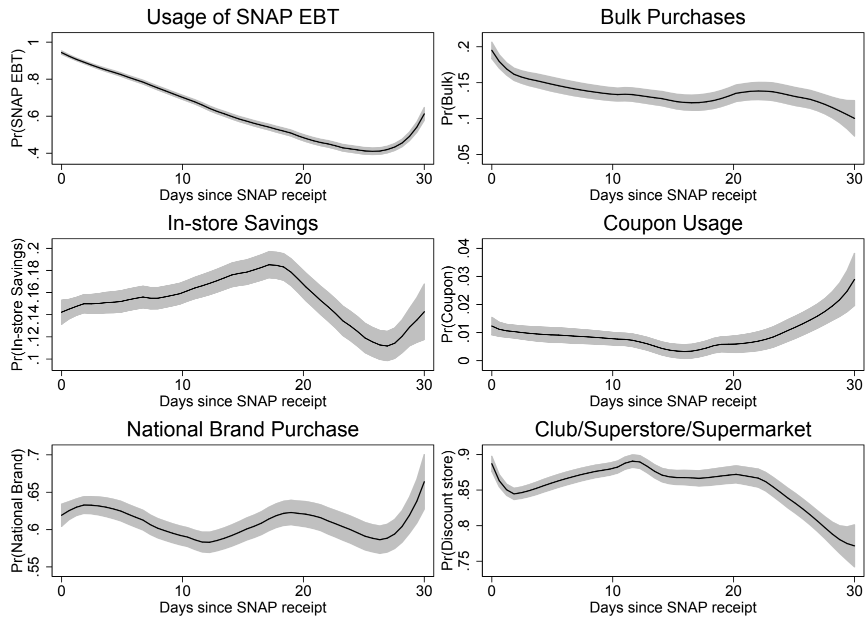

Figure 3.2: Unconditional probability of price-seeking behavior over the SNAP benefit month

ber (35 states); seven states had a single distribution day (FNS-USDA, 2016). Thus, we believe the causal identification of the impact of benefit arrival on unit prices is plausible under the correct specification as discussed below.

As mentioned earlier, in FoodAPS similar to other household surveys actual prices paid for food are not available and we only observe expenditures and physical quantities. Thus, as is common in the literature, we use unit values as a proxy for food prices. According to Deaton (1988, 1997), unit value v can be expressed as the product of the actual price paid p and a quality index π (i.e., $v = p \times \pi$). The quality index is sometimes referred to as an expensiveness index since price increases with quality (e.g., Beatty, 2010). Therefore, if the quality effects are trivial (i.e., $\pi = 1$) or if one can condition on quality, then unit values are (conditionally) equivalent to actual prices.

To account for differences in demand for quality between households, we follow Cox and Wohlgenant (1986) by assuming that quality effects are induced by household characteristics, both observable (e.g., presence of children and income) and unobservable (e.g., tastes). More specifically, given the relatively short survey period (i.e., 7 days), we assume that between-household quality effects are fixed and can be modeled as a household specific fixed effect. Thus, for household h buying food type k on day t, the equation capturing the response to the benefit arrival is:

$$log(p_{hkt}) = \alpha_h + \delta_t + \phi_k + X'_{hkt}\eta + \beta f(DAYS) + \epsilon_{hkt}, \tag{3.1}$$

where $log(p_{hkt})$ is the logarithm of unit price of food k on day t purchased by household h; α_h, δ_t, and ϕ_k are household, diary day and (534) food type fixed effects, respectively. It is important to note that, although conditioning on household fixed effects could control for differences in demand for quality across households, it may not account for within-household changes in demand for quality over the benefit month.[12] X_{hkt} includes dummies for national brand, 16 store formats, coupon usage, in-store sale, bulk size purchase defined as the item sizes larger than 90th quantile of the package size within each food type, and a SNAP dummy indicating whether households used their EBT cards to pay (in part or entirely) for their transactions. As well, X_{hkt} includes indicators for days of the calendar month and week. We include these variables to capture variation in unit prices associated with decisions tied to the calendar day (e.g., bills due

[12]We assume that within household changes in demand for quality over the benefit month do not change the relative quality effects between households. For example, if we consider two households A and B, with household A buying higher quality foods than household B, if both households decide to buy lower-quality foods at the end of the benefit moths, these shifts are such that relative quality effects between A and B are not changed.

on the first of the month) and the day of the week (e.g., weekends, see Castellari et al., 2015 as an example). Moreover, there may be normal variations in shopping behavior over the course of the week and/or congestion in grocery stores. Thus, it is important to capture these sorts of fluctuations such that they are not falsely attributed to SNAP benefit arrival. $f(DAYS)$ can be a flexible function for days since SNAP benefit receipt. Here, we use a set of 11 indicators in two or three day increments (as defined in table 3.2) with the first two days of the month (i.e., days 0 and 1) as the reference period. Finally, ϵ_{hkt} is assumed to be an idiosyncratic error.

3.3.1 Price Analysis – Main Results

Main results from estimating equation 3.1 are presented in figure 3.3 and appendix table B.1. All coefficient estimates are accompanied by 90% confidence intervals calculated using standard errors clustered at the household level. Each estimate is relative to the reference period (days 0 and 1), shown as the horizontal solid line. As the month proceeds, households continually pay lower prices until the end of the third week (i.e., days 19-21) when prices paid are about 22% lower than the reference period. As the fourth week begins (i.e., days 22-24), prices begin to increase and during the final three days (i.e., days 28-30) are no longer statistically different than the base period.

To gauge the magnitude of the impact of SNAP benefit arrival on prices paid, we present coefficient estimates on selected control variables in table 3.3. For instance, nationally branded items are about 23% more expensive than generic or private label items. Likewise, prices are on average about 33% lower when using a coupon and 12% lower when taking advantage of in-store sales. The insignificant coefficient estimate for

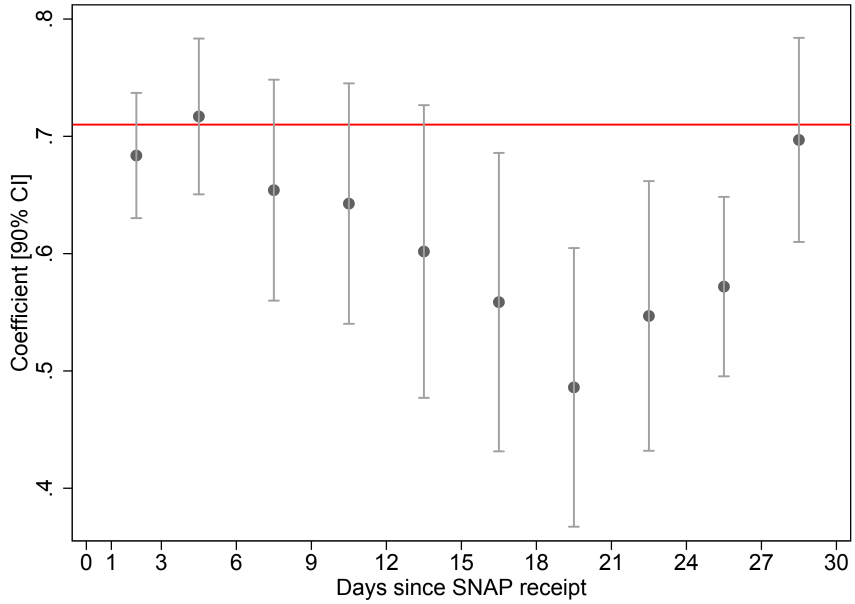

Figure 3.3: Unit price pattern over the SNAP benefit month for the full SNAP sample

SNAP dummy suggests that the source of income (i.e., SNAP vs. Non-SNAP) used to pay for the transaction does not have a significant impact on prices paid, as would be expected from a mental accounting (or fungibility) hypothesis.

Because we do not find evidence of a systematic increase in the intensity of observable price-seeking strategies toward the end of the benefit month (i.e., figure 3.2), the observed 22% drop in unit prices could be due to to other unobservable (to the researcher) price-seeking behavior. For example, following arguments from Aguiar and Hurst (2005), it is possible that households are more intensely using time to seek out prices, such as searching through grocery circulars, the Internet or speaking with friends.

An alternative explanation is that the decline in unit prices are driven by lower-quality foods purchased later in the month. To further investigate this hypothesis, we re-

Table 3.3: Selected coefficient estimates from equation 3.1

Variables	Estimates
Brand	0.23***
	(0.02)
Coupon	-0.33***
	(0.10)
Savings	-0.12***
	(0.02)
SNAP (EBT)	-0.03
	(0.03)
Bulk	-0.40***
	(0.02)
Observations	18,479

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses are clustered at the household level.

estimate equation 3.1 with "price per kilocalories" (kcal) of food as the dependent variable. Put differently, we use food calorie content as a proxy for food quality such that a lower-quality food (e.g., high-fat beef) has a lower price per kcal (see, Drewnowski and Specter, 2004). Estimation results are presented in figure 3.4 and appendix table B.1. Although we still observe a similar declining pattern in price per kcal,[13] we observe fewer significant point estimates during the fourth week, suggesting that the decline in unit prices are more likely to be driven by within food type substitution toward lower quality foods than intensified price-seeking behavior.

[13]This could be because food calorie content alone may not reflect all aspects of food quality for various food types. For example, in the case of fruits and vegetables or legumes, calorie content may not vary by quality.

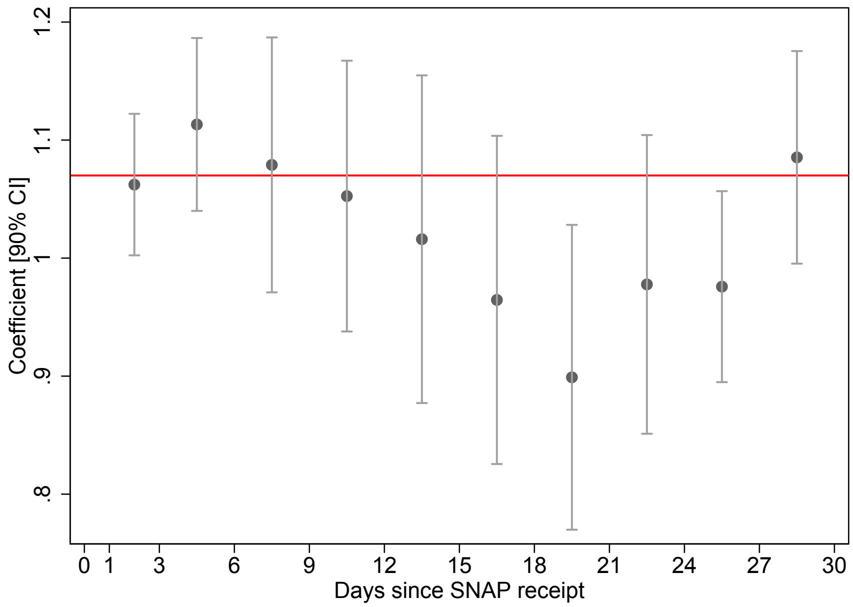

Figure 3.4: Price per kcal pattern over the SNAP benefit month for the full SNAP sample

3.3.2 Price Analysis – Heterogeneity

To examine the existence of heterogeneity in paid unit prices within the SNAP population, we repeat our analysis for several subsamples of SNAP households. To do so, we divide our sample based on household income relative to 100% of the FPL (below 100% of the FPL and above the 100% of the FPL), financial condition (secure and insecure), primary store selection reason (shop for price versus other reasons such as quality or variety), and travel time to primary store (less than 15 minutes and more than 15 minutes). For each subsample, we estimate the equation 3.1. The main results are presented in figures 3.5 and 3.6.

Our results indicate that some SNAP households pay lower prices as the month proceeds while others do not. As we can see in the left panels of figure 3.5, households with income less than 100% of the FPL as well as those who are financially insecure exhibit a declining unit price pattern. These households are more likely to face more severe resource and credit constraints than other SNAP households. Thus, they are more responsive to the timing of benefits arrival and are thus less likely to smooth food expenditures (see, Smith et al., 2016). Consequently, we observe higher variability in unit prices paid by these households.

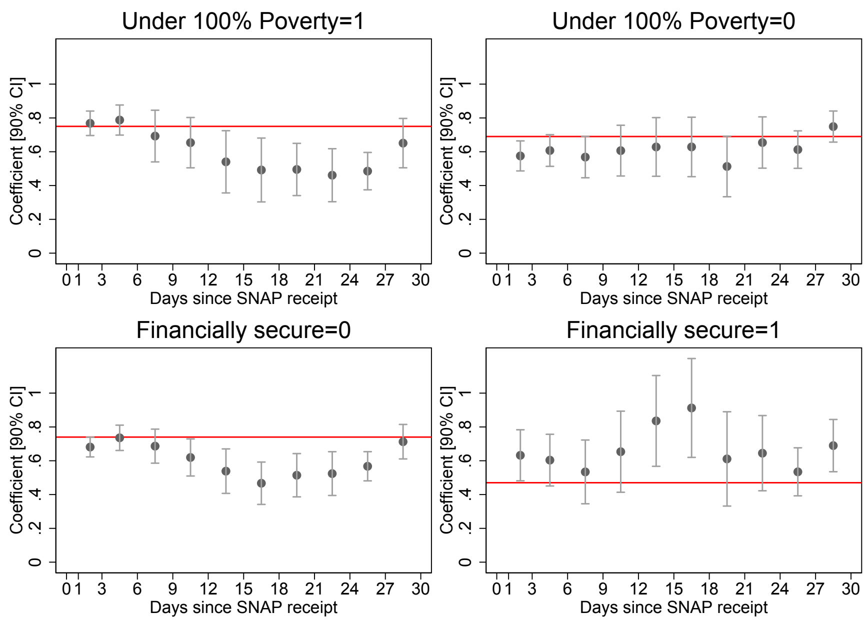

Figure 3.5: Unit price pattern over the SNAP benefit month by income level relative to federal poverty guidelines and financial condition

Similarly, the left panels of figure 3.6 show that households that state factors other than price determining their choice of a primary grocery stores and those who spend less time traveling to their primary grocery stores pay lower prices toward the month's end. One explanation could be the lack of financial planning and budgeting skills. Households with better financial planning skills are more likely to smooth expenditures and consequently, the prices they pay (see, Parker, 2015). On the other hand, households that shop for prices are more prone to compare prices to find the best deals throughout the month and not just during the last days. Moreover, when spending more time to get to their primary store, households might be more likely to plan ahead and seek out the best prices or choose to travel farther to a specific store that generally offers lower prices. Or, it could be that those who do not cite price as the main factor have preferences for higher priced products (perhaps because of perceived quality or personal tastes) but must cut back on those goods later in the benefit month.

