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

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

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Abstract

This paper 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. A distributional approach is taken to address heterogeneity in the effect of ARRA-induced SNAP benefit changes on well-being. Using a fixed-effects quantile estimator, we find that the ARRA implementation led to a first-order improvement in the material well-being of SNAP participants as defined by their food expenditure. The distribution of total nondurable spending, a more aggregate measure of well-being, showed relatively smaller and insignificant improvements. With respect to the ARRA expiration, we find no significant effect on the distribution of food spending but do see a decrease in total nondurable spending in the bottom quintile of distribution. This latter finding appears to be operating off SNAP participants' reluctance to decrease food spending when benefits were cut at the expense of decreasing nonfood, nondurable spending. Together these results highlight the important role SNAP benefits play in the overall budget of low-income households.

Key Words: SNAP, the American Recovery and Reinvestment Act, material well-being, fixed-effects quantile estimation

JEL Classification: I38, I30, Q18

1. Introduction

The Supplemental Nutrition Assistance Program (SNAP) is the largest federal food assistance program in the United States and the cornerstone of the nation's program for reducing hunger and food insecurity. In April 2009, following the Great Recession and in response to the rapid rise in food prices, Congress implemented the American Recovery and Reinvestment Act (ARRA), which increased SNAP benefits at an unprecedented level for all participants, 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). These changes were intended to provide SNAP beneficiaries with adequate resources to purchase food. In November 2013, due to lower-than-expected food price inflation, the ARRA expired, and benefits were cut 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]

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. Because SNAP benefits account for approximately 50% of at-home food spending of low-income households (Wilde 2013), 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' ability to meet their food needs and cause hardship (see, Dean and Rosenbaum 2013; Bruich 2014 and citations within). Indeed, implementation of the ARRA improved food security (Nord and Prell 2011), while the subsequent expiration increased food insecurity (Katare and Kim 2017).

This paper takes a more holistic view of welfare and examines the extent to which the implementation and the expiration of the ARRA affected the material (or money-metric) well-being of SNAP households. We focus on nondurable consumption as a measure of material well-being

[1] Participants in Hawaii, due to the higher food price inflation, did not experience a cut in SNAP benefits in November 2013 (Dean and Rosenbaum 2013).

rather than income, because for both theoretical and empirical reasons, it provides a more reliable measure of well-being (Cutler et al. 1991; Meyer and Sullivan 2004). We examine two forms of nondurable consumption: food spending and total nondurable spending, where the former is a rough proxy (see, Attanasio and Weber 1995; Lusardi 1996) and the latter is a broader measure. We choose food spending because it is important for judging the effectiveness of the program. Because the income effect of SNAP benefit changes will affect other nondurable expenditures (i.e., nondurable nonfood spending), we examine total nondurable spending.

Previous studies examining the impacts of the ARRA's SNAP benefit changes have focused on the average treatment effects of either the 2009 benefit enhancements (Nord and Prell 2011; Beatty and Tuttle 2015; Tuttle 2016; Kim 2016) or the 2013 benefit cuts (Bruich 2014; Katare and Kim 2017) on food spending and/or food security. Although mean impacts provide useful information for many policy decisions, it is unlikely that the well-being effects of the ARRA-induced SNAP benefit changes are constant within the SNAP population. Clearly, differences in observed (e.g., income levels) and unobserved household characteristics (e.g., food preferences, the desire to participate in food assistance programs, and the propensity to spend SNAP benefits) could influence the distribution of outcomes not entirely captured by the mean.

An example of heterogeneous outcomes is the differing effects of SNAP benefits on the food spending of extramarginal SNAP participants (i.e., those whose SNAP benefits exceed their desired food-at-home spending) as opposed to inframarginal households (i.e., those whose at-home food spending are at least as much as their SNAP allotment). According to Southworth's (1945) theory, for extramarginal households, SNAP benefits will increase food expenditure by more than an equivalent cash transfer would, whereas inframarginal participants are predicted to treat SNAP benefits no differently than an equivalent cash income.[2] Consequently, a change in SNAP benefits

[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, Martini, and Ohls 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

is predicted to strongly affect food spending of extramarginal households and weakly affect food expenditure of inframarginal households.

Heterogeneity in SNAP treatment effects, however, is not confined to the behavioral differences between extramarginal and inframarginal households. Indeed, the vast majority of SNAP participants are inframarginal (Hoynes, McGranahan, and Schanzenbach 2014) and there are good reasons to expect heterogeneous outcomes within the inframarginal households due to differences in unobserved and observed household characteristics. Thus, it is important to account for the heterogeneity in spending responses to changes in SNAP benefits as they can assist policymakers in identifying the SNAP subpopulations that are the most sensitive to variations in SNAP benefits. Put differently, investigating the heterogeneity in expenditure responses to changes in SNAP benefits can tell policymakers more about for whom SNAP benefit enhancements/cuts did or did not work. Accordingly, in this study, we allow for the possibility of heterogeneous outcomes by estimating the impacts of both the implementation and the expiration of the ARRA at various points in the distribution of our well-being measures. This makes the quantile regression an attractive candidate. Therefore, we estimate the quantile treatment effects of changes in SNAP benefits on food and total nondurable expenditures of SNAP participants as we move from low expenditures to high expenditures.

The impact of a policy change can be measured as the difference between *what happens* in the presence of the policy change and *what would have happened* in its absence (i.e., the counterfactual). Establishing the counterfactual is usually accomplished by investigating a population that has not been subject to the policy change. SNAP households self-select into the program for reasons that are not easily observed (Gundersen, Kreider, and Pepper 2011; Kreider et al. 2012; Hoynes, McGranahan, and Schanzenbach 2014; Bitler 2014). These selective processes may make SNAP participants different from those who do not participate in the program in systematic ways. For

out of non-SNAP income.

instance, households with larger unmet food needs/stronger preferences for food are more likely to participate in the program. Therefore, simple comparisons of SNAP participants to nonparticipants cannot identify the true impacts of SNAP benefit changes.

Drawing on panel data from the Consumer Expenditure Survey (CEX), we use a 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. Identification stems from within household variation through the use of panel data, as done elsewhere (e.g., Wilde and Nord 2005; Beatty and Tuttle 2015; Katare and Kim 2017). Thus, QRPD coefficient estimates give the desirable policy interpretation for the policy question at hand—how did the ARRA affect households prone to low food/nondurable expenditure separately from those inclined to high expenditure?

We find that the ARRA implementation increased average food spending of SNAP households but did not significantly affect average total nondurable spending. Our distributional results provide a more comprehensive picture of the well-being effects of the ARRA's SNAP benefit changes. We find that benefit enhancements had positive impacts throughout the food spending distribution with especially large impacts in the bottom quintile of the food spending distribution. Despite these significant positive impacts on the distribution of food expenditure, we find almost no significant effects on the distribution of nondurable spending. Overall, our findings suggest that the ARRA implementation led to a first-order improvement in the material well-being of SNAP households as measured by their food spending.

With respect to the 2013 SNAP benefit cuts, we find no significant adverse impacts on the distribution of food expenditure. We do, however, find that the ARRA expiration led to a decrease in total nondurable spending within the lower quartile of the expenditure distribution with no significant impacts at mid-to-upper quantiles. We show that SNAP households attempted to maintain their food expenditures by cutting their nondurable nonfood expenditures.

