HETEROGENEOUS IMPACTS OF CONDITIONAL CASH TRANSFERS: EVIDENCE FROM NICARAGUA*

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Abstract

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. Given that transfers depend on regular school attendance and use of preventive health care services, theory predicts differential effects on household behavior. This paper examines heterogeneous effects using random-assignment data from the Red de Proteccion Social (RPS) 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 and a small impact on the probability of engaging in labor activities. Estimated quantile treatment effects indicate that the there is considerable heterogeneity in the impacts of RPS on the distribution of total and food expenditures. In particular, households at the lower end of the expenditure distribution experienced a smaller increase in expenditures.

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I. INTRODUCTION

The most popular policy tool used in the last decade to increase human capital has been the conditional cash transfer program, which provides cash payments to households upon compliance with a defined set of requirements such as regular attendance to school 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, Bolsa Escola in Brazil, 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 interested 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 the target population.

This paper contributes to the small but growing literature on the estimation of heterogeneous effects of social programs in developing countries, in particular one type of program, the conditional cash transfer. 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 (RPS) or Social Safety Net. All households in participant RPS localities that satisfy eligibility rules and comply with its requirements receive treatment. 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 represent the true mean impact of the program.

Conditional cash programs, such as the RPS, have differential effects on household behavior given that transfers depend on 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 did not complete the 4th grade. Households with children in this age category who are currently attending school will not incur any extra cost and will receive monetary food and school transfers. The grant acts as a pure income effect and higher expenditures after the program are expected. For households with children age 7 to 13 years who have not completed fourth grade and are not 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. 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).

This paper contributes to the existing state of knowledge in several ways. First, the existing literature focuses on RPS's mean impacts, in the full sample and in demographic subgroups. This paper goes beyond mean impacts and interaction variables and test whether there are heterogeneous impacts of conditional cash transfers on the distribution of expenditures as the theoretical framework predict. In particular, it analyzes the impacts of the RPS program on the distribution of total and food expenditures using quantile treatment effect (QTE) estimation. The QTE is the difference in outcomes at various quantiles of the treatment and control group distributions, which tell us how the expenditure distribution changes when we assign RPS treatment randomly. Second, the literature on QTE has been limited mostly to the US context. Recent papers have used QTE to assess the impacts of training program 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 a welfare reform experiment 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. Third, 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. The first method interacts the treatment indicator with observable characteristics. Results show that boys experienced a larger positive impact of the program on schooling and smaller impact on the probability of engaging in labor activities and hours worked. Results 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.

Main results show that children located in more impoverished areas experienced a larger impact on schooling and a smaller impact on working hours.

The second method, not tied to covariates, is the QTE, which allows us to test whether conditional transfers lead to larger or smaller changes in some parts of the outcome distribution. Estimates from the quantile regression 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. Estimates show that program impacts are larger for households who had lower levels of food shares prior to the program. These findings could not have been revealed using mean impact analysis.

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, to encourage educational attainment and help impoverished households in rural households, the Nicaraguan government implemented the *Red de Proteccion Social* or Social Safety Net. 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 of the Inter-American Development Bank and the Government of Nicaragua, the RPS program was expanded in 2002 with a US$20 million budget for an additional coverage of three years. The RPS program was made after the

Mexican program PROGRESA (now called Oportunidades): it provided benefits conditional on school attendance and health checkups and participants were identified using a detailed targeting process aimed at reached poor population in rural areas.

For Phase I of the RPS, the Government of Nicaragua selected the states of Madriz and Matagalpa from the northern part of the Central Region. This selection was based on the states 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. First, officials selected six municipalities within these two states because of the presence of a concurrent poverty alleviation program. Second, officials ranked all 59 rural comarcas in the selected six municipalities using a 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 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 is the area. Based on this score, localities were selected as eligible and ineligible for the RPS program. 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 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 (include 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[2], 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.[3] In addition, for each 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, house infrastructure, among others.[6] Table 2 presents descriptive statistics for each year. Prior to the program, households spend on average C$3885 or about US$298.9 a year, where 70 percent is allocated to food consumption. Head of households are on average 44 years old, 86 percent are male, and have on average 1.65 years of schooling. 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. There is a greater chance 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. As would be expected from the random assignment process, characteristics of the treatment group are very similar to the control group for all households and subgroups of the population (Table 3). T-tests of the equality of means suggest that for the majority of variables measured prior to the random assignment to the treatment and control groups do not differ. This suggests that the sample is well balanced across the treatment and control groups.[7]

III. THEORETICAL FRAMEWORK

This section outlines a model of household decision making in the presence of conditional cash transfers 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 marginal costs of a child's time in school equal marginal benefits, considering the opportunity cost of schooling which are the forgone earnings 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. 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 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 will 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 change: 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 child's wage and the marginal increase in earnings due to the transfer.