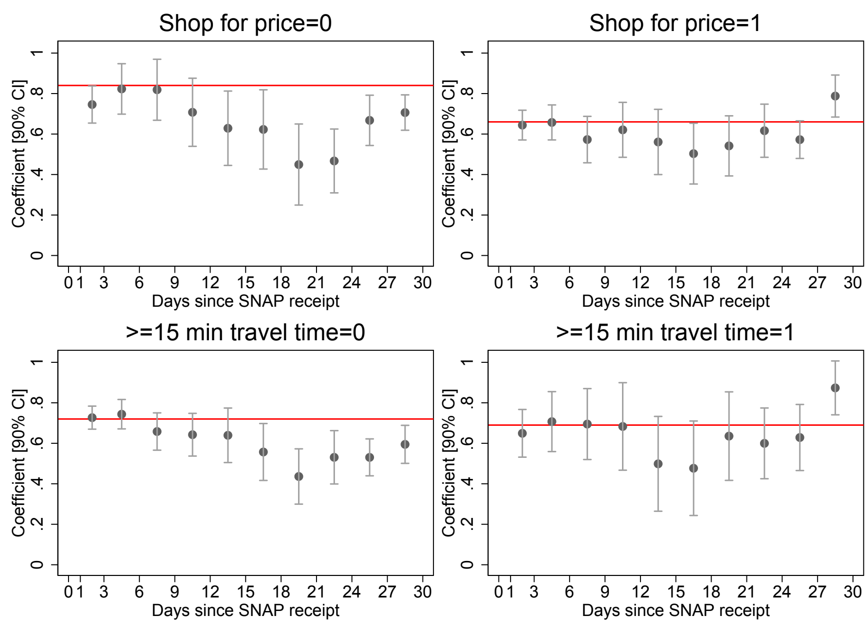

Figure 3.6: Unit price pattern over the SNAP benefit month by primary store selection reason and travel time to primary store

3.4 Welfare Implications

Marshall's concept of consumer surplus, defined by the area below the Marshallian (uncompensated) demand curve and above the new price is the canonical measure of welfare. However, Deaton and Muellbauer (pp.184-186, 1980) point out that "the use of [Marshallian] consumer surplus as an analytical tool frequently seems to lead to errors and confusion." They suggested that taking the area under the Hicksian (compensated) demand curve over a price change would be a better measure of monetary welfare. This is because the Hicksian demand function is the derivative of the cost function and the integration of the demand function at two different price vectors gives the differences in costs of reaching the same indifference curve .

Willig (1976), Shonkwiler (1991), and Just et al. (2005) propose approximations of Hicksian welfare to correct Marshallian consumer surplus. However, Hausman (1981) argues that when one is considering welfare implications of a single price change (e.g., FAH price change in this study) no approximation is necessary. He derives a measure of the exact consumer surplus from an indirect utility function which is retrieved from an estimate of the market demand curve.[14]

The basic idea of Hausman's (1981) method in deriving the exact measure of consumer surplus is to use the observed market demand curve to derive the unobserved market demand curve. The latter is then used to calculate the compensating variation (CV) as the exact measure of consumer surplus. In general, holding income constant at

[14]Hausman's (1981) approach is fairly tractable and easy to implement and has been used to estimate the consumer surplus associated with the introduction of new services and products (e.g., Hausman, 1997; Hausman, 1999; Hausman and Leonard, 2002) and also to evaluate consumer benefits of entry and expansion of supercenters into retail food markets (Hausman and Leibtag, 2007).

y_0, for a single price change from p_0 to p_t, CV is the amount of money that a consumer would give or would need to be given to be as well off after the price change as she was before the price change. In terms of expenditure function

$$CV(p_0, p_t, y_0) = e(p_t, u_0) - e(p_0, u_0) = e(p_t, u_0) - y_0 \tag{3.2}$$

where u_0 denotes the consumer's utility level at the initial level of prices.

Hausman (1981) derives the CV for two cases: 1) two-good case, and 2) many-good case. In both cases, only the price of one good changes. The two-good case is often used in empirical analysis with a separability assumption between the good whose price changes and all other goods (Hausman, 1981). Therefore, we employ the two-good case analysis in this study under two assumptions. First, we assume consumer preferences are separable between food and nonfood commodities. Second, we assume that prices for nonfood goods are constant.

Starting with the market demand curve, Hausman (1981) uses Roy's Identity to derive the corresponding indirect utility function. He derives the CV for several demand curve specifications. For the constant elasticity specification used in this study, the first step is to write down Roy's identity:

$$q_{hkt} = e^{Z'_{hkt}\gamma} p_{hkt}^{\alpha} M_{ht}^{\delta} = -\frac{\partial \nu(p_{hkt}, M_{ht}) / \partial p_{hkt}}{\partial \nu(p_{hkt}, M_{ht}) / \partial M_{ht}}, \qquad \delta \neq 1 \tag{3.3}$$

where p_{hkt} again denotes the price of food type k purchased by household h on diary day t, q_{hkt} is the corresponding quantity, and Z_{hkt} is a vector including α_h, δ_t, ϕ_k, and X_{hkt} as defined in equation 3.1. M_{ht} is total daily food expenditure for household h

on diary day t as a proxy for household's income (i.e., y). Finally, $\nu(p_{hkt}, M_{ht})$ is the household's indirect utility function. Such a constant elasticity specification is often estimated in log-linear form as:

$$log(q_{hkt}) = Z'_{hkt}\gamma + \alpha log(p_{hkt}) + \delta log(M_{ht}) + \epsilon_{hkt}. \tag{3.4}$$

By solving the linear partial differential equation in equation 3.3, the indirect utility function is derived as:

$$\nu(p_{hkt}, M_{ht}) = c = -e^{Z'_{hkt}\gamma} \frac{p_{hkt}^{1+\alpha}}{1+\alpha} + \frac{M_{ht}^{1-\delta}}{1-\delta} \tag{3.5}$$

where c is the constant of integration and is chosen to be equal to u_0. Since the indirect utility function of equation 3.5 is monotonically increasing in M_{ht}, it can be easily inverted to derive the corresponding expenditure function:

$$e(p_{hkt}, \bar{u}) = \left[(1-\delta) \left(\bar{u} + e^{Z'_{hkt}\gamma} \frac{p_{hkt}^{1+\alpha}}{1+\alpha} \right) \right]^{1/1\,-\,\delta} \tag{3.6}$$

This expenditure function gives the minimum amount of income required to achieve the utility level from the indirect utility function in equation 3.5. By combining equations 3.2 and 3.6, the exact CV for a change in price is

$$CV(p_0, p_t, M_h) = \left\{ (1-\delta) \left[\frac{e^{Z'_{hkt}\gamma}}{1+\alpha} (p_t^{1+\alpha} - p_0^{1+\alpha}) \right] + M_h^{1-\delta} \right\}^{1/1\,-\,\delta} - M_h \tag{3.7}$$

where p_0 is the average unit price paid during the reference period (i.e., days 0 and 1), p_t is the average unit price paid during other periods (e.g., days 19-21), and M_h is the average daily food expenditure for household h. Thus, the welfare change is measured as the CV for each household following a change in the FAH average paid price from p_0 to p_t. The calculated CV is then interpreted as the amount of money that a household needs to be given in period t to purchase the food at the same quality of the reference period.

3.4.1 Welfare Analysis Results

The main coefficient estimates from the observed market demand for FAH for different specifications are presented in table 3.4. The first column shows the estimation results from a specification without household and food type fixed effects, the second column with household fixed effects, the third column with food type fixed effects, and the last column with both household and food type fixed effects. In all specifications, the estimated price and income elasticities are statistically significant and have the expected signs, indicating that FAH is a necessity. Further, we see that, as expected, household fixed effects are correlated with the income elasticity of demand for food, whereas the food type fixed effects are correlated with the price elasticity of food demand.

Coefficient estimates from column (4) are used to calculate the exact CV using equation 3.7. Results for the average SNAP household are given in column (1) of table 3.5. The largest change in the household welfare is at the end of the third week (i.e., days 19-21) at \$4.94 per day which is expected given that households pay the lowest prices during these days. The interpretation is that the average SNAP household would need to be compensated \$4.94 per day on days 19-21 to purchase the same basket of food (i.e., the same quality) at prices equivalent to those paid on days 0 and day 1.

Table 3.4: Demand Estimation Results, Full Sample

	(1)	(2)	(3)	(4)
$log(p_{hkt})$	-0.86***	-0.86***	-0.67***	-0.67***
	(0.01)	(0.01)	(0.02)	(0.02)
$log(M_{ht})$	0.15***	0.12***	0.16***	0.13***
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.04	0.22**	-0.54***	-0.25
	(0.14)	(0.11)	(0.13)	(0.19)
Household Fixed Effects	No	Yes	No	Yes
Food Type Fixed Effects	No	No	Yes	Yes
Observations	18,479	18,479	18,479	18,479

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions use survey weights. Standard errors in parentheses are clustered at the household level. All regressions include controls for national brand, store formats, coupon usage, in-store sale, bulk size purchase, SNAP, and days of the calendar month.

To better conceptualize the magnitude of this welfare change and to understand its economic importance, we can express the estimated change in welfare as a percentage of daily food expenditure. Since average daily food expenditure during days 0 and 1 is \$74.58 (see, table 3.2), the largest change in welfare is 6.6%.[15]

Columns (2) through (5) of table 3.5 present the estimated change in money metric welfare for the groups of households with statistically significant declining prices paid over the benefit month.[16] In general, similar patterns emerge—changes in welfare reach their peak around the end of the third week and also beginning of the fourth week. The largest changes in welfare are seen for households for households that do not shop for prices and those with income below the 100% of the FPL at \$8.38 (11% of the

[15] Although the average daily food spending for the entire benefit month is much smaller (see, table 3.2), we should notice that SNAP households redeem a large portion of their benefits shortly after receiving their benefits and are paying higher prices during this period. Thus, it is more relevant to use the average daily food expenditure from the reference period.

[16] The CV for each group is calculated using its specific market demand coefficient estimates.

Table 3.5: Exact Compensating Variation Estimates

	Full Sample	Under 100% Poverty	Financially Insecure	Do not Shop for Price	Travel Time < 15 Min
Days 2-3	-0.61	0.42	-1.41	-2.15	0.15
Days 4-6	0.16	0.87	-0.12	-0.39	0.55
Days 7-9	-1.28	-1.29	-1.29	-0.47	-1.38
Days 10-12	-1.54	-2.16	-2.85	-3.00	-1.73
Days 13-15	-2.45	-4.59	-4.61	-4.70	-1.77
Days 16-18	-3.39	-5.58	-6.17	-4.84	-3.56
Days 19-21	-4.94	-5.52	-5.16	-8.38	-6.06
Days 22-24	-3.65	-6.21	-4.96	-8.03	-4.13
Days 25-27	-3.10	-5.72	-3.99	-3.86	-4.14
Days 28-30	-0.30	-2.22	-0.67	-3.01	-2.77
Observations	18,749	9,629	14,774	6,047	12,700

average day 0/1 daily food expenditure) and \$6.21 (8% of the average day 0/1 daily food expenditure) per day, respectively. Financially insecure households and those who spend less than 15 min traveling their primary store experience slightly smaller welfare changes.

3.5 Discussion and Policy Implications

This study finds that part of the SNAP benefit cycle – the observation that expenditures spike upon benefit receipt – can be explained by a decline in prices paid by recipients. After controlling for a host of price-seeking strategies (e.g., coupon usage, store format, bulk purchases), we find that prices fall precipitously before bottoming out at the end of the third week at 22% lower than prices paid on the first two days of the benefit month.

It is important to note that our estimated price changes are conditional on a host of price-seeking strategies (e.g., coupon usage, store format, bulk purchases, branding), as well as fixed product characteristics (e.g., cheesecake versus milk) and household fixed effects. This implies that other time-varying factors (e.g., other price-seeking behavior unobservable to the researcher, buying lower quality foods) must be driving the decline in prices. Specifically, we find some evidence indicating that the decline in unit prices over the benefit cycle is likely to be driven by within food type substitutions from higher-quality to lower-quality foods.

We use this decrease in price to calculate a money-metric change in welfare. The idea is to ask, how much more money would the average household need on the day t (e.g., days 19-21) during the benefit month to purchase the food with the quality purchased on the day benefits arrive? Using the end of the third week as an example, when prices reach their lowest, we calculate this value to be \$4.94 per day, or 6.6% of the average daily spending on the first two days of the month.

A leading hypothesis in the SNAP cycle literature is that SNAP households exhibit hyperbolic discounting (sometimes interpreted as impatience). The basic idea is that the desire to consume today over later days in the month can lead households to over-purchase at the beginning of the month, leaving themselves with fewer resources later in the month. Our results indicate this squeeze on resources can lead to purchasing lower-quality (less-expensive) foods towards the end of the benefit cycle. SNAP's goal is to improve food security of low-income households and to help these households achieve a healthful diet. Purchasing lower quality foods towards the month's end such as those with high saturated fats and added sugar could reduce diet quality of SNAP participants and thus have implications for their health-outcomes such as obesity rates. Indeed, one mechanism that could explain the changes in body mass index (BMI) levels due to changes in the SNAP benefits is transition from higher-quality (more expensive) to lower-quality (less expensive) foods and vice versa (see, e.g., Almada and Tchernis, 2016; Drewnowki and Specter, 2004; Meyerhoefer and Yang, 2011).

This result suggests that efforts to help participants smooth the prices paid over the benefit cycle, such as budgeting, planning purchases, and financial literacy may help improve welfare and in turn, food security. Further, there exists growing evidence that budgeting, planning and financial literacy is associated with healthier food purchases (Chang et al., 2016; Lyford et al., 2016). Indeed, we find that prices are relatively stable for those with longer commutes (i.e., more likely to plan ahead) and who frequent grocery stores for their low prices (i.e., more likely to be budget conscious). USDA's SNAP-Ed program could further focus on budgeting and planning education, not just nutrition education and food budgeting. By targeting educational efforts through programs such as SNAP-Ed, benefit redemption could become more efficient with no increase in benefit allotment.

Chapter 4

Aging out of WIC and Child Nutrition: Evidence from a Regression Discontinuity Design

4.1 Introduction

Poor dietary quality in childhood may impair growth and development and affect dietary behaviors in adulthood (Carlson et al., 2003; Beydoun and Wang, 2009). Subsequently, longer-term poor nutrition is associated with major causes of cardiovascular disease, type 2 diabetes, and cancer (Jemal et al., 2008). The goal of the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is to improve health and nutritional well-being of pregnant and postpartum women, infants and children up to the age of 5 years who are low income and nutritionally at risk. A body of literature evaluates the extent to which WIC has accomplished its goal. In general, these studies find that WIC participation is associated with improving birth outcomes and nutritional intake (see, Currie, 2003, and Colman et al., 2012 for a review of this literature).

Yet, there are three major limitations with this literature. First, despite the fact that children (aged 1 to 4 years) comprise over half of all WIC participants, much of the literature has focused on maternal and infant health (Currie, 2003; Kreider et al., 2016). Second, most studies have employed research designs that compare outcomes for WIC participants and non-participants. However, since selection into WIC is non-random (Besharo and Geramins, 2001; Bitler and Currie, 2005; Hoynes et al., 2011; Kreider et al., 2016) such comparisons lead to biased estimates of the program's effect.[1] Third, the current literature has only focused on estimating the *average* effects of WIC participation. While estimating the average effects could provide useful information for many policy applications, it may limit what we can learn about the heterogeneity in the WIC's effects, unless the program affects all parts of the outcome distribution in the same way.

Existing studies have used a variety of non-experimental approaches to account for non-random selection into WIC. Some studies have employed a multivariate regression analysis to control for a detailed set of personal characteristics, such as demographic characteristics of the mother and socioeconomic factors of the family, while restricting their samples to more homogenous WIC participant and non-participant women in terms of important observed characteristics (e.g., Bitler and Currie, 2005; Joyce et al., 2005).[2] Although this approach could help mitigate the issue of selection bias, it does not fully

[1]Depending on the direction of selection into the program, the effects of WIC could be overestimated or underestimated. If WIC participants are positively selected (e.g., they are healthier or have better access to health care) then the program's impact might be biased upward. Conversely, if WIC participants are negatively selected, then such comparisons might understate the WIC's effect. By comparing characteristics of WIC and non-WIC mothers, Bitler and Currie (2005) suggest that WIC mothers are negatively selected from the pool of eligible mothers.

[2]Bitler and Currie (2005) limited their sample to women who had Medicaid-funded deliveries. Joyce et al. (2005), on the other hand, restricted their sample to women who initiated prenatal care during the first trimester of their pregnancy.

eliminate the problem as these comparisons may still suffer from omitted variables bias. Other studies have used maternal fixed-effects models on a sample of siblings to control for unobserved family background characteristics (e.g., Kowaleski-Jones and Duncan, 2002; Chatterji et al., 2002; Foster et al., 2010). Results from these studies, however, could be underestimated due to measurement error, spill-over in the effects of WIC from the participating siblings to the nonparticipating ones, or changes in family conditions between births (Bitler and Currie, 2005; Hoynes et al., 2011). Another set of studies have used geographic variations in eligibility and benefit rules across states as instrumental variables for WIC participation (e.g., Brien and Swann, 2001; Chatterji et al., 2002). Although WIC is a state-run program, there is very little geographic variation in either eligibility requirements or benefit levels. Thus, these instruments have limited power in predicting WIC participation (Bitler and Currie, 2005; Hoynes et al., 2011).

