Conditional Cash Transfers, Credit, Remittances, Shocks, and Education: An Impact Evaluation of Nicaragua’s RPS

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Abstract

This work estimates the impact on school enrollment of a Nicaraguan conditional cash transfer program, Red de Protección Social (RPS). RPS is one of a growing number of these programs (e.g. Progresa, Bolsa Escola, and PRAF) that pays households regular cash transfers on the condition that their children attend school and all household members visit health clinics and seminars. A household model highlighting the decision between child labor and education is presented to estimate the impact of cash transfers based upon key structural variables: wealth, credit access, remittances, and exposure to weather shocks (specifically droughts). The results of the model lead to the paper’s central hypothesis: conditional cash transfers will have the greatest impact on credit constrained households hit by negative economic shocks. This work contributes to the literature by examining empirically the effect of credit constraints and negative economic shocks on the impacts of a cash transfer program. A difference-in-difference estimator is used to calculate the impact of RPS depending on key structural variables within the household. Consistent with a previous study (Maluccio and Flores, 2004) the results show that RPS helped to substantially increase school enrollment. The results are also consistent with the central hypothesis: that credit constrained households experiencing a negative economic shock are the most impacted by RPS. Additionally, as part of the empirical analysis, propensity score matching is utilized to test the validity of the assumptions of randomization in the RPS data, that justify the use of difference-in-difference estimation; the use of difference-in-difference estimation is supported by the results of the matching estimator.

Key Words – education, credit, natural disaster shocks, remittances, and Nicaragua.

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Section 1: Introduction

Increasing educational attainment is widely believed to be one of the most effective means of fostering economic development. Seminal works in the macro-growth literature such as Mankiw et al. (1992) and Lucas (1988) found a strong link between economic growth and human capital. National governments in many developing countries have made commitments to provide universal education as part of the United Nations Millennium Development Goals. International organizations such as the World Bank and the Inter-American Development Bank have provided billions of dollars to start programs that encourage households to send their children to school by providing economic incentives.

Over the last decade, perhaps the most popular policy tool in this effort to increase human capital has been the conditional cash transfer program, which provides cash payments to households that adhere to certain requirements. These requirements typically include school attendance and visiting health clinics and seminars. Such programs exist in a number of countries (Bangladesh, Brazil, Columbia, Honduras), though arguably the most famous is Progresa in Mexico. Most evaluations show that conditional cash transfer programs have been successful at raising human capital in poor rural households (Rawlings and Rubio, 2003).

The work presented here focuses on a particular conditional cash transfer program in Nicaragua: Red de Protección Social (RPS) i.e. “Social Safety Net,” which was modeled after the previously mentioned Progresa. An empirical analysis tackles new ground not covered in the impact evaluations of either Progresa or of other conditional cash transfer programs and examines the impacts on education of two critical factors: credit access and remittances.¹

The empirical impact evaluation is motivated by a two-period unitary household model of the decision between child labor and education. This model builds on previous attempts to
predict decisions regarding child labor versus education (Glewwe and Jacoby, 2004; Jacoby and Skoufias, 1997) and explicitly incorporates conditional cash transfers and key structural variables such as credit access and remittances. It also explores how school enrollment is impacted by shocks that are both ex-ante and ex-post of the labor-education decision. Additionally, the model highlights how credit access and remittances act as insurance to provide households a buffer to respond to shocks.

The design of RPS as in several other conditional transfer programs includes the random assignment of communities to a treatment or “control” group so that program impacts may be measured as exogenous effects. This work will utilize that experimental design, while also carefully examining the nature of the randomization. Like other impact evaluations of conditional cash transfer programs, this paper utilizes difference-in-difference (DID) to calculate impacts. Conversely, unlike most previous impact studies, estimates of programs impacts are based on key structural variables (e.g. credit, remittances, wealth, and shocks).

One possible problem with DID is that while randomization is performed at the community level, estimations are calculated at the individual child outcome level. Using the emerging literature on program evaluation, the paper addresses the concern that typical DID estimators may not control for differences among the communities, thus creating heterogeneity problems between treatment and control groups. The paper then tests for heterogeneity between the control and treatment communities, and finds that households in the control community were indeed somewhat more likely to have credit access. The household model hypothesizes that these differences in credit access could create a bias which in turn would tend to overestimate program impacts, given that a massive drought hit 85% of the communities in the program’s first year of implementation. This bias is potentially important because recent research reveals that
households with credit access are more likely to maintain school enrollment during poor economic times (Jacoby and Skoufias, 1997; Park and Brown, 2002; Gitter and Barham 2005).

In order to test for bias caused by heterogeneity between treatment and control groups, the paper utilizes the technique of propensity score matching (PSM) to calculate average treatment effects, while controlling for possible heterogeneity between the two groups. When compared to the standard DID estimator this estimation yields similar, but smaller, estimates of program impacts.

The remainder of the paper is organized as follows. Section 2 places this work within the context of the current literature. Section 3 presents a theoretical household model. Section 4 provides an overview of the current state of education in rural Nicaragua and offers background information on RPS along with descriptive statistics of variables of interest. Section 5 presents a difference-in-difference model, details the methods used to select RPS and the control communities, and considers the selection process in the context of recent work on evaluating program impacts. Section 5 also highlights the problems that arise from randomizing at the community level when the outcomes of individual children are used in the analysis. A second estimation technique propensity score matching is utilized as a check on the difference-in-difference estimator. The empirical analyses on program impacts of RPS on school enrollment takes into account key structural variables utilizing the RPS data. Results of this analysis are compiled in Section 6, with conclusions and suggestions for further study offered in Section 7.

Section 2: Literature Review and Conceptual Framework

Randomized experiments have been used in a wide variety of situations including health insurance, electricity use, and job training (Burtless, 1995). Studies of randomized experiments
are popular with researchers, because the effects of treatments can be measured with greater reliability than non-experimental studies. The increase in reliability stems from the removal of correlation between treatment status and characteristics of those observed (Burless, 1995; Heckman and Smith 1995). In order to perform impact evaluations, conditional cash transfer programs have created randomized experiments by collecting information on a non-treated “control” group. The experiential design has been utilized to justify the use of difference-in-difference techniques in impact evaluations by Schultz (2004) on Progresa, and by Glewwe and Olinto (2004) on PRAF.

This work is not the first to use the RPS data. An impact evaluation study by Maluccio and Flores (2004) measured the effect of RPS participation on education, consumption, and health. They found that RPS increased school enrollment in the target population by 23 percentage points, led to an 18% increase in per capita household expenditures (most of which was spent on food) and decreased by 5 percentage points the number of children with stunted growth. A second analysis by Maluccio (2005) examines the impact of RPS on the household’s ability to cope with falling coffee prices and provides a point of departure for this work. Although not central to the analysis, Maluccio (2005) finds that households in RPS were less likely to receive remittances and that households without credit access had larger program impacts on enrollment. Additionally, Maluccio (2005) used difference-in-difference estimators to show that households in eligible communities were less impacted by the coffee crisis.

Impact studies of Mexico’s Progresa program have tended to focus solely on program impacts without considering other possible exogenous factors (Schultz, 2004; Skoufias, 2005). One notable exception is De Janvry et al. (2004), which measures the impact of Progresa on children’s educational attainment in households facing economic shocks. The work finds that
conditional transfers serve as a safety net, making households faced with negative economic shocks less likely to remove their children from school. Although De Janvry et al. (2004) posit that credit constraints are in part to blame for the inability of poor households to continue education post shocks, their analysis does not explicitly analyze credit access because fewer than 2% of the Progresa households were able to obtain credit, compared to over 13% of the households in the RPS sample.

