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Conditional Cash Transfers, Shocks, and School Enrolment in Nicaragua

SETH RICHARD GITTER* & BRADFORD L. BARHAM**
*Towson University, USA, **University of Wisconsin-Madison, USA

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ABSTRACT *This work estimates the impacts of a Nicaraguan cash transfer programme that pays households conditional on school attendance and family visits to health clinics and seminars. A model explores the impact on school enrollment of cash transfers given differences in household wealth, labour market opportunities, and negative shocks. A difference-in-difference estimation for distinct wealth cohorts reveals that the programme led to a significant improvement in school enrollment outcomes among poor households in coffee-cultivating communities. The results cast doubt, however, on proposals that broadly link conditional cash transfers to negative shocks.*

I. Introduction

Over the last decade, conditional cash transfer programmes have been a popular policy tool in the effort to increase school attainment in Latin America. These programmes provide cash payments to households that adhere to certain requirements, which include regular school attendance as well as visits to social service agencies, such as health care clinics or child nutrition programmes. Conditional cash transfer (CCT) programmes exist in a number of countries (such as, Brazil, Chile, Colombia, and Honduras), though the best known and studied is Progresa in Mexico (Hoddinott and Skoufias, 2004; Schultz, 2004; Behrman et al., 2005; Skoufias, 2005; De Janvry et al., 2006a). Most evaluations find CCT programmes effective at raising human capital and contributing to poverty alleviation among poor rural households (Rawlings and Rubio, 2005).

This article evaluates a CCT programme, Red de Protección Social (RPS), which was modelled after Progresa using a randomised experimental design and introduced as a pilot in 2001 and 2002 in two regions of Nicaragua. RPS also pursued a multipronged approach to increasing human capital, providing cash payments in return for participation in health services, nutritional education and school. Previous

Correspondence Address: Seth Richard Gitter, Department of Economics, Towson University, 8000 York Road, Towson, 21252 USA. Email: srgitter@gmail.com
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work by Maluccio and Flores (2004) shows that RPS led to significant improvements in diet, child height and weight for age, and school enrollment. Our evaluation focuses on household decisions regarding children's time in school and working. A two-period model frames the basic trade-off between the opportunity cost of current foregone and future consumption generated by investing in human capital. Building on Glewwe and Jacoby (2004), and Jacoby and Skoufias (1997), the model explores how wealth, economic shocks, and conditional cash transfers shape the school versus work decision for children among poor, rural households.

Not surprisingly, the model shows that rural households are more likely to choose child labour over school to improve current consumption prospects if: they are limited by low wealth; they encounter favourable labour market options for children; and/or they insure *ex ante* or *ex post* against exposure to shocks through child labour. However, labour market options and risk management are intertwined and can cut in different directions, if a shock has the effect of broadly lowering returns to rural labour. Combined, low wealth, child labour market options, and shock exposure make the predicted effects of a CCT ambiguous, because they have counteracting effects; low wealth and negative shocks encourage more child labour effort to generate income, yet negative shocks can lower returns to child labour. Whether cash transfers help to promote higher education enrollment under adverse conditions depends on which effects dominate. This ambiguity motivates the need for empirical evaluation of the impact of cash transfers under different types of shocks. In the RPS sample, droughts and major coffee price declines provide two distinct types of negative shocks.

The evaluation deploys a conventional difference-in-difference comparison of control and treatment communities to examine how the introduction of RPS affected child education outcomes. In addition to the randomised-trial experimental design, two specification refinements help to identify the effects of wealth, child labour market opportunities, and the negative shocks on educational attainment outcomes. The first refinement divides the sample into three wealth categories by initial (baseline) land holdings (in rural areas of Latin America land accounts for the vast majority of tangible wealth and correlates highly with other wealth holdings – Broegaard (2005), Torche and Seymour (2007)). The second refinement explores the dual effects of:

1. being a rural household in a community where coffee is cultivated and thus *ceteris paribus* facing better labour market options for children; and also,
2. being a household more directly exposed to the major coffee price shocks that occurred in 2001 and were diminishing by 2002.

This latter effect is explored by conducting a separate analysis of education outcomes in coffee cultivating communities where year indicators help to distinguish price shock effects from the typical labour market effects of higher returns to child labour.

We find that RPS has the largest positive impacts on school enrollment for children in poorer households and for those in coffee communities during higher price years. As expected, RPS helps poor households to meet current consumption objectives by providing payments that substitute for current child labour market earnings. A more surprising but consistent result is that for households with little or no land wealth, droughts increased school enrollment in control communities, presumably by reducing returns to child labour and hence the opportunity costs of school attendance. Thus, in terms of school enrollment objectives, the generally positive effects of RPS are reduced by negative economic shocks in the form of droughts or coffee price declines, both of which have depressing effects on returns to child labour.

Our results verify the positive impact of CCTs on school enrollment outcomes among poor households. Yet, they also call into question broad application of a second-generation reform under consideration for CCT programmes, which would condition payments on negative economic shocks (de Janvry and Sadoulet, 2006 and De Janvry et al., 2006b).[1] While droughts and price shocks to cash crops are reasonable examples of observable exogenous shocks that could potentially be used to determine payment size, our empirical results cast doubt on the value of linking payments to these negative shocks. If the goal is to increase school enrollment, certain negative economic shocks and increased CCT payments can both be pushing in the same direction. Our findings highlight the need for a nuanced view of how shocks shape educational attainment outcomes before linking CCT payments to shocks.

The next section briefly reviews the current literature on education attainment, shocks, and CCT programmes. The third section presents a model of the impact on education of CCTs conditional on differences in wealth, labour market options, and shocks. Then, background information on RPS' experimental design, along with select descriptive statistics, sets up the econometric analysis, which is presented in two sections. We conclude with reflections on what our findings suggest for policy and future research.