The remainder of the paper is organized as follows. Section 2 provides abbreviated background information on SNAP and the ARRA. Section 3 discusses our choice of nondurable consumption as the measure of material well-being. Section 4 describes the data and provides summary measures. Section 5 presents the empirical methodology. Section 6 presents the results and finally, the last section concludes.

2. Background on SNAP and ARRA

SNAP has a clearly defined dual mandate: to "alleviate hunger" and to "permit low-income households to obtain a more nutritious diet" (Food, Conservation, and Energy Act of 2008). SNAP aims to accomplish these goals by "increasing the food purchasing power for all eligible households" through in-kind transfers. SNAP benefits are distributed monthly to recipients via an Electronic Benefit Transfer (EBT) card. To become eligible for SNAP, households' monthly gross income must be less than 130% of the federal poverty line (FPL). The amount of SNAP benefits is a function of household size, net income, and a maximum benefit which is calculated based on the cost of the USDA's Thrifty Food Plan (TFP).

Figure 1 plots the USDA's estimated cost of TFP and monthly maximum SNAP benefit for a household of four from the fiscal year 2002 to 2015. As can be seen in this figure, the cost of TFP has been increasing over time due to food price inflation. 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 TFP in the June of the prior fiscal year. This annual adjustment based on the lagged prices in conjunction with the rapid increase in food prices during the Great Recession rendered SNAP benefits increasingly less adequate to enable households to afford the TFP (Rosenbaum 2008).

Food prices were expected to continue rising in the fiscal year 2009, which could exacerbate the problem (Rosenbaum 2008). Therefore, in April 2009, the ARRA substantially increased the maximum monthly benefits by 13.6% for a SNAP household.[3] The amounts of increase in benefits for households of one to four were $24, $44, $63, and $80, respectively.[4] These changes were intended to improve the adequacy of food stocks for low-income households following a period of economic turmoil.

The ARRA mandated that maximum monthly benefit levels would remain constant at the new higher amount for the next several years until the food-price inflation caught up with the ARRA benefit add-on. When the ARRA was enacted, food-price inflation was projected to be high and the cost of TFP was expected to surpass the ARRA level in the fiscal year 2014 (Dean and Rosenbaum 2013). However, food-price inflation was lower than expected over the 2009–2013 period, leading to the early sunset of the ARRA. In November 2013, the ARRA expired and SNAP benefits were reduced the first time in history (see, figure 1). The amounts of the benefit cuts for one- to four-person households were $11, $20, $29, and $36, respectively.[5]

3. Measurement of Well-Being

To measure the material well-being of SNAP households, we focus on the consumption-based measures rather than the income-based approaches. For theoretical and empirical reasons consumption data are preferred to income data in evaluating the economic well-being (Cutler et al. 1991; Meyer

[3]The 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 the well-being of low-income households. These provisions are assumed to affect SNAP participant and nonparticipant households similarly.

[4]Since households that received less than the maximum benefit (i.e., households with positive net income) also experienced the same constant dollar increase, the percentage change in their benefits was larger than households of the same size with no net income. Within the SNAP population, average SNAP benefits went up about 15% to 20% because of the ARRA implementation (Keith-Jennings and Rosenbaum 2015).

[5]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).

and Sullivan 2004). 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 as households can smooth consumption and maintain their welfare status through "saving" and/or "dissaving." Thus, current income can be a misleading measure of the economic well-being of households as it is more susceptible to temporary fluctuations that do not necessarily reflect changes in the material well-being. 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). Additionally, income data do not capture in-kind transfers, whereas expenditure data reflect them. For these reasons, we find it preferable to focus on consumption measures to assess the well-being. This requires constructing a measure of consumption using households expenditure data because in practice actual consumption cannot be estimated.

We represent consumption using household food spending and total nondurable expenditure (i.e., food plus nondurable nonfood spending). Theoretically, the in-kind nature of SNAP only distorts spending of extramarginal households, and as mentioned before, the vast majority of SNAP participants are inframarginal. This implies that for inframarginal households a change in SNAP benefits can be considered as a pure income effect that will affect spending on both food and nonfood goods.[6] For instance, Kim (2016) finds that the ARRA implementation, on average, increased household spending on food as well as some nonfood expenditure categories (e.g., housing, utility fee, and transportation). Thus, in this paper food spending is considered a strict measure of ma-

[6]Although empirical evidence indicates that inframarginal participants do not treat SNAP income in the same manner as cash income in that they have a higher MPS on food out of SNAP income than from cash income, the estimated MPS from SNAP is, in effect, less than 0.50 (e.g., Moffitt 1989; Fraker 1990; Levedahl 1995; Breunig and Dasgupta 2005; Beatty and Tuttle 2015). For instance, Beatty and Tuttle (2015) using data from the CEX, estimate that infra-marginal households' MPS on food at home out of an increase in SNAP benefits is 0.48. More recently, 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.

terial well-being, and we utilize expenditure on nondurable goods to construct a broader measure of material well-being. Finally, in the empirical section we make use of total expenditure (i.e., nondurable plus durable spending) as a representation of total household resources in our Engel curve specification.

4. 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 quarter.

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 interview. 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., before and after the ARRA's SNAP benefit changes) and to control for their observable and unobservable fixed 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 between the interviews. We also exclude households if changes in the number of children or adults are greater than three in absolute magnitude.

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) and Johnson, Parker, and Souleles (2006), 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 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 total nondurable expenditure as they can be inferred as an investment (e.g., Attanasio and Weber 1995; Meyer and Sullivan 2004).[7]

Moreover, we construct a more refined measure of consumption referred to as "food consumption" by summing spending on food at home, food away from home, and alcoholic beverages (see, Lusardi 1996). As discussed before, we treat food consumption as a more strict measure of material well-being in the sense of Engel. These consumption measures are then expressed in real (2009) dollars using Consumer Price Index (CPI) for nondurables and CPI for food.[8] By deflating the expenditure measures, we adjust for the annual cost of living adjustment in SNAP benefits. In this way, we are examining the effects of real changes in SNAP 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.

[7] See table A1 in appendix for more details on each expenditure group.

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

4.1. Summary Measures

To estimate the well-being impacts of the implementation and the expiration of the ARRA, we consider three-year periods from January 2008 to December 2010, and from July 2012 to June 2015, respectively.[9] Although the ARRA's 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 compare changes in the expenditure levels of SNAP households to changes in the expenditure levels of non-SNAP households before and after SNAP benefit changes. However, since SNAP participants self-select into the program, simple comparisons between SNAP participants and the full population of nonparticipants would be misleading. Thus, we limit our analysis to low-income households with annual income (defined as household financial income before tax minus the value of their SNAP benefit) less than 185% of the FPL.[10] From here, we define SNAP participants as households who received any positive amount of SNAP benefits in the previous 12 months.[11] All other households are considered as comparable nonparticipants.[12]

Table 1 shows the summary statistics for household demographic characteristics. As can be seen, demographics are different between SNAP participants and nonparticipants within each pe-

[9]For each study period, due to the implausibly small expenditures, we drop the bottom one percent of the real nondurable expenditure after adjusting for household size.

[10]We choose this threshold for two reasons. First, this threshold is used as an upper bound on the cutoff for many federal nutrition assistance programs such as SNAP and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). Thus, it is a policy-relevant threshold. Second, it restricts our sample to income-eligible households. Although the gross income cutoff for SNAP eligibility is 130% of the FPL, due to the categorical eligibility in many states, households with higher gross income may become eligible for SNAP.