The work by De Janvry et al. (2004) is part of an emerging literature which uses economic shocks at the household or macro level to examine the impact of wealth on education. Interestingly, some empirical studies have found a positive relationship between negative economic shocks and education, suggesting household income may not always be positively correlated with education. Duryea and Arends-Kuenning (2003), Binder (1999), and Schady (2001) found higher enrollment rates in school during poor economic times in Brazil, Mexico, and Peru, respectively. These results imply that opportunity costs of education decrease because negative macroeconomic shocks depress children’s wages, and that the reduction in opportunity costs may outweigh the income effect caused by the shock. These studies are countered by Thomas et al. (2004) and Rucci (2004), which found negative economics shocks to decrease schooling in Indonesia and Argentina. Conversely, Thomas et al. (2004) found that households tended to maintain their investment in older children, and Rucci (2004) found that the negative relationship existed in poorer households only.

With credit access, households may borrow funds when faced with a negative economic shock, and this may allow a household to maintain a child’s enrollment in school during a shock. Central to this line of literature is Jacoby and Skoufias (1997), which shows that having access to credit helped Indian households maintain their children’s school attendance in the face of
weather shocks to crops. Similar results by Brown and Park (2002) and Beegle et al. (2003) used samples of Chinese and Tanzanian households, respectively. However, these three works fail to control for the fact that access to credit is related to other factors that influence education, particularly wealth, and that credit access itself may be endogenous. Gitter and Barham (2005) studied education in Honduras and controlled for the endogeneity of credit access. They utilized a two-step estimator in the first stage estimating credit access and in the second educational attainment. This work finds a positive role for credit access on secondary school attainment. More importantly, it also finds a significant interaction between credit access and the household’s ability to maintain education in the face of a major negative economic shock (from Hurricane Mitch). These results hold even when controlling for wealth.

Remittances, like credit access, can also act as insurance against negative shocks. Amuedo-Dorantes and Pozo (2004) have shown that remittances received by a household tend to increase when that household is hit by a negative economic shock. Their work also adds to the growing body of literature that shows a positive relationship between remittances and educational attainment (Edwards and Ureta (2003); Kandel and Kao (2000)). Conversely, Skoufias (2005) suggests that conditional cash transfers may crowd out remittances, as migrants may feel less need to send money home when a household is receiving cash from a program.

Section 3.A: Household Response of Education to a Stochastic Shock

In this section a two-period household utility maximization model examines the impact on education of shocks to the household’s and the individual child’s potential income, contingent on a household’s access to credit, to conditional cash transfers, and/or to remittances. The model is used to motivate the empirical analysis with the RPS data presented in Sections 5 and 6. The
key hypothesis of the household model is that children’s schooling in credit-constrained households is more affected by shocks than households with credit. Because of this imbalance, conditional cash transfers impact school enrollment more in the credit constrained households, especially those experiencing negative economic shocks.

This section presents the model of a household that lacks access to any of the three types of transfers (credit, conditional cash, or remittances). It is assumed that both household wealth and adult labor are fixed and that no direct costs to schooling exist. This model contains a single decision on the allocation of the child’s time in the first period. It is assumed that the child in the household is endowed with one unit of time to use in the first period to spend time in school, e, or add labor to add to the household production function $F(1-e, w)$, where, w, is household wealth. In the second period, the child attends no school and only works. In the first period, households have a utility function, $U^1$, with two inputs consisting of the consumption of a single good, c, purchased at the unit price and time spent in school, e. The second period utility, $U^2$, with discount rate, $\delta$, is only a function of the consumption of the single good. It is also assumed that the utility functions exhibit positive and decreasing marginal utility and that in the first period education does not affect the marginal utility of consumption or vice-versa (i.e. the cross partials are zero). Income in period 1, is generated by a decreasing returns to scale production function, $\theta_1 F(1-e, w)$, which is a function of child labor, 1-e, the household’s wealth, w, and stochastic parameters, $\theta_1$. In the second period output is generated with a production function $\theta_2 f(w)$ and stochastic parameter $\theta_2$. It is assumed that wealth is fixed and that wealth increases the marginal productivity of child labor, (i.e. $F_{ew} < 0$). Low values of the stochastic parameters would indicate a negative economic shock. For the initial analysis $\theta_1$ is assumed to be ex-ante to the education decision. In the second period children are assumed to spend all of their time in
Besides enhancing first-period utility, education increases income in the second period by \( \Phi e \) (where \( \Phi \) is the return to human capital). Thus, the household problem is to maximize two period utility:

\[
\text{Max } \frac{\partial}{\partial \theta_i} (U^1(\theta F(1-e, w), e) + \delta U^2(\theta f(w) + \Phi e), \quad \text{where } 0 \leq e \leq 1
\]

Since “e” is the only choice variable, only one interior solution exists. The first order condition is provided in equation (2).

\[
U^1e \theta F_e + U^1e \delta \theta \delta U = 0
\]

\[
\frac{\partial e}{\partial \theta_i} = \frac{[F_e U^1_e + F \theta F_e U^1_{ee}]}{-[U^1_{cc} (\theta F_e)^2 + U^1_e \theta F_{ee} + \delta \theta \delta^2 U^2_{cc} + U^1_{ee}]} = 0
\]

This result highlights the relationship between shocks and education. An examination of equation (3) shows that the four parts within the brackets in the denominator are negative (due to decreasing marginal utility and returns to scale). If \([ U^1_e + F \theta U^1_{ee} < 0 \] \), then the relationship between shocks and education is positive (i.e. negative economic shocks decrease schooling), since \( F_e < 0 \). However, since it cannot determine whether or not the result is true, the model leaves an ambiguous conclusion regarding the relationship between shocks and schooling, which is consistent with results in the empirical literature. Although the results of this model are inconclusive, they nevertheless help to provide a comparison for the response to shocks of credit-constrained and unconstrained households alike as presented in the next section.
Section 3.B: The Impact of Shocks on Schooling in a Household with Credit Access

In this section the two-period household model examines the effects of credit access. Suppose that households with credit access can borrow money in the first period and repay it in the second at an interest rate \((1+r)\), but that the household of interest receives no conditional cash transfers or remittances. The amount borrowed is denoted as \(B\). With the addition of credit access the household may borrow in the first period to gain the benefits of education in the second. This section will also show that households with credit access will be less likely to remove their children from school when they experience a shock. Below equation (4) shows the household maximization problem with credit access. The first order conditions of \(B\) and \(e\) for an interior solution are shown in equations 5 and 6.

\[
\begin{align*}
\text{Max}_{\theta, B} & \quad U^1(\theta F(1-e, w) + B, e) \\
\quad & \quad + \delta U^2(\theta f(w) - (1+r)B \Phi e), \quad \text{where } 0 \leq e \leq 1 \\
U^1_{e} & = E(\delta(1+r)U^2_{e}) \\
U^1_{e}\theta_{F_{e}} + U^1_{e} + \delta \Phi U^2_{e} & = 0 \\
(5), (6) \rightarrow \\
\delta(1+r)U^2_{e}\theta_{F_{e}} + U^1_{e} + \delta \Phi U^2_{e} & = 0
\end{align*}
\]

Equation (7) calculates the derivative of time spent in school, \(e\), with respect to the shock term, \(\theta_1\), and the results of this calculation are presented in equation (8).

\[
\frac{\partial e}{\partial \theta_1} = \frac{-\delta(1+r)U^2_{e}}{[\delta \Phi U^2_{e} + (1+r)U^2_{e}\theta_{F_{e}} + U^1_{e}]} (8)
\]
As in the previous analysis of the relationship between shock and education, the derivative’s sign is ambiguous. In equation (8) the numerator is positive, but the denominator is ambiguous. The sign of the denominator is determined by the relationship between the second period value of the marginal product of child labor, \((1+r)\theta_1 F_\theta\), and returns to education, \(\Phi\). The latter term can be seen as the benefit seen in the second period of child labor conducted in the first period. These two values will be equal if the household sets the marginal benefit in period two of first period child labor equal to the marginal benefit of education in period two. When these two values are equal the sign of the denominator will be negative and therefore the derivative will be negative.

