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IZA DP No. 3653

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ABSTRACT

Heterogeneous Impacts of Conditional Cash Transfers: Evidence from Nicaragua

In the last decade, the most popular policy tool used to increase human capital in developing countries has been the conditional cash transfer program. A large literature has shown significant mean impacts on schooling, health, and child labor. This paper examines heterogeneous effects using random-assignment data from the Red de Proteccion Social in rural Nicaragua. Using interactions between the targeting criteria and the treatment indicator, estimates suggest that children located in more impoverished localities experienced a larger impact of the program on schooling in 2001, but this finding is reversed in 2002. Estimated quantile treatment effects indicate that there is considerable heterogeneity in the impacts of the program on the distribution of food expenditures, as well as total expenditures. In particular, households at the lower end of the expenditure distribution experienced a smaller increase in expenditures. This paper also presents evidence of the rank invariance assumption to help clarify the interpretation of the quantile treatment effect in the development literature context.

JEL Classification: O15, I38

Keywords: Nicaragua, conditional cash transfers, quantile treatment effect

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I. INTRODUCTION

The most popular policy tool used in the last decade in developing countries to increase human capital has been the conditional cash transfer program, which provides cash payments to households conditional on regular school attendance and visiting health clinics. Many governments implemented experimental frameworks to assess the impacts of conditional cash transfers on employment, schooling, and health among poor eligible households (PROGRESA in Mexico and PRAF in Honduras, among others). Though conditional cash transfers have achieved quantified success in reaching the poor and bringing about short-term improvements in consumption, education, and health (Schultz 2004; Gertler 2004; Rawlings and Rubio 2003), most of the literature has focused on mean impacts. As Heckman, Smith, and Clements (1997) point out, however, judgments about the "success" of a social program should depend on more than the average impact. For example, it may be of interest to investigate whether social programs have differential effects for any subpopulation defined by covariates, for example gender effects, or whether there is heterogeneity in the effect of treatment. Knowledge of whether a program's impacts are concentrated among a few individuals is important for the effectiveness of the program in reaching its target population.

This paper contributes to the small but growing literature on the estimation of heterogeneous effects of conditional cash transfers in developing countries. This type of program has received a great deal of attention among policymakers, influencing adoption of new policies in Latin and Central America. The assessment of heterogeneous impacts is done with a unique data set from a social experiment in Nicaragua designed to evaluate a conditional cash transfer program targeted to poor rural households, the Red de Proteccion Social (hereafter RPS) or Social Safety Net. The analysis takes advantage of the random assignment of localities to treatment and control groups so that program participation is not correlated in expectation with either observed or unobserved individual characteristics and outcome differences provide an unbiased estimate of the true mean impact of the program. The purpose of this paper is to investigate the degree of heterogeneity in program impacts of the RPS program for education, health, and nutrition in Nicaragua. This paper explores the heterogeneity of impacts as a function of observable characteristics (age, gender, poverty, and household head characteristics) and the criteria used by the RPS to select beneficiaries. This paper also investigates the overall heterogeneity of program impacts using quantile treatment effects (QTE), which allows us to test whether conditional transfers lead to larger or smaller changes in some parts of the outcome distribution.

This paper adds to the existing literature in several ways. First, the existing literature on conditional cash transfers focuses on mean impacts, in the full sample and in demographic subgroups. This paper goes beyond mean impacts and interaction variables and tests whether there are heterogeneous impacts of conditional cash transfers on the distribution of expenditures. Conditional cash programs, such as the RPS, have differential effects on household behavior given that transfers affect regular school attendance and health visits. For example, the school cash transfer is conditional on regular attendance of children age 7 to 13 years who have not yet completed the 4th grade. For households with children age 7 to 13 years who have <u>not</u> completed fourth grade and are <u>not</u> attending school, the program has income effects of the cash transfer and substitution effects of a lower price of schooling driven by the attendance requirement. Some households may have to bear the cost of children's foregone labor earnings due to the implicit reduction in labor time, in which case the impact on household expenditures may be negative if the RPS transfer does not make up for losses in income from market work. The monetary transfer received to buy food (or food cash transfer), however, has a positive effect on household expenditure. The net effect on expenditures could be positive or negative. Conditional cash transfers have differential effects based on whether the household is meeting the requirements prior to the implementation of the program. Knowing more about this heterogeneity is relevant to anti-poverty policies (Ravallion, 2005).

Second, the literature on QTE has been limited mostly to the US context. Recent papers have used QTE to assess the impacts of training programs on labor outcomes such as Heckman, Smith, and Clements (1997); Black, Smith, Berger, and Noel (2002); Abadie, Angrist, and Imbens (2002); and Firpo (2007). Bitler, Gelbach, and Hoynes (2005, 2006) examine the impact of welfare reform experiments on earnings and total income. Overall, the main finding is that variation in the impact of treatment across persons is an important aspect of the evaluation problem. To the best of my knowledge, Djebbari and Smith's (2005) study represents the first to analyze heterogeneous impacts of social programs in a developing country using QTE.

Third, QTE corresponds, for any fixed percentile, to the horizontal distance between two cumulative distribution functions. Under the rank preservation assumption, QTE can be interpreted as the treatment effect for individuals at particular quantiles of the control group outcome distribution or the treatment effect for each quantile in the distribution (Bitler, Gelbach, and Hoynes, 2005). Without the rank preservation assumption, QTE represents how various quantiles of the outcome distribution change in the treatment and control groups, but we cannot make inference on the impact on any particular person. This paper presents evidence of rank invariance in the RPS context to help clarify the interpretation of the QTE impacts for the development literature.

The main results show that impact estimates vary among the eligible population. From the analysis on subgroups, the estimates show that boys experienced a larger positive impact of the program on schooling and a negative impact on the probability of engaging in labor activities and hours worked. The estimates also show that older children experienced a smaller impact of the program on schooling and participation in labor activities. There are also differential impacts by whether the child is living with a male head of household and with education of the head of household. To assess the effectiveness of the targeting criteria, the analysis considers the interaction between the treatment indicator and marginality index and household per capita expenditures, separately. The main results show that children located in more impoverished areas experienced a larger impact on schooling and a smaller impact on working hours.

From the QTE analysis, the estimates suggest that the positive program impact in per capita food expenditures and total per capita expenditures is smaller for households who are in the lower tail of the expenditure distribution. The estimates show that program impacts are larger for households who had lower levels of food shares prior to the program. These findings are consistent with the theoretical prediction that treatment effects on expenditures are lower for households whose costs of complying with the program requirements are highest. Tests of the null hypothesis of constant treatment effects reveal that these findings could not have been revealed using mean impact analysis. Finally, joint tests of rank preservation show that the distributions of observable characteristics in all ranges of the expenditures distribution do not vary significantly between the treatment and control group.

The rest of the paper is organized as follows. Section II presents the RPS Program and data and Section III outlines the theoretical framework. Section IV outlines the empirical strategy and this is followed by a discussion of the empirical results in Section V. Section VI concludes.

II. THE RPS PROGRAM

II.1 Program Structure and Benefits

Nicaragua is a lower-middle income country. With an estimated per capita GDP of US$817 in 2004, Nicaragua remains the second poorest country in the Latin America and Caribbean region after Haiti. In 2000, the Nicaraguan government implemented the *Red de Proteccion Social* or Social Safety Net to encourage educational attainment and help impoverished households in rural areas. Phase I of the program started with a budget of US$11 million, representing approximately 0.2 percent of Nicaragua's GDP (Maluccio and Flores 2005). With financial assistance from the Inter-American Development Bank and the government of Nicaragua, the RPS program was expanded in 2002 with a US$20 million budget for coverage for an additional three years. The RPS program provided benefits conditional on school attendance and health checkups, where participants were identified using a detailed targeting process aimed at reaching poor people in rural areas.

For Phase I of the RPS, the Government of Nicaragua selected the departments of Madriz and Matagalpa from the northern part of the Central Region. This selection was based on the departments' ability to implement the program in terms of institutional and local government capacity, high poverty levels within the communities, and proximity to the capital of Nicaragua. In 1998, approximately 80 percent of the rural population in Madriz and Matagalpa was poor and half was extremely poor.