[11]Households that were not SNAP participants before the ARRA implementation but participated afterwards are excluded from the sample. These households are more likely to select into the program due to the larger SNAP benefit. Nord and Prell (2011) refer to these households as "ARRA-induced" participants. However, fewer households likely drop out of the program due to a reduction in benefits. Thus, ARRA expiration would pose fewer issues with this source of selection-bias (i.e., the potential for gain).

[12]The CEX asks for income information including SNAP benefit receipts only in the second and fifth interviews. The data collected in the second interview are then applied to the third and fourth interviews. This imputation could be a potential problem because SNAP participation status of households may change over time. As a robustness check, we excluded the third and fourth interviews from the sample and estimated the models again. Similar results were obtained but as expected, the confidence intervals were wider due to smaller sample size.

riod. In both periods, SNAP participants are less likely to be: married, headed by a male, employed, of 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 be less problematic if the program participation was also exogenous. Households, however, select into the program for reasons that are not easily observed which may make them different from comparable nonparticipants in important ways. If these unobservable factors do not change over the survey period then using panel data and conditioning on household fixed effects helps identification. Put differently, by conditioning on household fixed effects and assuming conditional exogeneity, unobservable and observable time-invariant household characteristics associated with program participation are no longer confounding.

Tables 2 provides summary statistics for household quarterly expenditures. Similarly, in panels A and B, we observe that mean expenditures are different between SNAP participants and nonparticipants with the latter having higher spending than the former. Further, the results of a two-sample Kolmogorov-Smirnov test indicate that in both periods nonparticipants' spending distributions first-order stochastically dominate the corresponding spending distributions of participants.[13] Using the differences between the outcome distributions of participant and nonparticipant households over time, we can describe the effects of the ARRA on the material well-being of SNAP households.

Figures 2 and 3 show the empirical cumulative distribution functions (CDFs) for household quarterly spending. Panel (A) of figure 2 presents the empirical CDFs of food spending for SNAP participant and nonparticipant households, denoted by F_P and F_{NP}, and the difference between them before the ARRA implementation. Panel (B) likewise for after the ARRA implementation.

[13] 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).

In comparing the differences in subpanels (A) and (B), we see a smaller gap between the distributions of food spending for participants and nonparticipants following the ARRA implementation.[14] This is formally shown in panel (C) by taking the difference (between before and after the ARRA implementation) in the differences. As can be seen in panel (C), there is good evidence that the impacts of the ARRA implementation are not uniform. For instance, we see larger effects towards the top of the distribution. These ununiform effects, however, could suggest a first-order improvement in the distribution of food spending following the rise in SNAP benefits.

Similar to panel (A), panel (D) of figure 2 shows nonparticipant households spend more on food at all points in the distribution prior to a policy change. However, contrary to ARRA implementation, panel (E) shows the spending gap becomes larger after the 2013 benefit cuts. Thus, the unconditional difference-in-differences in panel (F) reveals that the ARRA expiration decreased food spending at all points in the distribution. Similar conclusions can be drawn from figure 3 for nondurable expenditure.

None of the aforementioned descriptive findings, however, control for factors known to impact spending such as total household resources, household size, and seasonality. Moreover, the presence of unobservable characteristics (e.g., a preference for food versus nonfood) further casts doubt on drawing casual inferences from figures 2 and 3. In the following section, we employ regression methods to better isolate the impacts of the ARRA's SNAP benefit changes on the material well-being of SNAP households.

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

5. Empirical Methods

5.1. Average Impacts

We first discuss the regression model for estimating the average treatment effects (ATEs) as they are needed for comparison purposes and are typically estimated in the literature. Let $SNAP_{it} = 1$ if household i in interview quarter $t = \{2, 3, 4, 5\}$[15] is a SNAP participant. We divide each study period into pre- and post-policy change periods. We consider the period from January 2008 to April 2009 as the pre-ARRA implementation period and the period from May 2009 to December 2010 as the post-ARRA implementation period.[16] Likewise, we consider the period from July 2012 to November 2013 as the pre-ARRA expiration period and the period from December 2013 to June 2015 as the post-ARRA expiration period.[17] Accordingly, we define a dummy variable, $post_t$, which takes on the value of zero in the pre-ARRA implementation/expiration period and one in the post-policy change periods.[18] Then, the OLS fixed-effects model for estimating the ATEs of the ARRA implementation and expiration is:

$$log(Y_{it}) = \beta_1 SNAP_{it} + \beta_2 post_t + \beta_3 SNAP_{it} \times post_t + \beta_4 log(X_{it}) + \gamma_{htm} + \alpha_i + \epsilon_{it}, \tag{1}$$

where Y_{it} is either food spending or nondurable expenditure for household i in interview quarter t and X_{it} is household total expenditure (i.e., nondurable plus durable expenditure) to control for total household resources in the sense of an Engel curve specification. Including the total expen-

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

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

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

[18]Households surveyed in May 2009 report expenditures for February, March, and April of that year. Thus, only April's expenditures reflect new level of benefits. Therefore, $post_t$ will take on the value of 0.33 in May 2009 and with a similar argument, it will take on the value of 0.66 in June 2009. Similarly, $post_t$ will take on values of 0.33 and 0.66 in December 2013 and January 2014, respectively.

diture in the regression model could also control for household "need", which is considered as a common source of self-selection bias (see, Fox, Hamilton, and Lin 2004). γ_{htm} is an interaction term based on household size $h = \{1, 2, 3, 4, 5^{+}\}$, interview quarter t, and calendar month m.[19] This interaction term is particularly important in our quantile regression specification below (i.e., equation [2]) because it allows the expenditure distributions 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 expenditure. α_i is the household fixed effect, and 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.

5.2. Distributional Impacts

The mean regression in equation (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 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, also called the rank variable, which 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 (1):

$$\begin{aligned} log(Y_{it}) = {} & \beta_1(U_{it})SNAP_{it} + \beta_2(U_{it})post_t + \beta_3(U_{it})SNAP_{it} \times post_t + \beta_4(U_{it})log(X_{it}) \\ & + \gamma_{htm}(U_{it}). \end{aligned} \tag{2}$$

[19]We first interact each interview quarter with household size. This new interaction term is then interacted with each calendar month.

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, $U_{it} = f(\alpha_i, \epsilon_{it})$. Therefore, the model tells us how the ARRA impacted well-being at different points in the distribution, as defined by $U_{it} = f(\alpha_i, \epsilon_{it})$. These impacts (i.e., the quantile treatment effects [QTEs]) are again captured by β_3.

However, as with mean regression, the model yields endogenous results when attributes 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.[20]

The specific estimation approach taken in this paper is to utilize the quantile regression estimator for panel data (QRPD) with nonadditive fixed effects proposed by Powell (2016).[21] 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,

[20]We remind the reader that several OLS specifications lead to the same fixed-effect 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."

[21]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).

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 QRPD estimates.[22] Inferences are then drawn from the posterior distribution.

6. Results

6.1. Average Treatment Effects (ATEs)

ATEs estimates are presented in table 3. The first two columns show coefficient estimates from pooled OLS (POLS) and fixed-effects OLS (FE-OLS) for food spending, respectively. Likewise, the last two columns for nondurable spending. 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, POLS estimates tend to bias ATEs of the ARRA implementation. For instance, POLS results indicate that ARRA implementation increased nondurable spending 2.95%, whereas FE-OLS shows no significant impact on nondurable spending. This finding suggests that unobserved household characteristics are positively correlated with program participation. Thus, conditioning on household fixed effects is important for identification as it accounts for the time-invariant unobserved and observed household characteristics associated with the selection into the program.