In this study, we estimate effects of *aging out* of WIC on child's dietary quality, as quantified by the Healthy Eating Index 2010 (HEI-2010), as well as rates of household, adult, and child food insecurity. According to federal WIC eligibility criteria, children remain eligible for WIC up to the age of 5 years and in the month following their fifth birthday (i.e., at the age of 61 months) WIC eligibility ends—presumably because the vast majority of children are expected to attend kindergarten and elementary school by this age, and thus transition into federal school meal programs such as the National School Lunch Program (NSLP).[3] Using a regression discontinuity design (RDD), we exploit this age-related discontinuity in WIC participation and compare child's outcome (e.g., diet quality) on either side of the age cutoff point of 61 months. Our main

[3]According to the U.S. Census Bureau (2015), in 2014, 2.2% of 3-year-old children attended kindergarten, whereas 75% of the 5-year olds were enrolled in either kindergarten or elementary school.

identifying assumption is that observed and unobserved determinants of outcome vary continuously around the age cutoff of point. Under this assumption, a change in the outcome at the age of 61 months can be interpreted as the effect of aging out of WIC.

This article is perhaps closet in spirit to the recent work by Arteaga et al. (2016). To estimate the effects of becoming age-ineligible for WIC on the rates of household food insecurity, the authors use a RDD and estimate changes in the food insecurity rates as children age out of WIC in a sample including children who were receiving WIC benefits since their fourth birthday and had not yet started started kindergarten. More specifically, using data from Early Childhood Longitudinal Study Birth Cohort (ECLS-B), the authors use a so-called *sharp* RDD and find that becoming age-ineligible for WIC leads to an increase in the rates of 30-day household food insecurity. This impact is mainly driven by "later school-starters" in the sample.

When such longitudinal data are not used, as in our study, using a sharp RDD will lead to biased estimates of the program's true effects. In other words, since with non-longitudinal data the sample cannot be restricted to only WIC participants, households with WIC age-eligible children are still able to choose to participate in the program — not all age-eligible children participate in WIC. To deal with this issue of "imperfect compliance" to WIC eligibility, we utilize the exogenous assignment to WIC by the child's age as a natural instrumental variable for WIC participation. In other words, we employ a so-called *fuzzy* RDD to estimate the effects of aging out of WIC on both child's diet quality and food insecurity rates.

Further, in this study we go beyond estimating the average treatment effect of aging out of WIC and allow for heterogeneous impacts across the diet quality distribution.

That is, by taking a distributional approach, we investigate whether aging out of WIC affects children who are prone to low-quality diets (perhaps due to parental or environmental factors) differently than those who are prone to higher-quality diets. Clearly, due to unobserved differences between WIC children such as differences in the intakes of foods and nutrients targeted by WIC food packages, effects of aging out of WIC could vary throughout the outcome distribution. For example, we may expect aging out of WIC to have larger adverse effects on lower-quality diets if the risk of inadequate intakes of food items targeted by WIC (e.g., fruits and vegetables) is likely to be greater towards lower parts of the diet quality distribution. By employing a quantile regression estimator within a fuzzy RDD framework, we estimate the effects of aging out of WIC at different points of the dietary quality distribution as we move from low-quality diets to high-quality diets.

Drawing on the data from the National Health and Nutrition Examination Survey (NHANES), we find that aging out of WIC on average has a fairly substantial negative effect of about 10 HEI-2010 points (20%) on child's overall diet quality. By examining the major sub-categories of HEI-2010, we find that this effect is primarily driven by the adequacy foods (i.e., foods for which higher intakes indicate better diet quality) as opposed to the moderation (i.e., foods for which lower intakes indicate better diet quality) foods. We find no significant impact on the percentage of total energy intake from added sugar and the percentage of total energy intake from saturated fat. Similarly, we find no significant effects on the rates of food insecurity. Our distributional approach expands our understanding of the effects of losing WIC benefits beyond the mean effects. Specifically, we find that the estimated adverse effects of aging out of

WIC are largest for children with the lowest-quality diets—a finding relevant to food policymaking as WIC appears to have the largest beneficial effects on children prone to the lowest-quality diets. Lastly, our findings indicate that transition into school meal programs may pick up some of the otherwise decreases in diet quality due to aging out of WIC.

The rest of this chapter proceeds as follows. Section 4.2 provides more details about the WIC program. Section 4.3 describes our data as well as measurements of dietary quality and food insecurity. Section 4.4 outlines our empirical methodologies. Section 4.5 presents the main results. In section 4.6, we provide concluding remarks and derive policy implications.

4.2 Background

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), the third-largest federally-funded food and nutrition assistance program in the U.S., provided over 7 million individuals with nearly $5.6 billion in benefits in 2017 (FNS-USDA, 2018a). WIC eligibility is limited to three broad groups: pregnant and postpartum women, infants up to the age of 1 year, and children aged 1 to 4 years. In 2014, 23.6% of WIC participants were women, 23% were infants, and 53.3% (more than half) were children (FNS-USDA, 2018b).[4] In addition to this categorical eligibility, individuals must live in households with family income below 185% of the federal poverty line (FPL), or be adjunctively eligible through participation in another welfare program

[4]Among children, 19.6% were 1-year old, 13.8% were 2-years old, 12.3% were 3-years old, and 7.6% were 4-years old.

such as Medicaid, Temporary Assistance to Needy Families (TANF), or Supplemental Nutrition Assistance Program (SNAP). They must also be certified to be at nutritional risk by a health care professional.[5]

The goal of WIC is "to improve birth outcomes, support the growth and development of infants and children, and promote long-term health in all WIC participants" (FNS-USDA, 2018c). To achieve this goal, WIC provides participants with non-monetary benefits in the forms of food package, nutrition education, and health referrals. Food packages are typically provided on a monthly basis in the form of vouchers to purchase specific foods that are rich in nutrients found to be lacking in the diets of the WIC target population (iron, calcium, protein, and vitamins A and C).[6]

Similar to other federal nutrition assistance programs, food and services provided by WIC must align with the Dietary Guidelines for Americans (DGA) (NASEM, 2017). Since the DGA are revised every five years, review and revision of nutrition assistance programs are required, accordingly. In December 2007, following the recommendations by the Institute of Medicine (IOM), WIC food packages were revised the first-time since the program's inception to align the food packages with the 2005 DGA as well as infant feeding guidelines by the American Academy of Pediatrics (AAP). The update was required to be in effect by October 2009 and included several changes such as introduction of a cash-value voucher to purchase a variety of fruits and vegetables, addition of whole-wheat bread, eliminating juice from the infant food packages, imposing restric-

[5]Five major types of nutritional risk for WIC eligibility are recognized by federal regulations (see, Oliveira and Frazão, 2015). In practice, almost all categorically eligible applicants are certified to be at risk due to an inadequate dietary patterns even if other risks criteria are not identified (Bitler et al., 2003).

[6]In some states benefits are issued through electronic benefit transfer (EBT) cards and all state are required to migrate to EBT systems by 2020.

tion on the fat content of milk, and reducing the amount of milk that can be replaced by cheese. Currently, the food package for children[7] includes juice, milk, breakfast cereal, eggs, fresh vegetables and fruits, whole wheat or whole grain bread, and legumes and/or peanut butter (FNS-USDA, 2018d).[8] In 2010, the average monthly cost of this food package was about $37.

4.3 Data

To study the effects of aging out of WIC on diet quality and rates of food insecurity, we use data from the 1999-2014 continuous cycles of the NHANES, conducted by the National Center for Health Statistics, Centers for Disease Control (CDC). Each NHANES cycle is an independently drawn, nationally representative sample of about 10,000 individuals, about half of whom are children. The survey provides rich dietary intake information as well as detailed demographic characteristics. From 1999 to 2002, respondents reported one 24-hour dietary recall and since 2003 an additional day of dietary recall was collected. Day-one dietary recalls were administered in-person by trained interviewers in each survey. Day-two intakes were obtained 3-10 days after the initial interview in a follow-up phone interview. Since we pool data from 1999 to 2014 to ensure sufficient representation of WIC children, we utilize only day-one dietary intake data in the construction of our diet quality measures.

[7]Food packages provided by WIC are not the same for all eligible groups. There are seven food packages which accommodate different physiological state categories of women (i.e., pregnant; breastfeeding; or postpartum, non-breastfeeding), different development ages of infants, and different ages of children (NASEM, 2017).

[8]Individuals are prescribed quantities of foods instead of dollar values, except for the monthly cash value vouchers (currently $8 for children) to buy a variety of fruits and vegetables.

Our analysis focuses on children aged 24-72 months. We further restrict our sample to children who live in households with family income less than 200% of the FPL.[9] Using a self-reported (by the parent/guardian) measure of the child's WIC participation status, we define WIC participants as those who reported receiving WIC benefits at the time of the interview (i.e., current WIC participants). WIC nonparticipants are defined as those who were not on WIC either at the time of interview or during the 12-month period prior to the interview. We also exclude from our sample all children who reported getting at least one complete meal (breakfast or lunch) during a week from school. We refer to this sample as the *no-school-meal* sample. This is particularly important because school meal programs (e.g., the NSLP) are found to improve diet quality of low-income children (Smith, 2017). Therefore, including these children in the sample could confound the effects of aging out of WIC on diet quality and food insecurity rates. However, to test the sensitivity of our results to the potential effects of school meal programs, we also examine a larger sample including children receiving food from school. This sample is referred to as the *full* sample and includes 176 (4.5%) 3-year old, 489 (12.5%) 4-year old, and 17 (0.4%) 5-year-old children who reported receiving food from school.

4.3.1 Measurement of Diet Quality

We quantify dietary quality using the Healthy Eating Index (HEI) which is a measure of diet quality in terms of compliance to the DGA. The HEI has been evaluated as a

[9]Although individuals must have income less than 185% of the FPL in order to be eligible for WIC, due to categorical eligibility households with higher incomes may be eligible for WIC. In April 2014, 1.6% of WIC participants had income at or above 185% of FPL (FNS-USDA, 2015).

valid and reliable measure of diet quality (Guenther et al., 2008, 2014) and is widely used in studies of WIC and other nutrition assistance programs (see, e.g., Basiotis et al., 1998; Hiza et al., 2013; Tester et al., 2016; Gu and Tucker, 2017; Smith, 2017). The original HEI was created in 1995 to measure compliance to the 1990 DGA and since then has been revised several times to reflect key changes in the DGA. In this paper, we use the HEI-2010, corresponding to the 2010 DGA, as a measure of child's overall dietary quality.

The HEI-2010 is a continuous, scalar measure which is calculated as the sum of 12 components based on the per-calorie consumption of various food and nutrients. There are nine *adequacy* components (total fruit, whole fruit, total vegetables, greens and beans, whole grains, dairy, total protein foods, sea food and plant proteins, and fatty acids) for which higher scores indicate higher intakes, and three *moderation* components (refined grains, sodium, and empty calories) for which higher scores reflect lower intakes. Each component assigns a score ranging from 0 to 5 (total fruit, whole fruit, total vegetables, greens and beans, total protein foods, seafood and plant proteins), 0 to 10 (whole grains, dairy, fatty acids, refined grains, sodium), and 0 to 20 for empty calories (calories from solid fats, alcoholic beverages, and added sugars). The total HEI is scored from 0 to 100. Table 4.1 provides exact details of the scoring. Using day-one dietary intake data from the NHANES, we construct the HEI-2010 according to Guenther et al. (2013).

In addition to examining the effects of aging out of WIC on the HEI-2010 total score, as a measure of child's overall diet quality, we also investigate the effects on the two main sub-categories of the HEI-2010—adequacy and moderation. In other words,

Table 4.1: Healthy Eating Index 2010 Components and Scoring Standards

Component	Score Range	Standard for Max Score	Standard for Min Score
Adequacy:			
Total Fruit	[0,5]	$\geq$ 0.8 cup equivalent/1,000 kcal	No Fruit
Whole Fruit	[0,5]	$\geq$ 0.4 cup equivalent/1,000 kcal	No Whole Fruit
Total Vegetables	[0,5]	$\geq$ 1.1 cup equivalent/1,000 kcall	No Vegetables
Greens and Beans	[0,5]	$\geq$ 0.2 cup equivalent/1,000 kcal	No Dark/Green Vegetable or Beans and Peas
Whole Grains	[0,10]	$\geq$ 1.5 oz equivalent/1,000 kcal	No Whole Grains
Dairy	[0,10]	$\geq$ 1.3 cup equivalent/1,000 kcal	No Dairy
Total Protein Foods	[0,5]	$\geq$ 2.5 oz equivalent/1,000 kcal	No Protein Foods
Seafood and Plant Proteins	[0,5]	$\geq$ 0.8 oz equivalent/1,000 kcal	No Seafood or Plant Proteins
Fatty Acids	[0,10]	(PUFAs + MUFAs)/SFAs* $>$ 2.5	(PUFAs + MUFAs)/SFAs $\leq$ 1.2
Moderation:			
Refined Grains	[0,10]	$\leq$ 1.8 oz equivalent/1,000 kcal	$\geq$ 4.3 oz equivalent/1,000 kcal
Sodium	[0,10]	$\leq$ 1.1 g equivalent/1,000 kcal	$\geq$ 2.0 g equivalent/1,000 kcal
Empty Calories	[0,20]	$\leq$ 19% of energy	$\geq$ 50% of energy

*PUFAs: polyunsaturated fatty acids. MUFAs: monounsaturated fatty acids. SFAs: saturated fatty acids.
Source: Recreated from Guenther et al. (2013)

we want to better understand whether the adverse effects of losing WIC benefits if any, are due to lower intake of adequacy foods or higher intake of moderation foods. As well, because WIC food packages are arguably designed to reduce individuals' energy intake from added sugar and fat, for instance by restricting the amount of added sugar in breakfast cereal and providing low-fat milk,[10] we examine changes in children's intakes of added sugar and saturated fat in terms of percent of total energy intake as they age out of WIC.

[10]Indeed, previous research suggests that WIC is associated with improved diets among children as measured by the intakes of added sugars and fats (see, Colman et al., 2012).

4.3.2 Measurement of Food Insecurity

Official food insecurity rates in the U.S. for households with children, over a 12-month period, are calculated using a series of 18 questions posed in the Core Food Security Module (CSFM). These questions are designed to manifest food insecurity in a manner consistent with how the presence of food insecurity is perceived by experts (see, Gundersen and Kreider, 2008). In the NHANES household interview, households with children respond to all CSFM questions which refer to all household members, not just NHANES participants—10 questions refer to adults and 8 questions to children.

Following official definitions, we classify a household with children as "food insecure" if the respondent responds affirmatively to three or more questions and "very low food secure" if the respondent responds affirmatively to eight or more questions. Similarly, we categorize an adult (child) in the household as "food insecure" if the respondent responds affirmatively to two (one) or more questions and "very low food secure" if the respondent responds affirmatively to six (five) or more questions. We then estimate the effects of aging out of WIC on these rates of food insecurity.

4.3.3 Summary Measures

Table 4.2 displays summary statistics for our *no-school-meal* sample. The first column presents the information for all children, the second column for WIC age-eligible children (aged 24-60), and the third column for WIC age-ineligible children (aged 61-72). Panel A shows the socio-demographic characteristics, panel B measures of dietary quality, and panels C and D measures of food insecurity.