Targeting of poor households was implemented at the RPS headquarters in two stages: (1) officials selected six municipalities within these two departments based on criteria similar to those used at the department level, (2) officials selected eligible comarcas within the selected municipalities based on the marginality index constructed from the 1995 National Population and Housing Census. Comarcas (hereafter called localities) are administrative areas within municipalities including between one and five small communities averaging 100 households each. This marginality index used locality-level information on the illiteracy rate of persons over age 5, access to basic infrastructure (running water and sewage), and average family size. The higher the value of the marginality index, the more impoverished the area. Out of 59 localities, 42 eligible rural localities were identified as having a high or very high marginality index and thus pre-selected for the program.[1]

Program benefits are conditional income transfers composed of (Table 1):

1. Each eligible household received money to buy food (called the food cash transfer) every other month. In order to receive this transfer, a household member (typically the mother) is required to attend educational workshops and bring their children under the age of five for preventive health care appointments (including vaccinations and growth monitoring). Children younger than age two were seen monthly and those between age two and five, every other month. In September 2000, the food transfer was US$224 a year, representing 13 percent of total annual household expenditures in beneficiary households before the program.
2. Contingent on enrollment and regular attendance, each household with children age 7 to 13 who had not completed the fourth grade of primary school received a fixed cash transfer every other month. [2,3] In addition, for each eligible child in the household enrolled in school, the household received an annual lump sum transfer for school supplies and uniforms (called the school supplies transfer).[4] In September 2000, the school attendance transfer and the school supplies transfer were US$112 and US$21, respectively.

To enforce compliance with program requirements, beneficiaries did not receive the transfer if they failed to carry out the conditions previously described. Less than 1 percent of households were expelled during the first two years of delivering transfers, though 5 percent voluntarily left the program, e.g., by dropping out or migrating out of the program area (Maluccio and Flores, 2005).

II.2 The Experimental Design and Data

The evaluation design is based on an experiment with randomization of localities into treatment and control groups. One-half of the 42 localities were randomly selected into the program. The selection was done at a public event in which the localities were ordered by their marginality index scores and stratified into seven groups of six localities each. Within each group, randomization was achieved by blindly drawing one of six colored balls without replacement; the first three were selected in the program and the other three in the control group.[5]

All households in selected localities are interviewed before and after the random assignment. The evaluation dataset consists of panel-data observations for 1,359 households over 3 rounds of survey (baseline: September 2000, follow-ups: October 2001 and October 2002). Surveys at the individual and household level collected information on socioeconomic and demographic characteristics such as parental schooling, labor market outcomes, health, nutrition, and attributes of the physical infrastructure of the household, among others.[6] Table 2 presents descriptive statistics for each year. Prior to the program, the mean per capita consumption was 3885 Nicaraguan Cordobas (hereafter C$) or about US$298.9 a year, with 70 percent allocated to food consumption. Table 2 also shows that 77 percent of children aged 7 to 13 years attend school and 15 percent participate in labor activities for an average of 23.5 hours per week.

The randomization is at the locality level rather than at the household or individual level. One reason for doing the random assignment at the locality level was to avoid spillover effects between treated and untreated individuals in the same locality. This was part of the motivation for doing the random assignment at the village level in the PROGRESA evaluation as well.[7] Assignment by randomization at the locality level ensures the treatment and control groups are similar on average in terms of observable and unobservable characteristics. There is a chance, however, of observing some non-randomness in terms of differences between localities selected for the control and treatment groups at the household level prior to the program, since estimates of average quantities are more reliable with large sample sizes and the sample subject to randomization is small (42 localities) (Behrman and Todd, 1999). Table 3 shows t-tests of the equality of means at the household and individual level. Main results show that the majority of variables measured prior to the random assignment do not differ between the treatment and control groups, which suggest that the sample is well balanced across these groups.[8]

III. THEORETICAL FRAMEWORK

This section outlines a model of household decision-making in the presence of conditional cash transfers in order to get a better understanding of potential heterogeneous impacts of the RPS program. The model is based on Skoufias and Parker (2001) and Djebbari and Smith (2005). The analysis begins first by considering household time allocation in the absence of the conditional cash transfer. Neoclassical models of household decision-making are commonly employed in this analysis. In this framework, parents make decisions about the allocation of a child's schooling time, the time of other household members, and the purchase of goods and services. Parents will invest in each child's schooling up to the point where the marginal costs of a child's time in school equal the marginal benefits considering the opportunity cost of schooling, which is the foregone earning from work.

The opportunity cost of children's time is likely to vary with observed characteristics. For example, it is expected to see gender and age differences in child labor if boys and girls have different returns to education or older children have a comparative advantage in the labor market. It is important to note that the RPS does not provide higher payments for female enrollment in school as in PROGRESA where the main idea was to equalize the incentive for girls in the face of higher wages, on average, for boys in the labor market. Girls in secondary school received slightly higher subsidies (by about $2 per month) than boys in Mexico.

The existing literature on heterogeneity of treatment effects predominantly looks at the impact of the program as if it varies with observed characteristics or subgroups of the population. In the case of conditional cash transfers, other papers have found evidence of differential impacts on schooling and child labor for girls vs. boys, and primary school-age children vs. secondary school-age children, and by socioeconomic status (for example Maluccio and Flores (2005) for RPS; Skoufias (2005) for PROGRESA; Schady and Araujo (2006) in Ecuador; Behrman, Sengupta, and Todd (2005) for PROGRESA; Filmer and Schady (2008) for Cambodia; Djebbari and Smith (2005) for PROGRESA).

The conditionality of the transfer has important effects on household behavior as well. The monetary school and food cash transfers are linked to the school attendance of children aged 7 to 13, participation in health clinics, and other criteria. If they were not conditioned, transfers would act as a pure income effect. Conditionality of the transfers results in changes in the marginal cost of investment in schooling. If children participate in the program with full compliance of the requirements, time devoted to schooling changes: children now receive transfers for attendance and school supplies but lose wages for the extra time the child devotes to schooling. What matters is the ratio of the child’s wage and the marginal increase in income due to the transfer.

Participation and compliance with the RPS program might affect household behavior as follows: [9]

- Households with no children in the targeted age ranges or with children under age 5 (but without children aged 7 to 13 who have not completed the fourth grade) receive the food cash transfer. These transfers will have a pure income effect and it is expected that these households will have higher expenditures after the program.
- Households with children aged 7 to 13 years old who have completed fourth grade at primary school and are attending school without the program will be eligible to receive food transfers but not school transfers. Food cash transfers will have a pure income effect and it is expected that these households will have higher expenditures after the program.
- Households with children aged 7 to 13 years, who have not completed fourth grade but are attending school even without the program, are eligible for both the food and school cash transfers. These transfers will have a pure income effect and it is expected that these households will have higher expenditures after the program.
- For households with children aged 7 to 13 years who have not completed fourth grade and are not attending school without the program, the RPS program combines the income effect of the school transfer with the substitution effect of a lower price of schooling driven by the attendance requirement. Some households may have to bear the cost of children's foregone labor earnings due to the implicit reduction in labor time, in which case the impact on household expenditures may be negative if the RPS transfer does not make up for losses in income from market work. Thus the program impact on household expenditures may be negative. At the same time, the food cash transfer will have an income effect. The net effect of the school and food transfer on expenditures could be positive or negative for these households.

In sum, the predicted effect on expenditures is heterogeneous. At the top of the expenditures distribution, or the richest households among the eligible ones, households are meeting or almost meeting program requirements prior to the program and thus, the impacts will be larger. For some part of the bottom of the expenditures distribution are located households who are not meeting the requirements (e.g. children are not going to school the minimum required time) and for which the cost of participation is the highest (children's contribution is significant), for them the program impacts could be positive or negative. These households are likely to be the ones who rely greatly on child labor. As Basu and Van's (1998) seminal model shows a household will send children to work if adult income or family income from non-child labor sources becomes very low. In between these extremes, the effect of the program depends on whether the child is attending school the minimum required time or not. The extent to which the program has a significant impact on different parts of the expenditure distribution can only be determined through empirical analysis.

IV. EMPIRICAL STRATEGY

Let Y_1 denote the potential outcomes of interest in the presence of treatment, $T_i = 1$, and Y_0 without treatment $T_i = 0$. Each individual experiences only one of these treatment and untreated outcomes, thus the critical objective is to establish a credible comparison group or a group of individuals who in the absence of the program would have had outcomes similar to those who were exposed to the program. In this paper the treatment and control group are randomly selected, so that program participation is not correlated in expectation with either observed or unobserved individual characteristics and outcome differences provide an unbiased estimate of the true mean impact of the program.[10]

What we are interested is in estimating the expected average effect that the RPS program have on different outcomes, or the average treatment effect (ATE),

$\Delta_{ATE} = E(Y_1 - Y_0)$. The literature also focuses on the mean impact of treatment on the treated (ATET), given by $\Delta_{ATET} = E(Y_1 - Y_0 \mid T = 1)$. Under the assumptions of no equilibrium effects and no randomization bias, the randomized experiment identifies the ATET (Djebbari and Smith, 2008).

I.V.1 Impacts at the Subgroup Level

The first method generates impact estimates that vary among the eligible population by considering variation in impacts as a function of observable characteristics through the interaction of the treatment indicator in equation (1) with a variety of individual and locality characteristics as follows:

$$y_i = \beta_0 + \beta_1 * C_i + \beta_2 T_i + \beta_3 T_i * C_i + X_i \alpha + \omega_i , \qquad (1)$$

where y_i is some outcome measure, C_i is the characteristic of interest, T_i is a dummy variable representing whether the locality was randomly assigned to the treatment or control group, and $T_i * C_i$ represent the interactions between the characteristics and the treatment indicator.