II. Literature on Wealth, Child Schooling, Shocks, and CCTs

The strong links between household wealth and child schooling are well known (Jacoby and Skoufias, 1997; Glewwe and Jacoby, 2004). Basu's (1999) model of the child labour and schooling decision highlights how returns to child labour and rural household wealth may be closely intertwined, because land holdings are both the main form of holding wealth and a determinant in the value of child labour. At the margin, additional household land holdings may increase the potential productivity of child labour particularly for households with relatively small land holdings. Thus, at low levels of wealth, additional land holdings may be negatively related to schooling. Then, once wealth and the resulting income potential reach a certain point, future earnings potential and altruism of parents can dominate the marginal utility of current consumption, yielding a positive relationship between schooling and wealth. At the household level, therefore, non-linear relationships can arise between wealth and child labour.

A related phenomenon will be the strength of local labour demand which can vary based on the types of crops cultivated in the region. In much of Latin America, coffee cultivation provides substantially greater returns to child labour, especially for boys, than do other crops and could influence household allocation of child labour time (Gitter and Barham, 2007; Maluccio, 2005). Thus, both local cropping patterns and a child's gender can also affect the opportunity cost of the forgone labour.

The effect of economic shocks on educational attainment in developing countries is under intense scrutiny. In some of the earlier work, Duryea and Arrends-Kuenning (2003), Binder (1999), and Schady (2001) examined the impact of macroeconomic downturns on school enrollment in Brazil, Mexico, and Peru, respectively. These studies reveal that enrollment rates increase, presumably because negative macroeconomic shocks depress labour demand and child wages, reducing the opportunity costs of education sufficiently for that effect to prevail over the negative income effects. In contrast, Thomas et al. (2004) and Rucci (2004) find that exchange rate crises in Indonesia and Argentina led to declines in schooling. Ambiguous results are not surprising according to Schady (2001), because the schooling choice should depend on the extent to which shocks affect household earning prospects and the relative opportunity cost of forgone child labour.

Two works in the conditional cash transfer literature, de Janvry et al. (2006a) and Maluccio (2005) evaluate how economic shocks and conditional cash transfers combine to shape school enrollment. However, neither work conditions the analysis on household wealth. De Janvry et al. (2006a) test the impact of Progresa and economic shocks on children's educational attainment utilising an analytic model similar to Jacoby and Skoufias (1997), and they depict the relationship between shocks and school enrollment as monotonic. They then estimate an econometric model with linear relationships between shocks, schooling, and programme impacts considering a variety of shocks, but not differentiated by household wealth.

Consistent with the macroeconomic shock literature, they find that only some negative economic shocks (earthquakes, floods, and hurricanes) increase child labour and correspondingly decrease school attendance. However, children in localities facing droughts are less likely to work than in communities with no shocks. Their explanation for this latter result is that droughts are a frequent occurrence in Mexico, so households may have devised *ex-ante* coping mechanisms that do not include increases in child labour. Two other possible explanations are that during droughts the opportunity costs of labour on and off farm are less attractive than otherwise, while for the other types of weather/natural shocks named above the opposite is likely to hold given the need to repair, restore, and recover. De Janvry et al. (2006a) suggest also that with other non-drought natural disaster shocks there is greater potential for severe household losses of both assets and people, which would increase the opportunity costs of time spent in school.

Maluccio (2005) shows that RPS helped Nicaraguan households to maintain children's school enrollment during a period of poor coffee prices. Maluccio (2005) finds, for example, that children in coffee farming communities at the baseline were around 15 per cent less likely to go to school than those in non-coffee communities, but that the gap narrowed to 10 per cent in 2001 and to 5 per cent in 2002. However, his analysis does not distinguish the effects of price shocks from coffee cultivation, because the variable for coffee year effects combines both dimensions.[2] Moreover, his analysis also does not control for wealth and hence the potential for CCT effects to vary across different types of households. Our regression strategy attempts to separate more carefully the effects of the price shocks and the differences between coffee and non-coffee communities among households of different wealth levels.

III. Two-Period Model of Education and Child Labour Choice

In this section, we develop a two-period household utility maximisation model to explore how wealth, labour market opportunities, negative shocks, and CCT programmes affect education and child labour trade-offs. The model shows how child schooling decisions depend on two critical factors: the contribution of child labour to current expected consumption and the opportunity cost of future forgone consumption. The model shows first how negative economic shocks can decrease both children's productivity and current expected consumption, which in turn make the direction of the relationship between shocks and schooling ambiguous. The impact of CCTs is to provide an unambiguous price effect that reduces the potential consumption contribution from child labour, but when and where it has the effect of increasing school enrollment is more subtle.

The Basic Model

Assume a household with fixed wealth (w) and endowments of adult labour that lacks access to both credit and a conditional cash transfer.[3] Further, assume no direct costs to schooling (free public education). The two-period model contains a single decision on the allocation of the child's time in the first period given the potential returns to more education in their labour outcomes in a second period. A child in the household is endowed with one unit of time in the first period that is divided between school, e, and child labour $(1-\mathrm{e})$. Besides enhancing first-period utility, education increases income in the second period by Φet (where Φ is the return to human capital). Income in the first period is generated by a diminishing marginal returns production function, $\theta_1\mathrm{F}(1-\mathrm{e}, \mathrm{w})$, where θ_1 is a stochastic shock parameter, realised before the education decision.[4] In the second period income is generated with a production function, $\theta_2\mathrm{f}(\mathrm{w})$, and an unknown stochastic parameter θ_2. Households consume their entire income in both first and second periods as shown in the budget constraints in Equation 1. There is no savings potential between periods.[5]

In the first period, households have a utility function, U^1, with two inputs: the consumption of a single good, c, purchased at the unit price and time spent in school, e. Second period utility, U^2, with discount rate, δ, is a function only of consumption. Because the second period shock outcome is not known when the first period education decision is made, the household maximises the sum of the first period utility and the expected value of utility in the second period, $\mathrm{E}[\mathrm{U}^2]$. We also assume 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 (that is, the cross partials are zero).[6] The household problem is to maximise two period utility:[7]

$$\max_e U^1(c^1, e) + \delta E[U^2(c^2)], \quad \text{where } 0 \leq e \leq 1 \tag{1}$$

subject to budget constraints $c^1 = \theta_1 F(1-e, w)$ and $c^2 = \theta_2 f(w) + \Phi e$