The FE-OLS results indicate that the ARRA implementation on average increased food spending by just over 6%. To better understand the magnitude of this impact, we can use the conditional (counterfactual) mean of food spending which is \$816.61. Therefore, the ARRA implementation

[22] We use an adaptive MCMC algorithm (see, Baker 2014; Powell, Baker, and Smith 2014) applied to equation (2) in conjunction with a two-step procedure suggested by Yin (2009). The first step uses a Metropolis-within-Gibbs (MWG) sampling with 600 draws. Coefficient estimates from this step are then used as the initial values of the second step which uses a global sampling approach with 7000 draws.

on average raised food spending about \$49. Despite this relatively large positive impact on the food spending, no significant positive impact is observed for nondurable spending. Likewise, from panel B we see that the 2013 benefit cuts did not impose significant negative impacts on either expenditure category. One possible explanation is that the benefit changes were too small to have a significant negative effect on household expenditures. An alternative reason might be that mean regression model masks the impacts of the benefit changes on other parts of the distribution. For instance, ATE could average together positive and negative expenditure responses and obscure the extent of the ARRA's effects.

6.2. Quantile Treatment Effects (QTEs)

The distributional impacts (i.e., QTE estimates) of the ARRA implementation on food and nondurable spending are presented in figures 4 and 5, respectively.[23] The left panel plots estimates from POLS and pooled QR models and the right panel plots estimates from FE-OLS and QRPD. Pooled QR results are presented to show how accounting for household fixed effects in QRPD would affect the estimation results. In both figures coefficient estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. The shaded areas represent 90% confidence interval (CI) and are calculated pointwise from the posterior of MCMC draws. The quantiles on the *x*-axis refer to the counterfactual expenditure distribution (i.e., absence the policy change), which gives the QTE estimates a *ceteris paribus* interpretation.

The main finding from figure 4 is that the ARRA implementation had a positive effect throughout the food expenditure distribution (i.e., a first-order improvement). As can be especially seen from the QRPD estimates, the 2009 benefit increase had a larger effect on lower quantiles of the food spending distribution. Throughout the remainder of the distribution, impacts are similar to the average impact, as denoted by the dashed line. These larger impacts in the bottom twenty percent

[23] Results are also summarized in tables A2 and A3 in appendix.

of the distribution conform to theory as this part of the distribution has a relatively higher probability of containing extramarginal households. Another finding from this figure is that the effect on inframarginal households' food expenditure is almost uniform. In other words, we do not find any evidence of significant heterogeneity within a majority of the distribution which most likely contains the inframarginal subpopulation.

Turning to figure 5, we see that there are differences between pooled QR and QRPD, especially at low quantiles and somewhat at very high quantiles. While these offsetting tails could in part explain why no significant mean effect on nondurable spending was detected, the main reason seems to be the small size of the benefit enhancements relative to expenditure on nondurables. Again, we do not find evidence of substantial heterogeneity in spending responses following the ARRA implementation. One might be inclined to roughly interpret the results as a first-order improvement in the total nondurable spending distribution, in the sense that we observe some significant positive effects without any significant negative effects. However, it is equally possible that we cannot reject the null hypothesis that the quantile regression is curve is equal to no effect at all.[24] We therefore take the conservative stance that the ARRA implementation led to a first-order improvement in the material well-being of SNAP participants as defined by food expenditure, but when examining the broader measure of nondurables there appears to be at least no disimprovement.

Figure 6 shows the impacts of the ARRA expiration on food expenditure. The fixed-effect (QRPD) coefficient estimates are all insignificant. However, we do see especially large negative impacts within the lower part of the distribution (as one would expect given the extramarginal nature) and positive effects within the upper half of the distribution. Moreover, while the insignificant QTE estimates could again be due to the relative small size of the benefit cuts, another hypothesis is that, instead of adjusting their food spending, households reduced their expenditure on other items to maintain their food consumption. To test this hypothesis, we estimate the equation (2)

[24]Chernozhukov and Hansen (2006) provide a method to test such hypotheses in the case of standard quantile regression. Similar methods have not been applied to the case of QRPD.

for nondurable nonfood spending of SNAP households. Results are presented in figure 7. As can be seen, the benefit cuts shifted the distribution of the nondurable nonfood spending to the left, as indicated by the mostly negative coefficient estimates. Therefore, it appears that SNAP households attempted to smooth their food spending in response to benefit cuts by decreasing their nonfood expenditure.

Finally, figure 8 plots the estimated QTEs of the ARRA expiration on total nondurable spending. Again, we observe negative impacts at lower quantiles and positive but insignificant impacts on higher quantiles. We might expect positive impacts at high quantiles if this part of the distribution is characterized by households who use other resources (e.g., savings, selling off assets, and/or borrowing) to avoid a reduction in total spending.

7. Conclusions

This paper investigates the heterogeneity in the well-being effects of the implementation and the subsequent expiration of the ARRA. We use nondurable consumption, represented by food spending and total nondurable expenditure, as the measure of material well-being. 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-induced benefit changes impacted the average household in the context of policy applications. However, it is equally desirable to understand how such policy changes impact households differently given, for example, the substantial heterogeneity in food preferences and resource constraints. Moreover, SNAP participation is endogenous given participants self-select into the program for unobservable reasons. We simultaneously account for heterogeneity in benefit changes and endogeneity due to self-selection using a fixed-effects quantile estimator.

Consistent with previous studies (Nord and Prell 2011; Beatty and Tuttle 2015; Kim 2016), we find that the ARRA implementation increased average quarterly food spending of SNAP households. We extend such results and, as predicted by Southworth's (1945) theory, the ARRA implementation had much larger effects at lower quantiles of the food-spending distribution, where one would expect to find a high proportion of extramarginal households. Within the remaining portion of the distribution containing mostly inframarginal households, we find responses to the benefit increase to be almost uniform. This finding is of importance for policymakers as it indicates that the average rise in food spending is due to the increase in food spending of all SNAP households and not a subgroup of households with stronger preferences for non-food. Simply put, our findings suggest that ARRA implementation "worked" in that it had its intended impact across the distribution.

We find a similar increase in the distribution of nondurable spending, whereby spending mainly increased across the distribution. We do note that results for this more aggregate measure of well-being are weaker, but this is to be expected since food spending represents a relatively smaller share of total nondurable spending. Overall, our results imply that ARRA implementation led to a first-order improvement in the material well-being of SNAP participants, as measured by their food spending.

With respect to the ARRA expiration, both average and quantile estimates suggest no significant adverse effect on food expenditure. However, we find that benefit cuts led to a first-order disimprovement in the distribution of nondurable nonfood expenditure of SNAP households. One implication of this finding is that SNAP households must have preferred to maintain their food spending level by reducing their spending on other nondurable items. For example, Dean and Rosenbaum (2013) calculate that the benefits cuts were equal to taking away 21 meals per month for a family of four. This behavior highlights the importance of food as well as the role of the SNAP benefits in the overall budget of low-income households.