The interpretation of the coefficients is as follows: for example, in the specification that tests for heterogeneous impacts by gender, C_i is a dummy variable equal to one if the child is male, the coefficient β_1 is an estimate of the difference in the outcome between boys and girls, the RPS effect for girls is given by β_2, the corresponding effect for boys is given by the sum of the coefficients $\beta_2 + \beta_3$. If β_3 is statistically significant different from zero, there is evidence of heterogeneity of treatment effects by gender.

C_i also includes characteristics of the head of household and locality. I also use a criterion used by PRS to select beneficiaries, the locality marginality index. Program officials using data from the 1995 Nicaraguan Household Survey, collected prior to the program, constructed this index. Following the analysis in Djebbari and Smith (2005) for Mexico, the impact of conditional cash transfers is expected to be largest for households living in more impoverished localities as defined by the marginality index. If the targeting mechanism is efficient, then households in the most marginal localities get a greater program impact than less marginal places. Equation (1) also controls for other baseline household and individual characteristics (X_i) to take into account any differences that were present despite randomization and to increase the precision of the coefficient estimates. The standard errors are clustered at the locality level.

I.V.2 Quantile Treatment Effects

The method described in the previous section emphasized differences in means. While the mean is important, comparisons of means only account for shifts in the central tendency of a distribution. For many questions, knowledge of distributional parameters is required; for example, the proportion that benefit from treatment, the proportion that gain at least a fixed amount, or the quantiles of treatment effect (Heckman, Smith, and Clements, 1997). One particular feature of interest in the RPS context is the behavior at the left tail of the consumption distribution, as this measures consumption of those households that most likely are not meeting the requirements (children are not going to school the minimum required time) and for which the cost of participation is the highest (children’s contribution is significant). In order to capture responses across the entire distribution of consumption, the second econometric method uses the QTE approach.

Most of the existing literature on QTE is based on social experiments in employment, training and welfare programs in the US. Heckman, Smith, and Clemens (1997), find strong evidence that heterogeneity is an important feature of impact distributions using experimental data from the National Job Training Partnership Act Study. Black, Smith, Berger, and Noel

(2002), using experimental data from the Worker Profiling and Reemployment Services program, find that the estimated impact of treatment varies widely across quantiles of the outcome distributions. The pattern of impacts suggests that the treatment has its largest effect on persons whose probability of unemployment insurance benefit exhaustion without treatment would be of moderate duration. In evaluating the economic effects of welfare reform, Bitler, Gelbach, and Hoynes (2005, 2006) find strong evidence against the common effect assumption using experimental data from the Connecticut's Job First Waiver program and the Canadian Self-Sufficiency Project. Their estimates suggest substantial heterogeneity in the impact of welfare reform on earnings and total income, which is consistent with the predictions from the static labor supply model.

Let Y_1 and Y_0 denote the outcome of interest in the treated and control states with corresponding cumulative distribution functions $F_1(y) = \Pr[Y_1 \le y]$ and $F_0(y) = \Pr[Y_0 \le y]$. Let θ *d*enote the quantile of each distribution

$$y_\theta(T) = \inf\{y : F_T(y) \ge \theta\},\ \mathrm{T} = 0,1\ , \qquad (2)$$

where " inf " is the smallest attainable value of *y* that satisfies the condition stated in the braces. The quantile treatment effect at quantile θ is defined as $\Delta_\theta^{QTE} = y_\theta(T=1) - y_\theta(T=0)$. For example, suppose that *y* represents family income in a given year, $y_{0.25}$ is that level of income for households in the treatment (control) group such that 25 percent of treatment (control) households have income below it. $\Delta_{0.25}^{QTE}$ is given by the difference between the income of households in the 25th percentile of the treated distribution and the 25th percentile of the control distribution.

The impact estimate for a given quantile θ is the coefficient on the treatment indicator from the corresponding quantile regression as follows:

$$Q_\theta(y_i \mid T) = \alpha(\theta) + \beta(\theta)T_i\ , \qquad \theta \in (0,1)\ , \qquad (3)$$

where $Q_\theta(y_i \mid T)$ denotes the quantile θ of expenditures conditional on treatment.[11]

As presented above in table 2, the RPS sample is well balanced and there are few statistical differences in the observable characteristics in the two groups. To correct for any differences not accounted for by the randomization of localities into treatment and control groups and to obtain more precise estimates, I have included covariates as in Djebbari and Smith (2005).[12] The vector of control variables includes characteristics of the head of household (age, education, gender, employment) and household demographic composition.[13] The advantage of the QTE approach relative to the common effect model is that the impact of the program on different quantiles of the outcome distribution does not have to be constant.

Note that although average differences equal differences in averages, the treatment effect at quantile θ is not the quantile of the difference (Y_1- Y_0). The QTE corresponds, for any fixed percentile, to the horizontal distance between two cumulative distribution functions. Under the rank preservation assumption, QTE can be interpreted as the treatment effect for individuals at particular quantiles of the control group outcome distribution or the treatment effect for the person located at quantile θ in the distribution (Heckman, Smith and Clements 1997; Bitler et al. 2005). Without the rank preservation assumption, QTE represents how various quantiles of the outcome distribution change in the treatment and control groups, but we cannot make inference on the impact on any particular person.

Rank preservation across treatment status is a strong assumption as it requires that the rank of the potential outcome for a given individual would be the same under treatment as under non-treatment. There are two ways to deal with cases where the rank invariance assumption is not valid. Heckman, Smith, and Clements (1997) suggest computing bounds for the QTE, allowing for several possibilities of reordering of the ranks. The second approach argues that even without this assumption, QTE estimates are informative about the overall impacts of the program and therefore still meaningful parameters for policy purposes.

In the absence of rank invariance, the interpretation of QTE is the difference in the treated and control distributions, not the treatment effects for identifiable people in either distribution (Bitler, et al., 2005 and 2006). The last section of the paper analyzes whether the rank invariance assumption is valid in the RPS context.

Outcomes of Interest

The outcomes of interest in the empirical section include household and individual level variables. The RPS program aims at improving the educational and health outcomes of children. I focus on children aged 7 to 13 years old at the baseline because they are most likely to be affected by the conditionality of the cash transfers. Outcomes of interest include child labor (participation and working hours) and school attendance. Child labor refers to children who are engaged in market work, which includes wage employment, self-employment, agriculture, unpaid work in a family business, and helping on the family farm.[14] Impacts for schooling and child labor outcomes are estimated using OLS. One important feature of the data is the presence of a substantial number of children reporting zero hours of work, thus I also include the estimates from the Tobit regression.

To analyze the QTE on household welfare the empirical literature uses household consumption rather than income because data on expenditures are likely to be more accurate and consumption expenditures have a stronger link with current levels of welfare (Deaton 1997). At the household level, this paper analyzes three outcomes of interest: per capita total expenditure, per capita food expenditure, and food share of total expenditures. The analysis of food expenditures is important because one of the keys of the program is supplementing income to increase expenditures on food so as to improve household nutrition. The expenditure variables include food, non-food items, and the value of food produced and consumed at home.[15]

V. RESULTS

V.1 Impacts along Observable Characteristics

Table 4 reports estimated coefficients of the treatment indicator interacted with covariates of interest. The main results show that the program has different impacts on children with different observable characteristics. For example, boys aged 7 to 13 years experienced a statistically significant larger impact on school attendance than girls. Estimates suggest that the RPS program increased school attendance by 12 percentage points for girls and by 18 percentage points for boys in 2001. In addition, the reduction in the probability of engaging in market activities is larger for boys. Estimates show that boys experienced a greater impact of the program on reducing the probability of engaging in market work and hours worked. The results show that the RPS program decreased participation in labor activities for boys by 11 percentage points in 2001 and 14 percentage points in 2002, while the negative effect of the RPS program on labor participation for girls is small, just one percentage point in both years. These findings are important given that the program did not provide differential transfers to boys and girls. For instance, PROGRESA provided slightly more money to girls enrolled in secondary school and the results show that the program had a greater impact on secondary age girls (Skoufias and Parker, 2001). In addition, it is important to note that this definition of work does not include other activities usually not remunerated and performed in the same household, such as taking care of younger siblings, cleaning, and cooking, among other household chores. A broader definition including detailed household chores may decrease this gender difference in participation rates.

The coefficient on the interaction term between treatment and age shows that older children experienced a smaller impact of the program on schooling, as well as on the probability of engaging in market work and hours worked. This is related to previous findings that with higher age potential earnings increase, thus transfers might not be high enough to compensate for foregone earnings. The interaction with household head education shows that children with more educated head of households experienced a smaller impact of the program on schooling. The empirical literature has shown that more educated parents use the information provided in health clinics about nutrition more efficiently and value schooling more and child labor less (Strauss and Thomas, 1996). Thus, school attendance among children living in more educated households would be higher and the margins for improvement lower than among children in households with lower parental education. In addition, children living with a male head of household experienced a smaller impact of the program on school attendance, participation in labor activities, and hours worked. Similarly, children living in larger households experienced a smaller impact of the program on school attendance, labor force participation and hours worked.