In panel A, we build two indicator variables which we will make use of in the empir-

Table 4.2: Summary Statistics by Child's Age in Months, No-School-Meal Sample

	Ages 24-72	Ages 24-60	Ages 61-72	*p*-value
Panel A: Demographics				
T = $1\{\text{Age} \geq 61 \text{ months}\}$	0.12	0.00	1.00	-
	(0.01)	(0.00)	(0.00)	
D= $1\{\text{Child off WIC}\}$	0.62	0.57	1.00	0.00
	(0.01)	(0.02)	(0.00)	
Child Female	0.48	0.49	0.40	0.06
	(0.01)	(0.01)	(0.04)	
Household Size	4.66	4.65	4.78	0.26
	(0.04)	(0.04)	(0.12)	
Income-to-Poverty Ratio	0.99	0.98	1.04	0.24
	(0.02)	(0.02)	(0.05)	
Reference Female	0.54	0.54	0.54	0.95
	(0.01)	(0.01)	(0.05)	
Panel B: Measures of Diet Quality				
HEI-2010	56.49	56.72	54.80	0.09
	(0.36)	(0.37)	(1.09)	
Adequacy Score	26.43	26.54	25.62	0.14
	(0.22)	(0.24)	(0.59)	
Moderation Score	30.06	30.18	29.18	0.16
	(0.21)	(0.20)	(0.71)	
%Energy from Added Sugar	13.78	13.54	15.49	0.00
	(0.24)	(0.25)	(0.60)	
%Energy from Saturated Fat	11.87	11.83	12.13	0.46
	(0.13)	(0.13)	(0.40)	
Panel C: Rates of Food Insecurity				
Household	0.30	0.30	0.28	0.84
	(0.01)	(0.01)	(0.04)	
Adult	0.26	0.26	0.26	0.92
	(0.01)	(0.01)	(0.04)	
Child	0.15	0.15	0.13	0.32
	(0.01)	(0.01)	(0.02)	
Panel D: Rates of Very Low Food Security				
Household	0.08	0.08	0.08	0.80
	(0.01)	(0.01)	(0.02)	
Adult	0.09	0.09	0.10	0.46
	(0.01)	(0.01)	(0.03)	
Child	0.01	0.01	0.01	0.43
	(0.00)	(0.00)	(0.00)	
Observations[a]	3,223 [3,199]	2,922 [2,900]	301 [299]	

[*] *Notes*: Standard errors in parentheses. All calculations use survey weights.
[a] Number of observations for the sample in panels C and D in brackets.

ical methods section. T is an indicator variable for the child's age and takes on a value of 1 if child's age is greater than or equal to 61 months. In total, 12% of children in the sample age 61 months and over. D is a binary variable that takes on a value of 1 for WIC non-participants and 0 otherwise. Overall, 62% of children are classified as WIC non-participants. The reported WIC non-participation rate for age-eligible children (i.e., the second column) is 57%, suggesting that child's age is not perfectly correlated with WIC participation.[11] With respect to other demographic characteristics, the sample seems to be balanced on both sides around the cutoff point of 61 months, except for child's gender. For example, the average household size is a little less than 5 and the average household income is about 100% of the FPL.

In panel B, we see that age-eligible children on average tend to have a better diet quality than their age-ineligible counterparts. For instance, we observe that their HEI-2010 score is higher and a smaller share of their total energy (kcal) intake is from added sugar. Moving down to panel C, we see that rate of household food insecurity is almost identical between age-eligible and age-ineligible groups with about 30% of households being classified as food insecure. We see similar patterns among adults and children with the latter being less likely than the former to be food insecure. Similarly, in panel D, rates of very low food security are almost identical between age-eligible and age-ineligible children. As expected, however, the prevalence of very low food security is lower.

[11]The high reported non-participation rate can be explained by several factors. While some households may simply be unaware of their eligibility, for others the cost of program participation may outweigh its benefits (Kreider et al., 2016). Further, since WIC is not an entitlement program, in principle, even eligible households are not guaranteed to receive benefits. In recent years, however, there has been enough funding to serve all eligible households that sought benefits (U.S. GAO, 2013). Lastly, self-reported measures of WIC participation are underreported (Kreider et al., 2016).

Table 4.3 reports summary statistics for the *full* sample. As one can see, overall 23% of the sample age at least 61 months. The larger share of WIC age-ineligible children in this sample reflects the fact that vast majority children have started attending kindergarten and/or elementary school and enrolled in school meal programs by the time they are 5-years old. As can be seen in panel A, overall 17% percent of children in the full sample have reported receiving at least one complete meal per week from school. These children constitute 57% of the WIC age-ineligible group and are excluded from the no-school-meal sample. In addition, with more observations on either side of the cutoff point of 61 months, we can see that sample is more locally balanced and no significant difference is observed between demographics of the two groups. With respect to the diet quality, on average WIC age-ineligible children seem to have a slightly higher-quality diet than those in the no-school-meal sample. One plausible explanation could be due to the potential effect of school meal programs on diet quality. Lastly, similar to the no-school-sample food security rates are almost identical between WIC age-eligible and age-ineligible children.

Figure 4.1 shows the unconditional empirical cumulative distribution functions (CDF) of the HEI-2010. Panel A presents the CDFs by child's WIC participation status in the no-school-meal sample and panel B by child's age. Likewise, panels C and D for the full sample. In panel A, we see that WIC participants seem to have a better diet quality than WIC nonparticipants across the distribution of HEI-2010, with larger differences within the first and third quartiles of the distribution. Similarly, in panel B, we observe that distribution of diet-quality of WIC age-eligible children to dominate that of the age-ineligible children at the first order. Unlike panel A, however, we observe that the

Table 4.3: Summary Statistics by Child's Age in Months, Full Sample

	Ages 24-72	Ages 24-60	Ages 61-72	p-value
Panel A: Demographics				
T = 1{Age $\geq$ 61 months}	0.23	0.00	1.00	-
	(0.01)	(0.00)	(0.00)	
D= 1{Child off WIC}	0.67	0.57	1.00	0.00
	(0.01)	(0.02)	(0.00)	
On School Meal	0.17	0.05	0.57	0.00
	(0.01)	(0.01)	(0.03)	
Child Female	0.50	0.49	0.51	0.61
	(0.01)	(0.01)	(0.02)	
Household Size	4.67	4.65	4.71	0.52
	(0.04)	(0.04)	(0.07)	
Income-to-Poverty Ratio	0.98	0.98	0.97	0.95
	(0.01)	(0.02)	(0.03)	
Reference Female	0.55	0.55	0.57	0.41
	(0.01)	(0.01)	(0.03)	
Panel B: Measures of Diet Quality				
HEI-2010	56.41	56.77	55.21	0.02
	(0.33)	(0.37)	(0.62)	
Adequacy Score	26.44	26.59	25.94	0.13
	(0.21)	(0.23)	(0.37)	
Moderation Score	29.97	30.18	29.27	0.02
	(0.19)	(0.20)	(0.38)	
% Energy from Added Sugar	13.93	13.54	15.21	0.00
	(0.22)	(0.24)	(0.39)	
% Energy from Saturated Fat	11.80	11.80	11.79	0.94
	(0.11)	(0.13)	(0.20)	
Panel C: Rates of Food Insecurity				
Household	0.30	0.30	0.30	0.87
	(0.01)	(0.01)	(0.02)	
Adult	0.27	0.27	0.26	0.94
	(0.01)	(0.01)	(0.02)	
Child	0.15	0.15	0.14	0.51
	(0.01)	(0.01)	(0.02)	
Panel D: Rates of Very Low Food Security				
Household	0.09	0.08	0.09	0.72
	(0.01)	(0.01)	(0.01)	
Adult	0.10	0.09	0.11	0.34
	(0.01)	(0.01)	(0.02)	
Child	0.01	0.01	0.01	0.42
	(0.00)	(0.00)	(0.00)	
Observations[a]	3,905 [3,875]	3,102 [3,077]	803 [798]	

[*] *Notes*: Standard errors in parentheses. All calculations use survey weights.
[a] Number of observations for the sample in panels C and D in brackets.

gap between the CDFs is larger below the median. Similar patterns for the full sample are observed in panels C and D. The gaps between the CDFs, however, are smaller which is expected given the potential positive effects of the school meal programs on diet quality.[12]

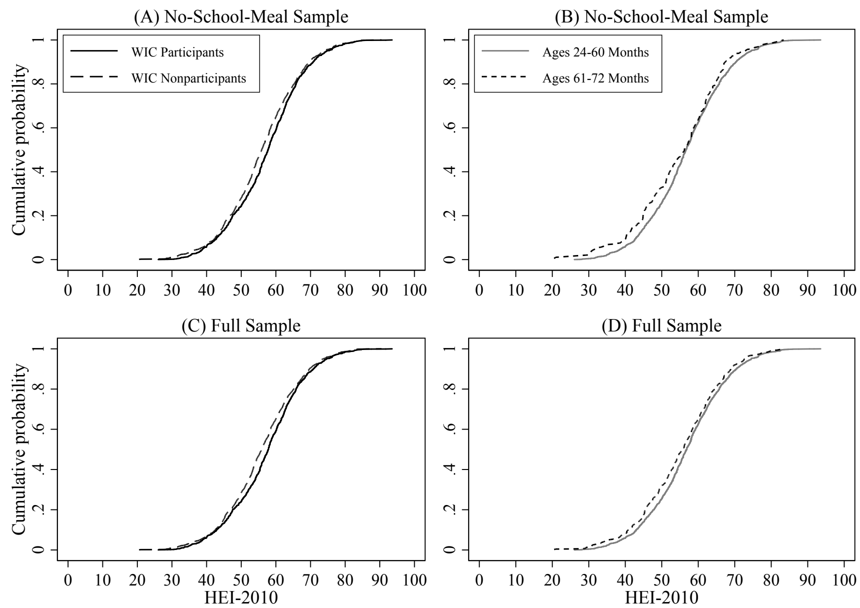

Figure 4.1: Unconditional cumulative distribution functions (CDF) of HEI-2010 by WIC participation status and child's age

The descriptive analyses presented in tables 4.2 and 4.3 and figure 4.1 cannot be used to draw causal inferences about the effects of aging out of WIC on mean or any other points of the outcome distribution. Because the participant and non-participant comparison presented in figure 4.1, panel A, suffers from omitted variable bias due to non-random selection. In panel B, although child's age randomly assigns children to WIC eligible and ineligible groups, households could still refuse to enroll their children

[12]The empirical CDFs for sub-categories of diet quality are presented in appendix figures C.1 and C.2.

in the program. That is, self-selection into WIC is still an issue among age-eligible children. In the following section, we outline our empirical approach to deal with these problems.

4.4 Empirical Methods

4.4.1 Identification

In order to understand the difficulties inherent in estimating the causal effect of WIC on diet quality or a food insecurity rate, it is useful to specify the following mean regression model:

$$Y_i = \beta_0 + \beta_1 W_i + X_i' \gamma + u_i + \epsilon_i, \tag{4.1}$$

where Y_i is either a measure of diet quality or food insecurity rate for child i, W_i is an indicator for WIC participation which takes on a value of 1 if child i is *on* WIC and 0 otherwise. X_i is a vector of observed characteristics for child i, u_i is the child i's unobserved characteristics, and ϵ_i is an idiosyncratic error term. β_1 captures the average treatment effect of WIC participation on the outcome. A primary obstacle to identification, however, is that selection into WIC is non-random. Specifically, selection into WIC on the basis of unobserved characteristics by eligible individuals may result in a non-zero correlation between treatment and unobserved characteristics, cov$(W, u) \neq 0$. Thus, the Ordinary Least Squares (OLS) estimate of β_1 lead to biased estimates of the program's true effects.

To recover the causal effects of WIC, we exploit the fact that WIC participation is a discontinuous function of child's age. Figure 4.2 displays the share of age-eligible children participating in WIC by child's age. As one can see, in both samples, the probability of WIC participation drops significantly at the cutoff point of 61 months. Assuming that child's observable and unobservable characteristics vary continuously in the vicinity of the cutoff point, we can identify the effects of aging out of WIC by comparing child's outcomes just below and just above the threshold. This identification strategy is referred to as a regression discontinuity design (RDD).

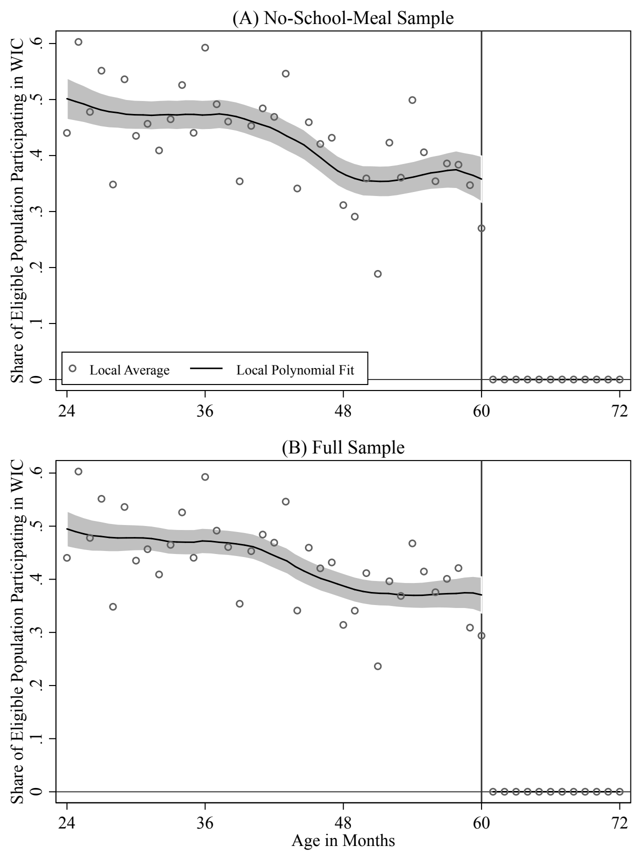

Figure 4.2: Discontinuity in WIC participation by child's age in months

If WIC participation is solely determined by the "assignment variable" (i.e., child's age), under the assumption that the outcome variable is a continuous function of the assignment variable, any discontinuity in the outcome in proximity of cutoff point can be interpreted as the effect of treatment (i.e., aging out of WIC). In figure 4.3, panel A, we see that mean HEI-2010 shows a (insignificant) drop at the cutoff, whereas in panel B which includes children on school meal programs no discontinuity is observed.[13] However, this simple comparison of mean on either side of the cutoff point – referred to as a *sharp* regression discontinuity design (SRDD) – cannot identify true effects of aging out of WIC on the outcome. Because, as mentioned earlier and can be seen in figure 4.2, compliance of age-eligible children to WIC participation is imperfect. That is, not all age-eligible children participate in WIC.

To deal with endogeneity problems caused by the "imperfect compliance," as is standard in the literature, we use an instrumental variable (IV) estimation method by utilizing the exogenous assignment to WIC participation by child's age as an instrumental variable for WIC participation (see, Angrist and Pischke, 2008; Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Specifically, we use a *fuzzy* regression discontinuity design (FRDD) in which the probability of receiving the treatment does not need to be a deterministic function of the assignment variable. Instead, within a FRDD framework assignment to treatment is probabilistically determined as a discontinuous function of the assignment variable (see, Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

[13]Discontinuity graphs for other outcomes are presented in appendix figures C.3-C.6.