Participation and compliance with the RPS program might also affect household behavior as follows: [8]

- Households with <u>no</u> 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 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 <u>not</u> completed fourth grade but are attending school, 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 <u>not</u> completed fourth grade and are <u>not</u> attending school, the program combines the income effects of the cash transfer with the 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. The grant impact on household expenditures may be negative for these households. The food cash transfer will have a positive effect on household expenditure. The net effect on expenditures could be positive or negative.

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 (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 (1998) seminal model show 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.

IV. EMPIRICAL STRATEGY

The most commonly used estimator provides information about the program mean impacts. In this context, the implicit common effect assumption is present, where the program is assumed to have the same impact for all individuals in the sample. 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) for Ecuador). In addition, Behrman, Sengupta, and Todd (2005) find that program impacts differ based on children's propensities to attend school in PROGRESA. Filmer and Schady (2005) show heterogeneity of program effects by the aggregate measure of parental education and by distance to school in Cambodia.

For many questions, though, knowledge of distributional parameters is required, for example the proportion that benefit from treatment, proportion that gain at least a fixed amount, or the quantiles of treatment effect. Heckman, Smith, and Clements (1997) and Heckman and Smith (1995) emphasize that many criteria for the evaluation of social programs require information on the distribution of the treatment effect. To the best of my knowledge, only Djebbari and Smith (2005) use quantile treatment effects to study heterogeneous impacts of social programs in a developing country (Mexican program PROGRESA). This section describes two preferred econometric estimations used for obtaining impact estimates that vary among the eligible population.

I.V.1 Impacts at the Subgroup Level

The first method generates impacts estimates that vary among the eligible population. It considers variation in impacts <u>as a function of observable characteristics</u> 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. Since the specification does not condition on household participation in the program, but only on whether the household resides in a locality were the program is available, the estimates reflect the "intent-to-treat" effect of the program or the effect for individuals who are offered the treatment.[9]

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 representing 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 there is evidence of heterogeneity of treatment effects by gender. C_i include also 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, construct 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 second econometric method used to derive estimates that vary among the eligible population is the quantile treatment effect. The second method uses a quantile regression to provide a convenient framework to characterize the heterogeneous impact of treatment on different points of the outcome distribution. Most of the existing literature on QTE is based on social experiments from U.S. employment, training and welfare programs. 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 (JTPA). 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 $F_0(y) = \Pr[Y_0 \le y]$. Denote the θ^{th} quantiles of each distribution

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

Thus, we can define the quantile treatment effect 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 25 percent sample quantile of the treated distribution and the 25 percent sample quantile of the outcome distribution.

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

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

where $Q_\theta(y_i \mid T)$ denotes the quantile θ of expenditures conditional on treatment. Following Koenker and Basset (1978), the θ^{th} quantile estimator can be defined as the solution to the problem:

$$\min_\beta \frac{1}{n}\left[\sum_{i:y_i \geq T'\beta} \theta \mid y_i - \beta_\theta T_i \mid + \sum_{i:y_i < T'\beta} (1-\theta) \mid y_i - \beta_\theta T_i \mid \right] = \min_\beta \frac{1}{n}\sum_{i=1}^{n} \rho_\theta(u_{\theta_i}) \tag{4}$$

where $\rho_\theta(.)$ is known as the check function.

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 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) by estimating the QTE on $\tilde{Y}$, as follows:

1. Estimate the effects of observable characteristics (*X_i*) on the outcome of interest (*Y_i*) as $Y = \alpha + \sum_{i=1}^{K} X_i \beta_i + \upsilon$.
2. Predict the residuals by removing the effect of household characteristics from the outcome as $\tilde{Y} = y - [\tilde{\alpha} + \sum_{i=1}^{K} X_i \tilde{\beta}_i]$.
3. Estimate the QTE on the residuals $\tilde{Y}$ against the treatment indicator as $Q_\theta(\tilde{Y}_i \mid T) = \alpha(\theta) + \beta(\theta)T_i \ , \qquad \theta \in (0,1)$.

