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Are Conditions on Cash Transfers Necessary to Improve Rural Education Outcomes? Evidence From Nicaragua

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Abstract: Across Latin America, conditional cash transfer programs (CCTs), in which governments pay poor families conditional on their children attending school, have successfully increased enrollment and attendance rates. No empirical evidence supports the need for costly conditionality, however, and I compare the effect of unconditional remittances to the effect of CCTs to determine which more strongly influences educational investment. I test the outcomes of school enrollment and attendance and find that unconditional transfers more strongly impact enrollment, while conditional transfers more strongly increase attendance.

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I. Introduction

Conditional cash transfer programs (CCTs) are government safety net programs in which poor families receive monthly cash transfers conditional on child school attendance.[1] By tying the cash to education and health outcomes, the government theoretically achieves dual goals of relieving short-term poverty and investing in long-term human capital. Handa and Davis's (2006) assertion that CCTs "have come to dominate the social protection sector in Latin America" is beyond debate. Mexico was the first of at least eight countries to use a CCT to pay poor, rural families a stipend so long as they ensured that their kids enrolled in and attended school (Schultz 2006). Families were required to take young children for health checkups and vaccinations, as well as ensure enrollment in school and attendance of a minimum of 85% of available school days. The monthly transfers, often worth 50% or more of a family's budget, reduced short-term poverty while serving as an investment in children's human capital (Schultz 2006). By 2004, Latin American governments were allocating over $5 billion annually to these hybrid social safety net-human capital development programs (Caldés, Coady & Maluccio 2006).

Despite this popularity, a recent cohort of studies highlights various forms of CCT inefficiency as investments in human capital. Caldés, Coady and Maluccio (2006) show that program administrative and condition monitoring costs represented 40% of the Nicaraguan CCT's budget, suggesting large cost-savings if conditions are not necessary. In their study of CCTs, Sadoulet and de Janvry (2006) show that because different families have different thresholds below which they cannot afford school, differentiating

[1] Most CCTs, including the one in Nicaragua, also transfer cash conditional on doctor visits for children.

transfer levels based on this threshold could save CCTs 11% of their budgets by ensuring that they are not paying families whose children would attend school regardless.

Sadoulet and de Janvry's proposed targeting adjustments would result in a resource allocation in which each family receives exactly what it needs to enroll or invest in attendance. They conclude that this type of efficiency is especially important in poor countries like Nicaragua that face governmental budget constraints and extensive poverty. Interestingly, Handa and Davis (2006) conclude that the need for conditions on Latin American transfers has not been established empirically.

The objective of this paper is to perform an empirical test of Nicaragua's CCT panel data to establish the effect of the *Red de Proteccion Social* (RPS) on education relative to the effect of unconditional remittance income. I respond to Sadulet and de Janvry's (2006) distinction between the two goals of CCTs: to internalize positive externalities associated with long-run continued attendance in school *and* reduction in short-term poverty such that families can enroll their children in school. Hence, I test the effects of unconditional and conditional transfers on both attendance and enrollment.

This paper is organized as follows. Section II reviews the relevant literature about cash transfers, to situate this analysis in an empirical niche. Section III develops a theoretical model to guide empirical analysis. Section IV summarizes and discusses the Nicaraguan CCT data. Section V presents the empirical technique and discusses its associated estimation issues. Section VI compares panel probit and tobit estimates of the relative effects of conditional and unconditional transfers on education outcomes. Section VII concludes.

II. Previous Literature

Gammeltoft (2002) provides evidence that remittances are the least costly and most prolific form of unconditional transfers in Latin America. Authors concur that both types of transfer improve educational outcomes, with conditional cash transfers having raised school enrollment and attendance rates unambiguously in CCT program countries (see Schultz (2004); Hoddinott & Skoufias (2004); Skoufias & Parker (2001); Maluccio & Flores (2006); Sridhar & Duffield (2006); Schady & Araujo (2008); Glewwe & Kassouf (2009)). Remittances also improve education and they do so without the presence of a condition (see Edwards & Ureta, 2003; Calero et al (2004); Malone (2006)).