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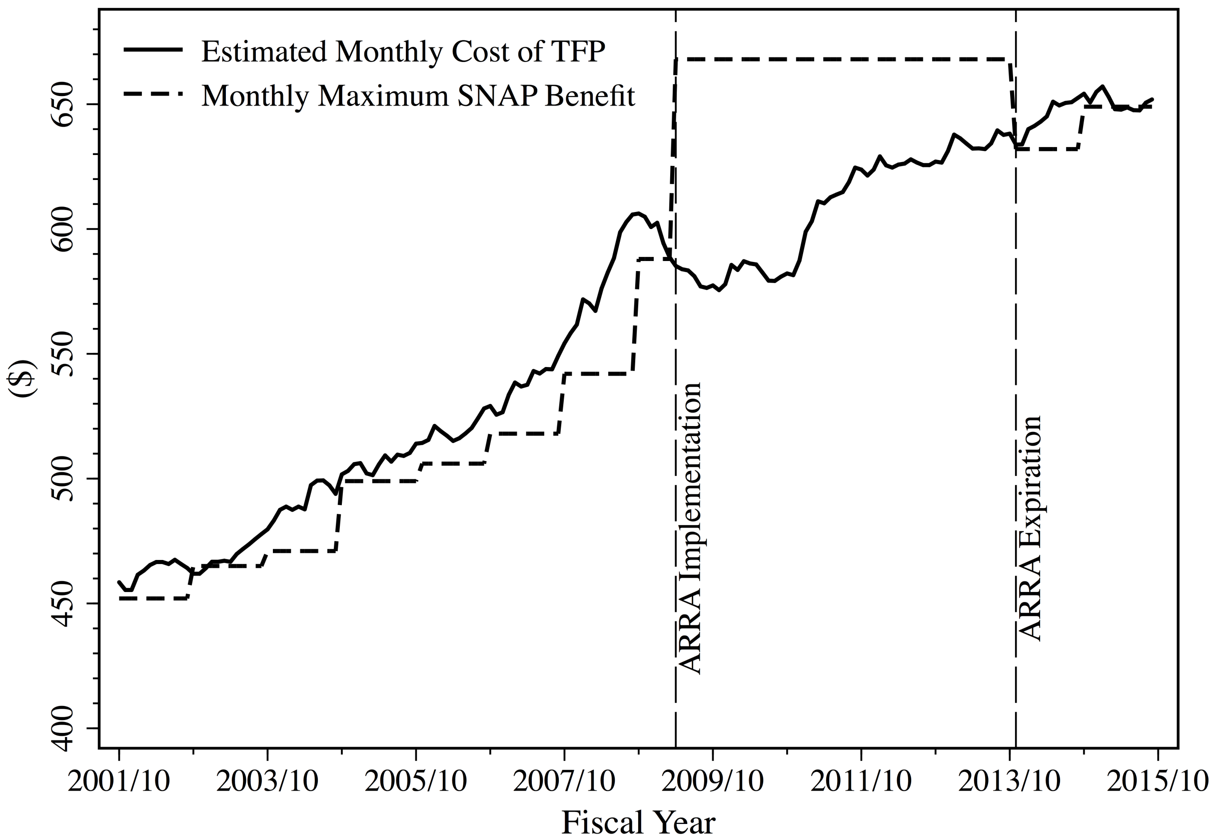

Figure 1. USDA estimated monthly cost of the Thrifty Food Plan (TFP) and monthly maximum SNAP benefit for a household of four, fiscal year 2002 – 2015

Source: Authors calculation using USDA data from "USDA Food Plans: Cost of Food", available at: https://www.cnpp.usda.gov/USDAFoodPlansCostofFood/reports>

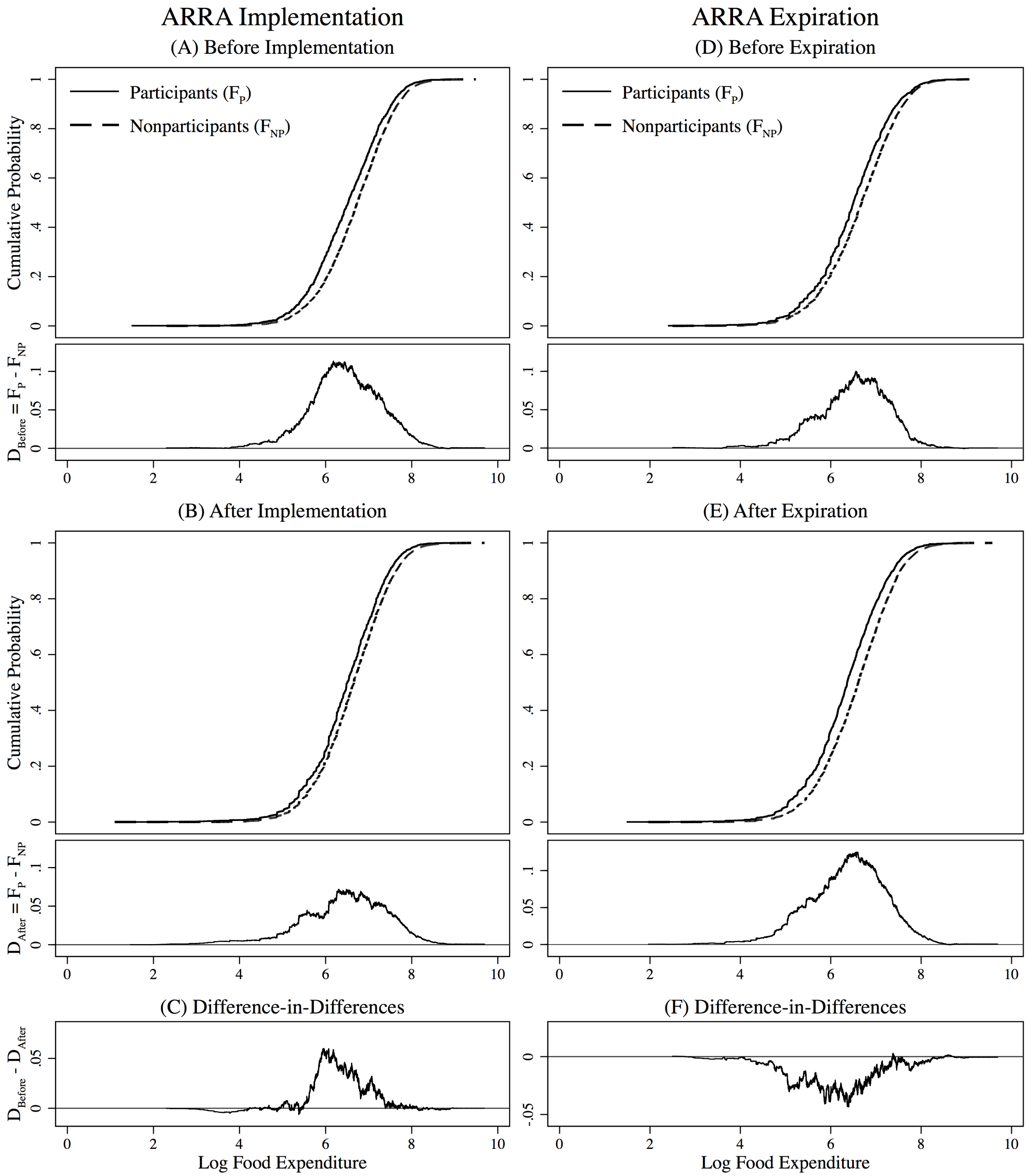

Figure 2. Unconditional cumulative distribution functions (CDFs) of food spending