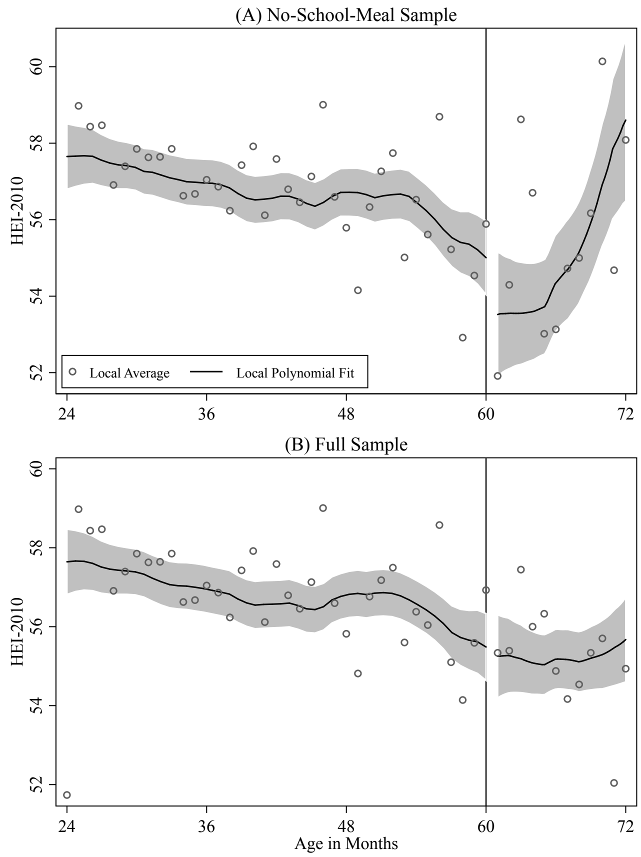

Figure 4.3: Discontinuity in HEI-2010 by child's age in months

4.4.2 Estimation

4.4.2.1 Average Effects

We employ a parametric RDD method and formally estimate the average effects of aging out of WIC using the following Two-Stage Least Squares (TSLS) model:

$$Y_i = \beta_0 + \beta_1 D_i + \sum_{k=1}^{K} \alpha_k \tilde{Age_i}^k + \sum_{k=1}^{K} \lambda_k T_i \tilde{Age_i}^k + X_i'\gamma + \eta_y + U_i, \quad \forall k = 1, ..., K,\ K \in \{1, 2, 3\} \tag{4.2}$$

$$D_i = \delta_0 + \delta_1 T_i + \sum_{k=1}^{K} \phi_k \tilde{Age_i}^k + \sum_{k=1}^{K} \theta_k T_i \tilde{Age_i}^k + X_i'\omega + \eta_y + V_i, \quad \forall k = 1, ..., K,\ K \in \{1, 2, 3\} \tag{4.3}$$

where Y_i is either a measure of child's diet quality or food insecurity rate as listed in table 4.2. X_i is a vector of covariates including a dummy for child's gender, a dummy for child's reference person gender, and second-order polynomials of household size[14] as well as household's income-to-poverty ratio. D_i, as defined before, is an indicator variable for non-WIC children and is instrumented for by T_i, an indicator variable for children aged at least 61 months. $\tilde{Age}$ is a centered age variable, $\tilde{Age} = age - 61$, representing the number of months before and after the age threshold of 61 months. η_y is a set of indicators for pooled NHANES cycles and lastly, U_i and V_i are idiosyncratic error terms. In equation 4.2, under the assumption that the outcome variable is a continuous

[14]Results are robust to using more flexible functions of household size (e.g., household size dummy variables).

function of the assignment variable (i.e., $\tilde{Age}$), the coefficient estimate for β_1 captures the effect of aging out of WIC.

Equation 4.2 models dietary quality as a flexible function of the child's age.[15] Choosing the correct order of the polynomial of the assignment variable, however, is one of the challenges of the parametric RDD approach. One solution is to try and report a number of specifications to explore the sensitivity of the results to the degree of polynomials (see, Lee and Lemieux, 2010). Here, we use linear (i.e., K=1), quadratic (i.e., K=2), and cubic (i.e., K=3) models for our analysis.[16] We then use the Akaike information criterion (AIC) of the model selection to choose our preferred specification. It turns out that the first-order and second-order polynomial models generally provide a better fit.

4.4.2.2 Distributional Effects

To estimate the distributional effects of aging out of WIC, we use a linear-in-parameter quantile regression model corresponding to equations 4.2 and 4.3:

$$\begin{aligned} Y_i = \beta_0(U_i) + \beta_1(U_i)D_i + \sum_{k=1}^{K} \alpha_k(U_i)\tilde{Age}^k + \sum_{k=1}^{K} \lambda_k(U_i)T_i\tilde{Age}^k + \eta_y, \\ \forall k = 1, ..., K,\ K \in \{1, 2\}, \\ U|T, \tilde{Age} \sim \text{Uniform}(0, 1) \end{aligned} \tag{4.4}$$

[15]Once we control for a smooth function of the child's age, the side of the cutoff on which the child happens to fall can be excluded from the structural equation. Put differently, having controlled for the influence of age in a smooth way, whether or not the age of child exceeds the cutoff value of 61 months affects the outcome only through its effects on the probability of WIC participation (i.e., D).

[16]Although it is common in RDD analysis to control for higher-order polynomials of the assignment variable, we avoid doing so because employing higher-order polynomials could lead to noisy estimates of the treatment effect (Gelman and Imbens, 2017).

$$D_i = f(T_i, \tilde{Age}_i, V_i) \tag{4.5}$$

where the maximum polynomial order is set to be 2 (i.e., linear and quadratic models) and U_i is a non-separable error term also called "rank" variable and is interpreted as unobserved "proneness" for the outcome variable (Doksum, 1974). This rank variable determines the relative position of children with the same observables (e.g., age) throughout the distribution of outcome such that children with relatively higher values of rank (e.g., higher-quality diets) are placed at higher quantiles of the outcome distribution.

It is important to note that U_i in general depends on the treatment status. For instance, if we consider all children with the same age, the median child when all these children are exposed to treatment (e.g., aging out of WIC) need not to be the median child when the treatment is withheld from all of them (Guiteras, 2008). The key identifying assumption, however, behind our quantile RDD analysis is that the ranks of children with the same age does not change systematically between treated and untreated states. This assumption is referred to as *rank similarity* and requires the conditional distribution of ranks to be identical in all treatment states (see, Chernozhukov and Hansen, 2005; Guiteras, 2008).

As an example, suppose there are two children, A and B, with the same age in months, and that child A has a higher proneness for a higher quality diet than child B. We assume child A will be ranked higher than child B in the counterfactual treatment states where they both get WIC, or both do not get WIC. The rank similarity assumption requires that if both children lose WIC, child A cannot experience detrimental effects so great that she would be ranked below B when aging out of WIC. We should note that A or B can experience different effects (in magnitude) from aging out of WIC, but these differences cannot be so large that they "switch" in ranking.

Moreover, because the rank variable determines the conditional quantiles over which estimation occurs, the interpretation of coefficients in the quantile regression is directly tied to preserving the ranking structure. Including covariates in the quantile regression model changes the ranking structure as some parts of the unobserved proneness (i.e., U_i) become observed (Powell, 2016a). This changes the interpretation of the coefficient estimates and they must be interpreted as the effect of treatment on the *conditional* distribution of outcome (Powell, 2016a). We are, however, interested in the impact of aging out of WIC on the *unconditional*[17] distribution of diet quality as unconditional quantile estimates give the desirable interpretation for the policy question at hand – how does aging out of WIC impact children prone to low-quality diets separately from those prone to high-quality diets? Since the key identification assumption behind a RDD analysis is that observable and unobservable characteristics do not vary discontinuously around the cutoff point, then conditioning on covariates is not required for identification.[18] Therefore, to maintain the ranking structure and obtain unconditional quantile treatment effects, we do not include covariates in our quantile regression specification.

To proceed with estimation, we use the "instrumental variable" or "inverse quantile regression" (IVQR) estimator developed in Chernozhukov and Hansen (2006). IVQR

[17]The term "unconditional" used here refers to "mean unconditional on the covariates" but the resulting distribution is still conditional on the treatment variable (see, Powell and Goldman, 2016).

[18]In practice, however, it is common to include covariates in regression models to reduce sampling variability in the RDD estimator (Lee and Lemieux, 2010).

estimator involves minimizing the following objective function:

$$Q(\tau, \beta_1, \alpha_1, \lambda_1, \gamma_1) = 1/n \sum_{i=1}^{n} \rho_\tau(Y_i - \beta_1(\tau)D_i - \alpha(\tau)f(\tilde{Age}) - \gamma_1(\tau)T_i) \tag{4.6}$$

where τ denotes the τ^{th} quantile of the outcome distribution, ρ_τ is the standard quantile check function, $f(\tilde{Age})$ is a flexible function of the assignment variable and its interaction with T as in equation 4.4, and for simplicity of notation we exclude β_0 and η_y from the objective function. As one can see, the instrument T is directly included in the objective function. Intuitively speaking, since the instrument can be excluded from the structural equation, its coefficient in a correctly specified regression of outcome on the treatment variable should be zero. Therefore, the IVQR estimate for $\beta_1(\tau)$ can be obtained by finding a value for β_1 that makes the coefficient on the instrument (i.e., $\gamma_1(\tau)$) as close to zero as possible (Chernozhukov and Hansen, 2006).

In practice, the computation of IVQR estimator is conducted using a grid search optimization procedure (see, Chernozhukov and Hansen, 2008). Inferences are conducted using the analytical estimated variance-covariance matrices following the formulae given in Section 4.3 of Chernozhukov and Hansen (2008).

4.4.3 Test of Assumptions

The fundamental assumption behind a RDD that generates local random assignment result is that individuals are not able to precisely manipulate the assignment variable. Although this assumption cannot be tested directly (because only one observation on the assignment variable is observed per individual), an intuitive test of this assumption can be conducted by investigating whether there is a discontinuity in aggregate distribution of the assignment variable at the cutoff point (see, Lee and Lemieux, 2010). McCrary (2008) proposes a simple procedure for testing whether the density of the assignment variable shows discontinuities around the cutoff.

Figure 4.4 displays the results of the McCrary test for both no-school-meal and full sample. Visual inspection of the graph as well as the estimates of the discontinuities suggest that the difference between the frequency to the right and to the left of the threshold is not statistically significant. Therefore, we fail to reject the null hypothesis that the discontinuity in the density of the child's age at the cutoff is zero, confirming that parents are not likely to (precisely) change the age of their children (for instance, by arguing that their children are younger than they actually are when enrolling in WIC) to keep receiving WIC benefits.

An alternative way to testing the validity of RDD is to examine whether the observable characteristics (i.e., baseline covariates) are locally balanced on either side of the cutoff point. In fact, in the absence of manipulation of the assignment variable, we expect that children just below and above the cutoff point to be very similar in terms of both observables and unobservables. To investigate this issue we conduct both a graphical analysis and a formal discontinuity estimation, replacing the dependent variable in equation 4.3 with each of the baseline covariates in X. However, instead of estimating each equation individually, we run a Seemingly Unrelated Regression (SUR) model where each equation represents a different covariate and test for the joint significance of discontinuity gaps in all equations (see, Lee and Lemieux, 2010).

The results of discontinuity tests for the no-school-meal sample are presented in fig-

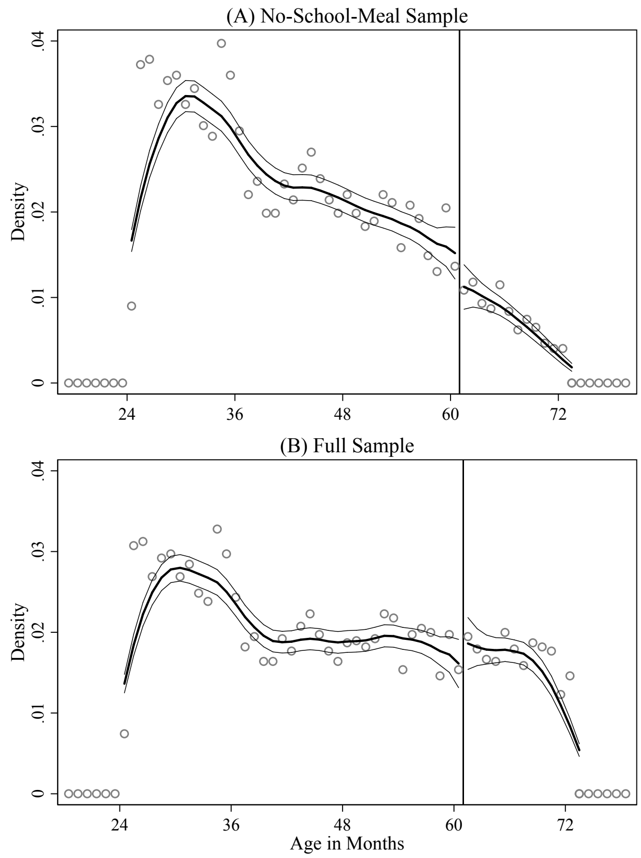

Figure 4.4: McCrary Test for manipulation of the assignment variable

Note: Discontinuity estimates in panels A (-0.24, S.E. = 0.26) and B (0.23, S.E. = 0.20) are calculated using the defaults bandwidths.

ure 4.5 and table 4.4. As one can see in figure 4.5, all demographic characteristics vary smoothly over the threshold of 61 months. Estimation results from linear, quadratic, and cubic specifications and the corresponding *p*-vales from the joint tests of all discontinuities being zero, presented in table 4.4, confirm the graphical evidence. Although in the quadratic specification the income-to-poverty ratio variable drops significantly at the cutoff, in all specifications discontinuity gaps are jointly statistically insignificant.

Discontinuity results for the full sample are shown in appendix figure C.7 and table C.1. We see that with a larger sample, covariates are even more locally balanced. These findings reassure us about the validity of our RDD analysis.

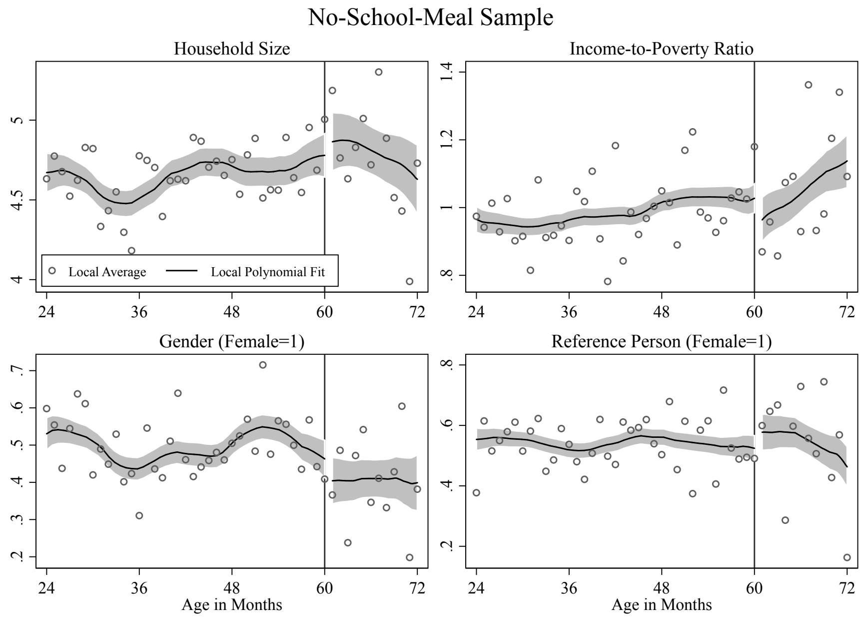

Figure 4.5: Discontinuity in baseline covariates, no-school-meal sample

4.5 Results

4.5.1 Average Effects

We first present our results for the average effects of aging out of WIC on dietary quality measures and rates of food insecurity. Table 4.5 summarizes the estimation results for the HEI-2010 for the no-school-meal sample. Panel A reports the FRDD estimates, panel B SRDD estimates, and panel C the first-stage results. Additionally, in panel D,

Table 4.4: Discontinuities in Baseline Covariates, No-School-Meal Sample

	Order of Polynomial of $\tilde{Age}$		
	1st	2nd	3rd
Child Female	-0.09	-0.16	-0.04
	(0.09)	(0.11)	(0.12)
Income-to-Poverty Ratio	-0.12	-0.20**	-0.18
	(0.08)	(0.10)	(0.11)
Income-to-Poverty Ratio Squared	-0.25	-0.43**	-0.38
	(0.17)	(0.21)	(0.23)
Household Size	0.26	0.07	0.26
	(0.21)	(0.28)	(0.37)
Household Size Squared	2.59	0.96	3.14
	(2.03)	(2.60)	(3.51)
Reference Female	0.11	0.01	0.16
	(0.07)	(0.09)	(0.11)
Joint Test p-values[a]	0.59	0.17	0.50
Observations	3,223	3,223	3,223

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights.
[a] p-values for tests of linear restrictions that all discontinuities are jointly zero.

we report the fuzzy RDD estimates for the periods before and after the 2009 WIC revision. The first two columns present the estimates from the linear model, the third column from the quadratic model, and the last column from the cubic model. For brevity, we only report estimation results for the key parameter, β_1.