A negative derivative would indicate that negative economic shocks increase education for households with access to credit. The relationship of these marginal benefits is supported by Jacoby and Skoufias (1997) who suggest that the elimination of credit constraints allows households to send their children to school until future costs can be equated with future benefits. When compared to the previous section this result suggests that households without credit access will be more likely to remove their children from school when faced with an economic shock. The proposition that negative economic shocks decrease educational attainment in credit-constrained households but not in those which are unconstrained is supported by findings previously cited in the empirical literature, particularly Gitter and Barham (2005).

**Section 3.B.2: The Effects of Conditional Transfers and Remittances**

This section extends the model to include conditional cash transfers and remittances. Conditional cash transfers are based on time spent in school, and are denoted as \(\tau_c(e)\). Many conditional cash transfer programs such as RPS and Progresa require children to maintain a minimum level of attendance (this level is denoted as \(e'\)): once that level is met the household
receives a lump-sum payment, denoted as $\Gamma$. In addition, only households with sufficiently low levels of wealth receive transfers and a household’s maximum wealth to qualify for transfers is denoted as $w'$. It is also assumed that conditional cash transfers are paid to the household in the same period in which the child attends school, as these programs are intended to replace the income generated by children. Therefore, $\tau_c(e, w)$ has the following functional form in our analysis:

$$
\tau_c(e) = 0 \quad (\text{if } e < e' \text{ and/or } w > w')
$$

$$
\tau_c(e) = \Gamma \quad (\text{if } e \geq e' \text{ and } w' \geq w)
$$

Consistent with the literature on remittances, our model assumes that they are negatively correlated with shocks to household production. Therefore, transfers from remittances are denoted as a function of the stochastic income-generating parameter $\tau_r(\theta_i)$. Households that are eligible for remittances and conditional cash transfers alike are represented by $\tau(e,\theta_i)$ where $\tau(e, w, \theta_i) = \tau_c(e, w) + \tau_r(\theta_i)$.

The children whose enrollments are most impacted by a conditional cash transfer are those whose households are willing to meet the attendance conditions and in the absence of transfers would have a maximized schooling value, $e^*$, less than the required amount of schooling, $e'$. Those who would in the absence of transfers have $e^* < e'$, but with transfers would have $e \geq e'$, are considered to have accepted the transfer. Below it is demonstrated when a conditional cash transfer will be accepted. If the equality in equation 9 is not met, then the household’s marginal utility of education at the minimum education point is higher than the marginal utility of consumption, and the transfer will be accepted. If equation 9 does not hold, but the household is still better off with transfers (shown when the equality in equation 10 holds), then transfers will be accepted. When equation 10 does not hold the household will not accept
the conditions of the transfer and will not receive the payment, so clearly the program will have no impact.

\[
-U'(\theta F(1-e',w) + \Gamma , e') \theta F_e (1-e', w)) > U'_c(\theta F(1-e', w) + \Gamma, e') \\
+ \delta E[U'_c(\theta F(1,w) + \Phi e')] \\
U'(\theta F(1-e',w) + \Gamma , e') + \delta \Phi E[U^2(\theta f(w) + \Phi e')] \geq U'(\theta F(1-e*,w), e*) \\
+ \delta \Phi E[U^2(\theta f(w) + \Phi e*)]
\]

(9) 

(10)

The above equations show that as the stochastic income term \( \theta \) decreases – as it would during an economic shock – it is more likely that equation 10 will hold (see Appendix A.) This is consistent with empirical findings of De Janvry et al. (2004), which found Progresa particularly effective when households faced economic shocks. Furthermore, the empirical literature suggests that youths in households lacking credit access attend less school (Jacoby and Skoufias, 1997; Park and Brown, 2002; Gitter and Barham, 2005). If this is indeed the case, youths in households lacking credit access would have been less likely to attend school in the absence of transfers (or have smaller values of \( e^* \)). It is important to note that conditional cash transfers would have similar impacts when the shock is ex-post to the education decision. If all households were risk averse a guaranteed source of income from transfers would be more desirable to credit-constrained household who could not smooth consumption through loans. These comparative static results motivate our key hypothesis that conditional cash transfers will have a greater impact on credit rationed household particularly when negative economic shocks occur.\(^8\)

Wealthier households are less likely to change schooling in the presence of conditional cash transfers. There are two reasons for this. First wealthier households will be unaffected by the program because they are ineligible for transfers (i.e. \( w > w' \)). Second, when considering
households below the wealth threshold, those with more wealth will have higher marginal products of child labor at any given effort level (since $F_{ew} < 0$), making it less likely that the conditions stated above will hold (equations 9 and 10).

The addition of remittances in the ex-ante shock case results in only small changes to the previous analysis. The additional income from remittances in the first period will lower the marginal utility of consumption in that first period, making a child more likely to attend school. If remittances are also expected in the second period, the marginal utility of income from human capital will decrease, making a child less likely to attend school. These two opposing forces of remittances on education make their effects ambiguous in this model. Additionally, it is important to note that remittances work as an insurance policy against negative shocks to the household’s income. In the ex-ante shock case, this will mean that the variance of income in the second period will be lower if a household receives remittances. This result will have a positive effect on education if the returns to education, $\Phi$, are stochastic. If the shock is ex-post of the education-labor decision, then remittances will also decrease variance of first period income, which may in turn allow risk-averse households to send their children to school. Both the ex-ante and ex-post shock results suggest that additional income from remittances can increase education by lowering income variance.

Section 4: Nicaraguan Education and RPS

Two-thirds of Nicaraguans over the age of 25 have not completed primary school, while half have no formal education at all (Maluccio and Flores, 2004). In 2000 (the baseline year) more children were receiving an education with enrollment rates at about 80% for nine-year olds
but are about half of that for age 13. Despite these recent increases, Nicaragua has one of the lowest primary school enrollment rates in Latin America (Maluccio and Flores, 2004).

To encourage educational attainment and to help impoverished households in Nicaragua, a conditional transfer program, titled RPS, was created 2000. The program is a joint effort of the Inter-American Development Bank, the Nicaraguan government and the International Food Policy Research Institute: These organizations helped to implement the program and collect the data. RPS, as in conditional cash transfer programs in Mexico and Honduras, also provides transfers for health care and food.

RPS has collected data on 42 rural communities (comaracs) in Nicaragua. These 42 communities were randomly assigned to treatment and control groups, with half in each group (Maullcio and Flores, 2004). Households in treatment communities were eligible for several types of transfers unless their asset level indicated they did not need a transfer. These benefits included a C$2,880 ($224) food security transfer. Households with children ages 7-13 who had not completed the fourth grade could receive a bi-monthly transfer for school attendance of C$1440 per year and an annual school supplies transfer of C$275. The average household received C$3885 ($302), or about 18% of total annual household expenditure. In order to receive these transfers, households members had to attend bi-monthly meetings, their children had to have regular health care appointments provided by RPS, and the children had to maintain regular attendance in school. These regulations were enforced with around 10% of participants having their benefits suspended because they failed to fulfill their obligations.