This dichotomy between the effectiveness of conditions relative to unconditional transfers is the center of a debate about improving rural education outcomes. Studies suggest that poor families in the developing world choose less-than-optimal amounts of education for children, but they disagree about why. Inchauste (2000) shows that cash transfers do not significantly improve schooling in Bolivia for women or indigenous minorities, suggesting that constraints like geography, parental education and culture may dominate pure liquidity constraints.

An opposing body of work shows that poor rural families respond to changes in income with respect to education. Levison and Moe (1998) and Levison, Moe and Knaul (2001) show this response with respect to non-market "shadow" wages of children, while Rosenweig (1990), Brown and Park (2001), Lloyd et al. (2006) and Glewwe and Jacoby (2004) show responsiveness to educational investment with respect to liquid income.

In its recent study on CCTs, the World Bank (2009) outlines a different theoretical debate about the necessity of conditions on cash transfers, centering on under-investment in schooling. Educational investment may be below the *private* optimum if parents and children underestimate the future returns to schooling. There may also be a less-than-optimal *social* level of education if schooling can lead to positive social externalities like civic engagement or lower crime. Internalizing these positive externalities with subsidies and transfers may also be an important political tool to attract popular support for publicly-funded social safety nets. In contrast, however, public cash may have a steep opportunity cost if it is not invested in alternative public projects. The World Bank ultimately concludes that market-driven economic growth is likely the best poverty-reduction plan, but that public projects like CCTs may be a necessary supplement. It does not provide empirical support for this claim.

Earlier studies provide theoretical and empirical evidence against CCTs and unconditional transfers as tools for economic development. Cox and Jimenez (1990) find that (unconditional) public transfers "crowd out" private cash. In the presence of such transfers, the marginal value of each remittance diminishes, reducing migrants' incentives to remit at all. This "crowding out" effect may also eliminate improved credit markets that result from the presence of remitted cash (Taylor 1999). Additionally, Martinelli and Parker (2003) show theoretically that conditionally transferred cash can alter incentive structures and result in an overinvestment in human capital. Parents suffer in the short run as they allocate an inefficient level of resources to education, and children ultimately pay

the cost as they receive lower bequests later in life. In these cases, allocating investment money privately may result in a more efficient outcome.[2]

Additional studies provide evidence against the effectiveness of remittances in improving educational outcomes. Chami et al. (2003) show with a panel from over 100 countries through 29 years that remittances are often countercyclical and do not go toward economic development. Instead of sending money home that can be invested in education, remitters only relieve temporary budget constraints during economic downturns. In these cases, the targeted and intentional nature of CCTs may be needed to achieve long-run gains in human capital.

The preceding discussion emphasizes the debate surrounding the value of conditions on cash transfers as an educational investment. The following section develops a theoretical model with which to apply this question to the Nicaraguan data.

III. Theoretical Model

Most CCT analysis papers include a set of general assumptions in place of a formal model. A simple family-utility maximization framework effectively frames the typical Nicaraguan family's tradeoff between education and other consumption and provides the basis for empirical analysis. This model assumes that the child's only opportunity cost of education is the value of her production in the home and that children do earn such a shadow wage when not in school.[3] The typical CCT's condition requires attendance of at least 85% of available days and I assume that families will not choose to allocate more than the necessary number of days, since they face an opportunity cost of

[2] It is not unreasonable to assume, however, that cash bequests are minimal or non-existent in rural Nicaragua. Parents often bequest land to children but land likely doesn't factor into schooling and consumption decisions.

[3] Levison and Moe (1998) demonstrate the empirical validity of this assumption.

consumption. Decisions in this model flow from the assumption that families accurately value the returns to a given level educational investment and that the future value of education is worth a family's present level of investment. This assumption likely does not hold in reality and relaxing it will help explain this paper's empirical results.