The last rows show the effect of the treatment interacted with the marginality index and household per capita expenditures, separately, to analyze the targeting mechanism of the program. Based on the marginality index, I group households into quintiles and interact the treatment indicator with the index categories. [16] If the actual targeting of the program is efficient then the impact in schooling should decrease from the poorest index (expenditures) quantiles to the richest index (expenditures) quantiles. As the estimates from Table 4 show, children living in more impoverished areas experienced larger impacts of the program on school attendance in 2001. Similar results are obtained with the interaction of the treatment indicator and quintiles of household per capita expenditures. In 2002, however, children living in more impoverished areas experienced a smaller impact of the program on schooling. For example, children in the first quintile of the marginality index (poorest) experienced an increased in school attendance of 8 percentage points, whereas children in the highest quintile experienced an increase in schooling of 19 percentage points. The estimates also show that children in the poorest households experienced smaller impacts of the program on the probability of engaging in labor activities. Most of the interactions between the treatment indicator and the quintiles of the marginality index (expenditures), however, are not statistically significant. Finally, the last row in Table 4 rejects the null hypothesis that all of the coefficients on the interaction terms equal zero.

V.2 Quantile Treatment Effect Regression

The quantile treatment effects provide information on how the impact at the household level varies at different points of the expenditure distribution. Figures 1 through 6 plot the quantiles using post-treatment data. The solid line represents the estimate of the RPS treatment in a given quantile. The associated 95 percent confidence intervals are obtained from the bootstrap with 1000 replications clustered at the locality level. These bootstrap confidence intervals are plotted on the graph with dashed lines. For comparison purposes, the mean treatment effect is plotted as a small dashed line.[17]

Overall, RPS treatment group expenditures are greater than control group expenditures, yielding positive impacts at each quantile of the distribution. For per capita total expenditures and per capita food expenditures, the difference increases from the lowest percentile to the highest percentile of the distribution. These findings suggest that households with lower expenditures tend to receive lower positive impacts from the program. As the theoretical framework suggests the impacts are greater for households with higher expenditures who are more likely meeting or almost meeting program requirements prior to the program. For households with lower expenditures who are more likely not meeting the requirements and for whom the cost of participation is therefore the highest, program impacts are still positive but smaller than for households at the upper end of the distribution. These results are similar to Djebbari and Smith's (2005) QTE findings for the Mexican's PROGRESA. They find that program impacts on wealth and nutrition are greater for households who were at higher levels of wealth and nutrition prior to the program. Similarly, for the share of food expenditure, the difference decreased from the lowest percentile to the highest percentile suggesting that the program impacts are higher for households who had lower levels of food shares prior to the program.[18]

Figure 1 shows that in 2001 the program impact on per capita total expenditures varies from about C$707 (US$ 54) to C$3087 (US$ 237). In 2002, the program impact on per capita total expenditures varies from about C$264 (US$20) to C$1293 (US$99) for the highest percentile (Figure 2). Many of the impacts are quite large compared to the mean impacts of C$1184 and C$820 in 2001 and 2002, respectively. These results suggest that households at the top of the outcome distribution receive more than five times the impact that households with lower expenditures do.

I test whether a constant treatment effect could lead to a range as large as that observed for the QTE point estimate as in Bitler et al. (2006). The test is as follows: first, keep only observations in the control group and assign a uniformly distributed random number to the i^{th} household in the b^{th} bootstrap sample.[19] Second, sort the sample of households using this random number and assign $t=1$ to households with a random number higher than 0.5 and in the b^{th} sample and $t=0$ to the remaining households in this bootstrap sample. Third, add the estimated mean treatment effect to households with $t=1$ to create a synthetic null treatment group distribution. Finally, use the synthetic null treatment group and the remaining control group to construct the QTE under the null hypothesis. From the resulting individual distributions, we can generate a confidence interval for testing the maximum minus minimum range, which compares the distribution for the range under the null with the real-data QTE range. This confidence interval is estimated with 1000 bootstrap replications. The test of constant treatment effects suggests that the null constant treatment range is [3187.1, 3384.1] and [2987.9, 3198.2] at a confidence level above 95 percent for

2001 and 2002, respectively. The QTE range estimated using the data is 2380.3 and 1029.8 for 2001 and 2002. These results show that the mean treatment effect is not sufficient to characterize RPS's effects on total per capita expenditures.[20]

Consistent with the RPS program's goal, additional expenditures as a result of the transfers were spent predominantly on food. Results for food expenditures suggest a large degree of treatment impact heterogeneity. In 2001, the program impact on per capita food expenditures varies from about C$367 (US$28) to C$3780 (US$290) for the highest percentile of the distribution (Figure 3). In 2002, the program impact on per capita food expenditures varies from about C$174 (US$13) to C$1846 (US$142) for the highest percentile of the distribution (Figure 4). The mean impacts are C$1004 and C$733 for 2002 and 2001, which are far below the impacts at the top of the distribution. The confidence interval for a null of constant treatment effects is [1963.6, 2104.9] and [1993.8, 2186.4] at a confidence level of above 95 percent, while the estimated range over all quantiles in the real data is 3412.2 and 1671.7 for 2001 and 2002, respectively. The positive impact of the program for households with the highest per capita food expenditures prior to the program is almost seven times the impact for households with lower food expenditures, which is not captured by the mean treatment effect estimate.

To further explore the impacts of RPS on the distribution of expenditures, Figures 5 and 6 show QTEs for the share of food expenditures in the household budget. In 2001, the program impact on food share ranges from about 7.79 percentage points to -0.18 percentage points for the highest percentile. In 2002, the program impact on food share varies from about 8.65 percentage points to -1.42 percentage points for the highest percentile. The mean impacts are about 4.0 and 3.8 percentage points in 2001 and 2002. The impact is higher for households who have a lower share of food expenditures prior to the program. Maluccio and

Flores (2005) have shown that not only the number of food items purchased increased but also their nutritional value.

Rank Preservation and Rank Reversal

The main QTE findings show that the impact of the RPS program varied across the distribution of total and food expenditures. As previously discussed, the impact of the treatment on the distribution is not the distribution of treatment effects. This interpretation is valid only under the rank preservation assumption. This section examines whether there is evidence consistent with rank preservation. As in Bitler et al. (2005), I use the treatment and control distributions of demographic characteristics to see if there is evidence against rank preservation or rank reversal in each quartile. For example, if the distribution of observable characteristics in some range of the expenditures distribution varies significantly between the treatment and control group, this would be evidence against rank preservation. Note, however, that rank reversal may have occurred among unobservables even if observable characteristics do not change.

Tables 5 and 6 present the mean difference by quartile and the p-value for statistical significance. Each row corresponds to a demographic variable separated by household head characteristics (gender, education, age, and employment) and household demographic composition (girls 0-5 years, boys 0-5 years, girls 6-15 years, and boys 6-15 years). The test is performed for a total of 8 variables and, thus, tables 5 and 6 present 32 tests for each distribution and year. Panel A classifies people by their position (quartile) in the per capita total expenditure distribution and Panel B classifies people by their position in the per capita food expenditure distribution. Table 5 presents the results from the exercise using the 2001 data, whereas Table 6 presents the results using the 2002 data.

Of the 128 differences, 18 are statistically significant at the 10 percent level or below in 2001 and 2002. The individual test suggests that some rank reversal may be present based on these demographic characteristics. The joint test for the significance of the differences within a given quantile range, however, fails to reject the null for all ranges of per capita food expenditure and per capita total expenditure.[21] While the individual tests suggest that some rank reversal may be present along observables, the joint test results show that strict rank reversal is not rejected.

VI. SUMMARY AND CONCLUSIONS

This paper assesses the importance of heterogeneity in impacts of conditional cash transfers using a social experiment from a poverty alleviation program in Nicaragua. Heterogeneity in program impacts is expected to arise because of the design and implementation of the program. The theoretical model shows that program impacts vary with observable characteristics, the targeting dimension, and the conditionality of the program.