The RPS data set is particularly advantageous for several reasons. First, the program has a fairly large sample size of about 1,400 households, with a total of about 1,800 eligible children. As stated previously, there is also a random assignment at the community level. Additionally, the
Section 4b: Descriptive Statistics

A total of 1,396 households completed all three rounds of the survey. The first round was a baseline sample taken in 2000 before program implementation. The second two rounds were taken in 2001 and 2002 while the program was active. Attrition rates were nearly identical in treatment and control communities (Maluccio and Flores, 2004). Our sample takes a subset of the 1,396 households, using only households with children between ages of 7-13 when children are eligible for cash transfers for school attendance. In the subsample of 964 households, 495 (51%), lived in communities where RPS was administered. Table 1 below presents descriptive statistics for household variables used within the analysis for the sample as a whole, while Table 2 compares the descriptive statistics of households in treatment and control groups.

This section compares five main variables: consumption, rationing in the formal credit market, rationing in the informal credit market, the presence of household migrants, and household size. These variables correspond to key structural variables within the household model presented in Section 4. Ideally, a direct measure of wealth would exist; however detailed information on asset holding was not collected using RPS. Information was collected on total household consumption including expenditures on education, food, utilities, fuel, and health care. Consistent with the permanent income hypothesis, total expenditures in 2000 were used as a proxy for a household’s wealth before program implementation.

Credit access is another key component of the model presented above and it has been empirically been shown to be an important factor in determining education in developing countries (Jacoby and Skoufias, 1997; Park and Brown, 2002; Gitter and Barham, 2005). The
analysis is concerned with two different types of credit access (i.e. loans): those from formal and informal credit markets. The formal market consists of loans from banks, cooperatives, and micro-credit NGOs, while the informal market consists of friends, relatives, or other individual lenders. Households are classified as not being rationed in a credit market if in the baseline year, 2000, they either had a loan or said they could get one. A binary dummy was created, taking the value of one if a household is rationed, and these variables are denoted as $F_{ration}$ and $I_{ration}$, for formal and informal markets, respectively. Table 3 below shows that about ¾ of households are rationed in both formal and informal credit markets; these households had on average lower consumption levels. Not surprisingly, those with credit access in both markets had the highest average consumption, over 50% more than those who were fully rationed. Additionally, Table 3 shows that households with formal credit only had total consumption averaging about C$9000 more than fully rationed households. Meanwhile, households, with informal credit access had less than C$3000 more than their fully rationed counterparts.

{Table 1, 2, and 3 about here}

In the model presented in the previous section, remittances depend on a household’s stochastic income outcome. The model assumes that households with migrants that have good outcomes may receive smaller remittances than those with bad outcomes. However, if the household makes the education decision ex-ante to its realization of income, the possibility of receiving remittances will ultimately determine the child’s education. Therefore, the analysis utilizes a binary dummy variable $Migrants$, which takes the value of 1 if the household contains a migrant living elsewhere, whether within in the country or abroad. About 7% of households
Households with migrants had 25% higher consumption levels in the year 2000 (C$26692 compared to C$21710).

Household size represented by the variable \((\text{Size})\) is important to the model in two ways. First, households with more members earn higher incomes. On the other hand, this is counteracted to the extent that there are more family members represented in the household utility problem, because households that are larger will have higher marginal utility of consumption at the same levels of income. Table 4 below shows the relationship between household size and average per-capita consumption in 2000. From the table it is clear that smaller households had higher per capita consumption.

{Table 4 about here}

The comparative static analysis of negative shocks examines the presence of droughts, which were reported in a survey given to a community official. In 2001, 37 of the 42 total communities experienced a severe or very severe drought. In 2002, only 21 communities experienced a severe or very severe drought. Shocks were not measured in the baseline year, 2000. Additionally, these surveys ranked the shocks on a three point scale (not severe, severe, and very severe). Table 5 below shows the differences in school enrollment by shock severity and rationing in the formal credit market. In communities that experienced a severe drought in 2001, 84% of the children in credit-constrained households enrolled in school, while 91% of those in unconstrained households did so. For comparison in the baseline year, 2000, 82% children in unconstrained households enrolled compared to 76% of those in constrained households. Interestingly, credit-constrained households were almost as likely as their non-constrained counterparts to enroll their children in school when hit by a non-severe drought or no drought at all. This outcome supports the empirical findings that children are more likely
to leave school during economic shocks if the household is credit constrained. Due to the limited number of observation of those with very severe shocks (around 2% of the sample in 2001) a household is denoted as hit by a shock if their community reported a severe or very severe drought.

(Table 5 about here)

Section 5: Difference-in-Difference Estimation

Maluccio and Flores’ (2004) impact study of RPS on education provides an excellent framework for a difference-in-difference analysis. Before the program, if randomization worked properly, we would expect similar enrollments for the treatment group (I) and for the control group (C). Put differently, the quantity \((I_0 - C_0)\) would be expected to be close to zero (where the subscript “0”, represents the period before the program). In fact, randomization in terms of school enrollment appears to be successful, as there is not a statistically significant difference between treatment and control groups in the baseline year. One way to measure the program effects is to measure the difference in the period after the treatment is received \((I_1 - C_1)\), where 1 represents the period after the program). While Maluccio and Flores (2004) note that sometimes the difference at the initial period will not be zero, in this case the difference-in-difference estimator \((I_1 - C_1) - (I_0 - C_0)\) is more robust. Additionally, they note that if other factors that affect both the intervention and control group actually increase schooling, then the double-difference estimator will more accurately reflect the program’s impact.

The Maluccio and Flores (2004) equation for estimation of program effects of RPS provides the basic model necessary for our analysis. The RPS survey includes three panel waves. The first was collected in 2000 before the program was implemented. The second two were collected after implementation in 2001 and 2002. Their basic impact equation is shown in equation 11:
\[ E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 P_c + \delta_1 A_1 P_c + \delta_2 A_2 P_c + \mu_{ic} + \nu_{ict} \quad (11) \]

Where:

\( E_{ict} \) = outcome variable of interest for household (or individual) \( i \) in community \( c \) at time \( t \)

(in our case school enrollment is the key variable of interest)

\( A_1 = (1) \) if Year 2001

\( A_2 = (1) \) if Year 2002

\( P_c = (1) \) if program intervention in community \( c \)

\( \mu_{ic} = \) all (observed and unobserved) household- (or individual-) level time-invariant factors

\( \nu_{ict} = \) unobserved idiosyncratic household (or individual) and time-varying error

and all the \( \alpha \) and \( \delta \) are unknown parameters.

The two parameters of interest are \( \delta_1 \) and \( \delta_2 \), which measure the double-difference impact of the program relative to the year 2000 for 2001 and 2002, respectively. With random assignment of communities to receive the program \( (P_c) \), there should be zero correlation between the program effects and the error term. Additionally, household and individual fixed effects can be added as a robustness check.

This paper extends the Maluccio and Flores (2004) difference-in-difference analysis in two ways. First is the inclusion of a community-level weather shock, specifically droughts, which were reported in surveys in 2001 and 2002. Second, this paper includes interaction terms of credit access with shocks and treatment to test RPS’s ability to act as a safety net for credit constrained households during economic shocks. One concern is that the conditional cash transfer program may shape a household’s current credit access or receipt of remittances. Therefore, in this analysis, these variables are instrumented by using the household’s credit
access or remittances from the baseline data set collected before program implementation in 2000.