Substituting the budget constraints into equation 1 yields equation (1A):

$$\max_e U^1(\theta_1 F(1-e, w), e) + \delta E[U^2(\theta_2 f(w) + \Phi e)], \quad \text{where } 0 \leq e \leq 1 \tag{1A}$$

Since ‘e’ is the only choice variable, one interior solution exists for the utility maximising value of time spent in school presented in the first order condition of equation (2).[8]

$$U_1^1\theta_1 F_{e*} + U_{e*}^1 + \delta\Phi E[U_{e*}^2] = 0 \tag{2}$$

More wealth and positive economic conditions have two opposing impacts on child labour: they decrease the marginal utility benefit of income from a child, but also make child labour more productive. The two opposing impacts have the potential to create a non-linear relationship between wealth or economic conditions and child labour. These impacts are present in the first term in equation 2, which can be broken into two key parts U_1^1 and θF_e. The first part, U_1^1, is the marginal utility of consumption. The benefit of a child’s income will be smaller for households who have higher incomes due to the assumption of diminishing marginal utility of consumption. Higher initial incomes can come either through two means: wealth or positive economic conditions. However, wealth and economic conditions also shape the marginal product of child labour. More wealth or positive economic conditions increase the benefit to schooling, shown in equation 2 as the marginal opportunity cost of more time in school θF_e.

To demonstrate directly the potential non-linear relationship discussed above, it is useful to consider the comparative statics of the relationship between child schooling and the stochastic term (representing economic conditions).[9] Equation (2) is used to derive $\frac{\partial e*}{\partial \theta_1}$, which represents the relationship between *ex-ante* negative shocks to labour productivity in the first period and education:

$$\frac{\partial e^*}{\partial \theta_1} = \frac{F_e[U_1^1 + F\theta_1 U_{11}^1]}{-[U_{11}^1(\theta_1 F_e)^2 + U_1^1\theta_1 F_{ee} + E[\delta\Phi^2 U_{ee}^2] + U_{ee}^1]} \tag{3}$$

An examination of equation (3) shows that the denominator is positive, because all four terms in the brackets are negative (due to decreasing marginal utility and diminishing marginal returns). If the numerator term $[U_1^1 + F\theta_1 U_{11}^1 < 0]$, then the relationship between shocks and education is positive (that is, negative economic shocks decrease schooling), because $F_e < 0$. However, because *a priori* it cannot be determined whether $[U_1^1 + F\theta_1 U_{11}^1 < 0]$, the model provides an ambiguous conclusion on the link between shocks and schooling that is consistent with results in the empirical literature.

The ambiguity is caused by two key influences on child labour choices: the productivity of child labour and the opportunity cost of school. During negative economic shocks, child labour income provides a higher marginal utility as seen through the first term in the numerator, U_1^1. At the same time, children are less productive decreasing the opportunity costs shown in the second part, $F\theta U_{11}^1$. Because the shock term, θ_1, is included in the numerator directly and indirectly (through U_1^1 and U_{11}^1), the relationship between shocks and education will vary based on the value of the shock term and the marginal utilities associated with child income.

The relationship between shocks and education will also depend on household wealth. Household wealth can increase the incentive for the child to work (that is, households with more land have higher rates of return to child labour) and decrease the marginal consumption utility of that work (that is the impact of lost child income is smaller for richer households due to decreasing marginal utility). Thus, the derivation of the relationship between wealth and schooling would look similar to equation 3.

One factor not addressed by the model is how the income production technology might impact the child labour versus education decision. This factor could be particularly important in distinguishing between communities with and without coffee production. *Ceteris paribus*, the marginal product of child labour may be higher for households that live in coffee cultivating communities.

Conditional Cash Transfers

A household's decision to accept a transfer and send a child to school will depend on the size of the transfer, their utility of educating the child, and the opportunity costs of forgone child labour. As discussed earlier, a negative economic shock can have two opposing impacts on the opportunity costs of schooling. On the one hand, it can decrease child labour productivity and earnings. On the other hand, it can also decrease total household income thereby increasing the marginal utility of child income.[10] In the absence of a conditional cash transfer programme, the need for child income can outweigh the decrease in labour productivity and lead to a decline in enrollment with a negative economic shock. Conditional cash transfers can effectively dominate the opportunity cost decision if the cash transfer for schooling is sufficiently large. In those cases, the impact of a conditional cash transfer programme on school enrollment will depend mostly on e^*, the amount of schooling a child would have attended without the transfer.

Because the key trade-off is between the cash transfer amount and the household's opportunity cost of the forgone child earnings, it is also feasible to imagine situations where a very severe shock drives down labour productivity sufficiently to make it infeasible for the child to earn as much as the conditional transfer whereas under less severe conditions they might be able to earn more than the transfer. It is the potentially different thresholds for child earnings compared with the transfer, and the family's shifting marginal utility of that income, which can also generate non-linear responses of enrollment to shock severity. This non-linear relationship is potentially a driving force between the contrasting results found in previous empirical studies of the relationship between economic shocks and school enrollment. Therefore, while ambiguous, the possibility of a non-linear outcome developed here contrasts with the strictly negative relationship between shock and education that is presented in the formal model of de Janvry et al. (2006a).

IV. Nicaraguan Education and RPS

Nicaragua is one of the poorest countries in Latin America, and that status is reflected in the country's education indicators. While educational attainment of Nicaraguan children is better than that of previous generations, with about 80 per cent of nine-year-old children enrolled in school in the year 2000, only 40 per cent of children age 13 in 2000 were still in school. Overall, Nicaragua has one of the lowest primary school enrollment rates in Latin America. Thus, a better understanding of how the pilot programme RPS affected children's school attendance is vital to efforts to transform the educational situation of the poor in Nicaragua.