The first part of the paper analyzes impacts at the subgroup level by estimating the interaction between the treatment indicator and covariates of interest. The estimates also show that children living in more impoverished localities experienced larger impacts of the program on schooling in 2001, but this result is reversed in 2002. The second part of the paper analyzes quantile treatment effects. The results suggest evidence against the common effect assumption. The estimates show that the impact of the program is lower for households who were at a lower level of expenditures prior to the program. That is, the RPS program has a greater effect on households who would otherwise have had a high per capita total and food expenditures. Quantile treatment effect estimates show there was considerable heterogeneity in the impacts of the RPS on the distributions of expenditures, which is missed by looking only at average treatment effects. As the theoretical framework suggests, the impacts are greater for households with higher expenditures who are more likely to be meeting or almost meeting program requirements prior to the program. For households with lower expenditures who are more likely to not be meeting the requirements, and for whom the cost of participation is the highest, program impacts are still positive but smaller than for households at the upper end of the distribution. Tests of the null hypothesis of constant treatment effects reveal that these findings could not have been obtained using mean impact analysis. In addition, joint tests of rank preservation show that the distributions of observable characteristics in all ranges of the expenditures distribution do not vary significantly between the treatment and control group. These results have important implications for the implementation and evaluation of conditional cash transfers that are spreading rapidly in developing countries.

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Table 1: Nicaraguan RPS Beneficiary Requirements

	Household Type		
	With no targeted children	With children aged 0-5	With children aged 7 -13 who have not completed 4th grade
	(A)	(B)	(C)
Attend bimonthly health education workshops	✓	✓	✓
Bring children to prescheduled health care appointments Monthly (0-2 years) Bimonthly (2-5 years)		✓	
Adequate weight gain for children under 5[a]		✓	
Enrollment in grades 1 to 4 of all targeted children in the household			✓
Regular attendance (85%) of all targeted children in the household			✓
Promotion at end of school year [b]			✓
Bono a la Oferta or Teacher transfer			✓
Up-to-date vaccination for all children under 5 years		✓	

[a] This requirement was discontinued in Phase II in 2003
[b] This condition was not enforced
Source: Maluccio and Flores (2005)

Table 2: Descriptive Statistics for Children and Their Households

	2000	2001	2002
Household Level			
Per Capita Total Consumption	3885.08	3852.74	3880.91
Per Capita Food Consumption	2672.33	2669.20	2634.18
Food Share	0.704	0.688	0.683
Head of Household			
Age	44.27	46.05	47.01
Male	0.858	0.858	0.858
Years of Education [a]	1.652	-	-
Children 7-13 years at baseline			
Gender	0.521	0.521	0.521
Age	9.847	10.938	11.969
School Attendance	0.766	0.885	0.855
Participation in Labor Activities	0.150	0.109	0.177
Weekly Working Hours	3.51	2.90	5.31
Weekly Working Hours conditional on employment	23.47	26.49	30.07

[a] Data available only for 2000

Note: Expenditures levels are in Nicaraguan Cordobas, the equivalent exchange rate is US$1 = C$13. Sample includes 1359 households with observations in the panel 2000-2001-2002.

Table 3: RPS Summary Statistics: 2000 Baseline
(*Standard errors in parentheses*)

	Treatment	Control	Difference
Household Level			
Per Capita Total Consumption	4020.90	3738.24	282.66
	(203.22)	(212.23)	(290.34)
Per Capita Food Consumption	2759.87	2577.68	182.193
	(129.02)	(128.33)	(179.81)
Food Share	0.70	0.71	-0.01
	(0.01)	(0.01)	(0.01)
Household Head			
Age	44.64	43.86	0.77
	(0.85)	(0.73)	(1.11)
Male	0.87	0.85	0.02
	(0.01)	(0.01)	(0.02)
Years of Education	1.70	1.60	0.10
	(0.14)	(0.09)	(0.16)
N	706	653	1359
Children 7-13 years			
Gender	0.53	0.51	0.02
	(0.02)	(0.02)	(0.02)
Age	9.82	9.87	-0.05
	(0.07)	(0.07)	(0.10)
School Attendance	0.77	0.77	0.00
	(0.01)	(0.01)	(0.02)
Participation in Labor Activities	0.14	0.16	-0.02
	(0.01)	(0.01)	(0.02)
Working Hours	3.26	3.78	-0.52
	(0.33)	(0.37)	(0.49)
N	916	829	1745

**Statistically significant at 5% level, *Statistically significant at 10% level (only for differences).
Note: Expenditures levels are in Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. Robust standard errors, clustered at the locality level. Sample includes 1359 households with observations in the panel 2000-2001-2002.

Table 4: RPS Program Impacts along Observables Characteristics for children 7 to 13 years at baseline
(*Standard errors in parentheses*)

	2001				2002			
	School Attendance	Participation in labor activities	Hours Worked (OLS)	Hours Worked (Tobit)	School Attendance	Participation in labor activities	Hours Worked (OLS)	Hours Worked (Tobit)
T*male	0.060**	-0.099**	-4.034**	-8.085	0.063**	-0.124**	-4.771**	-7.330
	(0.03)	(0.04)	(1.07)	(9.27)	(0.03)	(0.04)	(1.41)	(7.59)
T	0.117**	-0.012	-0.383	-12.335	0.110**	-0.014	-0.901	-11.778
	(0.03)	(0.01)	(0.31)	(8.52)	(0.02)	(0.02)	(0.67)	(8.74)
T*age	-0.003	-0.019**	-0.739**	-1.677	0.018	0.000	-0.716*	3.712**
	(0.01)	(0.01)	(0.30)	(1.91)	(0.01)	(0.01)	(0.38)	(1.75)
T	0.185*	0.145*	5.605*	1.068	-0.068	-0.073	5.165**	-65.299**
	(0.10)	(0.08)	(2.81)	(23.47)	(0.02)	(0.11)	(3.96)	(24.75)
T*Household Head Schooling	-0.028**	-0.006	-0.203	-1.328	-0.014*	-0.007	-0.029	-0.612
	(0.01)	(0.01)	(0.22)	(1.90)	(0.01)	(0.01)	(0.26)	(1.44)
T	0.195**	-0.053**	-2.148**	-16.812**	0.165**	-0.067*	-3.352**	-16.408**
	(0.03)	(0.03)	(0.80)	(5.77)	(0.03)	(0.04)	(1.01)	(5.70)
T*Household Head is Male	-0.019	0.076*	2.577*	35.752**	-0.034	0.005	0.588	-1.361
	(0.07)	(0.04)	(1.39)	(13.54)	(0.07)	(0.06)	(2.20)	(10.54)
T	0.165**	-0.131**	-4.759**	-51.842**	0.172**	-0.082	-3.905*	-16.191
	(0.06)	(0.04)	(1.25)	(12.70)	(0.07)	(0.06)	(2.26)	(12.08)
T*Household Size	-0.002	0.011**	0.172	2.377**	-0.006	-0.001	0.003	-0.383
	(0.01)	(0.01)	(0.15)	(1.19)	(0.01)	(0.01)	(0.27)	(1.41)
T	0.164**	-0.155**	-3.921**	-39.437**	0.195**	-0.070	-3.410	-14.120
	(0.07)	(0.05)	(1.28)	(11.56)	(0.05)	(0.09)	(2.69)	(15.04)

**Statistically significant at 5% level, *Statistically significant at 10% level. *Note:* Expenditures levels are in Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. Robust standard errors, clustered at the locality level. Sample includes 1359 households with observations in the panel 2000-2001-2002.

Table 4: RPS Program Impacts along Observables Characteristics for children 7 to 13 years at baseline (continued)

(*Standard errors in parentheses*)

	2001				2002			
	School Attendance	Participation in labor activities	Hours Worked (OLS)	Hours Worked (Tobit)	School Attendance	Participation in labor activities	Hours Worked (OLS)	Hours Worked (Tobit)
Quintiles of the Marginality Index								
T	0.190**	-0.078*	-3.117**	-24.558**	0.081**	-0.072	-1.262	-15.598
	(0.08)	(0.05)	(1.38)	(11.17)	(0.03)	(0.08)	(1.25)	(11.52)
T*2nd Quintile	-0.039	-0.001	-0.036	2.590	0.112**	-0.008	-2.443	-3.149
	(0.11)	(0.06)	(1.88)	(15.22)	(0.05)	(0.09)	(2.20)	(15.89)
T*3rd Quintile	-0.122	0.100*	3.495**	24.086	-0.018	0.075	0.374	9.348
	(0.10)	(0.06)	(1.70)	(17.03)	(0.05)	(0.09)	(2.16)	(15.85)
T*4th Quintile	-0.056	0.005	0.689	8.794	0.069	0.009	-2.396	4.925
	(0.09)	(0.05)	(1.81)	(12.04)	(0.06)	(0.11)	(2.31)	(14.58)
T*5th Quintile (richest)	-0.012	0.017	0.537	4.535	0.107*	-0.055	-5.112**	-13.667
`	(0.11)	(0.06)	(1.74)	(13.35)	(0.06)	(0.09)	(1.92)	(14.32)
Quintiles of Household Per Capita Expenditures								
T	0.197**	-0.032	-2.219*	-13.496*	0.200**	-0.062	-2.749**	-17.976**
	(0.06)	(0.04)	(1.20)	(8.38)	(0.05)	(0.04)	(1.36)	(7.65)
T*2nd Quintile	-0.001	-0.058	-1.501	-17.453	-0.030	-0.055	-1.228	-4.258
	(0.06)	(0.05)	(1.75)	(12.42)	(0.06)	(0.06)	(1.99)	(10.06)
T*3rd Quintile	-0.033	0.014	1.547	6.149	-0.103	0.012	-0.048	5.637
	(0.06)	(0.05)	(1.42)	(11.30)	(0.07)	(0.06)	(1.83)	(10.74)
T*4th Quintile	-0.147**	-0.075*	-0.748	-14.530	-0.133*	0.010	-0.079	7.082
	(0.06)	(0.04)	(1.42)	(10.62)	(0.07)	(0.06)	(1.95)	(10.29)
T*5th Quintile (richest)	-0.080	-0.040	-0.562	-3.207	-0.031	-0.048	-1.640	-2.381
	(0.06)	(0.06)	(1.35)	(15.80)	(0.07)	(0.06)	(2.02)	(10.90)
F-test for the null that all	4.55	2.48	3.97	23.97[b]	2.98	2.01	2.28	22.08[b]
interactions=0 (p-value)[a]	(0.000)	(0.013)	(0.000)	(0.031)	(0.004)	(0.045)	(0.023)	(0.054)