Using these two extensions, a new equation is created by adding additional terms to the Maluccio and Flores (2004) impact estimation. The equation includes the household’s credit access status, presence of migrants before the implementation of the program, and their interaction with program effects. Finally, interactions with shocks are included. The estimation equation therefore can be expressed as:

$$E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 P_c + \alpha_4 F_0 + \alpha_5 R_0 + \alpha_6 A_1 D_1 + \alpha_7 A_2 D_2 + \alpha_8 A_1 D_1 F_0 + \alpha_7 A_2 D_2 F_0$$

$$+ \delta_1 A_1 P_c + \delta_2 A_2 P_c + \delta_3 A_1 F_0 P_c + \delta_4 A_1 P_c F_0 + \delta_5 A_2 P_c F_0 + \delta_6 A_1 P_c R_0 + \delta_8 A_2 P_c R_0 +$$

$$\delta_9 A_1 P_c D_1 + \delta_{10} A_2 P_c D_2 + \delta_{11} \text{Consum} + \delta_{12} \text{Size} + \mu_{ic} + \nu_{ict}$$

(12)

Where

- $E_{ict}$ = Child’s Enrollment in School
- $F_0$ = (1) If household is rationed in the formal credit market in 2000
- $R_0$ = (1) If household has foreign migrants who could remit in 2000
- $D_1$ = (1) If household was in a community affected by a severe drought in 2001
- $D_2$ = (1) If household was in a community affected by a severe drought in 2002
- Consum = $\ln(2000$ total household consumption/10,000)
- Size = Number of persons living in the household in 2000

Section 5: Randomization of RPS and Its Implications

Because 75% of those living in poverty in Nicaragua reside in rural areas the program focused on rural communities (Maluccio and Flores, 2005). Two departments, Madriz and Madagalapa from the North Central region of Nicaragua, were chosen for two reasons. Those
reasons were their ability to implement the program, which required relatively strong institutional and local government capacity, and because of the high poverty rates within the communities and their proximity to the capital of Nicaragua, Managua, where RPS has its headquarters. About 80% of the rural populations within the two departments in 1998 were classified as poor.

Next, six municipalities within these departments were targeted because of the presence of a concurrent poverty alleviation program. Within these six municipalities were a total of 59 communities. Using the 1995 Nicaraguan Life household survey these communities were ranked according to an index that measured relative poverty. The 42 communities with index scores corresponding to the highest level of poverty were selected for the pilot phase of the program, and were randomly assigned to either a treatment group eligible for RPS benefits or to a control group, with 21 communities assigned to each group (Maluccio and Flores, 2004).

**Section 5.A: The LaLonde Critique and Experimental Evaluation**

Like other conditional cash transfer programs (Progresa, PRAF, Bolsa Escola), RPS performed random assignment of treatment at the community level rather than at the household level. By providing services in selected localities, RPS could save on administrative costs. Meanwhile, for the researcher, randomization at the community level may be problematic because it may prompt a greater incidence of nonrandomness among households simply because of initial differences between communities selected for control and treatment groups (Behrman and Todd, 1999). Bias can arise because of initial differences between treatment and control communities which suggests non-experimental methods should be considered and compared to experimental results.
LaLonde (1986) conducting a seminal study of biases present in non-experimental evaluations, examines a U.S. worker trainer program and suggests that care be taken when selecting non-experimental estimators. Utilizing a subset of LaLonde’s data, Dehejia and Waba (1999) showed that propensity score matching (PSM) performed better than the non-experimental methods used by LaLonde. The results of Dehejia and Waba have been questioned by Smith and Todd (2005), who in particular studied whether PSM solves the selection problem discussed by LaLonde. The debate between Dehjia and Waba and Smith and Todd highlights the fact that an estimator should be chosen to fit the available data. Ravallion (2005) claims that PSM is effective when identical survey instruments are used in control and treatment communities, as they were in the RPS data. This claim is confirmed in a study using PSM on the Progresa data (Diaz and Handa, 2004).

The literature on non-experimental methods typically uses difference-in-difference as its comparison for the effectiveness of the estimator. However, the main concern in the evaluation of RPS is that randomization at the community level may have created heterogeneous characteristics between the control and treatment populations. Ravallion (2005) and Ravallion and Chen (2005) suggest that a combination of PSM and difference-in-difference helps eliminate initial heterogeneity, while at the same time taking advantage of difference-in-difference’s ability to control for time variant characteristics. The main concern of using difference-in-difference estimators for analysis at the household level when randomization takes place at the community level is that there may be ex-ante differences between treatment and control community that may increase bias. By matching a household within the treatment community to a household in the control community, PSM eliminates some possible bias. In particular, this method of matching
reduces bias that could arise from the differences in the probability that a household has remittances or had access to credit based on community factors.

Section 5.B: Testing for Differences between Treatment and Control Groups

Behrman and Todd (1999) study of random assignment of Progresa utilizes three estimators to test for successful randomization of continuous, discrete, and binary variables: the Kolmogorov-Smirnov, T-test, and Pearson Chi-squared, respectively. This paper’s analysis uses all three types of variables. Each is tested using the appropriate test: consumption (a continuous variable); credit access, remittances, and child’s gender (binary variables); and family size and child age (discrete variables).

Of the variables tested only credit access in the formal market was statistically significant at the 5% level. The difference in credit rationing between the control and experimental communities was around 4 percentage points. However, given credit access’s strong impact on education - particularly the fact that households with credit access may have been less likely to be in the treatment group - this difference in household characteristics could have some impact on typical experimental analysis. Ravallion (2005) points out how such problems with randomization can lead to miscalculations of a program’s impacts.

In this data set the lack of randomization of households with credit access could affect difference-in-difference estimates of educational attainment, if this paper’s key hypothesis holds that program impacts are greater when households are both constrained and impacted by negative economic shocks. Therefore, a second analysis is performed using a propensity score-matching method that matches each household within the treatment group to an experimental
household to calculate the program impact. This method is used as a check on the standard
difference-in-difference estimator proposed in equation 12.

Rosenbaum and Rubin (1983) denote the probability that a household is given the
treatment \( P(X_i) \), where \( X_i \) is a set of observable characteristics. The value of \( P(X_i) \) is equals the
probability that a household is in the treatment group given the observed characteristics \( \Pr(D_i = 1 \mid X_i) \), which denotes the propensity score of unit \( i \) given the vector \( X_i \). Rosenbaum and Rubin
(1983) prove that if a few assumptions hold that outcomes are independent of \( P(X_i) \), just as
would be expected in the case of an actual random assignment. They further assume that
unobserved differences do not impact whether or not \( i \) is in the treatment group, that \( D_i \)'s are
independent over all \( i \).

As in other works in the literature, this paper estimates the propensity scores for each
observation, using a binary response model for each child’s enrollment in the RPS community
and for the control group (Ravallion and Chen, 2005). In order to control for differences in age
and gender, the force option of the Stata command match is used to ensure that children of the
same age and gender were compared. The sample was sufficiently robust that all children in the
treatment group were matched with one in the control. All observations were matched based
upon credit rationing in formal and informal markets in 2000, the presence of migrants in 2000,
total consumption in 2000, and the shock measures for the years 2001 and 2002. In addition,
separate estimates are calculated for the impact of RPS for households in 2001 using PSM by
whether the child lived in a community that experienced a shock. The default option of a single
match was used. Thus, average treatment effects (ATE) on enrollment (y) for each year using
the matching estimator are calculated as follows, where \( t \) denotes the year, \( n \) is the number of
children in rps communities, i-rps, is a child in an RPS community, and their match in a control community is denoted as i-con.

\[ \text{ATE}_i = \frac{\sum_{i=1}^{n} (y_{i-rps} - y_{i-con})}{n} \]  

(13)

The comparable program impact of ATE for 2001 or 2002 from PSM to the difference-in-difference estimate is equivalent to ATE\textsubscript{2001} - ATE\textsubscript{2000} and ATE\textsubscript{2002} - ATE\textsubscript{2000}.