RPS was created in 2000 as a joint effort of the Inter-American Development Bank, the Nicaraguan Government and the International Food Policy Research Institute. The pilot programme worked with 42 rural communities in the departments of Madris and Matagalpa (Maluccio and Flores, 2004). Communities were randomly assigned in equal proportions to treatment and control groups. Households in treatment communities were eligible for several types of transfers.[11] The largest transfer was a C$2,880 ($224) food security transfer.[12] Households with children aged 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 RPS household received C$3885 ($302), or about 18 per cent of total annual household expenditure. In order to receive the food security transfer, household members had to attend bi-monthly meetings, and children had to have regular health care check-ups provided by RPS. To receive the additional school transfer, children also had to maintain regular school attendance. These regulations appear to have been enforced, as 10 per cent of participants had benefits suspended due to unmet obligations.[13]

Descriptive Statistics

RPS collected three rounds of data with a total of 1396 households. The first was a baseline sample taken in September of 2000 before programme implementation. The second two were done in October 2001 and 2002 while RPS was active. Attrition rates in the panel were nearly identical in treatment and control communities (Maluccio and Flores, 2004).[14] We sample a subset of the 1396 households, using only households with children that meet the eligibility criteria of being between the ages of 7–13.[15] In the sub-sample of 964 households with 1751 eligible children, 495 households (51 per cent) lived in communities where RPS was administered.

The main dependent variable of interest is child school enrollment. Households were asked if the child had attended school that year. With survey data collected in September and October, this means the observation of school enrollment is taken toward the end of school year, which runs from February to November. The timing of school vacation coincides with much of the harvest season, which runs from October to January (Maluccio, 2005). Consistent with the theoretical model, in this sense the initial decision to enroll a child in school in February is *ex-post* of the harvest in January.

Average school enrollment rates by treatment status and year are shown in Table 1. The overall preprogramme enrollment rate was 77 per cent. A comparison of enrollment rates in the baseline between control and treatment groups shows less than a 0.5 per cent difference, and is not statistically significant using a simple t-test. In the programme's two years, treatment groups averaged 96 per cent and 97 per cent enrollment respectively, while the control group averaged 80 per cent and 85 per cent.

Ideally, to fully explore the decision between child labour and school enrollment a measure of child labour force participation would also be examined. Unfortunately,

Table 1. Enrollment and worked in last week by year and treatment status (%)

Group	Variable	2000	2001	2002
Control	Enrolled in school	77	80	85
Treatment	Enrolled in school	77	97	96
Control	Worked last week	11	9	11
Treatment	Worked last week	10	5	5

the only survey question on child labour asks if the child has worked in the last week. As Maluccio (2005) suggests, this could create problems with seasonality as the baseline survey is taken pre-harvest, while the other surveys were taken during the beginning part of the harvest season. In addition, a one-week look at child labour is likely to be subject to significant censoring if labour opportunities on the spot market vary. Nonetheless, as Table 1 shows, the labour force participation outcomes are consistent with the RPS impacts seen in enrollment, with RPS reducing child labour participation in the treatment years. Below, we discuss but do not present the results of estimations using this labour force participation measure. They are consistent in sign and weaker in their statistical significance than the school enrollment results.

As noted in the discussion of the theoretical model, wealth and particularly land holdings can be a key determinant in school enrollment. We utilise reported land holdings from a pre-baseline census collected in May 2000. In that way, we eliminate potential endogeneous impacts from RPS. Although previous education and land purchase/sale decisions could potentially be endogenous choices, limited land sales in rural Nicaragua make this possibility unlikely (Boucher et al., 2005). To be cautious though, we interpret the impacts of land holdings on school enrollment as relationships and not necessarily as causal influences on enrollment outcomes.

Households are divided into three land-holding categories: less than one manzana, one to three manzanas and more than three manzanas. Land holdings are generally positively related to school enrollment as shown in Table 2 where school enrollment levels are compared for children in three land wealth categories.[16] Although the differences in baseline (2000) enrollment rates between control and treatment are slightly larger when examining each of the three land wealth categories compared to the sample as a whole, none of those differences are statistically significant using a simple t-test. Additionally, Table 2 shows a positive relationship between wealth and household consumption, credit access, and household size. Table 2 also includes a measure of maternal education (a control variable used below in the education attainment regression). On average, this indicator is low across the three land-size categories and not differentiated by wealth.

A central variable in the analysis is the measure of droughts that impacted the community. The survey instruments provide information from both individual households and a community leader on drought shocks. The module on shocks for both individual and community leader responses was not included in the baseline survey, but was added in both of the treatment years (2001 and 2002). A close examination of the descriptive statistics suggests the community level measure of droughts is a more credible measure than the individual reports, because the

Table 2. Descriptive statistics by landholdings

Land holdings	Per cent of sample	2000 control enrollment (%)	2000 treatment enrollment (%)	2000 Consumption	Rationed in formal credit market* (%)	Household size	Female head years schooling
Less than 1 Manzana	22	73	69	19400	89	6.43	1.74
1–3 Manzanas	*47*	77	79	21301	87	6.85	1.65
3 + Manzanas	30	79	79	25467	83	7.50	1.62

Note: *See endnote 5 for definition of rationed in credit market.

individual measure shows a positive relationship between consumption and reported droughts. Households with larger landholdings appear more likely to report droughts than smaller ones. By contrast, for the community leader response measure, we find a negative relationship between average household consumption levels in the community and the report of drought.

The coffee measure is also determined at the community level through the same survey of a local leader. Twenty-two of the 42 communities reported active coffee production, and were almost evenly split between treatment and control communities. The community measure is used in part because of limited data on household own farm production or labour participation.[17] Maluccio (2005) offers evidence that nearly all households that farmed or earned income harvesting coffee resided in communities that leaders identified as cultivating coffee.