**Statistically significant at 5% level, *Statistically significant at 10% level. [a] F-test obtained from a equation including all interactions and main effects. [b]Chi-square test

Note: Expenditures levels are in Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. Robust standard errors, clustered at the locality level. Sample includes 1359 households with observations in the panel 2000-2001-2002.

Table 5: Tests of Rank Reversal from Distribution of Observables for Ranges in Expenditure Distribution, 2001

	$q \leq 25$		$25 \prec q \leq 50$		$50 \prec q \leq 75$		$q \succ 75$	
	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value
Panel A: Per capita Total Expenditure Distribution Ranges								
Household Head is male	0.000	0.016	-0.001	0.609	-0.002	0.497	0.000	0.112
Household Head Years of Education	0.007	0.605	-0.007	0.345	0.004	0.050	0.011	0.058
Household Head is employed	0.000	0.856	0.002	0.441	-0.001	0.649	0.000	0.445
Household Head age	-0.012	0.178	0.029	0.816	0.037	0.661	-0.003	0.455
Girls 0-5 years	-0.003	0.567	0.001	0.527	0.001	0.972	-0.005	0.617
Girls 5-15 years	0.004	0.515	0.001	0.986	0.002	0.948	-0.009	0.816
Boys 0-5 years	0.005	0.948	0.000	0.513	-0.005	0.002	-0.006	0.178
Boys 5-15 years	0.003	0.820	0.001	0.341	-0.002	0.048	-0.009	0.074
Panel B: Per capita Food Distribution Ranges								
Household Head is male	0.000	0.192	0.001	0.633	-0.004	0.503	0.000	0.150
Household Head Years of Education	0.008	0.583	-0.011	0.593	-0.009	0.140	0.011	0.130
Household Head is employed	-0.001	0.463	0.004	0.439	-0.001	0.411	0.000	0.687
Household Head age	-0.007	0.108	-0.083	0.471	0.171	0.749	-0.014	0.301
Girls 0-5 years	-0.003	0.525	-0.001	0.549	0.003	0.926	-0.004	0.583
Girls 5-15 years	0.007	0.359	-0.002	0.864	-0.002	0.333	-0.009	0.443
Boys 0-5 years	0.004	0.317	0.004	0.523	-0.007	0.002	-0.005	0.062
Boys 5-15 years	0.003	0.497	-0.002	0.222	0.007	0.036	-0.010	0.098

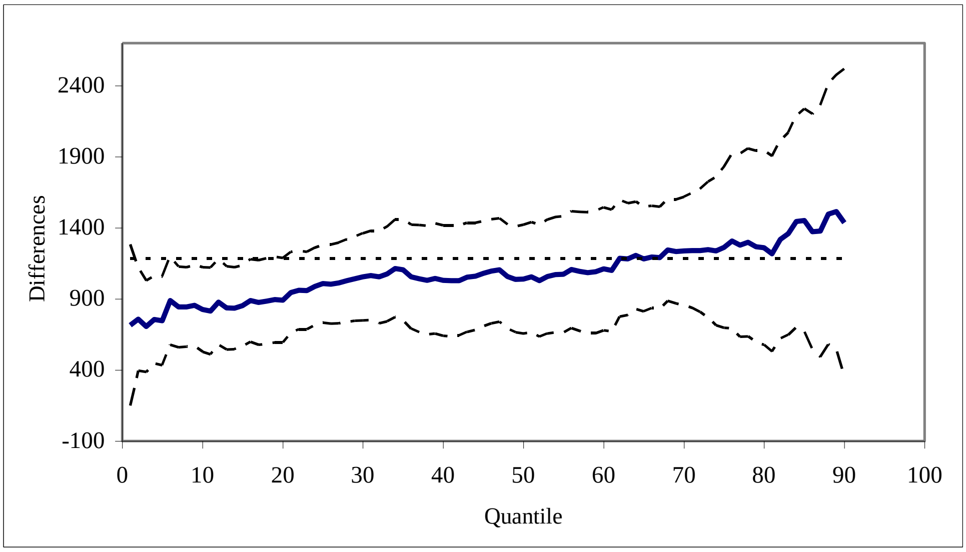
Note: Mean treatment-control differences and p-values for tests of individual differences being significant for each observable characteristic at each quartile. P-values obtained from the bootstrap with 1000 replications clustered at the locality level. Null distribution derived as in Bitler et. al. (2005).

Table 6: Tests of Rank Reversal from Distribution of Observables for Ranges in Expenditure Distribution, 2002

	$q \leq 25$		$25 \prec q \leq 50$		$50 \prec q \leq 75$		$q \succ 75$	
	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value
Panel A: Per capita Total Expenditure Distribution Ranges								
Household Head is male	0.000	0.695	-0.001	0.371	0.000	0.629	-0.001	0.271
Household Head Years of Education	0.009	0.854	0.011	0.160	0.000	0.749	0.005	0.573
Household Head is employed	0.000	0.617	0.002	0.934	0.000	0.467	-0.001	0.495
Household Head age	0.107	0.391	-0.108	0.774	-0.036	0.383	0.061	0.291
Girls 0-5 years	0.006	0.982	-0.002	0.196	0.000	0.539	-0.007	0.379
Girls 5-15 years	-0.003	0.022	0.011	0.112	0.004	0.389	-0.013	0.820
Boys 0-5 years	0.006	0.948	0.000	0.156	-0.001	0.056	-0.007	0.122
Boys 5-15 years	0.005	0.583	-0.002	0.028	0.003	0.944	-0.010	0.110
Panel B: Per capita Food Distribution Ranges								
Household Head is male	0.000	0.365	0.001	0.695	-0.003	0.948	0.000	0.425
Household Head Years of Education	0.014	0.160	0.011	0.904	-0.010	0.902	0.006	0.471
Household Head is employed	0.000	0.665	0.002	0.788	-0.001	0.894	0.000	0.583
Household Head age	0.087	0.423	-0.075	0.170	-0.046	0.866	0.059	0.273
Girls 0-5 years	0.007	0.733	-0.002	0.443	-0.001	0.481	-0.006	0.435
Girls 5-15 years	0.000	0.032	0.012	0.255	0.000	0.084	-0.012	0.890
Boys 0-5 years	0.004	0.471	0.005	0.072	-0.004	0.361	-0.006	0.092
Boys 5-15 years	0.004	0.950	0.002	0.008	0.004	0.549	-0.011	0.248

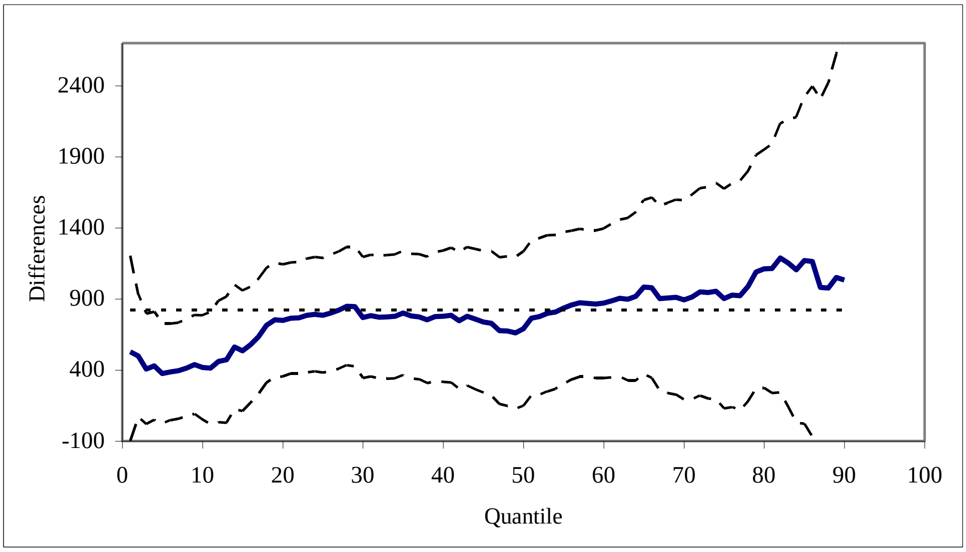
Note: Mean treatment-control differences and p-values for tests of individual differences being significant for each observable characteristic. P-values obtained from the bootstrap with 1000 replications clustered at the locality level. Null distribution derived as in Bitler et. al. (2005).