To see the relationship between the type of transfer – conditional or unconditional – and education outcomes, consider two possible family resource allocation scenarios in rural Nicaragua. The first suggests that an unconditional cash transfer regime is efficient and the second suggests that a conditional regime is efficient. The first scenario, depicted in Figure 1, illustrates a family that, along the first, pre-transfer, budget constraint, initially allocates more heavily toward schooling than other consumption. This family represents a typical unconditional-regime family in that it does not require conditions on cash transfers to improve school attendance. The presence of an unconditional transfer (remittance) shifts the budget constraint outward to the post-transfer constraint.[4] The difference in attendance levels between the unconditional allocation (utility curve 3) and the level required by the condition (utility curve 2) is small. In this case the condition itself does not induce a high increase in school and its cost may therefore be more efficiently spent elsewhere.

Figure 2 illustrates the analogous case for a typical family that does not originally allocate much school due to the non-liquidity constraints discussed in section I and therefore requires conditions on transfers. These conditional transfers shift the budget constraint outward to the post-transfer constraint. The dotted portion of the new budget constraint is unattainable under the conditional regime that requires 85% attendance. The

[4] This model assumes that remittances are equal in magnitude to the CCTs' cash transfer; the RPS data justify this assumption, as described in section III.

difference in attendance between the unconditional and conditional equilibria in this situation is large, suggesting that the conditions may be valuable in increasing school attendance. There is also a significant potential drop in family utility (from utility curve 3 to utility curve 2) if the family is required to allocate more education than they otherwise would. Taken together, these scenarios indicate that if the difference between the conditional and unconditional outcomes is small, then investing in the program may not be worthwhile, whereas if the difference is great, then investment in the program likely is valuable. Analysis of a family's enrollment decision would follow analogous reasoning.

The preceding discussion applies only to families who face some type of constraint with respect to education. There are also families that do not face educational constraints. The matrix in Table 1 includes these types of families. The top of the matrix divides families among those that face constraints – the type in this analysis – and those that do not. The left side of the matrix divides families by those who inherently value education and those who do not. Hence, families in the upper left quadrant that face constraints but do value education are the "typical unconditional regime" families that only require an expansion of the budget constraint. Those in the lower left that face constraints but do not inherently value education are the "typical conditional regime" families discussed above. Families in the upper right who do not face constraints and value education require no intervention. Finally, families in the lower right quadrant that do not face constraints and also do not value education simply justify laws requiring school attendance.

The prolificacy and success of CCTs at improving education for those families that do face educational constraints in Latin America suggest a null hypothesis that

families may face only liquidity constraints for education; the expected hypothesis is that conditions will encourage families to respond to non-liquidity educational constraints that unconditional transfers would not. The following guiding equations incorporate both conditional transfers from the RPS program and unconditional remittances, as well as other education-determinative controls for empirical estimation:

(1) $\text{Attendance}_{it} = f(\text{RPS}, \text{remittances}_{it}, X_{it})$

(2) $\text{Enrollment}_{it} = f(\text{RPS}, \text{remittances}_{it}, X_{it})$

The two regressions test conditional and unconditional cash transfers on the two primary education outcome indicators, attendance and enrollment. Comparison of the magnitude of conditional and unconditional transfers will suggest which more strongly impacts attendance and enrollment. Enrollment is a binary variable and both enrollment and attendance are indexed over time *t* and across students *i*, while X is a vector of household- and person-specific control variables. Variables such as age, whether the child works, grade and distance to school should have negative coefficients, since increases in each should decrease the probability of enrollment and attendance in school. The presence of the program or of remittances, as well as income, should increase school attendance and enrollment. Theory does not unambiguously predict signs for child gender, occupation and industry of the family. [5] The binary indicator *farmer* accounts for differences arising for families for which agriculture is the primary income source.

IV. Data and Summary Statistics

[5] Maluccio and Flores (2004) write the seminal analysis of RPS and do not indicate, specifically, which controls they use. In his analysis of Progresa in Mexico, Schultz (2004) includes child age, gender, level of parent education and distance to school. I include these, as well as other theoretically intuitive controls.

Nicaragua's *RPS* was allocated with randomized control and treatment groups within the entire rural poor population to isolate the effects of intervention from any "natural" improvement. The remittance recipients were not randomly assigned, presenting a potential endogeneity bias since decisions about education and remittances are likely made simultaneously. The dataset contains the necessary variables to measure school achievement and remittances, as well as most theory-specified control variables.