The impact of both coffee price shocks and droughts are evident in Table 3. Note the substantial declines in average household consumption in 2001 and 2002 as compared to the baseline year 2000. In 2001, for example, control households in coffee communities experienced a decline in average consumption of over 15 per cent from the baseline year, and another 15 per cent from 2001 to 2002. Severe drought shocks also decreased consumption although not as consistently. Table 3 also shows that RPS helped to increase household consumption levels in non-coffee communities over baseline levels, while in coffee communities RPS appears to have helped households to maintain baseline consumption levels despite negative price shocks.

The data in Table 4 depict school enrollment rates based on both drought experience and whether the household resides in a coffee community. The first striking difference is that children in coffee communities were 20 per cent less likely to attend school in the baseline year of 2000 than children in the non-coffee communities, with 65–70 per cent school enrollment rates in coffee communities, compared to 85–90 per cent in non-coffee communities. The table also breaks down the impact on school enrollment of drought for coffee and non-coffee communities. The baseline difference between enrollment in drought and non-drought communities varies by the presence of coffee in the community and the year. In many cases the enrollment rates are within 1 or 2 per cent, but at most they are 10 percentage points apart. Table 4 shows that once coffee is controlled for, the large baseline differences in school enrollment disappear between drought and no drought communities.

Table 3. Ratio of average household consumption levels, conditional drought and coffee community

	Control		RPS	
	No coffee	Coffee	No coffee	Coffee
Ratio of 2002 to 2000 consumption				
Drought 2002				
No	1.06	0.74	1.56	0.97
Yes	1.01	0.66	1.07	0.92
Ratio of 2001 to 2000 consumption				
No Drought 2001	1.07	0.82	1.10	1.08
Drought 2001	0.80	0.84	1.04	No data

Table 4. School enrollment by 2001 drought and coffee

	2000		2001		2002	
Coffee community						
Drought 2001	No	Yes	No	Yes	No	Yes
Treatment (%)	64	74	95	96	94	96
Control (%)	64	68	68	79	78	81
DID			28	11	17	9
No coffee						
Drought 2001	No	Yes	No	Yes	No	Yes
Treatment (%)	89	88	98	98	98	97
Control (%)	83	86	84	90	87	89
DID			7	7	4	5
Coffee						
Drought 2002	No	Yes	No	Yes	No	Yes
Treatment (%)	66	61	94	96	94	94
Control (%)	66	62	71	67	79	77
			24	30	16	18
No coffee						
Drought 2002	No	Yes	No	Yes	No	Yes
Treatment (%)	86	91	98	98	96	98
Control (%)	85	84	87	89	88	90
			10	2	7	1

Note: ** DID is ($\text{Treatment}_{2001/2002}$ – $\text{Control}_{2001/2002}$) – ($\text{Treatment}_{2000}$ – Control_{2000}).

The bottom row of Table 4 uses a simple difference-in-difference measure to show the varying impacts depending on the presence of a drought shock and if the community farmed coffee. In non-coffee farming communities, RPS appears to make a smaller impact in the case of drought. The results are mixed for coffee farming communities in terms of the impact of a drought. Finally, the results also show that the impact of RPS on increasing school enrollment was substantially larger in the coffee farming communities. This result is consistent with changing the price of non-attendance and lowering the opportunity costs of sending children to school.

V. Difference-in-Difference Estimation

Many recent evaluations of CCTs have used a difference-in-difference (DID) framework to estimate the impact on school enrollment or other outcomes (Skoufias and Parker, 2001; Hoddinott and Skoufias, 2004; Schultz, 2004). We begin this section by presenting the basic DID equation used to estimate the impact of RPS and then augment it with controls for wealth, drought, and coffee shocks. Specifically, we implement the DID analysis with separate regressions by land holding categories and by coffee and non-coffee communities to test for heterogeneity in RPS and other effects. We also present pooled regression results to make statistical comparisons across these groups within the same regression structure.

Equation 4 is the basic specification, with E_{ict} denoting the dependent variable, school enrollment, with subscripts i for child, c for community, and t for time. The simplest specification of independent variables for the DID estimation includes indicator variables for being in the intervention group (RPS_c) and time (A_1 and A_2), representing years 2001 and 2002, respectively. The error term is $v_{ict,}$ which captures the unobserved idiosyncratic time-varying error.[18] The unknown parameters are α and δ, with the terms δ_1 and δ_2 measuring the impact on school enrollment of being in an intervention community in the first and second year of programme implementation, respectively.[19]

$$E_{ict} = \alpha_{0+}\alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 RPS_c + \delta_1 A_1 RPS_c + \delta_2 A_2 RPS_c + v_{ict} \quad (4)$$

E_{ict} = child's enrollment in school, RPS_c = (1) if household is eligible for RPS eligible community, $A_{1,}$ = (1) if year equals 2001, $A_{2,}$ = (1) if year equals 2002, v_{ict} = time variant error term.

The next step is to expand the basic estimation equation to include droughts, where drought (D_{ct}) equals 1 if the community leader in community c reported a very severe drought in year t. This specification is shown below in equation 5. This modified specification also includes interaction terms to measure the negative impact of a drought and its interaction with RPS. However, some three-way interactions are dropped due to data limitations. As previously discussed, both wealth and production technology can influence the effect of a shock on school enrollment. To control for these impacts we provide separate estimations of equation 5 by both land wealth cohorts and coffee and non-coffee communities.

One limitation of the data is that drought information was not recorded in the baseline year so D = 0 for all of the baseline observations. As a result, the drought variable for 2001 or 2002 may instead indicate a propensity of the region to experience droughts and may be an inherent rather than time variant characteristic. However, from the descriptive statistics, we find that there is no statistical difference in baseline school enrollment between communities that experienced a drought in 2001 when households are separated into coffee farming and non-coffee farming communities. A second data issue is that in 2001 there were no treatment communities that experienced both a drought and were coffee communities. Therefore, we do not interact the time variable and the drought measure. In place of separate measures for drought impacts on RPS effects, we combine the two in a single term, D_t*RPS*T. Where D_tRPS*T = 1 if a community experienced a drought and was a treatment community, since droughts are only reported in 2001 and 2002, T*D_t*RPS = 1, the two treatment years. The descriptive statistics presented in Table 4 suggest that the impact of the drought on school enrollment was similar in 2001 and 2002, although the potential for a time variant bias exists.