As we can see from the first two columns, including covariates in the model does not have much impact on the fuzzy RDD estimates of β_1. This is expected because covariates vary smoothly around the cutoff point. Overall, estimates from the linear specification indicate that on average children experience a fairly large decrease of about

Table 4.5: Average Effects of Aging Out of WIC on HEI-2010, No-School-Meal Sample

	Order of Polynomial of $\tilde{Age}$			
	1st	1st	2nd	3rd
Panel A: Fuzzy RDD				
Off WIC (D)	-11.64**	-10.21**	-9.82	-11.27
	(5.54)	(4.97)	(7.10)	(9.04)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-3.78**	-3.60**	-3.47	-4.22
	(1.78)	(1.76)	(2.53)	(3.40)
Panel C: First-stage Estimates				
Age $\geq$ 61 months (T)	0.32***	0.35***	0.35***	0.37***
	(0.03)	(0.03)	(0.04)	(0.05)
Panel D: Pre-post 2009 Fuzzy RDD				
1999-2008:				
Off WIC (D)	-8.13	-6.65	-8.51	-3.99
	(6.39)	(5.51)	(6.56)	(8.64)
2009-2014:				
Off WIC (D)	-16.40*	-15.80	-11.44	-27.18
	(9.61)	(9.61)	(16.60)	(18.13)
Covariates	No	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
First-stage F-Statistic	93.16	59.57	52.87	49.18
AIC	24931.60	24712.67	24679.70	24823.95
Observations	3,223	3,223	3,223	3,223

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights.

10 HEI-2010 points in their overall diet quality as they age out of WIC.[19] Similar but statistically insignificant effects are estimated using the quadratic and cubic models.

[19]In interpreting these effects, one should note that since participation in WIC similar to other nutrition assistance programs is underreported, our fuzzy RDD estimates are an upper bound of the true effects of aging out of WIC. In other words, if there is no underreporting of WIC participation, the discontinuity in WIC participation at the age of 61 months (i.e., first-stage estimates) would be larger, making the fuzzy RDD estimates smaller.

This finding indicates that our parametric RDD analysis is not sensitive to the order of the polynomial of the assignment variable.

In panel B, we see that SRDD estimates, which ignore the problem of imperfect compliance to the assignment, are consistently smaller than FRDD across all specifications. Overall, SRDD estimates imply that on average diet quality of children aged 61-72 months is about 3.5 HEI-points lower than children aged 24-60 months. The first-stage results, in panel C, confirm that controlling for a flexible function of assignment variable, the instrument T strongly predicts WIC participation status. Moreover, sub-period fuzzy RDD estimates in panel D suggest larger effects on diet quality in the period after the 2009 WIC revision. Given that following the updates in 2009 WIC food packages were shifted towards even healthier foods, the larger effects within this period are expected.

Table 4.6 summarizes the estimation results for the full sample. As one may expect, magnitudes of the effects from both FRDD and SRDD models are smaller than those from the no-school-meal sample. The FRDD estimates from the linear model indicate a (insignificant) decrease of about 3 HEI-2010 points in the full sample as opposed to a 10 point decrease in the no-school-meal sample. This finding indicates that transition into school meal programs may offset the otherwise decreases in diet quality to a large extent.

Table 4.7 reports the estimation results for sub-categories of diet quality for the no-school-meal sample. Since results are not sensitive to the order of polynomial of the assignment variable, for brevity only estimates from the linear model are reported. In the first column, we observe that aging out of reduces adequacy score by about 7 HEI-2010

Table 4.6: Average effects of Aging Out of WIC on HEI-2010, Full Sample

	Order of Polynomial of $\tilde{Age}$			
	1st	1st	2nd	3rd
Panel A: Fuzzy RDD				
Off WIC (D)	-3.28	-2.91	-2.45	-2.83
	(3.56)	(3.42)	(4.93)	(6.17)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-1.11	-1.01	-0.84	-1.00
	(1.20)	(1.18)	(1.71)	(2.20)
Panel C: First-stage Estimates				
Age $\geq$ 61 months (T)	0.34***	0.35***	0.34***	0.35***
	(0.03)	(0.03)	(0.04)	(0.05)
Covariates	No	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
First-stage F-Statistic	93.50	69.78	61.53	57.06
AIC	29453.41	29414.82	29408.37	29419.55
Observations	3,905	3,905	3,905	3,905

* *Notes*: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights.

points. No significant impact, however, is found on the moderation score. This finding suggest that the decrease in overall diet quality is primarily driven by a decrease in the intakes of adequacy foods. In the last two columns we observe increases in the intakes of energy from added sugar and saturated fat. These effects, however, are not statistically significant. In table 4.8 which reports the results from the full sample we again observe that magnitude of the coefficient estimates are smaller than their counterparts in table 4.7 with no significant effects being observed.

Finally, tables 4.9 and 4.10 display the estimation results for the effects of aging out of WIC on rates of food insecurity and very low food security in the no-school-

Table 4.7: Average Effects of Aging Out of WIC on Sub-categories of Child's Dietary Quality, No-School-Meal Sample

	Dependent Variable			
	Adequacy Score	Moderation Score	%Energy from Added Sugar	%Energy from Saturated Fat
Panel A: Fuzzy RDD				
Off WIC (D)	-6.88**	-3.33	0.46	1.82
	(2.73)	(3.11)	(3.06)	(1.88)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-2.43**	-1.18	0.16	0.64
	(0.96)	(1.10)	(1.08)	(0.67)
Covariates	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	3,223	3,223	3,223	3,223

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from linear model are reported.

Table 4.8: Average Effects of Aging Out of WIC on Sub-categories of Child's Dietary Quality, Full Sample

	Dependent Variable			
	Adequacy Score	Moderation Score	%Energy from Added Sugar	%Energy from Saturated Fat
Panel A: Fuzzy RDD				
Off WIC (D)	-1.51	-1.40	-2.64	1.13
	(2.13)	(2.03)	(2.35)	(1.22)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-0.52	-0.48	-0.91	0.39
	(0.74)	(0.70)	(0.81)	(0.43)
Covariates	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	3,905	3,905	3,905	3,905

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from the linear model are reported.

meal sample and full sample, respectively. In both tables, FRDD estimates are obtained as marginal effects from the bivariate Probit estimation of equations 4.2 and 4.3 (see, Nichols, 2011 for more details) and SRDD estimates are marginal effects from the Probit model. Our results suggest that aging out of WIC has no significant effects on the rates of 12-month household/adult/child food insecurity or very low food security.

Table 4.9: Average Effects of Aging Out of WIC on Rates of Food Insecurity, No-School-Meal Sample

	Food Insecurity			Very Low Food Security		
	Household	Adult	Child	Household	Adult	Child
Panel A: Fuzzy RDD						
Off WIC (D)	4.69	11.68	-9.41	-1.47	9.91	0.83
	(13.57)	(12.07)	(9.30)	(4.67)	(7.50)	(0.93)
Panel B: Sharp RDD						
Age $\geq$ 61 months (T)	2.31	5.01	-2.49	0.21	5.80	0.55
	(5.86)	(5.78)	(4.09)	(2.98)	(3.91)	(0.93)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,199	3,199	3,199	3,199	3,199	3,199

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from the linear model are reported.

To check the robustness of our results to the inclusion of children who attend school but are not on school meal in our study sample, we repeated our analysis for a relatively smaller sample of "no-schoolers." Results are reported in the appendix tables C.2-C.4. As we can see similar results to those presented in this section are obtained.

Table 4.10: Average Effects of Aging Out of WIC on Rates of Food Insecurity, Full Sample

	Food Insecurity			Very Low Food Security		
	Household	Adult	Child	Household	Adult	Child
Panel A: Fuzzy RDD						
Off WIC (D)	6.19	9.92	-2.73	3.98	8.35	-2.78
	(8.21)	(7.84)	(6.28)	(5.80)	(5.30)	(3.17)
Panel B: Sharp RDD						
Age $\geq$ 61 months (T)	1.03	3.32	-2.06	1.27	4.55	-1.09
	(4.19)	(4.16)	(3.27)	(2.42)	(2.85)	(0.78)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,875	3,875	3,875	3,875	3,875	3,875

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from the linear model are reported.

4.5.2 Distributional Effects

In this subsection, we present the distributional effects of aging out of WIC on child's measures of dietary quality for the no-school-meal sample.[20] Figure 4.6 presents the the quantile treatment effects of aging out of WIC on the HEI-2010 distribution. In the figure, panel A presents the result from the linear fuzzy IVQR model. Likewise, panel B for the quadratic specification. In each panel, the solid line represents the fuzzy IVQR point estimates, the horizontal dashed line represents the average fuzzy RDD estimates, and the shaded area represents the 90% confidence interval (CI). The quantiles on the x-axis refer to the counterfactual or untreated diet quality distribution, which gives the estimated quantile treatment effects a *ceteris paribus* interpretation. The estimation of

[20]Similar to the average effect results, the estimated distributional effects of aging out of WIC on diet quality for the full sample are smaller than those from the no-school-meal sample. These estimates, however, are highly imprecisely estimated. Thus, they are not reported in this subsection.

IVQR is performed over the parameter space $\Re = [-25, 10]$ using β_1 equally spaced with a step size of 0.1 for quantiles $\tau = 5$ to $\tau = 95$ at 5-unit increments.

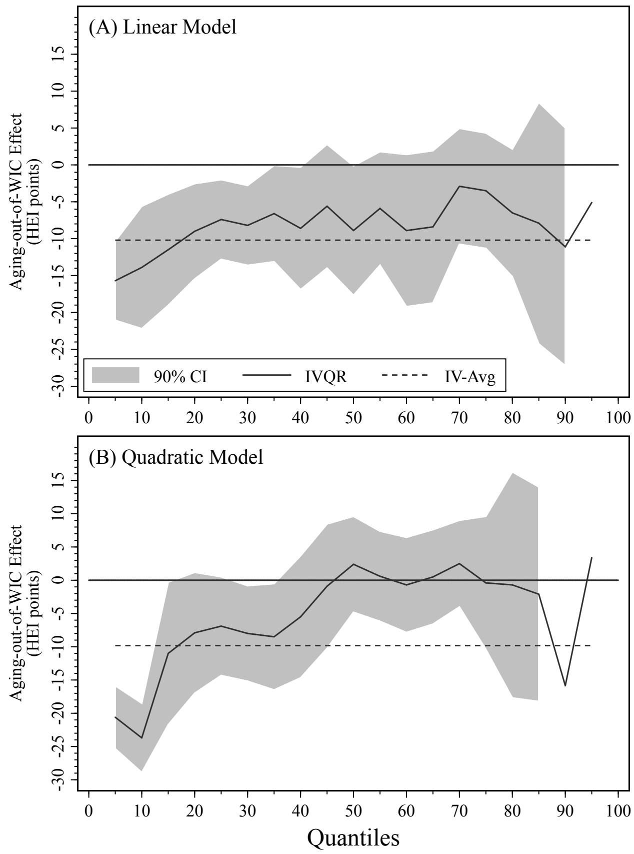

Figure 4.6: Distributional effects of aging out of WIC on HEI-2010

In panel A, we observe large significant effects at lower quantiles of the HEI-2010 distribution (i.e., lower-quality diets). As we move toward the the higher quantiles (i.e., higher-quality diets) magnitudes of the effects slightly shrink and they are no longer statistically significant. Similarly, in panel B, we see larger negative effects at lower-quality diet quantiles and as we move across the distribution these effects become smaller and statistically insignificant.

In figure 4.6, the IVQR estimates for some high-quality diet quantiles ($\tau = 95$ in panel A and $\tau = 90$ and 95 in panel B) are highly imprecisely estimated, and thus for presentation purposes the CIs for these point estimates are not drawn. In figure 4.7, we plot the objective function values from linear specification over the parameter space $\Re$ for quantiles $\tau = 5, 10, 25, 75, 90,$ and 95. As one can see, the objective functions for the lower quantiles are well-behaved, whereas they become more erratic for higher quantiles of the outcome distribution. One explanation could be that identification strength might vary at different parts (e.g., low vs. high quantiles) of the distribution (see, Chernozhukov and Hansen, 2008).

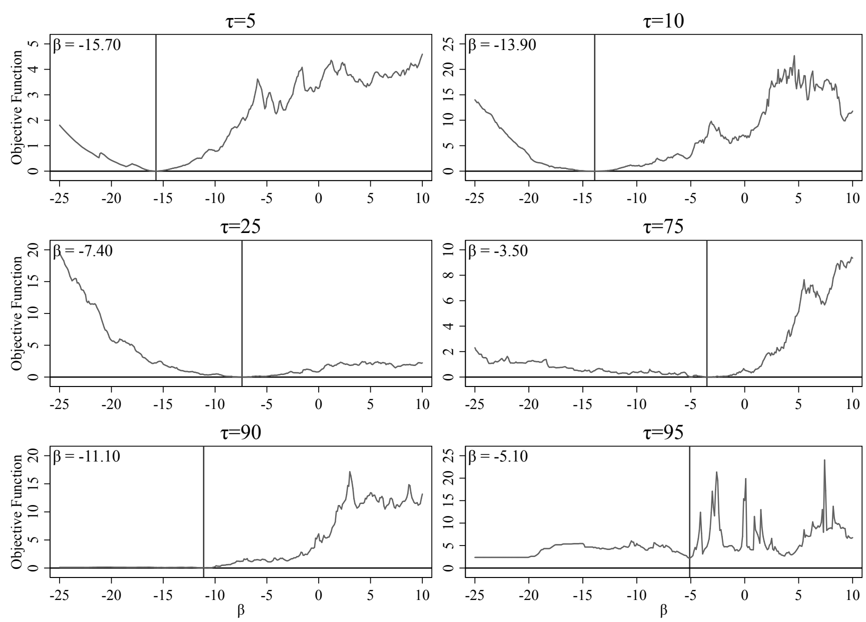

Figure 4.7: IVQR objective functions and β_1 coefficient estimates for selected quantiles

Lastly, estimated distributional effects on sub-categories of diet quality from the linear IVQR model are shown in figure 4.8. Panels A and B present IVQR estimates for adequacy and moderation, panels C and D for energy intake from added sugar and saturated fat as percent of total energy intake, respectively. The IVQR estimation for these outcome variables were conducted over the parameter space $\Re = [-10, 5]$ with a step size of 0.1 for β_1 for quantiles $\tau = 5$ to $\tau = 95$ at 5-unit increments.