RPS is the result of collaboration between the Nicaraguan government and the International Food Policy Research Institute (IFPRI), with World Bank funding. The first sample was collected in 2000 before initiation of the program and post-treatment data were collected in 2001 and 2002. The initial program covered 9750 people (1764 families) of whom 4920 were in the treatment group in 2000.[6] The school transfer had an annual value of US $112 and targeted children ages 7-13 who had not yet completed 4th grade of primary school.[7] All families in the treatment group also received a conditional food subsidy transfer with an annual value of $224, regardless of their receipt of the education transfer. There is therefore no way in these data to distinguish the education transfer from the food transfer, making the effective conditional transfer $336 annually.[8] For the typical rural Nicaraguan family, this represents nearly 20% of total expenditures. The school-age population is nearly perfectly divided between the treatment and control groups. All data are publically available on IFPRI's website.[9]

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[6] Program attrition saw this number fall to 4774 by 2002.
[7] I convert from Córdobas to dollars with September 2000 average Córdoba/dollar exchange rate of 12.80.
[8] Maluccio and Flores (2006) identify this problem and make the same assumption about the value of the conditional transfer.
[9] The International Food Policy Research Institute (IFPRI) makes available the Nicaragua data and other publications related RPS on its website. http://www.ifpri.org/

Tables 2 through 4 present summary statistics for all estimated variables in all years. Currency data are reported in year 2000 U.S. dollars relying on the 2000 Córdoba/dollar exchange rate of 12.83. In 2000, 59.7% of eligible students were enrolled in school; by 2002 71.3% were enrolled. Attendance is measured as the number of school days missed in the month prior to the survey, and for each year includes around 2000 individual children. This variable has strong right skew (see Figure 3); in 2000 63% had perfect attendance and by 2002 81.2% missed no school. I therefore take the natural log of the variable and rely on a tobit estimation technique to address this clustering (Figure 4). On average, in the baseline, the number of days missed is 2.92, and decreases as expected to 1.31 and 1.15 in 2001 and 2002, respectively. For each year there are a possible 22 days per month of school attendance.

The dataset records which families have members who have migrated, which migrants send remittances, and how much they remit. This variable is heavily right skewed, which potentially affects its comparison to the value of RPS's conditional transfers. The average annual remittance of the 807 people who lived in a remittance-receiving family is $309.27, close to the total value of the conditional transfer.[10] Reflecting its skewness, the median annual remittance is $140.62 and the average annual remittance after omitting 65 statistical outliers is $196.87.[11] On average, however, the magnitude of remittances is comparable to the conditional transfer.

Around 7000 people recorded that they worked in the week prior to each survey. The binary indicator *work* used in all regressions incorporates both paid hours and work defined as household production. Distance from school also explains attendance rates;

[10] The average yearly remittance is $8.70 for the entire program population including those who received no remittances.
[11] I consider any observation greater than 1.5(75th percentile-25th percentile) + 75th percentile an outlier.

this variable contains 2000 observations. The average student lives approximately one kilometer from school. The average age of the RPS population increases from 21.14 years to only 22.14 over the panel's three years, reflecting the fact that people are born into and move and die out of the sample population. There are 50 percent women in the sample.

The dataset does not measure family income, an important determinant of resource allocation. Hence, I use total yearly expenditures as a proxy for family income, under the assumption that private savings is minimal. In 2000, average yearly expenditures were $1735.70 and did not change substantially over the life of the program. The "farmer" dummy variable indicates that nearly 89% of all families worked primarily in agriculture. Finally, I control for parent education level by including the maximum level of education achieved by either parent in the family. This variable ranges from a minimum of one year to a maximum of six, with a mean of just over four years.