As shown in equation 5, the estimated impact of a drought on schooling is represented by α_4, while the impact of a drought on RPS' effect on schooling is δ_3. The specification shown below in equation (5) is presented for the three landholding categories and a comparison pooled model. The models are estimated using OLS with errors clustered at the community level to control for correlations in the error term at the community level.

$$E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 RPS_c + \alpha_4 D_{ct} + \delta_1 A_1 RPS_c + \delta_2 A_2 RPS_c + \delta_3 D_t{}^* RPS^* T + v_{ict} \quad (5)$$

where: E_{ict} = child's enrollment in school, RPS_c = (1) if household is eligible for RPS and the year of the observation = c, D_{ct} = (1) If household was lived in a community that experienced a drought in year t, T = (1) if it is a treatment year (2001 or 2002).

VI. Results

The DID regression results for coffee and non-coffee communities are presented in Tables 5 and 6 with separate estimations for each landholding category. Table 7 compares impacts by gender. In coffee communities, RPS has a differential impact across the three landholding sizes, and *ex-ante* differences between the landholding groups suggest a positive monotonic relationship between wealth and school enrollment. In non-coffee farming communities, RPS impacts are not statistically

Table 5. The impact of drought and RPS on school enrollment for coffee communities

	Less than 1 Manzana		1–3 Manzanas		More than 3 Manzanas	
Variable	Coef	(SE)	Coef	(SE)	Coef	(SE)
2001	−0.025	0.113	**0.094**	**0.041**	0.028	0.060
2002	0.083	0.099	**0.163**	**0.045**	**0.156**	**0.061**
RPS	−0.033	0.096	−0.028	0.077	0.003	0.068
Drought	0.171	0.145	−0.044	0.092	−0.016	0.069
2001* RPS	**0.434**	**0.128**	**0.235**	**0.071**	**0.230**	**0.076**
2002* RPS	**0.348**	**0.112**	***0.139***	***0.075***	0.087	0.073
Drought* RPS* T	−0.186	0.151	0.032	0.095	0.018	0.069
Size	−0.009	0.014	**0.012**	**0.006**	−0.008	0.009
Female Head School Years	***0.028***	***0.015***	**0.042**	**0.009**	**0.024**	**0.004**
Constant	**0.590**	**0.137**	**0.466**	**0.057**	**0.752**	**0.060**
	N = 418		N = 804		N = 860	
	R-Square = .19		R-Square = .15		R-Square = .13	

Table 6. The impact of drought and RPS on school enrollment for non-coffee communities

	Less than 1 Manzana		1–3 Manzanas		More than 3 Manzanas	
Variable	Coef	(SE)	Coef	(SE)	Coef	(SE)
2001	−0.099	0.073	0.027	0.060	0.046	0.041
2002	−0.070	0.061	0.033	0.039	0.063	0.046
RPS	0.017	0.075	0.060	0.043	0.008	0.046
Drought	**0.158**	**0.072**	−0.014	0.065	0.040	0.030
2001* RPS	0.080	0.087	0.076	0.070	0.060	0.052
2002 *RPS	0.101	0.082	0.056	0.051	0.010	0.053
Drought* RPS* T	−0.105	0.078	0.013	0.067	−0.033	0.032
Size	−0.001	0.013	0.002	0.003	−0.005	0.005
Female Head School Years	**0.017**	**0.010**	**0.012**	**0.006**	**0.010**	**0.004**
Constant	**0.866**	**0.101**	**0.790**	**0.051**	**0.889**	**0.052**
	N = 341		N = 1561		N = 719	
	R-Square = .06		R-Square = .05		R-Square = .05	

Table 7. By child gender the impact of RPS on school enrollment by land holdings with exogenous shocks pooled sample coffee and non-coffee communities

	Boys			Girls		
	Less than 1 Manzana	1–3 Manzanas	3+ Manzanas	Less than 1 Manzana	1–3 Manzanas	3+ Manzanas
2001	−0.073 (0.09)	0.062 (0.06)	0.055 (0.06)	−0.045 (0.08)	0.055 (0.04)	0.028 (0.04)
2002	0.011 (0.10)	**0.094 (0.039)**	**0.124 (0.042)****	0.006 (0.06)	0.053 (0.04)	**0.099 (0.047)**
RPS	0.145 (0.08)	0.059 (0.04)	0.01 (0.05)	−0.177 (0.09)	−0.004 (0.05)	−0.005 (0.05)
Drought	**0.236 (0.078)**	−0.094 (0.09)	0.075 (0.04)	0.08 (0.09)	0.007 (0.04)	−0.03 (0.04)
Coffee	**−0.249 (0.078)**	**−0.163 (0.041)**	**−0.125 (0.043)**	**−0.279 (0.063)**	**−0.206 (0.054)**	**−0.143 (0.048)**
2001*RPS	0.091 (0.11)	0.097 (0.07)	0.063 (0.07)	0.189 (0.11)	0.088 (0.06)	0.071 (0.05)
2002*RPS	0.054 (0.11)	0.045 (0.05)	−0.04 (0.06)	0.127 (0.09)	0.065 (0.07)	−0.009 (0.07)
Coffee*RPS*T	**0.281 (0.080)**	0.049 (0.05)	**0.165 (0.047)**	**0.296 (0.065)**	**0.181 (0.063)**	**0.126 (0.050)**
Drought*RPS*T	**−0.204 (0.081)**	0.06 (0.09)	−0.065 (0.05)	−0.081 (0.09)	0.001 (0.04)	0.027 (0.04)
Household size	−0.004 (0.01)	0.003 (0.01)	−0.003 (0.01)	−0.009 (0.01)	0.008 (0.01)	−0.011 (0.01)
Mother's years school	0.02 (0.01)	**0.03 (0.006)**	**0.019 (0.004)**	0.019 (0.01)	**0.016 (0.007)**	**0.016 (0.004)**
Constant	**0.756 (0.103)**	**0.72 (0.057)**	**0.822 (0.054)**	**0.983 (0.129)**	**0.768 (0.052)**	**0.97 (0.072)**
Observations	368	1231	812	391	1134	767
R-squared	0.25	0.11	0.11	0.2	0.12	0.11