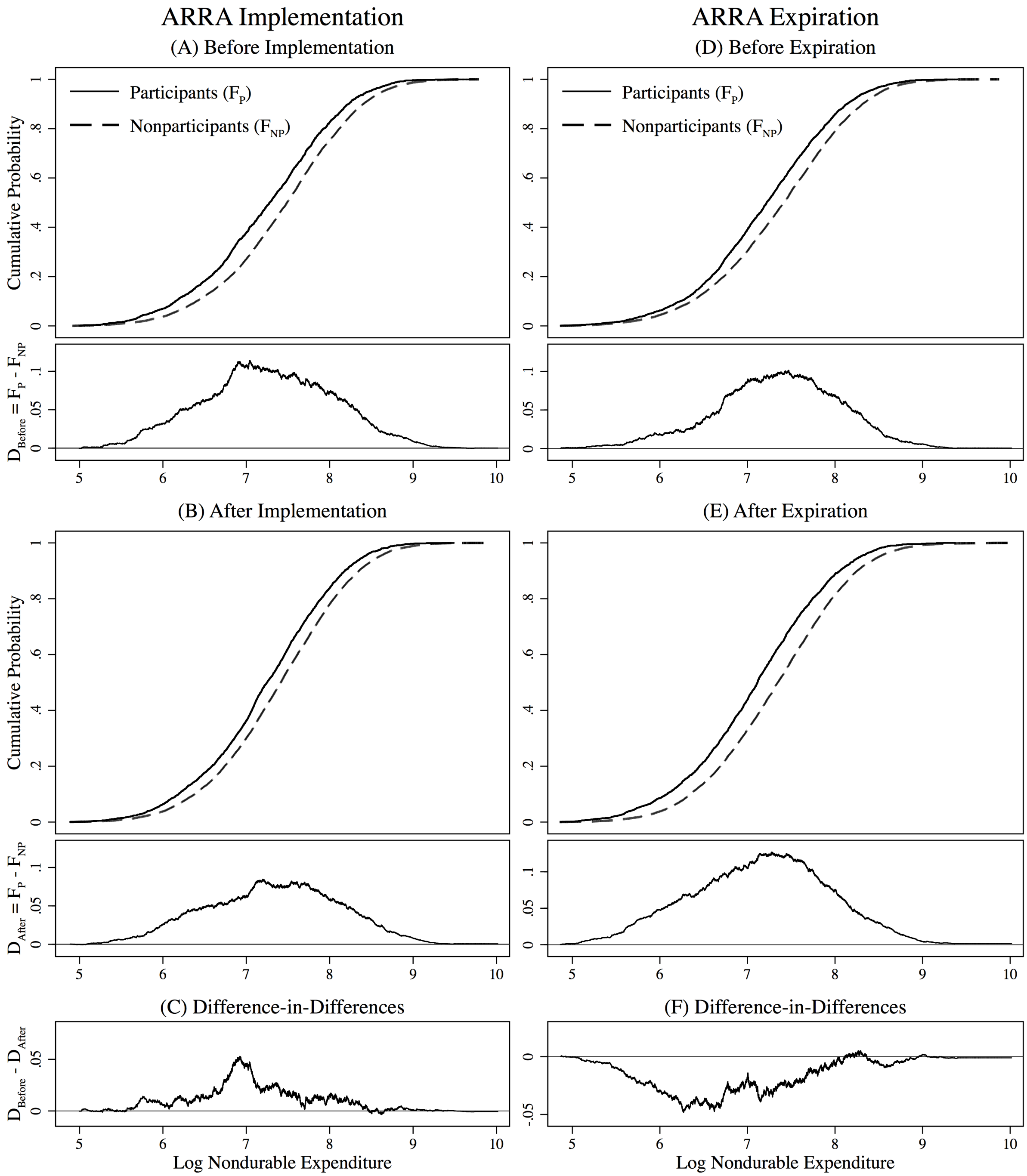

Figure 3. Unconditional cumulative distribution functions (CDFs) of total nondurable spending

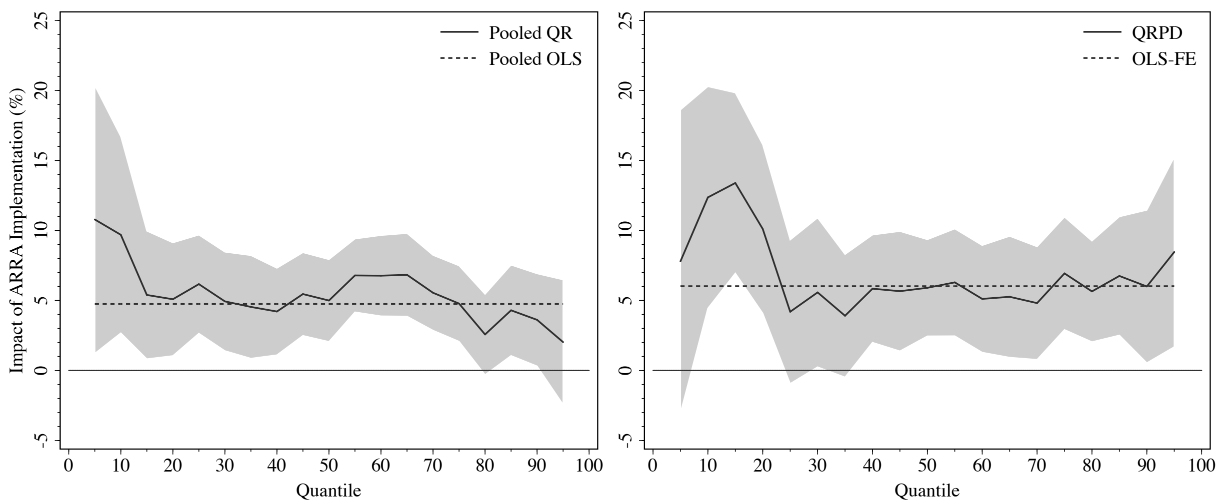

Figure 4. Impact of the ARRA implementation on the distribution of food expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. All calculations use survey weights (N = 33,248).

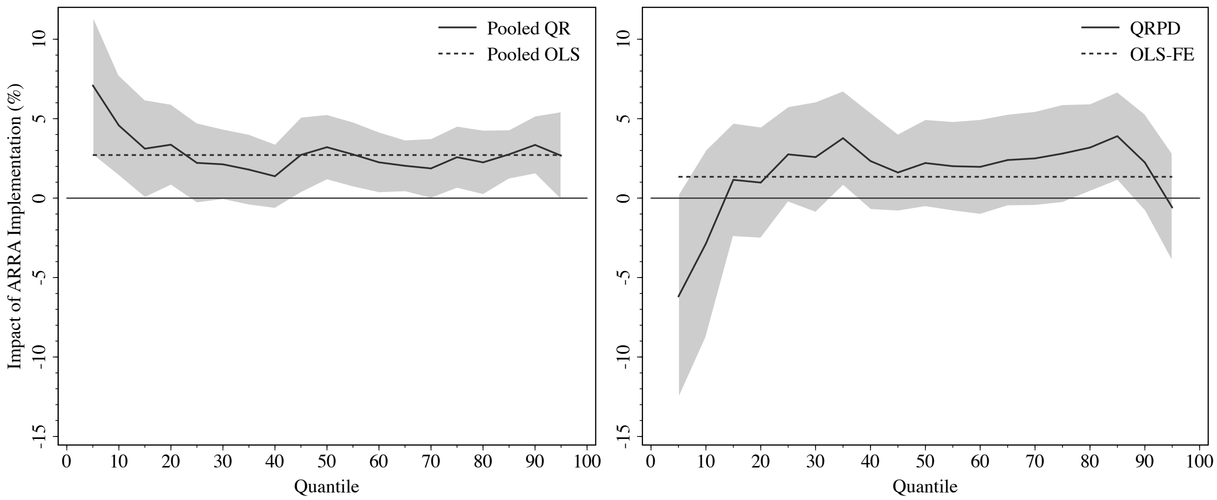

Figure 5. Impact of the ARRA implementation on the distribution of total nondurable expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. All calculations use survey weights (N = 33,248).