Estimating QTE on $\tilde{Y}$ is similar to estimating mean impacts conditioned on X. The control variables include characteristics of the head of household (age, education, gender, employment) and household demographic composition.[10] The advantage of 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 (Bitler et al. 2005). Without the rank preservation assumption, QTE represent 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 though 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 one can still have meaningful parameter 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. To analyze the impact 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.[11]

The RPS program also 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.[12] Impacts are estimated using OLS, except in the case of hours worked where the dependent variable is censored at zero therefore impacts are estimated using a Tobit model.

V. RESULTS

V.1 Impacts along Observable Characteristics

Table 4 reports the treatment indicator interacted by different 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 larger and statistically significantly different 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 the probability of engaging in market work and hours worked. 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 (1 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 with previous findings that with higher age potential earnings increase, thus transfers might not be high enough to compensate for forgone 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. Empirical literature has shown that higher educated parents use more efficiently the information provided in health clinics about nutrition and value more schooling and less child labor (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 male head of households experienced a smaller impact of the program on school attendance and larger impact on the probability of engaging in labor activities and hours worked.

Finally, to analyze the targeting mechanism of the program the last rows show the effect of the treatment interacted with the marginality index and household per capita expenditures separately. Based on the marginality index, I group households into quintiles and interact the treatment indicator with the index categories. If the actual targeting of the program is efficient then the impact in schooling should decrease from the poorest index quantiles to the richest index quantiles. As 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.

The estimates also show that children in poorest households experienced smaller impacts of the program on the probability of engaging in labor activities. This is consistent with the fact that the RPS is a conditional program. Households living in a more impoverished area face a trade-off between sending children to school and receiving the school cash transfer or not sending them to school and receiving their labor income or benefiting from their help at home or in the family farm/business. Results suggest that children appear as attending school and working part time rather than attending school only. These results are consistent with estimates using quintiles of per capita household expenditures.

V.2 Quantile Treatment Effect Regression

The quantile treatment effect provides information on how the impact at the household level varies at different points of the expenditure distribution. Figure 1 through 6 plots the quantiles using post-treatment data. The solid line represents the estimate of the RPS treatment in a given quantile. The associated 95% 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.[13]

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 suggest 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 cost of participation is 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 (2005) findings for the Mexican's PROGRESA, which provides benefits conditional on beneficiaries fulfilling human capital enhancing requirements: school enrollment of children aged 8–16, attendance by an adult at a monthly health seminar, and compliance by all family members to a schedule of preventive health checkups. 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.[14]

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 impact at the mean 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 can test whether a constant treatment effect could lead to a range as large as that for the QTE point estimate as in Bitler et al. (2006). The test is as follows, using the bootstrap. 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. Second, sort the sample of households using this random number and assign $t=1$ to households with number the 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 for 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 for the null with the real-data QTE range. This confidence interval is estimated with 99 bootstrap replications. This test suggest that a confidence interval for the null constant treatment range is [2896.2, 3699.5] and [2694.6, 3500.5] 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.

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 impact at the mean is C$1004 and C$733 for 2002 and 2001, which are far below the impact at the top of the distribution. The confidence interval for a null of constant treatment effects is [1928.2, 2549.7] and [1647.5, 2486.8] 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, Figure 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 impact at the mean is about 4.0 and 3.8 percentage points in 2001 and 2002. The impact is higher for households who have 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. Only under the rank preservation assumption, this interpretation is valid. This section examines whether there is evidence of rank preservation. As in Bitler et al. (2005) I can use the treatment and control distributions of demographic characteristics to see if there is evidence against rank preservation or rank reversal. 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 finding no significant differences in demographics does not imply rank preservation. In addition, even if observable characteristics do not change rank reversal may have occurred among unobservables. As in Bitler et al. (2005), the test of rank reversal is as follows. First, sampling with replacement from the real data and draw 500 bootstrap samples, each having the same number of observations as the real data. Second, for the b^{th} replication sample, randomly order the observations and assign $t=1$ to the first 706 households in the b^{th} sample and $t=0$ to the remaining households in this bootstrap sample. Third, calculate the mean of the observable characteristic in each synthetic group and take the difference of these sample means (d_b). Fourth, sort all sample means d_b from lowest to highest. Finally, use d_{25} and d_{475} respectively and construct a 90 percent confidence interval.

A real-data estimated difference is significantly different from zero if it falls outside the interval just defined. The p-value is defined as $p = 1-(k-1)/501$, where k is the index variable, which is estimate as follows: for each synthetic group create a vector with the sample means from the bootstrap (500 observations) and add the mean difference using real data, take the absolute value, and sort these observations. Let k be the index of the real data observation in the sorted data, for example if the real data estimate in the sorted data lies between the 300 and 301 bootstrap realization, then k = 301.