Section 6: Results

The results of the difference-in-difference estimation are presented in Table 6 below. Table 7 presents our estimations of average treatment effects using propensity score matching (PSM) and Table 8 shows program impacts using difference-in-difference estimation.\textsuperscript{13} The most noteworthy aspect of the results is that the impact measured by the difference-in-difference is similar to our estimates of the impact of RPS using the PSM estimator. Both estimators show that in 2001 RPS had twice the impact on enrollment for those households in a drought community. Consistent with our prediction that DID would over-estimate program impacts, PSM shows a smaller impact on enrollments than those suggested by DID. However, the coefficients are not statistically significantly different from one another. The PSM estimator found an increase in school enrollment of 22% for all children living in drought communities in 2001, compared to the DID estimates of 24% and 31% for non-rationed and rationed households, respectively. In 2002, when the drought did not show a significant impact on school enrollment, the PSM estimator and the DID estimator yield similar results with enrollment, increases of around 10% (although slightly higher for credit-constrained households in the DID estimator).
Interestingly, the impact of RPS in 2002 for all children was similar to the impact of RPS on children in non-drought communities in 2001.

The second noteworthy aspect seen is the effect of the 2001 drought on school enrollment, particularly for credit-constrained households. A comparison of program impacts by credit access status and the presence of a drought in 2001 are shown below in Table 8. RPS had the greatest effect on credit-constrained households with a drought in 2001. Table 8 shows that RPS increased school enrollment in 2001 by 31% for these households as represented by the sum of the variables ($AP01$, $APD01$, and $APF01$). These variables correspond to RPS’s impact in 2001 on all households, on households in a drought community, and on credit-rationed households, respectively. Taking away treatment impacts on credit rationed households ($APF01$), the DID estimates show RPS increased enrollment in 2001 by about 24% in households with credit access that experienced a drought in 2001 (the sum $AP01$ and $APD01$). The DID estimator results show that RPS increased enrollment by 23% for households that did not experience a shock but were credit rationed (the sum of $AP01$ and $APF01$). RPS had the smallest effect in 2001 on non-rationed households who did not experience a drought, only about 16% ($AP01$). The results show that rationed households who experienced a drought had program impacts nearly twice as large (31% compared to 16%) as those non-rationed households without a drought.

The variables for 2002 show that droughts in that year did not significantly impact enrollment whether in RPS or non-RPS communities ($dry02$); however, being in RPS cancels the negative effects on school enrollment of being credit rationed. Consistent with the prediction of the household model, our measure of household consumption ($consum$) was statistically significant and positively related to school enrollment. In addition, it is also found that large
households had lower school enrollments. The results, however, did not find statistically significant impacts on school enrollment from the presence of migrants within the household, nor for rationing in the informal market. Due to the lack of impacts of these variables their interaction terms with RPS and droughts are not presented in the results.

Tables 6, 7, and 8 about here

Finally, although gender was not a focus of this study, separate regressions for boys and girls are presented in Table 9. The results do not suggest major differences between boys and girls, although family size does negatively impact girls and not boys. This outcome reflects the fact that girls are more likely to leave school to take care of family members. This may be because payments in RPS were not tied to the child’s gender, specifically to girls’ enrollment unlike in Progresa.

Table 9 about here

Section 7: Conclusions

Conditional cash transfer programs such as RPS --which pay households in exchange for school attendance and required visits to health care facilities-- have been designed with the purpose of keeping children in school. Recent empirical work has shown that credit-constrained households are especially likely to remove their children from school during poor economic times. This fact is evident in the household model presented, which shows the important relationship between the impact of shocks on school enrollment and a household’s credit access.

Using difference-in-difference methods and PSM the results show that RPS significantly increased enrollment for children age 7-13 by 15-30% in 2001 and 10 to 15% in 2002 (compared to 2000). These results are consistent with the estimates reported by Maluccio and Flores (2004)
and compare favorably to the impacts of other conditional cash transfer programs. The key finding was that RPS helped credit-constrained Nicaraguan households maintain school enrollment in the face of a massive drought in 2001 which affected 85% of the communities surveyed. Especially important is the fact that credit-constrained RPS households seemingly do not have their school enrollment impacted by severe shocks.

To support the use of the difference-in-difference estimator, which assumes random assignment, we used a non-experimental method: propensity score matching. This estimator was chosen because treatment and control communities received the same survey at nearly identical times. The results from using propensity score matching are not substantially different from the difference-in-difference estimation but do suggest that DID may slightly overestimate program impacts.

These results suggest that conditional cash transfer programs can overcome the impacts of credit constraints on school enrollment. This raises a question: how do cash transfers overcome credit constraints? There are three possibilities: (1) that these households are impacted by the incentive and would prefer not to lose their transfer, (2) the transfer increases income to the point where credit constraints do not impact education, and (3) that cash transfers from RPS help households acquire collateral, which could then be used to obtain loans. These mechanisms should be considered in future research using the RPS data.
Appendix A: Shocks and Accepting Conditional Cash Transfers

Equation 10 claims that a household will accept a conditional cash transfer if the following is true:

\[
\begin{align*}
U'(\theta F(1-e',w) + \Gamma, e') + \delta \Phi E[U^2(\theta F(1,w) + \Phi e')] & \geq U'(\theta F(1-e^*,w) + \Gamma, e^*) + \delta \Phi E[U^2(\theta F(1,w) + \Phi e^*)]
\end{align*}
\]

(A1, 10)

If equation (A.1) does not hold then there are two necessary conditions if the household would be better off not accepting the transfer. First (Equation A.2) the optimum level of schooling in the absence of a transfer, e*, must be less than the minimum amount of school necessary for the transfer. Clearly, if it is not then the household would meet the requirement of schooling and receive the transfer, and since marginal utility of consumption is positive the transfer must increase utility. This result yields our second inequality (A.3): that households who do not accept the transfer must produce income greater than that from the transfer. Lacking such, they would not be utility maximizing since they would have lower consumption and education without transfers.

\[
e^* < e' \rightarrow \theta_1 F(1-e') + \Gamma < \theta_1 F(1-e^*)
\]

(A.3)

Suppose it is assumed if the household of interest does not receive transfers then they do not enroll the child in school, and therefore e* = 0. Furthermore, these households are less likely to enroll their children in school if there is a negative economic shock. To show the importance of the inequalities in A.2 consider the derivative of household income with respect to the shock term, \( \theta_1 \), where income with transfers accepted is \( Y' = \theta_1 F(1-e') + \Gamma \) and income with transfers rejected is \( Y^* = \theta_1 F(1-e^*) \).
\[ \frac{\partial Y'}{\partial \theta_l} = F(1-e') \]  
(A.3)

\[ \frac{\partial Y^*}{\partial \theta_l} = F(1) \]  
(A.4)

It is clear with this set of assumptions that income will fall at a greater rate for the maximum without transfers. This result implies that it is less likely that the necessary condition, that income without transfers must be greater if transfers are not accepted, will hold. These results show that households are more likely to accept transfers during negative economic shocks.
### Tables

#### Table 1: Descriptive Statistics for Full Sample (964 Households)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Mean (Standard Deviation)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Consum</td>
<td>Ln(Consumption/10000)</td>
<td>.63 (.59)</td>
</tr>
<tr>
<td>Formal</td>
<td>(1) Rationed (0) non-rationed</td>
<td>.86 (.34)</td>
</tr>
<tr>
<td>Informal</td>
<td>(1) Rationed (0) non-rationed</td>
<td>.81 (.39)</td>
</tr>
<tr>
<td>Migrants</td>
<td>(1) if Migrants (0) no migrants</td>
<td>.07 (.25)</td>
</tr>
<tr>
<td>Size</td>
<td>Number of Household Residents</td>
<td>7.0 (2.5)</td>
</tr>
</tbody>
</table>