Figure 1: Quantile Treatment Effect on the Distribution of Annual Per Capita Total Expenditures in 2001



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

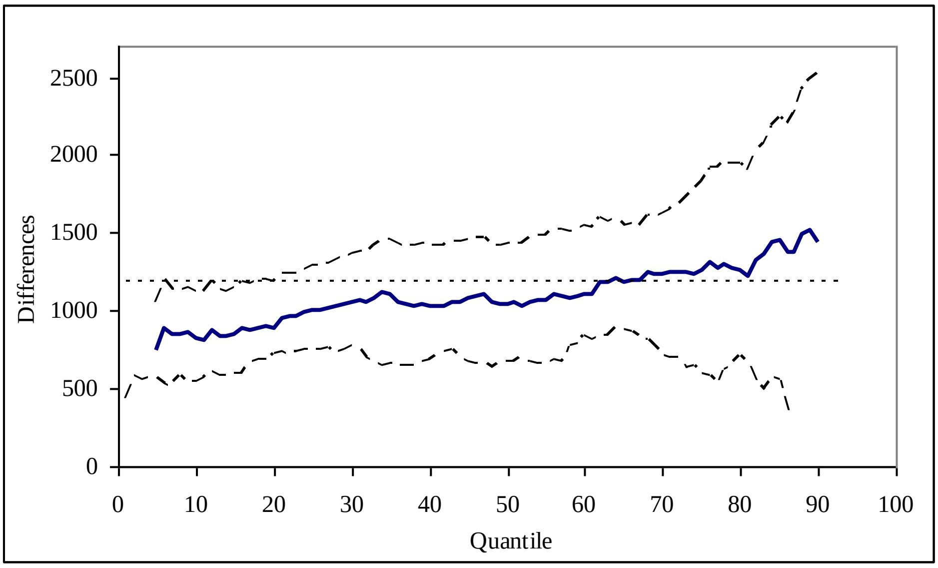
Figure 2: Quantile Treatment Effect on the Distribution of Annual Per Capita Total Expenditures in 2002



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is \$US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

Figure 3: Quantile Treatment Effect on the Distribution of

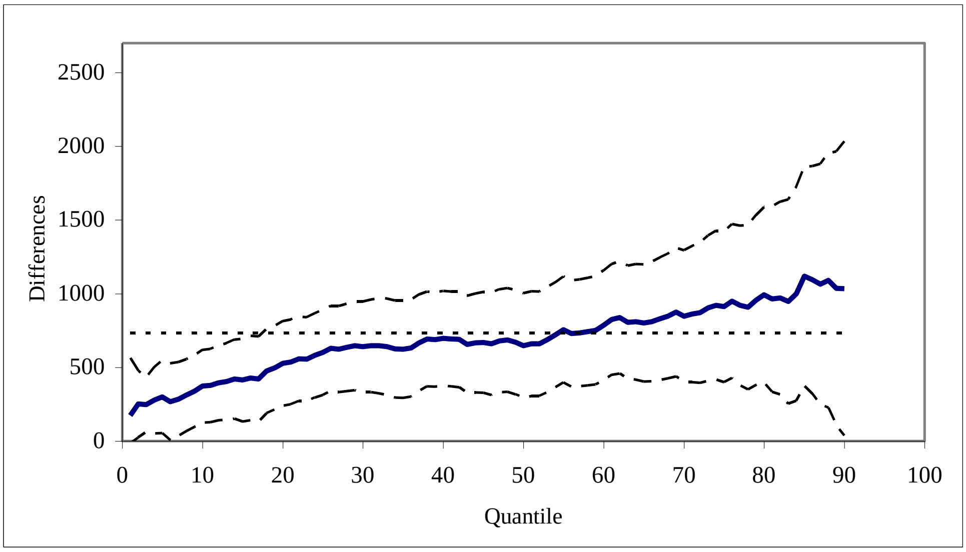
Annual Per Capita Food Expenditures in 2001



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

Figure 4: Quantile Treatment Effect on the Distribution of

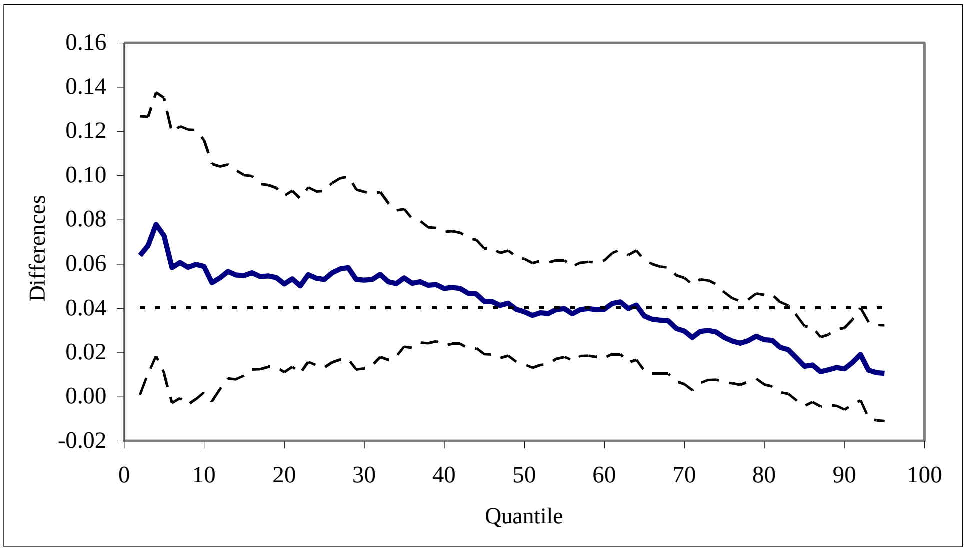
Annual Per Capita Food Expenditures in 2002



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is $US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

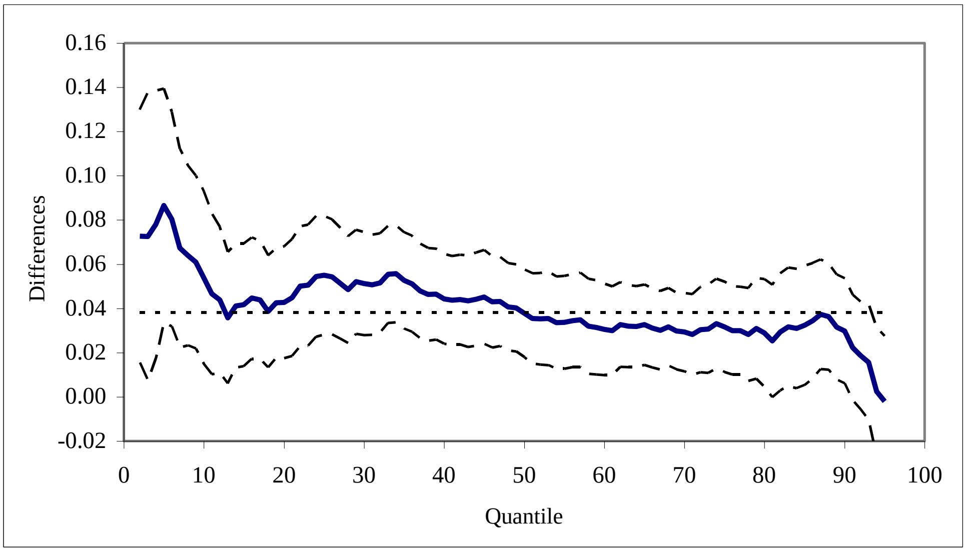
Figure 5: Quantile Treatment Effect on the