Tables 5 and 6 show the difference in the demographic variable between the treatment and control group within a given quantile and their p-value for statistical significance. I analyze 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). Panel A classifies people by their position (quantile) 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 this demographic characteristics. The joint test for the significance of the differences within a given quantile range suggests that the chi-square statistic fails to reject the joint test for all ranges of per capita food expenditure and per capita total expenditure.[15] More work, however, would be needed to go further in discussing rank reversal in the RPS program.

VI. SUMMARY AND CONCLUSIONS

Recent empirical work has shown that conditional programs are effective in raising school achievements and improving health outcomes in Mexico, Brazil, and Nicaragua. Much of the overall literature has focused on mean impacts. This study presents evidence on heterogeneous impacts of conditional cash transfers using a social experiment from a poverty alleviation program in Nicaragua. One way to analyze heterogeneous treatment impacts is to estimate interaction terms between the treatment indicator and the observable characteristic. For children, the results show that boys aged 7 to 13 years experienced a larger impact of the program on increasing schooling, but boys experienced a smaller impact of the program in reducing participation in labor activities. In addition, older children experienced a smaller impact of the program in reducing participation in labor activities and working hours. Finally, the results show that children living in more impoverished localities experienced larger impacts of the program on schooling and smaller impacts on working hours.

To illustrate heterogeneous impacts not tied to covariates, I use quantile treatment effect regression. The results suggest evidence against the common effect assumption. For the RPS program, the estimated treatment effect appears to differ across quantiles of the outcome distribution. In particular, for per capita total expenditures and per capita food expenditures, 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 greater effect on households who would otherwise have had 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 suggest 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 cost of participation is the highest, program impacts are still positive but smaller than for households at the upper end of the distribution. These findings could not have been revealed using mean impact analysis. This has 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]			✓
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 [a]			
Age	44.27	46.05	47.01
Male	0.858	0.858	0.858
Years of Education	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 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 error in parenthesis*)

	Treatment	Control	Difference
Household Level			
Per capita Total Consumption	4020.897	3738.242	282.656
	(203.22)	(212.23)	(290.34)
Per capita Food Consumption	2759.874	2577.680	182.193
	(129.02)	(128.33)	(179.81)
Food share	0.700	0.708	-0.008
	(0.01)	(0.01)	(0.01)
Household Head			
Age	44.637	43.864	0.774
	(0.85)	(0.73)	(1.11)
Male	0.868	0.847	0.021
	(0.01)	(0.01)	(0.02)
Years of education	1.698	1.602	0.096
	(0.14)	(0.09)	(0.16)
N	706	653	1359
Children 7-13 years			
Gender	0.532	0.509	0.023
	(0.02)	(0.02)	(0.02)
Age	9.824	9.874	-0.050
	(0.07)	(0.07)	(0.10)
School Attendance	0.766	0.767	-0.001
	(0.01)	(0.01)	(0.02)
Participation in labor activities	0.142	0.158	-0.016
	(0.01)	(0.01)	(0.02)
Working Hours	3.264	3.782	-0.517
	(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 parenthesis*)

	2001			2002		
	School Attendance	Participation in labor activities	Hours Worked (Tobit)	School Attendance	Participation in labor activities	Hours Worked (Tobit)
T*male	0.060*	-0.099*	-8.085	0.063*	-0.124*	-7.330
	(0.03)	(0.04)	(9.27)	(0.03)	(0.04)	(7.59)
T	0.117*	-0.012	-12.335	0.110*	-0.014	-11.778
	(0.03)	(0.01)	(8.52)	(0.02)	(0.02)	(8.74)
T*age	-0.003	-0.019*	-1.677	0.018	0.000	3.712*
	(0.01)	(0.01)	(1.91)	(0.01)	(0.01)	(1.75)
T	0.185**	0.145**	1.068	-0.068	-0.073	-65.299*
	(0.10)	(0.08)	(23.47)	(0.02)	(0.11)	(24.75)
T*HH schooling	-0.028*	-0.006	-1.328	-0.014**	-0.007	-0.612
	(0.01)	(0.01)	(1.90)	(0.01)	(0.01)	(1.44)
T	0.195*	-0.053*	-16.812*	0.165*	-0.067**	-16.408*
	(0.03)	(0.03)	(5.77)	(0.03)	(0.04)	(5.70)
T*HH is male	-0.019	0.076**	35.752*	-0.034	0.005	-1.361
	(0.07)	(0.04)	(13.54)	(0.07)	(0.06)	(10.54)
T	0.165*	-0.131*	-51.842*	0.172*	-0.082	-16.191
	(0.06)	(0.04)	(12.70)	(0.07)	(0.06)	(12.08)