Note: Standard errors in parentheses, bold indicates significant at 5 per cent level.

significant. Additionally, we find a non-linear *ex-ante* relationship between landholdings and school enrollment for non-coffee communities. Across the whole sample, the impacts of RPS tend to be largest for the poorest households, those with less than one manzana of land and for households in coffee farming communities. In non-coffee communities, droughts actually positively increase school enrollment for those with less than one manzana of landholdings and have no significant effects on school enrollment outcomes for households in the other land size categories.[20]

Coffee Communities

The basic relationship between land wealth and RPS impacts is evident for coffee community households in the econometric results in Table 5. Households with the smallest land holdings experienced the largest impacts from RPS. Those with less than one manzana had estimated increases in school enrollment of 40 and 32 percentage points in the two programme years, as compared to 23 and 14 percentage point effects for the one to three manzana group, and 23 and 9 percentage point effects for the more than three manzana group, with the second year for this largest land holding group being not statistically different from zero. These differences are supported by the pooled sample that shows those with less than one manzana of land had increases from RPS almost 15 percentage points higher than the other two groups (RPS*Land < 1 Manzana), while the difference between the one to three manzana group and the three or more manzana group was not statistically significant. An important non-result is that the coefficient estimate on drought is insignificant in explaining school enrollment in coffee communities.

The coefficient estimates on the 2001 and 2002 variables in Table 5 help to distinguish the effects of coffee price shocks across the different wealth groups. Although the changes could also represent other time variant impacts in the communities, similar impacts are not seen in the non-coffee community estimations. Recall that 2001 was the nadir of coffee prices during the study period, and note for the one to three manzana group in the control communities that enrollment increased 10 and 16 percentage points in 2001 and 2002 relative to the baseline year of 2000. For this land wealth group in particular, it appears that lower coffee prices had a rather strong effect in terms of increasing school enrollment. The magnitude of these effects is smaller in 2001 and similar in 2002 to the effect of RPS (24 per cent and 14 per cent). For the other two wealth groups, coffee price shocks do not appear to have had major effects on school enrollment outcomes, with only the coefficient estimate on 2002 for the over three manzana group being significant with a 16 per cent increase. This difference between wealthier and poorer households in coffee communities without RPS suggests that the lower marginal productivity of child labour associated with coffee price declines had a differential impact on child schooling decisions. Richer households significantly increased school enrollment relative to poorer households in the face of coffee prices decline.

Non-coffee Communities

For households in the non-coffee farming communities (see Table 6), RPS had no statistically significant effect on school enrollment for any of the three land holding groups. In terms of drought, we find that droughts positively impacted schooling in non-coffee communities for those with land holdings less than one manzana, but that the impact was muted in communities with RPS because the interaction of RPS and drought (*RPS*Drought*) is negative. An F-test for the non-coffee and less than one manzana of land group on the sum of the coefficients on Drought and RPS*Drought, is significantly different from zero and positive ($F = 7.4$). In other words, households in both control and treatment communities that experienced a drought increased school enrollment; however, the increase was smaller in treatment communities. In summary, these results suggest that droughts may decrease child labour market options and positively impact schooling enrollment among the poorest households. Also noteworthy is the contrast of large and significant positive impacts of RPS in coffee communities and the lack of significant impacts of RPS in the non-coffee communities.

Overall, the results presented in Tables 5–7 demonstrate that RPS was most effective at raising school enrollment in coffee farming communities. In terms of RPS impacts, the largest impacts were on those with the least amount of land; however, there were still substantial impacts for those with more than three manazanas of land, although only in coffee communities. Additionally, we found that droughts had a *positive* impact on school enrollment for households in non-coffee communities with less than one manzana of land and no significant impacts on other groups. All of these results underscore the key role that the opportunity cost of child labour plays in the schooling decision and the need to be clear on what kinds of shocks might be valuable to ameliorate to help maintain school participation.

Impacts by Child Gender

The final estimation compares boys and girls in terms of the impacts of droughts, coffee, and RPS in a model that pools coffee and non-coffee communities. Table 7 presents the results of six estimations broken down by gender and land holdings. As discussed in the literature review, boys are more likely to work off farm and therefore should see stronger impacts from opportunity cost concerns than girls. Indeed, droughts had the largest impacts on boys in households with less than one manzana of land, increasing school enrollment an estimated 23 percentage points. As shown above, an increase in school enrollment from the drought diminishes RPS impacts.

By contrast, the negative impacts on school enrollment of being in a coffee community do not differ substantially between boys and girls. RPS has equal impacts on boys and girls in coffee communities across the three land group strata except for households with one to three manzanas of land. In that case, boys experience no impacts from the RPS, while girls see an increase in enrollment of 18 percentage points. Again this may be driven by the higher opportunity cost of forgone child labour of boys, which may decrease the benefits to the family of accepting the conditional cash transfer. Another possibility is that improvements in nutrition and health of adult females, including mothers, through RPS payments, may decrease the need to use girls for domestic household chores. However, without additional information on individual time use these possibilities cannot be further explored.

VII. Conclusion

One of the goals set forth by policy makers for RPS and other conditional cash transfer programmes is to provide a safety net to help households maintain school enrollment during negative economic shocks. Additionally, the ability to maintain schooling during shocks is often seen as closely tied to household wealth. This paper provides a simple household model of the decision between child labour and schooling that highlights how wealth and economic shocks have two opposing forces on school enrollment. On the one hand, less wealth or negative economic shocks raise the potential marginal utility of income from child labour. On the other hand, negative shocks can also make child labour less productive. Due to these opposing forces the direction of the relationship between economic shocks and school enrollment outcomes cannot be determined *a priori*. This conclusion is consistent with previous research that shows negative shocks can have both positive and negative impacts on school enrollment in developing countries.