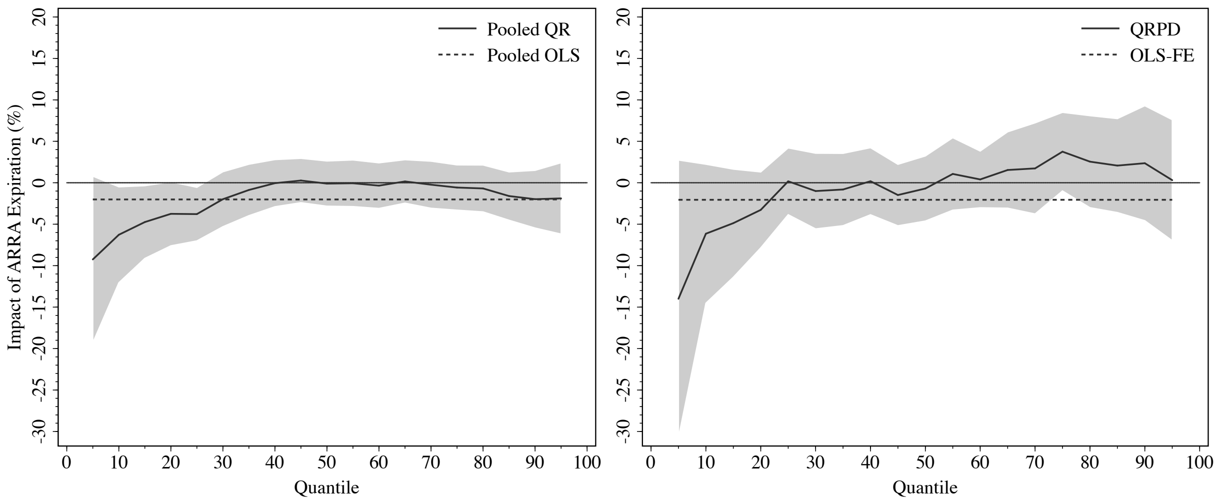

Figure 6. Impact of the ARRA expiration on the distribution of food expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. All calculations use survey weights (N = 31,837).

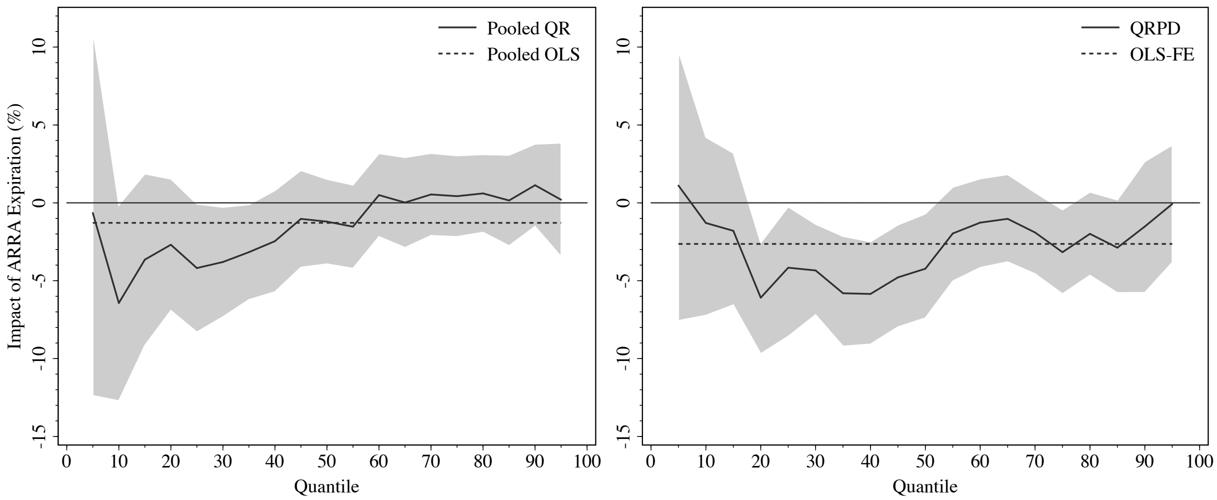

Figure 7. Impact of the ARRA expiration on the distribution of nondurable nonfood expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. All calculations use survey weights (N = 31,837).

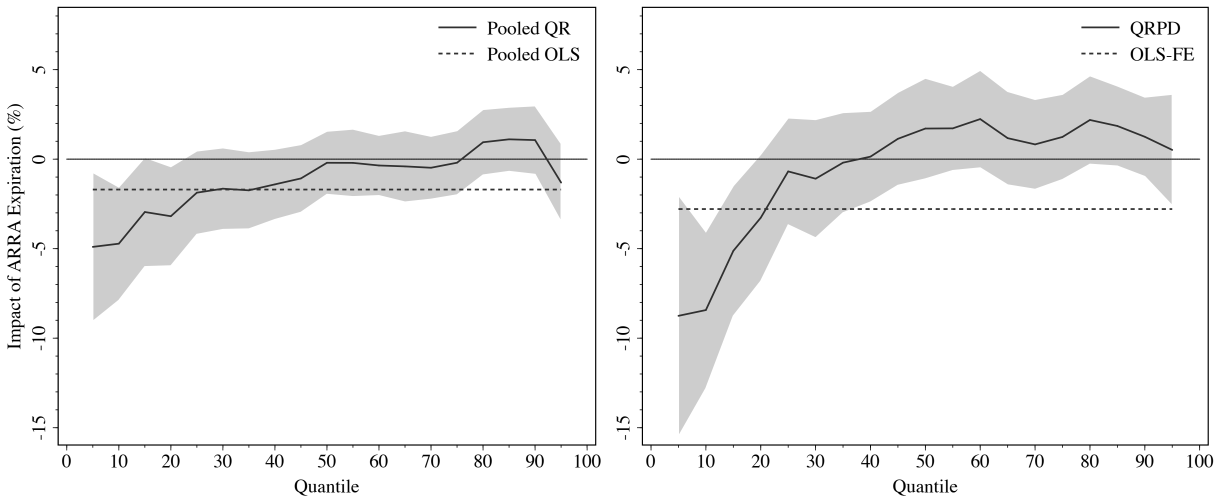

Figure 8. Impact of the ARRA expiration on the distribution of total nondurable expenditure

Notes: Shaded areas represent 90% confidence intervals (CI) and are calculated pointwise from the posterior of MCMC draws. QTE estimates are reported for quantiles 0.05 to 0.95 in 0.05 intervals. All calculations use survey weights (N = 31,837).