*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 (continue)

(*Standard errors in parenthesis*)

	2001			2002		
	School Attendance	Participation in labor activities	Hours Worked (Tobit)	School Attendance	Participation in labor activities	Hours Worked (Tobit)
Quintiles of Marginality Index						
T	0.190*	-0.078**	-24.558*	0.081*	-0.072	-15.598
	(0.08)	(0.05)	(11.17)	(0.03)	(0.08)	(11.52)
T*2nd Quintile	-0.039	-0.001	2.590	0.112*	-0.008	-3.149
	(0.11)	(0.06)	(15.22)	(0.05)	(0.09)	(15.89)
T*3rd Quintile	-0.122	0.100**	24.086	-0.018	0.075	9.348
	(0.10)	(0.06)	(17.03)	(0.05)	(0.09)	(15.85)
T*4th Quintile	-0.056	0.005	8.794	0.069	0.009	4.925
	(0.09)	(0.05)	(12.04)	(0.06)	(0.11)	(14.58)
T*5th Quintile (richest)	-0.012	0.017	4.535	0.107**	-0.055	-13.667
`	(0.11)	(0.06)	(13.35)	(0.06)	(0.09)	(14.32)
Quintiles of Household Per Capita Expenditures						
T	0.197*	-0.032	-13.496**	0.200*	-0.062	-17.976*
	(0.06)	(0.04)	(8.38)	(0.05)	(0.04)	(7.65)
T*2nd Quintile	-0.001	-0.058	-17.453	-0.030	-0.055	-4.258
	(0.06)	(0.05)	(12.42)	(0.06)	(0.06)	(10.06)
T*3rd Quintile	-0.033	0.014	6.149	-0.103	0.012	5.637
	(0.06)	(0.05)	(11.30)	(0.07)	(0.06)	(10.74)
T*4th Quintile	-0.147*	-0.075**	-14.530	-0.133**	0.010	7.082
	(0.06)	(0.04)	(10.62)	(0.07)	(0.06)	(10.29)
T*5th Quintile (richest)	-0.080	-0.040	-3.207	-0.031	-0.048	-2.381
	(0.06)	(0.06)	(15.80)	(0.07)	(0.06)	(10.90)

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

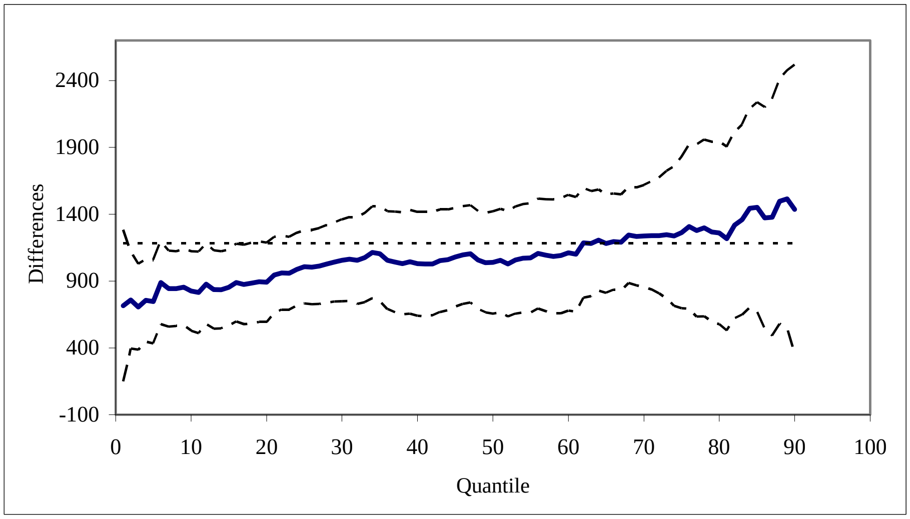
	$q \leq 25$		$25 \prec q \leq 50$		$50 \prec q \leq 75$		$q \succ 75$	
Panel A: Per capita Total Expenditure Distribution Ranges	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value
Household Head is male	0.000	0.016	-0.001	0.609	-0.002	0.497	0.000	0.112
Household Head Years of Education	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

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

	$q \leq 25$		$25 \prec q \leq 50$		$50 \prec q \leq 75$		$q \succ 75$	
Panel A: Per capita Total Expenditure Distribution Ranges	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value	Mean Diff	p-value
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