#### Table 2: Household Descriptive Statistics for RPS and Control Communities

<table>
<thead>
<tr>
<th>Variable</th>
<th>RPS Communities</th>
<th>Control Communities</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Std.</td>
</tr>
<tr>
<td>Consumption</td>
<td>22285.1</td>
<td>12514.4</td>
</tr>
<tr>
<td><strong>Formal</strong></td>
<td>.88</td>
<td>0.3</td>
</tr>
<tr>
<td>Informal</td>
<td>.81</td>
<td>0.4</td>
</tr>
<tr>
<td>Migrants</td>
<td>.06</td>
<td>0.2</td>
</tr>
<tr>
<td>Size</td>
<td>6.8</td>
<td>2.5</td>
</tr>
</tbody>
</table>

*Bold Indicates a statistically significant difference between treatment and control at 5% level using T-test

#### Table 3: Household Consumption by Credit Access Status

<table>
<thead>
<tr>
<th>Credit Access in the Formal Market</th>
<th>Credit Access in the Informal Market</th>
<th>Percent of Households</th>
<th>Average 2000 Consumption</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>No</td>
<td>72%</td>
<td>20367</td>
</tr>
<tr>
<td>No</td>
<td>Yes</td>
<td>9%</td>
<td>22816</td>
</tr>
<tr>
<td>Yes</td>
<td>No</td>
<td>14%</td>
<td>29137</td>
</tr>
<tr>
<td>Yes</td>
<td>Yes</td>
<td>5%</td>
<td>31947</td>
</tr>
</tbody>
</table>
## Table 4: Household Per Capita Consumption by Household Size

<table>
<thead>
<tr>
<th>Household Size</th>
<th>Per capita Consumption 2000</th>
<th>Percent of Households</th>
</tr>
</thead>
<tbody>
<tr>
<td>3 or less</td>
<td>6283</td>
<td>4%</td>
</tr>
<tr>
<td>4</td>
<td>5031</td>
<td>11%</td>
</tr>
<tr>
<td>5</td>
<td>4038</td>
<td>13%</td>
</tr>
<tr>
<td>6</td>
<td>3562</td>
<td>20%</td>
</tr>
<tr>
<td>7</td>
<td>2959</td>
<td>15%</td>
</tr>
<tr>
<td>8</td>
<td>2973</td>
<td>13%</td>
</tr>
<tr>
<td>9</td>
<td>2522</td>
<td>8%</td>
</tr>
<tr>
<td>10 or more</td>
<td>2419</td>
<td>15%</td>
</tr>
</tbody>
</table>

## Table 5: School Enrollment by Drought Severity and Formal Rationing

<table>
<thead>
<tr>
<th>Number of children</th>
<th>Children in Households without formal credit access</th>
<th>Total Enrolled</th>
<th>Enrollment Formal Rationed Households</th>
<th>Enrollment Formal Non-Rationed Households</th>
</tr>
</thead>
<tbody>
<tr>
<td>2000 All*</td>
<td>1751</td>
<td>15%</td>
<td>77%</td>
<td>83%</td>
</tr>
<tr>
<td>2001 All</td>
<td>1795</td>
<td>14%</td>
<td>88%</td>
<td>88%</td>
</tr>
<tr>
<td>2002 All</td>
<td>1775</td>
<td>13%</td>
<td>90%</td>
<td>90%</td>
</tr>
</tbody>
</table>

### 2001 Drought
- None: 165, 12% - 91% - 90% - 95%
- Not Severe: 787, 14% - 93% - 93% - 91%
- Severe: 806, 15% - 85% - 84% - 91%
- Very Severe: 37, 0% - 43% - 43% - NA

### 2002 Drought
- None: 763, 14% - 90% - 90% - 91%
- Not Severe: 514, 9% - 92% - 92% - 92%
- Severe: 390, 15% - 91% - 90% - 91%
- Very Severe: 108, 18% - 80% - 83% - 68%

* Information on droughts was not collected in 2000
Table 6: OLS Difference-in-Difference Estimates with Child Fixed Effects

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Coefficient</th>
<th>SE</th>
</tr>
</thead>
<tbody>
<tr>
<td>dum01</td>
<td>Dummy 2001</td>
<td>-0.032</td>
<td>0.028</td>
</tr>
<tr>
<td>dum02</td>
<td>Dummy 2002</td>
<td>0.025</td>
<td>0.028</td>
</tr>
<tr>
<td>RPS</td>
<td>RPS Dummy</td>
<td>-0.009</td>
<td>0.020</td>
</tr>
<tr>
<td>Fration</td>
<td>Formal Ration Dummy</td>
<td><strong>-0.048</strong></td>
<td><strong>0.026</strong></td>
</tr>
<tr>
<td>Fration</td>
<td>Informal Ration Dummy</td>
<td>0.003</td>
<td>0.014</td>
</tr>
<tr>
<td>migrant</td>
<td>Migrant Dummy</td>
<td>0.023</td>
<td>0.020</td>
</tr>
<tr>
<td>dry01</td>
<td>Drought 2001*</td>
<td><strong>-0.086</strong></td>
<td><strong>0.014</strong></td>
</tr>
<tr>
<td>dry02</td>
<td>Drought 2002</td>
<td>0.009</td>
<td>0.016</td>
</tr>
<tr>
<td>ADF01</td>
<td>Formal Rationed Household with Drought 2001</td>
<td><strong>-0.079</strong></td>
<td><strong>0.039</strong></td>
</tr>
<tr>
<td>ADF02</td>
<td>Formal Rationed Household with Drought 2002</td>
<td>0.061</td>
<td>0.048</td>
</tr>
<tr>
<td>AP01</td>
<td>RPS Household 2001</td>
<td><strong>0.159</strong></td>
<td><strong>0.021</strong></td>
</tr>
<tr>
<td>AP02</td>
<td>RPS Household 2002</td>
<td><strong>0.109</strong></td>
<td><strong>0.022</strong></td>
</tr>
<tr>
<td>APF01</td>
<td>Formal Rationed RPS Household 2001</td>
<td>0.074</td>
<td>0.030</td>
</tr>
<tr>
<td>APF02</td>
<td>Formal Rationed RPS Household 2002</td>
<td>0.062</td>
<td>0.029</td>
</tr>
<tr>
<td>APD01</td>
<td>RPS Household with Drought 2001</td>
<td>0.079</td>
<td>0.035</td>
</tr>
<tr>
<td>APD02</td>
<td>RPS Household with Drought 2002</td>
<td>-0.053</td>
<td>0.044</td>
</tr>
<tr>
<td>Size</td>
<td>Household Size</td>
<td><strong>-0.005</strong></td>
<td><strong>0.002</strong></td>
</tr>
<tr>
<td>Consum</td>
<td>Log of Consumption in unit of 10,000 SC</td>
<td>0.064</td>
<td>0.010</td>
</tr>
<tr>
<td>age_cur</td>
<td>Child's Age</td>
<td>0.234</td>
<td>0.029</td>
</tr>
<tr>
<td>age_cur2</td>
<td>Child's Age Squared</td>
<td><strong>-0.012</strong></td>
<td><strong>0.001</strong></td>
</tr>
<tr>
<td>_cons</td>
<td>Constant</td>
<td><strong>-0.287</strong></td>
<td><strong>0.146</strong></td>
</tr>
</tbody>
</table>

N = 5321             R-Squared = .09
*Bold indicates significance at 5% level and bold and italics at 10% level

Table 7: Treatment Impacts Using PSM

<table>
<thead>
<tr>
<th>Year</th>
<th>Average Treatment Effect</th>
<th>Standard Deviation</th>
<th>Observations In Treatment Group</th>
</tr>
</thead>
<tbody>
<tr>
<td>2000</td>
<td>0.011</td>
<td>0.023</td>
<td>918</td>
</tr>
<tr>
<td>2001</td>
<td><strong>0.165</strong></td>
<td><strong>0.017</strong></td>
<td>927</td>
</tr>
<tr>
<td>2002</td>
<td>0.093</td>
<td>0.016</td>
<td>911</td>
</tr>
</tbody>
</table>

2001 with Drought: 0.224 0.027 383
2001 no Drought: 0.111 0.020 544

*Bold indicates significance at 5% level

1 Note interaction terms with remittances were dropped as the results were not significant. Their omission does not substantially change the results.