Data Structure

Due to incompleteness of the dataset, several RPS variables required assumptions and restructuring in order to assemble variables for estimation. The remittance variable records the total amount of remittances received by each family from all relatives living abroad. For each migrant who sent money, the original dataset records the remitted amount and the period over which that amount was sent: semi-monthly, monthly, trimester, semi-annually or annually. Since RPS was allocated on a monthly basis, I normalized all remittance values to monthly transfers in order to match frequency between independent variables of interest. I then summed all family remittances for each household, to generate the final household monthly remittance variable. This sum also

includes the monetary amount of all in-kind transfers. Therefore, each member of a given family is recorded as receiving the same amount of remittances. I assume that families with no recorded remittance income received no remittances and therefore replace missing remittance values with zero.

The household parent education variable reflects the highest level of education attained by either of the parents. The binary dependent variable enrollment is a combination of all attendance data. That is, it is one for everyone who is recorded to attend or has attendance records and zero for all school-age children who are recorded under either the enrollment or attendance variables as not currently attending. Because the dataset only records attendance data for the month prior to the survey it is a potentially inaccurate representation of yearly attendance. Enrollment has more observations than either attends or attendance because it aggregates both variables.

V. Empirical Approach

The theoretical model presented above suggests that it is the nature of a family's educational constraint that determines whether conditional or unconditional transfers should have a greater affect on enrollment and attendance in school. The following guiding equation will yield coefficients on remittances and RPS for comparison:

$$(3)\ \text{Educational\_Outcome}_{it} = \alpha_0 + \beta_0\text{remittances}_{it} + \beta_1\text{RPS}_{it} + \beta_2\text{lnexpenditure}_{it} + \beta_3\text{age}_{it} + \beta_4\text{gender}_{it} + \beta_5\text{distance}_{it} + \beta_6\text{work}_{it} + \beta_7\text{parent\_education}_{it} + \varepsilon_{it}$$

In this equation *i* indexes individual children, *t* denotes years of the sample and ε_i is a stochastic error term. Expenditure records the total annual value of a family's

expenditures as a proxy for (non-existent) income data. To estimate enrollment, equation (3) will use a maximum likelihood panel probit estimator, presented with marginal effects coefficients. To estimate attendance, equation (3) will use a panel tobit regression to account for the fact that approximately 80% of all observations show perfect attendance throughout the panel. As robustness checks I estimate the effect of remittances isolated to the RPS control group and of CCTs with families who received no remittances, in order to establish both transfer types' "pure" effects.

Estimation Issues

In the RPS communities, decisions to fund migration and invest remittance income in education are likely made simultaneously, or are both jointly determined by unobserved variation. If a household decides to allocate resources to fund migration in order to receive remittances, it may be with the intention of improving education outcomes. This endogeneity will bias the coefficient estimates and likely over-predict the effect of remittances on education.

To address this bias I estimate an instrumental proxy for remittances using the concentration of families with a migrant in each community. This is the instrumental variable approach in Acosta (2006). This migrant "network" effect should help new people migrate and thus be highly correlated with remittance income. As a relative concentration, however, it should not be inherently correlated with individual family education decisions, thus mitigating the simultaneity. These instrumented observations are potentially biased in themselves in that they contain the same households in the regression, and that the neighborhood network effect may induce more migration for education investment. Hausman endogeneity tests and Wald Chi-squared exogeneity tests

with simple OLS estimators both suggest that remittances are endogenous with education outcomes. Hence, I include the instrumented remittance observations in place of remittances for both dependent variables. Equation five predicts the instrument:

$$(5)\ \ln\text{Remittances}_i = \alpha_0 + \beta_1 \ln\text{expenditures}_{it} + \beta_2 \text{age}_{it} + \beta_3 \text{gender}_{it} + \beta_4 \text{work}_{it} + \beta_5 \text{parent\_education}_{it} + \beta_6 \text{Farmer}_{it} + \beta_7 \text{PercentCommuityMigrant}_{it} + \varepsilon_{it}$$

where Percent Community Migrant is the excluded instrument used in all instrumental regressions. Table 7 presents the estimated coefficients and standard errors of the first stage instrumental regression. In this regression, expenditures, the excluded instrument and the level of parent education are all statistically significant in predicting the natural log of remittances.