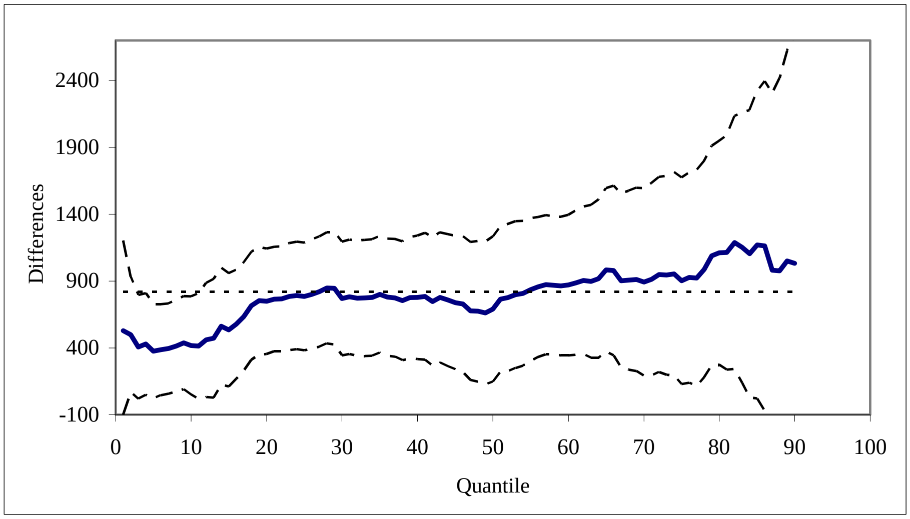
Figure 1: Quantile Treatment Effect on the Distribution of

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.

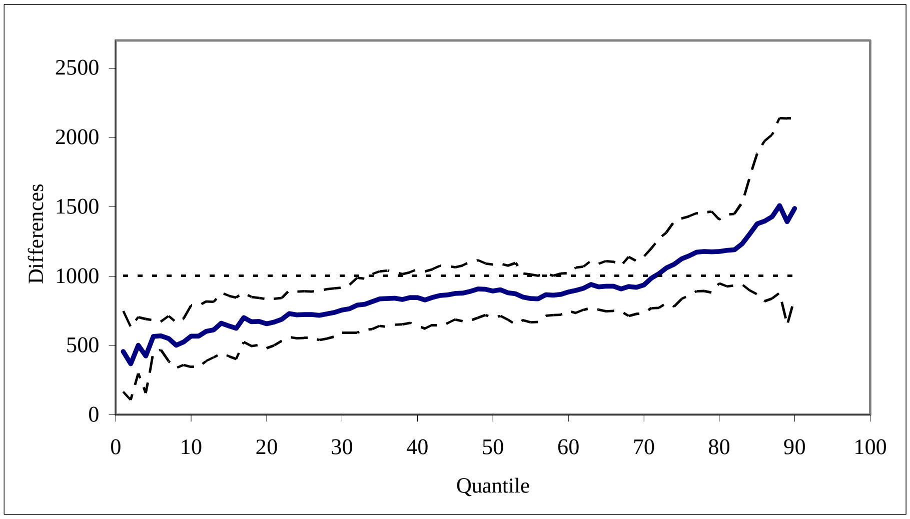
Figure 2: Quantile Treatment Effect on the Distribution of Annual Per Capita Total Expenditures in 2002



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.

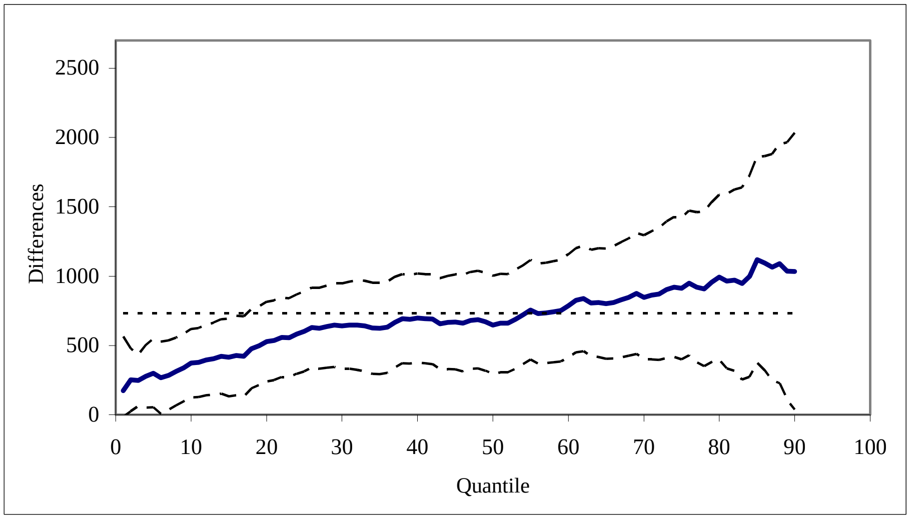
Figure 3: Quantile Treatment Effect on the Distribution of

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.