Our empirical analysis of Nicaragua's RPS conditional cash transfer programme explores the issue using data from communities that differ substantially in terms of labour opportunities for children for both structural and idiosyncratic reasons and differ depending on whether households were eligible for a cash transfer conditional on their children enrolling in school. Coffee cultivation provides better labour market opportunities for about half of the communities in the sample, severe and very severe droughts (as well as coffee price swings) provide stochastic shocks that reshape labour market opportunities in both coffee and non-coffee cultivating communities, and RPS provides a cash transfer that substantially alters the child labour-school enrollment calculus for households in the treatment villages. Examining how these differences in child labour market opportunities affect school enrollment outcomes across both control and treatment communities is very revealing.

We highlight two key results. First, RPS created the largest positive impacts on school enrollment in coffee farming communities and for those households with little or no land holdings, but had relatively little impact in the non-coffee communities. Second, negative economic shocks such as droughts or price shocks can actually increase school enrollment presumably by lowering the opportunity costs of school. This result is further supported by a comparison of school enrollment of boys and girls, which shows that the opportunity costs in terms of droughts are more likely to increase the school enrollment of boys than of girls. Thus, a nuanced understanding of how different types and severity of shocks shape child labour decisions and schooling outcomes is essential to making effective use of limited development funds aimed at boosting school attendance. This issue seems particularly important in countries such as Nicaragua where conditional cash transfer programmes are being piloted but have not yet been introduced in many parts of the country. It seems likely that the highest social return will come from more programmatic attention to the extensive margin of providing transfers to poor households in under-served regions, especially where labour market opportunities for children (such as coffee) are likely to draw them from school.

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Notes

1. In response to suggestions that negative shocks may limit CCT effects (de Janvry et al., 2006a), a follow-up programme to RPS is currently being discussed to link inversely the size of the cash transfer programme to rainfall data in recipient communities.
2. This distinction is important, because as Maluccio (2005) documents, a major negative coffee price shock began in 2000 with a 50 per cent decline. It was followed in 2001 with a further significant decline. Prices recovered in 2002 to their 2000 level, but were still only one third of their peak in 1997. During this period, coffee production fell by one third between 2000 and 2001, and then increased modestly in 2002.
3. With less than 20 per cent of the households having access to credit in either the formal or informal market, the assumption of no borrowing between periods is true for most households.
4. It is assumed that wealth is fixed and that wealth increases the marginal productivity of child labour, (that is $F_{ew} < 0$).
5. For around 85 per cent of the households credit is not available in the formal or informal markets. We define credit constrained households as those who did not have credit or did not believe they could not obtain it. Furthermore, nearly all income is consumed and there is little saving in the RPS households.
6. This is consistent with the models of Glewwe and Jacoby (2004) and Jacoby and Skoufias (1997).
7. In this section the statement of the maximisation 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.
8. 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 $U_1^1\ \theta_1 F_e > U_e^1 + \delta\Phi\ U_e^2$ at $e = 0$, and a corner solution for zero child labor when $U_1^1\ \theta_1 F_e < U_e^1 + \delta\Phi\ U_e^2$ at $e = 1$.
9. A similar result, not presented here, can be developed in terms of the relationship between wealth and schooling. Note that in this model household wealth and current economic conditions function in the same manner to shape marginal productivity of labour. We include wealth in order to provide motivation for separate effects of labor market opportunities across wealth groups. in terms of their impacts on the education decision.
10. See Gitter (2006) for formal treatment of the decision to accept conditional cash transfers and the impact of negative economic shocks on the decision.
11. Households with a motor vehicle or more than 20 manzanas of land were not eligible for benefits. However, data was collected only on eligible households in both treatment and control communities.
12. (C$) is September 2000, Nicaraguan córdobas, $1 US is about C$12.85.
13. Currently, there is no publicly available data on which participants had their funding revoked due to not following the guidelines of the programme. These data may become available at a later date, in which case this information could be incorporated into further analysis.
14. Maluccio and Flores (2004) conclude that the attrition rate is not a major concern for estimating programme impacts. They find that well educated and richer households are slightly more likely to have not completed all three rounds; however, given that RPS increased impacts on poorer households this should make estimates more conservative. In our sample, we find similar results when examining attrition by land holding categories.
15. The sample includes any eligible child in the treatment group and potentially eligible child in the control group that was aged 7–13 at any point during the 2000–2002 period.

16. A manzana is a unit of land equivalent to about 1.7 acres or 7000 square meters. We also tried breaking down the one to three manzana land group, however it did not substantially change the results, nor do those households have statistically significant differences in consumption. Additionally, the reporting of household land holding tends to be grouped around integer units of manzanas, which makes the use of continuous variables problematic.
17. Maluccio (2005) finds that only 10 per cent of the households in coffee communities reported working in the coffee sector and this figure dips to 5 per cent in the treatment years. However, it is worth noting that adult labour force participation is only measured over the previous week and the low numbers may reflect that the survey was taken before the harvest.
18. When household fixed effects are used in the non-pooled models the results are not substantially different.
19. The DID estimate of programme impacts in year 1 $(I_1-C_1) - (I_0-C_0)$ based on Equation (4) can be written as $[(E_{ict} \mid RPS=1) - (E_{ict} \mid RPS=0) \mid A_1=1] - [(E_{ict} \mid RPS=1) - (E_{ict} \mid RPS=0) \mid A_1=0]$. If we assume that the error terms are independent of all other variables, then algebraic manipulation shows that the DID estimate is equal to δ_1.
20. Appendix A (available online) includes a pooled model of coffee and non-coffee farming communities which shows that household differences between the two types of communities are largest among those with less than one manzana of land, but coffee communities had lower school enrollment for all strata of landholding households.

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