The sample of children who attend school is likely biased because it systematically excludes students who do not enroll. The Heckman model adjusts for this type of sample bias and ideally would be used in this analysis. Due to the way the enrollment data are collected, however, there is no accurate method to censor the attendance observations for a Heckman regression. Conclusions from attendance regressions must therefore be interpreted in light of this potential bias.

There is no significant heteroskedasticity or colinearity between independent variables (see Table 5 for pairwise correlation coefficients and Table 6 for Variance Inflation Factor results). The regressions include household fixed effects and have a relatively small horizon (three periods), indicating low risk of serial correlation or non-stationarity of the error terms.

VI. Results

Transfer Effects on Enrollment

Sadulet and de Janvry (2006) believe that CCTs will have a stronger effect on enrollment than attendance; families who have already decided to invest in school are likely to continue doing so. Thus, the more important outcome is encouraging families to invest for the first time. Hence, my primary estimation predicts enrollment as a function of RPS and remittances.

Table 8 presents marginal effects coefficients of panel probit estimations for four equations. Equation I predicts enrollment with RPS and the natural log of remittances to show the effect of each in the presence of the other. Equation II incorporates the instrumented variable in place of remittances; equations III and IV estimate enrollment with RPS and instrumented remittances separately.

Interpretation of the natural log of remittances coefficients is as follows. A one natural log-unit increase in remittances gives the corresponding coefficient's percentage increase in the likelihood of enrollment. Evaluated at the mean level of remittances, an increase of one natural-log unit represents approximately $300, which happens to equal the value of RPS's transfer. This coincidence allows roughly direct comparison between RPS and remittance coefficients.

Equation I predicts that remittances increase the likelihood of enrollment by 3.6% when controlling for RPS, while conditional transfers increase the likelihood of enrollment by 2.5%, controlling for remittances. Both variables' coefficients are significant at the one percent level. Eliminating the potential endogeneity in equation II increases the marginal effect of RPS to 2.6% and increases the effect of remittances

15.1%, both significant at one percent. Equations III and IV show that neither coefficient changes significantly when not controlling for the other. These comparative results indicate that unconditional transfers have a stronger effect on enrollment than conditional transfers. In all regressions, if the child works his or her probability of enrolling is 54% lower, significant at one percent. This strong effect is consistent with the theoretical prediction that the opportunity cost of school is child work and that the two are substitutes. In all regressions, all other coefficients are significant with the expected signs, except for farmer. In rural Nicaragua 89% of all families are farmers, so its lack of variation could explain its statistical insignificance.

Figure 5 plots cumulative density functions of the regression coefficients both with RPS and without RPS against remittances. The distance between the two functions shows the effect of RPS and their slope represents the effect of remittances. The two functions are close together and strongly upward sloping, verifying the result that unconditional transfers have a stronger effect than conditional transfers.

In the RPS population, 67.2% of all eligible students were enrolled in school. It is tempting therefore to use this fact to create a "naïve" rule by which to test the predictions of the probit model. Guessing randomly with no other information that every student is enrolled in school would be successful 67.2% of the time, whereas the econometric model predicts enrollment status correctly 78.7% of the time. The "fit" of the model is 78.7%, and it predicts enrollment 11.5% more accurately than a naïve guess.

Transfer Effects on Attendance

The long-run goal of RPS is to invest in human capital; that is, to ensure that once enrolled, students will stay in school. Table 9 presents a series of panel tobit regression estimates of RPS on attendance. Equation V does not include a remittance coefficient in order to establish the baseline effect of RPS on attendance. RPS is significant at the 1% level and predicts that children in the program miss 2.5 fewer days of school than control-group children. The variables that record whether children work, are primarily in agriculture and the level of parent education are all significant with expected signs. In this model, age, gender and distance to school do not have a statistically significant effect on attendance, though they do have signs in the expected directions with reasonable coefficients. Increases in income predict lower school attendance, but this coefficient also is not statistically different from zero.