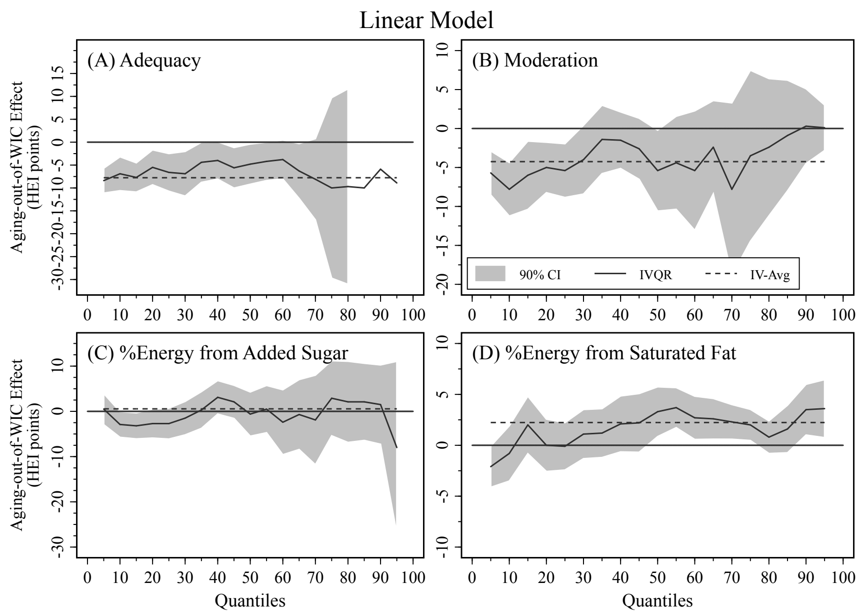

Figure 4.8: Distributional effects of aging out of WIC on sub-categories of child's dietary quality, linear model

Starting from panel A, we see uniform negative effects similar to the mean effect throughout the adequacy distribution. These effects are significant within the lower half of the distribution and become insignificant as we move toward higher quantiles. Again, for some adequacy quantiles IVQR estimates are imprecisely estimated and the CIs for these point estimates are not shown in the figure. A similar pattern is observed in panel B. Negative impacts, however, become statistically insignificant after quantile $\tau = 30$.

In panel C no significant effect is observed for the percentage of total energy intake from added sugar. In panel D, however, we see aging out of WIC increases the percentage of total energy intake from saturated fat for children above the median of the distribution. In figure 4.9 which displays the results from the quadratic model, we see very similar patterns, suggesting that our distributional results are robust to the choice of the degree of the polynomial of the assignment variable.

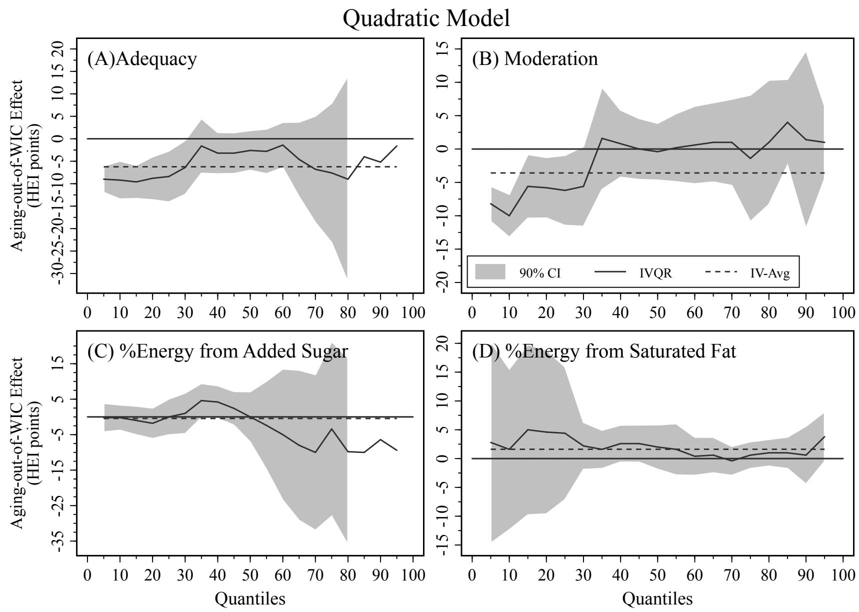

Figure 4.9: Distributional effects of aging out of WIC on sub-categories of child's dietary quality, quadratic model

4.6 Conclusions and Discussion

This study investigates the effect of aging out of the WIC program on the nutritional well-being of children aged 2-4 years. Specifically, using nationally representative data from the NHANES, we examine how losing WIC benefits at the age of 61 months affects child's diet quality, as measured by HEI-2010, as well as rates of 12-month food insecurity for households, adults, and children. Although there has been considerable amount of research examining the effects of WIC participation on maternal and infant health, fewer studies have investigated the effects of WIC on child's nutrition. More importantly, existing studies have struggled to fully address the issue of non-random participation into WIC, and therefore may not be able to make causal inferences about the effects of WIC on health and diet related outcomes. Further, current studies have mostly focused on estimating the average effects of WIC and full distributional effects of WIC participation are almost unknown.

To address the selection-bias problem, we use a fuzzy regression discontinuity deign. Using a sample of children who are not on school meal programs, we find that aging out of WIC has a fairly sizable adverse effect (about 20%) on the HEI-2010 as a measure of child's overall diet quality. To interpret the magnitude of the effects from this study, it is important to note that we are estimating the effects of losing WIC benefits and not effects of WIC participation in general. Given that children who stay on WIC until their eligibility ends are more likely to be more disadvantaged than the average WIC participant, then losing WIC benefits could have potentially larger effect on their diet quality.

Furthermore, our results for several subcategories of diet quality indicate that the estimated decrease in child's overall diet quality is mainly driven by adequacy foods and smaller impact is observed on moderation food. We find no significant increase in the percentage of total energy intake from added sugar and from saturated fat. Given that foods provided by WIC are all adequacy foods, observing larger effects on adequacy score due to losing food package is reasonable. Although WIC food packages target the moderation score for instance by imposing restrictions on the amounts of added sugar or saturated fat, smaller effect on moderation could be due to other unobserved factors. For example, after losing WIC benefits parents could still provide foods with lower added sugar or saturated fat content.

With respect to food insecurity rates, unlike Arteaga et al. (2016) we find that aging out of WIC has no significant effects on the prevalence of food insecurity or very low food security. One explanation is that Arteaga et al. (2016) examine the effects of aging out of WIC on a 30-day proxy for food insecurity, whereas in this study we use 12-month food insecurity measures. Given the subjective nature of the food-insecurity, it is more likely that households report as food insecure in the month following losing benefits from WIC.

Moreover, our distributional results show that losing WIC benefits has larger negative effects on lower quantiles of the HEI-2010 and its major sub-categories. These results indicate that the impacts of becoming age-ineligible for WIC are more detrimental for children falling in the lowest portion of diet quality distribution. This is a policy-relevant finding because WIC appears to have the largest benefits for children prone to the lowest quality diets.

Using a larger sample including children who report receiving food from school meal programs, we find that aging out of WIC has no significant effect on either measures of child's dietary quality. This finding suggests that school meal programs might pick up some of the otherwise decreases in diet quality due to becoming age-ineligible for WIC. Thus, one solution to avoid detrimental effects of losing WIC benefits on diet quality and fill the gap in the patchwork of federal food and nutrition assistance programs could be extending the WIC eligibility until school-entry. In other words, instead of ending eligibility for all children at the age of 61-months, eligibility could end upon enrolling in school meal programs.

Chapter 5

Conclusions

In this dissertation, I have explored several aspects of the Supplemental Nutrition Assistance Program (SNAP) and the Special Supplemental Program for Women, Infants, and Children (WIC) to better understand 1) the effects of changes in the amount of SNAP benefits on the material well-being of SNAP participants, 2) the welfare effects of the SNAP benefit cycle, and 3) and the impact of aging out of WIC on child's nutrition.

In the first analysis, I examined how changes in SNAP benefits due to the implementation and the subsequent expiration of the American Recovery and Reinvestment Act (ARRA) affected the material well-being of SNAP participants, as measured by their nondurable consumption. Specifically, I investigated three forms of nondurable consumption: total nondurable spending, food spending, and nondurable nonfood spending. I first examined and compared the expenditure decisions of households in response to both an increase and a decrease in SNAP benefits. Put differently, I examined whether households responded to benefit changes by modifying both their food and nonfood spending, as predicted by economic theory. Then, using a new fixed-effect quantile estimator, I allowed for the possibility of heterogeneous outcomes and estimated the impacts of benefit changes at various points of the spending distribution. Using data from the Consumer Expenditure Survey (CEX), I found that the ARRA implementation on average increased the quarterly total nondurable expenditure of SNAP households by around 6%, whereas the ARRA expiration led to a decrease of about 3.5%. Despite the fact that SNAP benefits should have an income effect for both food and nonfood spending, my findings indicated that households reacted to ARRA primarily by modifying their food expenditure. With respect to distributional effects, I found that the ARRA implementation led to an increase across the distributions of food and total nondurable spending, whereas the ARRA expiration had adverse effects on the most disadvantaged SNAP households. Together, these new findings have important implications for contemporary food policymaking, particularly in the light of the proposed cuts to the SNAP funding.

In the second analysis, I investigated whether the SNAP benefit cycle – the observation that food expenditures spike markedly in the days following benefit receipt and decline over the remainder of the benefit cycle – is in part induced by variation in food prices paid over the benefit month. Using data from USDA's National Household Food Acquisition and Purchase Survey (FoodAPS), I showed that the price component of expenditure is sensitive to benefit arrival. Specifically, I found that by the end of the third week (i.e., days 19-21) of the benefit month average food prices paid by households were 22% lower than the first two days of the month. My findings suggested that this fairly substantial decline in prices was more likely to be caused by within food type substitution from higher-quality (more expensive) to lower quality (less expensive) foods than increase in the intensity of price-seeking strategies. I then used these estimated price changes to calculate the welfare impacts associated with a decline in prices using an exact measure of Hicksian compensating variation following Hausman (1981). My findings indicated a change in money-metric welfare of $4.94 per day or 6.6% of the average amount spent on the first two days of the month. In other words, households need to be given $4.94 per day during days 19-21 of the benefit cycle to be able to purchase the foods with the quality of days 0 and 1. Overall, the results from this study suggest that by targeting educational efforts through programs such as SNAP-Ed, households could to be better educated how to smooth and/or find better prices over the benefit month, instead of buying lower-quality (less expensive) foods.

In the last analysis, I evaluated the impact of aging out of WIC on child's dietary quality, as quantified by the HEI-2010 and its major sub-categories, as well as rates of food insecurity. To address concerns about the non-random WIC participation, I used a fuzzy regression discontinuity design (RDD) and estimated changes in children's diet quality and rates of food insecurity as they become age-ineligible for WIC, thus exploiting a discontinuity in WIC participation that is directly linked to the child's age. Using data from the continuous waves of the National Health and Nutrition Examination Survey (NHANES), I found a fairly large decrease in overall diet quality for children who became age-ineligible for WIC and have not yet started receiving food from school meal programs. I showed that this result was mainly driven by the adverse effects on adequacy component of the HEI-2010 and a smaller effect was observed on moderation foods. I found no significant effects on other indices of diet quality such as energy intake from added sugar and saturated fat as a percentage of total energy intake. Similarly, I found aging out of WIC had no significant effect on prevalence of food insecurity and very low food security. Furthermore, by taking a distributional approach, I found that children prone to lower-quality diets experienced larger decreases in their diet quality upon losing their WIC benefits at the age of 61 months. This is a policy-relevant finding because WIC appears to have larger beneficial impacts on children prone to the lower-quality diets. I repeated the my analysis using a larger sample including children on school meal programs. I found no significant effect on either measures of child's diet quality or food insecurity rates. Overall, these findings suggest that, to fill the gap in the patchwork of federal food and nutrition assistance programs, policymakers should consider extending WIC eligibility until school-entry and transition into school meal programs.

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Appendix A

How Did the American Recovery and Reinvestment Act Impact the Material Well-being of SNAP Participants? A Distributional Approach

Table A.1: Expenditure Groups and Their Subgroups

Group	Subgroup
1. Food	1.1. Food at Home 1.2. Food away from home
2. Alcoholic beverages	2.1. Alcoholic beverages
3. Utilities	3.1. Natural gas 3.2. Electricity 3.3. Fuel oil and other fuels 3.4. Telephone services 3.5. Water and other public services
4. Public transportation, gas and motor oil	4.1. Public transportation on trips 4.2. Local public transportation 4.3. Gasoline and motor oil
5. Household operations	5.1. Domestic services including babysitting and childcare 5.2. Other household expenses
6. Apparel and services	6.1. Clothing for men and boys 6.2. Clothing for women and girls 6.3. Clothing for children under 2 6.4. Footwear 6.5. Other apparel product and services
7. Tobacco	7.1. Tobacco and smoking supplies
8. Personal care	8.1. Personal care
9. Miscellaneous expenditures	9.1. Miscellaneous expenditures

Appendix B

The Welfare Effects of Cyclical Spending among SNAP Beneficiaries

Table B.1: Price Analysis Estimation Results

	Dependent Variable	
	Log Price Per Pound	Log Price per Kcal
Days 2-3	-0.03	-0.01
	(0.03)	(0.04)
Days 4-6	0.01	0.04
	(0.04)	(0.04)
Days 7-9	-0.06	0.01
	(0.06)	(0.07)
Days 10-12	-0.07	-0.02
	(0.06)	(0.07)
Days 13-15	-0.11	-0.05
	(0.08)	(0.08)
Days 16-18	-0.15*	-0.11
	(0.08)	(0.08)
Days 19-21	-0.22***	-0.17**
	(0.07)	(0.08)
Days 22-24	-0.16**	-0.09
	(0.07)	(0.05)
Days 25-27	-0.14***	-0.09*
	(0.05)	(0.05)
Days 28-30	-0.01	0.02
	(0.05)	(0.05)
Brand	0.23***	0.25***
	(0.02)	(0.02)
Coupon	-0.33***	-0.30***
	(0.10)	(0.10)
Savings	-0.12***	-0.08***
	(0.02)	(0.02)
SNAP (EBT)	-0.03	-0.02
	(0.03)	(0.03)
Bulk	-0.40***	-0.37***
	(0.02)	(0.02)
Observations	18,479	18,479

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses are clustered at the household level. All regressions use survey weights.