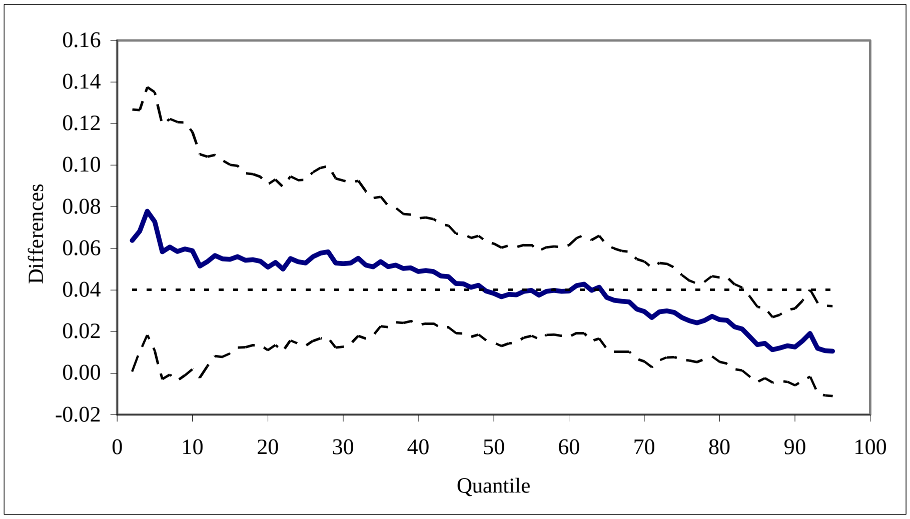
Figure 4: Quantile Treatment Effect on the Distribution of Annual Per Capita Food Expenditures in 2002



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.

Figure 5: Quantile Treatment Effect on the

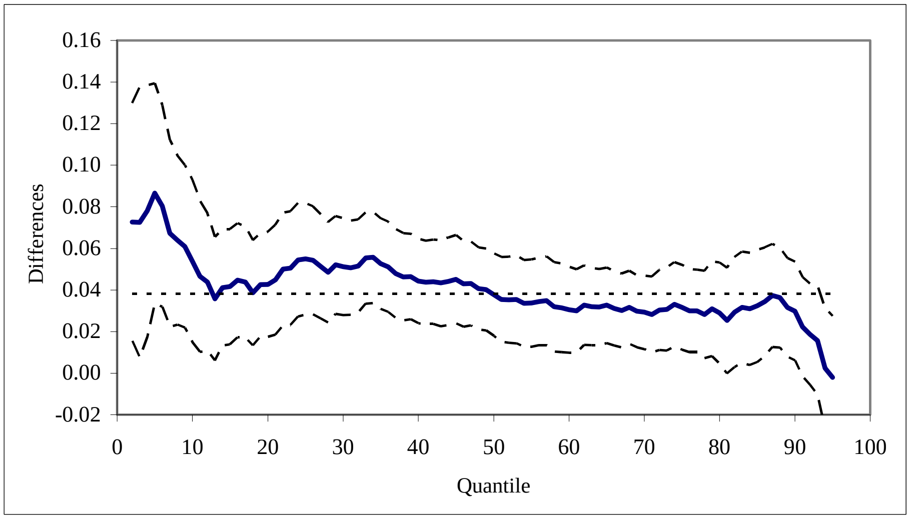
Distribution of Food Share in 2001



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 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.