Distribution of Food Share in 2001



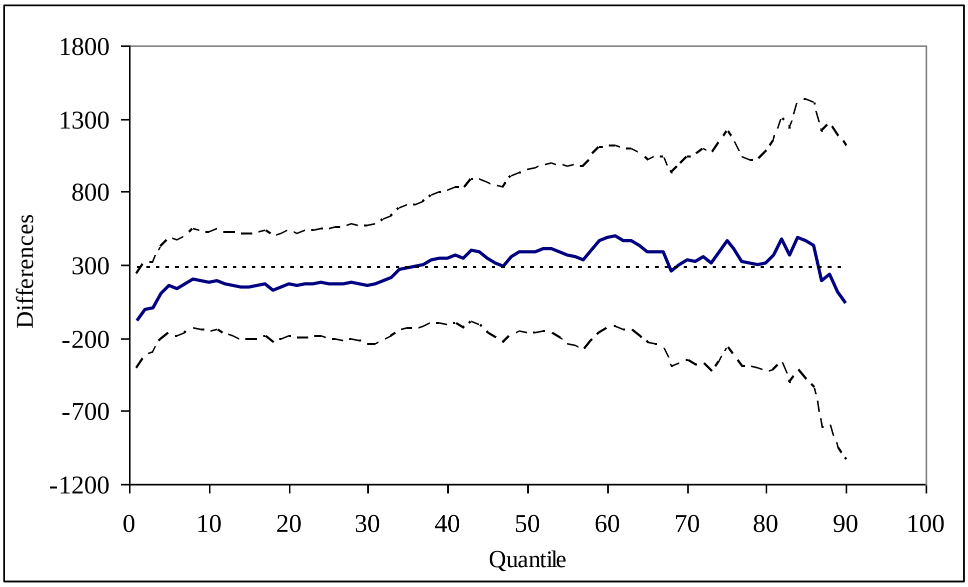
Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) QTE is computed for the 1st to 3rd quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

Figure 6: Quantile Treatment Effect on the Distribution of Food Share in 2002



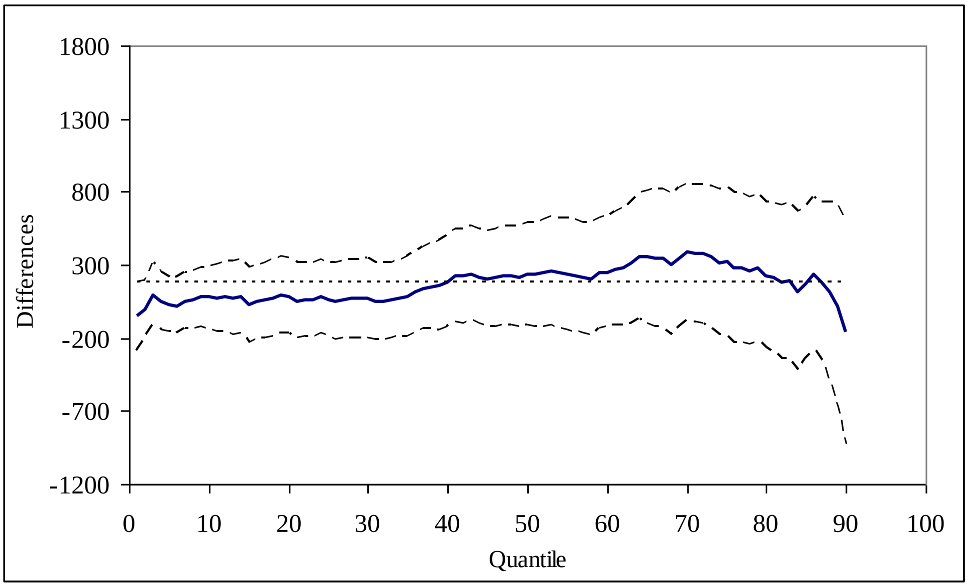
Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) QTE is computed for the 1^{st} to 3^{rd} quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures. (vii) The estimation controls for household head characteristics and demographic composition of the household.

Appendix **Figure A.1: Quantile Treatment Effect on the Distribution of Annual Per Capita Total Expenditures in 2000**



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is \$US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures

Figure A.2: Quantile Treatment Effect on the Distribution of Annual Per Capita Food Expenditures in 2000



Notes: (i) Solid line is the treatment quantile effect. (ii) Dashed lines provide confidence interval from the bootstrap with 1000 replications clustered at the locality level. (iii) Small dashed line is the mean impact. (iv) Sample includes 1359 households with observations in the panel 2000-2001-2002. (v) In Nicaraguan Cordobas, the equivalent exchange rate is \$US1 = C/13. (vi) QTE is computed for the 91st to 99th quantiles but they are not included in the figures because their variances are large enough to distort the scale of the figures.

* I am grateful for helpful comments and suggestions from Dan Black, Jeff Kubik, Jose Galdo, Hugo Ñopo, the editor, and two anonymous referees. I also thank comments and suggestions received at LACEA in Bogota and the Third IZA/World Bank Conference on Employment and Development in Rabat. I am grateful to IFPRI for permission to use the data. Some of the revision on this article was completed while the author was a postdoctoral fellow in the Economics Department at McMaster University, Canada. Any errors or omissions are my own responsibility. Correspondence to: Ana C. Dammert, Department of Economics and NPSIA, Carleton University, Loeb B846, 1125 Colonel by Drive, Ottawa K1S5B6, Canada.

[1] The RPS program did not identify poor households within targeted localities as in PROGRESA. See Maluccio and Flores (2005) for an assessment of the targeting procedure.

[2] Children are required to enroll and attend classes at least 85 percent of the time, i.e., no more than 5 absences every 2 months without valid excuse.

[3] This design seems to embody a perverse incentive for students to keep repeating the 4th grade so that families can continue to receive the subsidy. In order to eliminate this problem, the program design included a number of causes for which the household may be expelled from the program, among them, if the beneficiary child failed to be promoted to the next grade. This condition, however, was deemed unfair and never enforced. Thanks to the referee for pointing this out.

[4] The lump sum transfer for school supplies and uniforms varies with the number of eligible children while the school attendance transfer is a lump sum per household, regardless of the number of children.

[5] The evaluation was designed to last for one year but because of delays in funding the implementation of the program was postponed in control localities until 2003.

[6] In panel data, both non-response and attrition are potential concerns for the empirical analysis. In 2001 and 2002, about 92 percent and 88 percent of the targeted households were re-interviewed, respectively. The principal reasons for failure to interview targeted sample households were that household members were temporarily absent or that the dwelling appeared to be uninhabited. Maluccio and Flores (2005) examine the correlates of the observed attrition and conclude that attrition is not a major concern for estimating program effects and emphasize that using only the balanced panel is likely to slightly underestimate the effects.

[7] For PROGRESA, Behrman and Todd (1999) found that treatment and control groups had similar mean outcomes at the locality level before the program; however, they find small differences at the household and individual level. Thanks to the referee for pointing this out.

[8] Maluccio and Flores (2005) analyze 15 indicators and find small differences only in household size and number of children younger than 5 years old.

[9] In the RPS data, approximately 20 percent of the beneficiary households had no targeted children, 25 percent only children under age 5, 20 percent only children ages 7–13, and the remaining 35 percent both under 5 year-olds and 7–13 year-olds.

[10] See Duflo, Glennerster, and Kremer (2007) for a methodological discussion of randomization of experiments in developing countries.

[11] See Koenker and Basset (1978).

[12] Including covariates in the estimation of quantile treatment effects changes somewhat the nature of the treatment effect being estimated and the assumption that underlies it (see Djebbari and Smith 2008). Thanks to the referee for pointing this out.

[13] Unreported regressions show that the QTE estimates, without controlling for covariates, are in the 95 percent range of the QTE estimates controlling for covariates. With non-

experimental data, the estimation can adjust for differences in baseline observable characteristics by using propensity score weighting as in Bitler et al. (2006) and Firpo (2007).

[14] Labor laws in Nicaragua establish age fourteen as the basic minimum age for work. Children between the ages of 14 and 17 can work a maximum of six hours per day but not at night. The employment of youth is prohibited in places that endanger their health and safety such as mines, garbage dumps, and night entertainment venues (i.e. nightclubs, bars, etc.). Government enforcement, however, is far from strict.

[15] See Maluccio and Flores (2005) for more detailed information on the constructed variables.

[16] Quintiles of the marginality index are defined such as the lowest quintile includes people with the highest marginality index (poorest), whereas the highest quintile includes those with the lowest marginality index (richest).

[17] The bootstrap samples are drawn in a manner that mimics the stratified cluster sample design of the RPS survey by first drawing localities for each bootstrap sample and then sampling within the selected localities for each bootstrap sample. QTEs are calculated for each bootstrap sample and the process is repeated 1,000 times. The standard deviation of a QTE over the bootstrap replications is an estimator of the standard error.

[18] Figures A.1 and A.2 show the QTE estimates for the year before random assignment. The structure of the figures is identical to tat of Figures 1 to 6before; they show the mean effect, the QTEs, and the bootstrap 95 percent confidence interval of the QTEs. The effects are statistically not significantly different from zero for all quantiles.

[19] Note that each bootstrap sample has the same size as the control group and sampling is made with replacement from the observations from the control group only.

[20] Using the bootstrap, we can compare the mean range from the null of constant treatment effect with the mean range from the real data. One-sided tests show that we can reject the null of equality at the 5 percent level for all variables and years, separately.

[21] See Bitler et al. (2005) for a detailed explanation of the joint test for the significance of the demographic variables.