Appendix C

Aging out of WIC and Child Nutrition: Evidence from a Regression Discontinuity Design

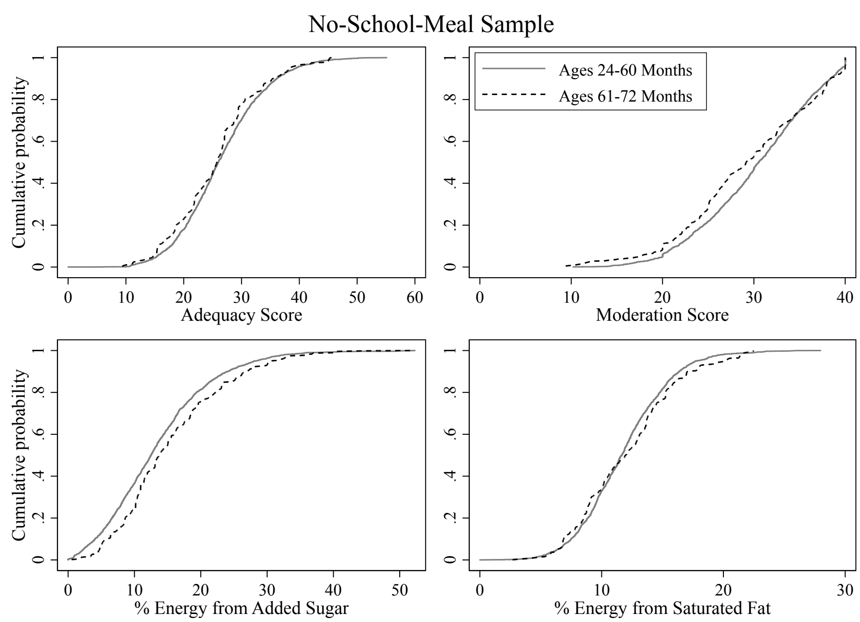

Figure C.1: Unconditional cumulative distribution functions (CDF) of sub-categories of child's dietary quality, no-school-meal sample

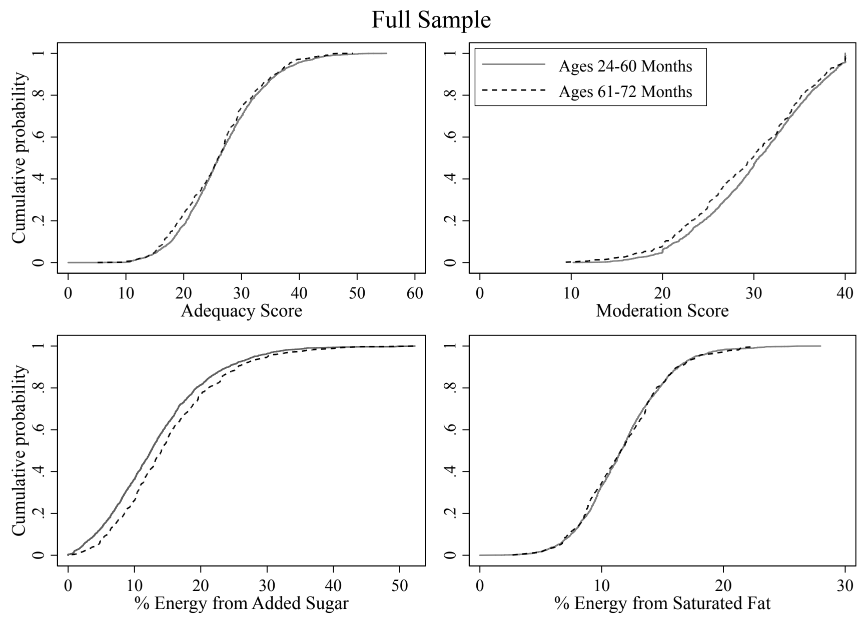

Figure C.2: Unconditional cumulative distribution functions (CDF) of sub-categories of child's dietary quality, full Sample

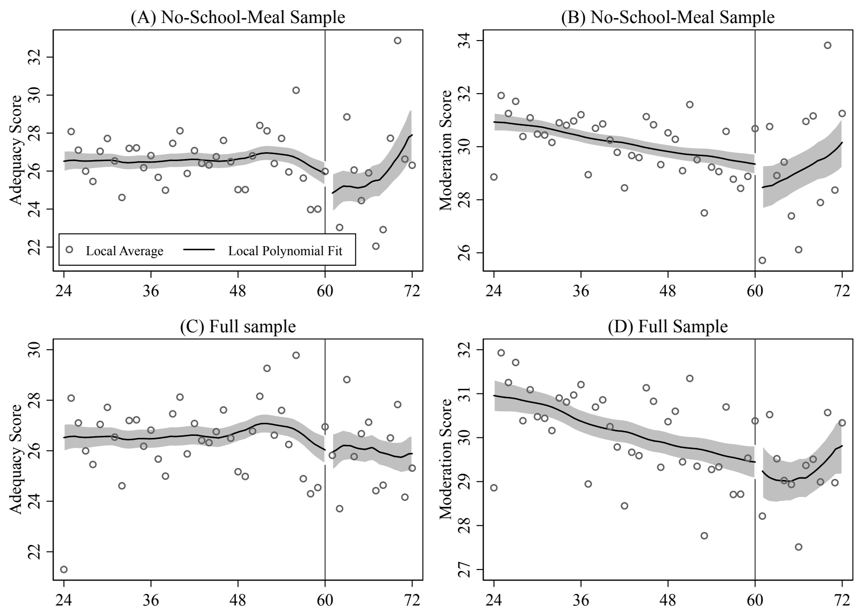

Figure C.3: Discontinuities in Adequacy and Moderation Scores

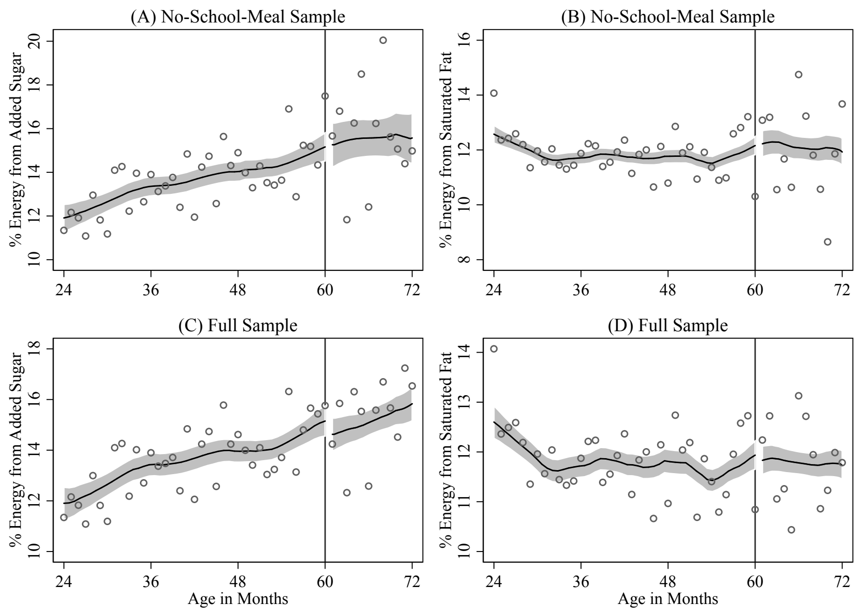

Figure C.4: Discontinuities in percentage of energy (kcal) from added sugar and saturated fat

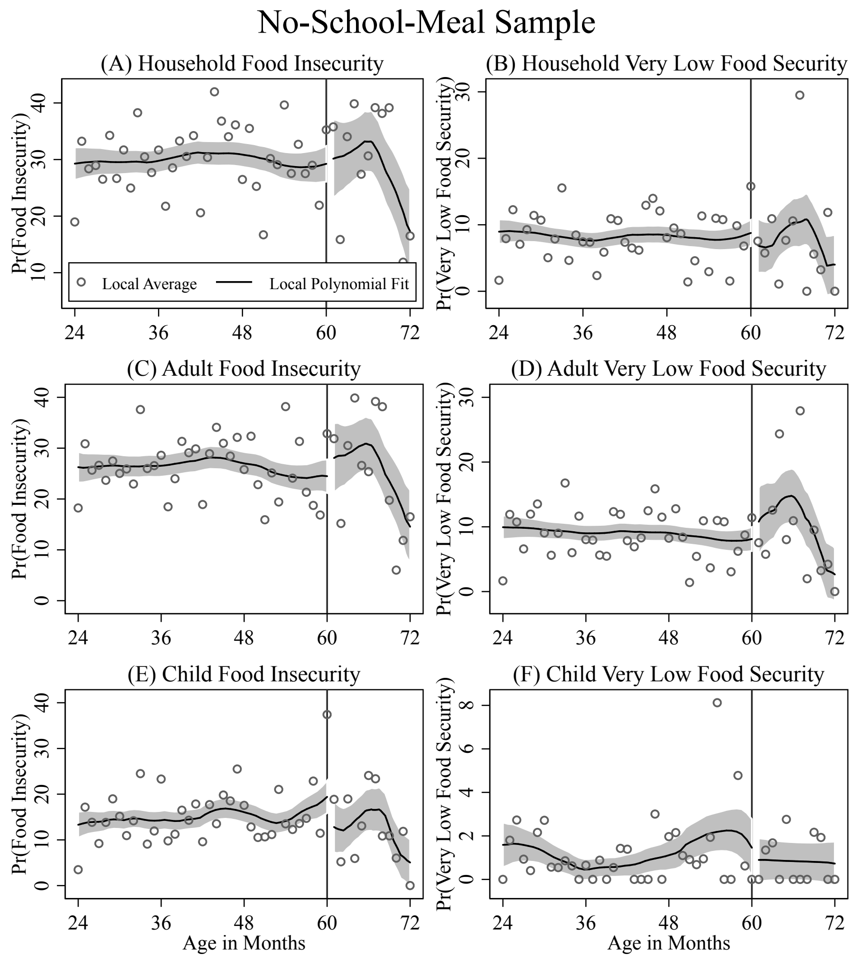

Figure C.5: Discontinuities in rates of food insecurity, no-school-meal sample

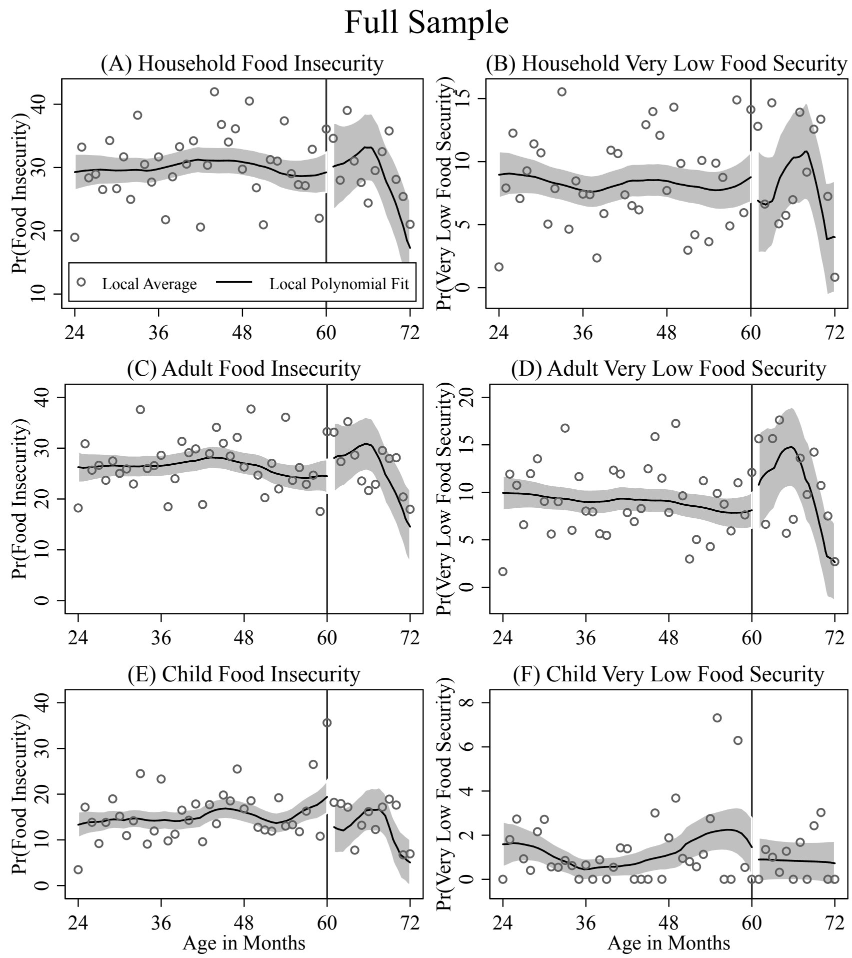

Figure C.6: Discontinuities in rates of food insecurity, full sample

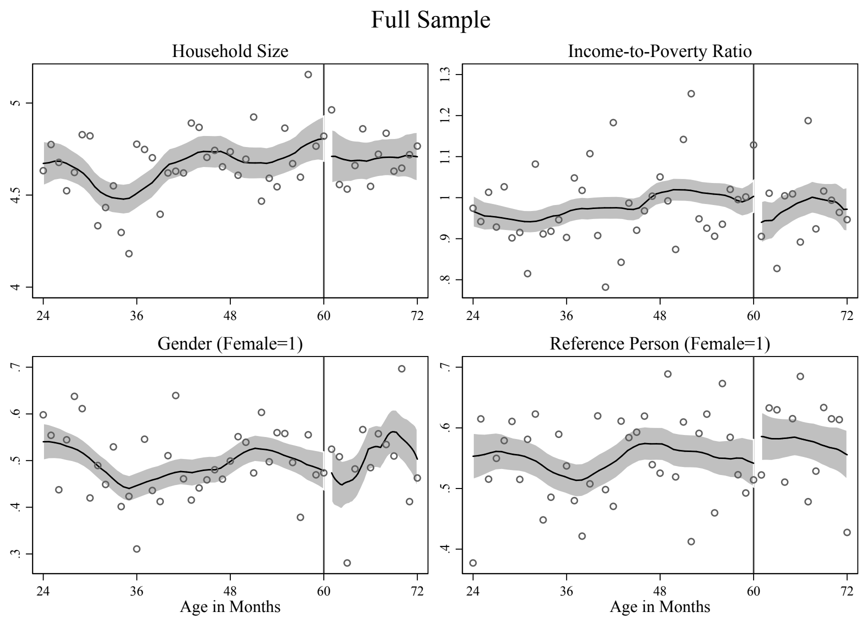

Figure C.7: Discontinuity in baseline covariates, full Sample

Table C.1: Discontinuities in Baseline Covariates, Full Sample

	Order of Polynomial of $\tilde{Age}$		
	1st	2nd	3rd
Child Female	-0.02	-0.09	0.05
	(0.06)	(0.08)	(0.09)
Income-to-Poverty Ratio	-0.06	-0.11	-0.04
	(0.06)	(0.08)	(0.09)
Income-to-Poverty Ratio Squared	-0.11	-0.24	-0.10
	(0.12)	(0.17)	(0.20)
Household Size	-0.03	-0.08	0.13
	(0.15)	(0.19)	(0.24)
Household Size Squared	-0.01	-0.39	1.71
	(1.41)	(1.83)	(2.42)
Reference Female	0.03	-0.01	0.04
	(0.05)	(0.06)	(0.08)
Joint Test p-values[a]	0.82	0.36	0.89
Observations	3,905	3,905	3,905

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights.

[a] p-values for tests of linear restrictions that all discontinuities are jointly zero.

Table C.2: Average Effects of Aging Out of WIC on HEI-2010, No-Schoolers Sample

	Order of Polynomial of $\widetilde{Age}$			
	1st	1st	2nd	3rd
Panel A: Fuzzy RDD				
Off WIC (D)	-13.51**	-11.65**	-9.57	-18.40*
	(6.54)	(5.72)	(8.83)	(10.08)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-4.33*	-4.18*	-3.30	-6.77*
	(2.21)	(2.19)	(3.17)	(3.69)
Panel C: First-stage Estimates				
Age $\geq$ 61 months (T)	0.33***	0.37***	0.36***	0.37***
	(0.03)	(0.03)	(0.05)	(0.05)
Covariates	No	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
First-stage F-Statistic	90.18	52.23	47.63	45.22
AIC	23632.61	23362.66	23167.35	24156.69
Observations	3,054	3,054	3,054	3,054

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights.

Table C.3: Average Effects of Aging Out of WIC on Sub-categories of Child's Dietary Quality, No-Schoolers Sample

	Dependent Variable			
	Adequacy Score	Moderation Score	%Energy from Added Sugar	%Energy from Saturated Fat
Panel A: Fuzzy RDD				
Off WIC (D)	-7.96**	-3.69	-0.52	2.53
	(3.62)	(3.11)	(3.37)	(1.83)
Panel B: Sharp RDD				
Age $\geq$ 61 months (T)	-2.81**	-1.37	-0.36	0.94
	(1.40)	(1.16)	(1.25)	(0.69)
Covariates	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	3,054	3,054	3,054	3,054

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from the linear model are reported.

Table C.4: Average Effects of Aging Out of WIC on Rates of Food Insecurity, No-Schoolers Sample

	Food Insecurity			Very Low Food Security		
	Household	Adult	Child	Household	Adult	Child
Panel A: Fuzzy RDD						
Off WIC (D)	2.42	7.73	-7.57	-0.96	4.76	-0.34
	(11.10)	(10.35)	(9.07)	(4.05)	(5.52)	(3.63)
Panel B: Sharp RDD						
Age $\geq$ 61 months (T)	-1.47	1.59	-4.22	-2.34	2.34	-0.45
	(6.63)	(6.53)	(5.05)	(3.35)	(3.68)	(1.15)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,030	3,030	3,030	3,030	3,030	3,030

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. All calculations use survey weights. Coefficient estimates from the linear model are reported.