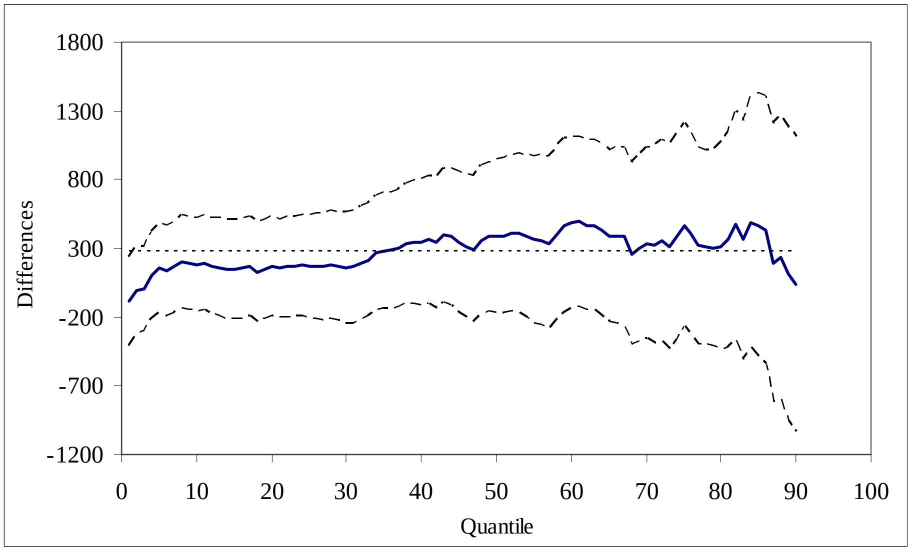
Figure 6: Quantile Treatment Effect on the

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 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

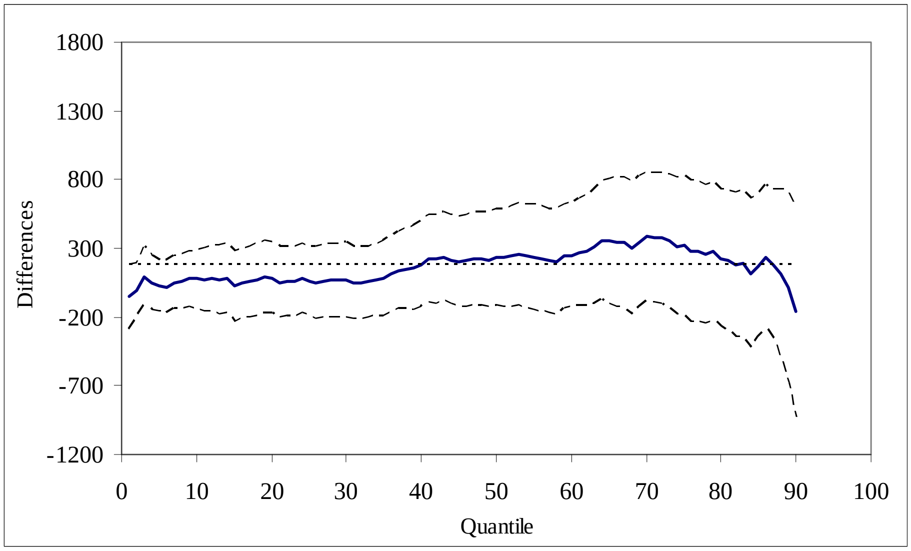
Appendix **Figure A.1: Quantile Treatment Effect on the Distribution of Annual Per Capita Total Expenditures in 2000**



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.

NOTES

[*] I am grateful for helpful comments and suggestions from Dan Black, Jeff Kubik, Jose Galdo, Hugo Ñopo, the editor, and two anonymous referees. I am grateful to IFPRI for permission to use the data. Any errors or omissions are responsibility of the author.

[1] The RPS program did not identified 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, 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] Maluccio and Flores (2005) analyze 15 indicators and find small differences only in household size and number of children younger than 5 years old. Similarly, the randomization is at the village level in Mexico's 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.

[8] 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.

[9] The effect of RPS participation – the effect of "treatment on the treated" (ATT) – may also be of interest. If the treatment effect on eligible non-participants is zero and if intent-to-treat (ITT) is the overall impact effect evaluated at the sample mean, we can estimate the ATT by rescaling the ITT by the fraction of program participants (Heckman, LaLonde, Smith, 1999). In this experiment, however, the ITT is unlikely to underestimate the effect on participating households by much as Maluccio, Murphy and Regalia (2006) show in their analysis of impacts of the RPS program on schooling outcomes, because about 90% of eligible households participate in the program.

[10] Thanks to the referee for pointing this out. Unreported regressions show that the QTE estimation 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).

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

[12] 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.

[13] The bootstrap samples are drawn in a manner that mimics the stratified cluster sample design of the RPS survey. The randomization was made at the comarca level, i.e. all households in the treatment comarca are assigned to the program. The standard errors were calculated by using a one-level bootstrap, that is, within each comarca *J* households are randomly drawn. 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.

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

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