Tables

Table 1. Household Summary Statistics

	ARRA Implementation		ARRA Expiration	
	Participants	Nonparticipants	Participants	Nonparticipants
Married	0.27	0.45	0.25	0.43
	(0.01)	(0.01)	(0.01)	(0.01)
Female	0.73	0.55	0.68	0.55
	(0.01)	(0.01)	(0.01)	(0.01)
Employed	0.43	0.58	0.40	0.57
	(0.01)	(0.00)	(0.01)	(0.01)
Household Size	3.03	2.37	2.86	2.33
	(0.04)	(0.02)	(0.04)	(0.02)
White	0.65	0.80	0.66	0.80
	(0.01)	(0.00)	(0.01)	(0.00)
Black	0.29	0.14	0.28	0.15
	(0.01)	(0.00)	(0.01)	(0.00)
Other Race	0.05	0.06	0.06	0.06
	(0.00)	(0.00)	(0.01)	(0.00)
Age	44.28	51.30	46.35	52.20
	(0.37)	(0.20)	(0.34)	(0.20)
No. of Observations	5,332	27,916	6,059	25,778
No. of Households	2,610	13,330	3,186	12,903

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

Table 2. Household Quarterly Expenditures

	Mean	SE	p5	p25	p50	p75	p95
			Panel A: ARRA Implementation				
Total Expenditure							
Participants	3708.28	(66.56)	733.74	1640.28	2844.39	4793.21	9567.64
Nonparticipants	5346.85	(41.10)	1037.85	2304.92	4001.06	6901.37	14503.94
Nondurable Expenditure							
Participants	1804.63	(30.74)	357.02	831.35	1415.73	2398.62	4510.54
Nonparticipants	2155.71	(15.85)	440.89	989.25	1687.59	2809.49	5410.61
Food Expenditure							
Participants	880.16	(15.10)	160.11	388.29	663.90	1182.80	2274.57
Nonparticipants	1035.58	(7.87)	195.82	454.15	786.80	1357.70	2627.56
Nondurable Nonfood Expenditure							
Participants	920.86	(18.20)	122.62	354.40	680.90	1216.26	2540.31
Nonparticipants	1113.33	(9.26)	158.52	447.96	825.14	1448.40	2987.57
			Panel B: ARRA Expiration				
Total Expenditure							
Participants	3536.41	(54.96)	728.64	1612.12	2743.51	4529.33	8846.20
Nonparticipants	5181.58	(40.67)	1013.20	2219.85	3851.37	6661.98	13926.26
Nondurable Expenditure							
Participants	1653.36	(25.40)	334.19	770.17	1290.29	2177.71	4192.82
Nonparticipants	2019.94	(15.07)	429.76	923.16	1596.18	2644.57	4996.48
Food Expenditure							
Participants	823.79	(13.31)	154.33	362.67	618.20	1064.64	2156.49
Nonparticipants	982.54	(7.44)	186.17	430.47	766.19	1298.21	2471.94
Nondurable Nonfood Expenditure							
Participants	828.86	(14.45)	109.73	327.48	622.80	1097.40	2238.75
Nonparticipants	1032.34	(9.01)	154.64	413.84	764.83	1350.53	2699.64

Notes: All calculations use survey weights. Standard errors (SE) in parentheses 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 corresponding Consumer Price Indices (CPIs). All differences between SNAP participants and nonparticipants are statistically significant at 1% significance level.

Table 3. Average Effects of the ARRA on Expenditures

Dependent variable:	Log Food Expenditure		Log Nondurable Expenditure	
	POLS	FE-OLS	POLS	FE-OLS
	Panel A: ARRA Implementation			
$SNAP_{it}$	-7.27***	-4.95*	-1.30	-0.33
	(1.65)	(2.72)	(1.04)	(1.62)
$post_t$	-3.95***	-0.14	-0.42	-0.58
	(0.83)	(1.63)	(0.61)	(1.08)
$SNAP_{it} \times post_t$	4.90**	6.02**	2.95**	1.34
	(2.04)	(2.99)	(1.28)	(1.79)
$log(X_{it})$	53.97***	41.22***	65.55***	46.78***
	(0.73)	(1.43)	(0.61)	(1.29)
	Panel B: ARRA Expiration			
$SNAP_{it}$	-5.61***	-2.62	-1.59*	-0.40
	(1.32)	(1.95)	(0.90)	(1.24)
$post_t$	0.36	2.19	1.35**	-0.06
	(0.86)	(1.63)	(0.61)	(1.10)
$SNAP_{it} \times post_t$	-2.08	-2.05	-1.90	-2.79
	(1.81)	(2.84)	(1.19)	(1.83)
$log(X_{it})$	52.75***	40.39***	63.04***	46.77***
	(0.78)	(1.41)	(0.61)	(1.21)
No. of Observations (Panel A) = 33,248				
No. of Observations (Panel B) = 31,837				

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 so they represent percentages.

Table A1. Expenditure Groups and Their Subgroups

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

Table A2. Quantile Treatment Effects of the ARRA Implementation on Expenditures

	Log Food Expenditure		Log Nondurable Expenditure	
Quantile	Estimate	SE	Estimate	SE
5	7.80	(6.58)	-6.19	(3.90)
10	12.36	(4.82)	-2.87	(3.59)
15	13.39	(3.92)	1.15	(2.17)
20	10.11	(3.66)	0.98	(2.13)
25	4.20	(3.12)	2.75	(1.83)
30	5.58	(3.24)	2.58	(2.12)
35	3.91	(2.67)	3.77	(1.81)
40	5.84	(2.34)	2.33	(1.86)
45	5.66	(2.60)	1.61	(1.48)
50	5.90	(2.10)	2.21	(1.67)
55	6.29	(2.33)	2.01	(1.71)
60	5.11	(2.32)	1.97	(1.82)
65	5.26	(2.63)	2.40	(1.76)
70	4.81	(2.45)	2.50	(1.80)
75	6.94	(2.45)	2.81	(1.88)
80	5.65	(2.19)	3.18	(1.68)
85	6.75	(2.57)	3.90	(1.70)
90	6.00	(3.32)	2.25	(1.86)
95	8.46	(4.12)	-0.59	(2.07)
No. of Observations = 32,248				

Notes: Estimates are from Powell's (2016) Quantile Regression for Panel Data (QRPD). All regressions use survey weights. Standard errors (SE) in parentheses are calculated pointwise from the posterior of MCMC draws.

Table A3. Quantile Treatment Effects of the ARRA Expiration on Expenditures

	Log Food Expenditure		Log Nondurable Expenditure	
Quantile	Estimate	SE	Estimate	SE
5	-13.99	(10.17)	-8.75	(4.10)
10	-6.15	(5.09)	-8.43	(2.66)
15	-4.87	(3.96)	-5.12	(2.21)
20	-3.27	(2.78)	-3.27	(2.15)
25	0.18	(2.45)	-0.68	(1.82)
30	-1.00	(2.77)	-1.09	(2.01)
35	-0.81	(2.65)	-0.19	(1.70)
40	0.18	(2.46)	0.14	(1.54)
45	-1.48	(2.25)	1.14	(1.58)
50	-0.69	(2.38)	1.71	(1.71)
55	1.06	(2.65)	1.72	(1.43)
60	0.40	(2.08)	2.24	(1.66)
65	1.54	(2.80)	1.17	(1.59)
70	1.73	(3.34)	0.82	(1.53)
75	3.75	(2.88)	1.24	(1.45)
80	2.55	(3.37)	2.19	(1.50)
85	2.07	(3.44)	1.85	(1.36)
90	2.36	(4.21)	1.25	(1.35)
95	0.31	(4.43)	0.52	(1.89)
No. of Observations = 31,837				

Notes: Estimates are from Powell's (2016) Quantile Regression for Panel Data (QRPD). All regressions use survey weights. Standard errors (SE) in parentheses are calculated pointwise from the posterior of MCMC draws.