2 The difference-in-difference using the matching estimator can be calculated using the difference between Average Treatment Effects of 2001 and 2000 or 2002 and 2000.
Table 8: Comparison of Enrollment by Drought 2001 and Credit Rationing Using Significant Variables from Table 6

<table>
<thead>
<tr>
<th></th>
<th>Drought</th>
<th>No Drought</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Credit Rationed Household</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>RPS</strong></td>
<td>-0.048 -0.086 -0.079 +0.159 +0.074 = 0.100</td>
<td>-0.048 +0.159 +0.074 = 0.185</td>
</tr>
<tr>
<td><strong>Control</strong></td>
<td>-0.048 -0.086 -0.079 = -0.213</td>
<td>-0.048</td>
</tr>
<tr>
<td><strong>RPS Impact</strong></td>
<td>0.313</td>
<td>0.233</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Drought</th>
<th>No Drought</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Non-Rationed Household</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>RPS</strong></td>
<td>-0.086 + 0.159 + 0.077 = 0.153</td>
<td>0.16</td>
</tr>
<tr>
<td><strong>Control</strong></td>
<td>-0.086</td>
<td>0</td>
</tr>
<tr>
<td><strong>RPS Impact</strong></td>
<td>0.239</td>
<td>0.16</td>
</tr>
</tbody>
</table>

Table 9: DID Estimates by Gender with Child Fixed Effects

<table>
<thead>
<tr>
<th></th>
<th>Boys N=2710</th>
<th></th>
<th>Girls N=2611</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Variable</td>
<td>Coefficient</td>
<td>SE</td>
<td>Coefficient</td>
<td>SE</td>
</tr>
<tr>
<td>dum01</td>
<td>-0.033</td>
<td>0.043</td>
<td>-0.035</td>
<td>0.036</td>
</tr>
<tr>
<td>dum02</td>
<td>0.017</td>
<td>0.041</td>
<td>0.035</td>
<td>0.038</td>
</tr>
<tr>
<td>RPS</td>
<td>0.036</td>
<td>0.028</td>
<td><strong>-0.057</strong></td>
<td><strong>0.028</strong></td>
</tr>
<tr>
<td>Fration</td>
<td>-0.057</td>
<td>0.036</td>
<td>-0.040</td>
<td>0.036</td>
</tr>
<tr>
<td>migrant</td>
<td>0.021</td>
<td>0.031</td>
<td>0.022</td>
<td>0.024</td>
</tr>
<tr>
<td>dry01</td>
<td><strong>-0.074</strong></td>
<td><strong>0.020</strong></td>
<td><strong>-0.101</strong></td>
<td><strong>0.019</strong></td>
</tr>
<tr>
<td>dry02</td>
<td>0.003</td>
<td>0.023</td>
<td>0.023</td>
<td>0.022</td>
</tr>
<tr>
<td>ADF01</td>
<td><strong>-0.101</strong></td>
<td><strong>0.058</strong></td>
<td>-0.060</td>
<td>0.050</td>
</tr>
<tr>
<td>ADF02</td>
<td>0.115</td>
<td>0.075</td>
<td>0.001</td>
<td>0.055</td>
</tr>
<tr>
<td>AP01</td>
<td>0.155</td>
<td>0.031</td>
<td>0.168</td>
<td>0.028</td>
</tr>
<tr>
<td>AP02</td>
<td>0.088</td>
<td>0.031</td>
<td>0.129</td>
<td>0.031</td>
</tr>
<tr>
<td>APF01</td>
<td>0.085</td>
<td>0.045</td>
<td>0.065</td>
<td>0.039</td>
</tr>
<tr>
<td>APF02</td>
<td><strong>0.083</strong></td>
<td><strong>0.042</strong></td>
<td>0.037</td>
<td>0.039</td>
</tr>
<tr>
<td>APD01</td>
<td>0.077</td>
<td>0.053</td>
<td>0.086</td>
<td>0.046</td>
</tr>
<tr>
<td>APD02</td>
<td>-0.075</td>
<td>0.071</td>
<td>-0.030</td>
<td>0.050</td>
</tr>
<tr>
<td>Size</td>
<td>-0.003</td>
<td>0.003</td>
<td><strong>-0.007</strong></td>
<td><strong>0.003</strong></td>
</tr>
<tr>
<td>Consum</td>
<td><strong>0.072</strong></td>
<td><strong>0.015</strong></td>
<td><strong>0.053</strong></td>
<td><strong>0.014</strong></td>
</tr>
<tr>
<td>age_cur</td>
<td>0.226</td>
<td>0.043</td>
<td>0.241</td>
<td>0.039</td>
</tr>
<tr>
<td>age_cur2</td>
<td><strong>-0.011</strong></td>
<td><strong>0.002</strong></td>
<td><strong>-0.012</strong></td>
<td><strong>0.002</strong></td>
</tr>
<tr>
<td>cons</td>
<td>-0.282</td>
<td>0.211</td>
<td>-0.274</td>
<td>0.195</td>
</tr>
</tbody>
</table>

*Bold indicate significance at 5% level and bold and italics at 10% level
References


http://are.berkeley.edu/~sadoulet/papers/CCTSafetyNet.pdf


Endnotes

1 The impact of Credit access has not been included in the Progresa evaluation due to the extremely small number of households with credit access (see De Janvry et al., 2005)

2 This is consistent with the models on which we build (Glewwe and Jacoby, 2004; Jacoby and Skoufias, 1997)

3 Households may generate income either through on farm production or by seeking wage labor; it is not important in this case. Income from wages may experience decreasing marginal returns to child labor, because wage labor may not always be available and time spent looking for work may have decreasing returns.

4 In this section the statement of the maximization problem will include the input variables for all functions. When first order conditions are taken the input variables are not written explicitly. In addition, we use subscripts to indicate derivatives; two subscripts indicate second order derivatives.

5 First please note that $F_e < 0$, as the negative sign is not explicitly brought out of the derivative. Second, a corner solution for zero schooling exists when $U_1^c \theta_1 F_e > U_1^e + \delta \Phi U_2^e$ at $e = 0$, and a corner solution for zero child labor when $U_1^c \theta_1 F_e < U_1^e + \delta \Phi U_2^e$ at $e = 1$

6 For now, we assume B (the borrowing limit) is exogenous.

7 Those who would have attended sufficient levels of school may have small impacts due to increased consumption, but since $e'$ is likely to be close to all of a child’s time the impact should be smaller.

8 If a household’s wealth was large enough it could sell off assets in order to fund education if hit with a negative economic shock, then credit access would not be as important. However, given the low wealth levels of those targeted by RPS, it seems unlikely that household wealth would be large enough to finance education.

9 “Census comarcas are administrative areas within municipalities that include one to five small communities averaging one hundred households each” (Maluccio, 2005).

10 Households with a motor vehicle or more than 20 manzanas of land were not eligible for benefits.

11 (C$) is September 2000, Nicaraguan códdobas, $1 U.S. is about C$12.85

12 Currently there is no publicly available data on which participants had their funding revoked due to not following the guidelines of the program. These data may become available at a later date, in which case this information could be incorporated into further analysis.
The results presented differ from Equation 13 in that interaction terms with regards to remittance have been dropped. This due to the fact that none of the interactions with remittances significantly impact enrollment and their omission does not substantially change results or conclusions. Similarly there was not found to be an effect of being rationed in the informal market, therefore only a single interaction term is included.