Incorporating the natural log of remittances in equation VI as an explanatory variable has no effect on the coefficients from equation V, and remittances are not statistically significant. Equation VII incorporates the instrumented proxy of remittances. This coefficient predicts that remittances raise attendance rates, but is not statistically different than zero. Its presence does not meaningfully affect any other coefficients. Equation VII estimates the effect of remittances not controlling for RPS, but this does not qualitatively change remittances' magnitude or level of significance.

Table 10 presents point estimates, standard errors and 95% confidence intervals of the differences between the RPS and remittance coefficients for each type of regression. There is a statistically significant difference between the IV probit RPS and remittance coefficient and the RPS and non-instrumented remittance coefficients in the tobit model. The other regressions do not estimate statistically different coefficients.

Robustness

To examine the results' sensitivity to specification, I change the sample size to include only observations effected by one type of transfer in order to identify the "pure" effects of each. Table 11 presents these pure effects with subsets of the data that 1) exclude RPS treatment observations, to isolate the effect of remittances; 2) exclude observations that received remittances, to isolate RPS effects; and 3) include only observations that received both conditional cash transfers and unconditional remittances. The primary specification forms from the earlier regressions are used for each data subset, including the instrumented remittance variable where possible and using panel probit and tobit regressions as appropriate. The first pane of Table 9 presents effects on enrollment and the second pane presents effects on attendance.

Isolated to the RPS control group, remittances improve the probability of enrollment by 21.9%, significantly greater than other specifications. Other variables remain significant at the 1% level and largely unchanged in magnitude. The coefficient on work is again high, predicting that working children have a 55% lower chance of enrolling in school. As in other specifications, remittances' effect on attendance is negligible and not statistically differentiable from zero. Eliminating observations that receive remittances raises RPS's effect on the probability of enrollment only slightly, from 2.5% to 2.8%, significant at the 1% level. In the very small sample of 169 observations that received both conditional and unconditional transfers, no variables are statistically different from zero. This likely reflects the small sample size of this subset.

Similarly, there are not enough observations to estimate a regression on attendance using the observations that receive both types of transfers. In the sample

excluding RPS treatment observations, remittances are again not statistically significant in explaining attendance variation. Other coefficients also remain unchanged from the original specification. Excluding observations that receive remittances raises the effect of conditional transfers on attendance to 2.63%. These alternative specifications confirm that the estimated coefficients are robust to changes in sample size and characteristics, as well as functional forms.

VII. Conclusion

As expected, the RPS transfers have a positive, significant effect on enrollment and attendance in school in rural Nicaragua. The data suggest, however, that unconditional remittances actually encourage higher rates of enrollment than conditional transfers. While there is no doubt that investing public money in human capital while relieving temporary budget constraints is valuable, this paper supports Sadulet and de Janvry's (2006) conclusions that efficiency gains could be realized with unconditional transfers.

For example, money saved from monitoring conditions on families with only young, already-enrolled children could be focused more heavily on families with older children who face a steeper opportunity cost to education. Alternatively, governments could invest in means to facilitate migration and remittances, which also invest in human capital and in some cases enhance rural credit markets.

These results should be interpreted with some care. The remittance data are endogenous and, as discussed, the included instrumental variable is not free from problems. For example, while the average remittance is similar in magnitude to the CCT, its median value is much smaller. This discrepancy could help to explain the difference in

remittances' effect on enrollment but not attendance. It is also possible that because the magnitude of remittances varies, it was their market-driven efficiency that resulted in a higher impact. Or perhaps the primary education constraint is monetary but families discount returns to continued education, making it easier to impact enrollment. Nicaragua is also the second poorest country in Latin America, so results from its CCT should be extrapolated with care. Rural poor populations in other countries may face entirely different constraints to education. Though problematic, Mexico's CCT provides a potentially rich source to test these same hypotheses in a more developed nation with a more robust CCT. Finally, it should be noted that CCTs and remittances represent purely demand-side interventions in the rural education market. Due to poor and inconsistent schools, it is possible that families appropriately discount returns to education and improving education outcomes without improving school quality is less marginally productive than other investments. Supply-side improvements may raise the value of education sufficiently that demand-side interventions of much smaller magnitude are needed.

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Figures

Figure 1: A typical family in an unconditional regime

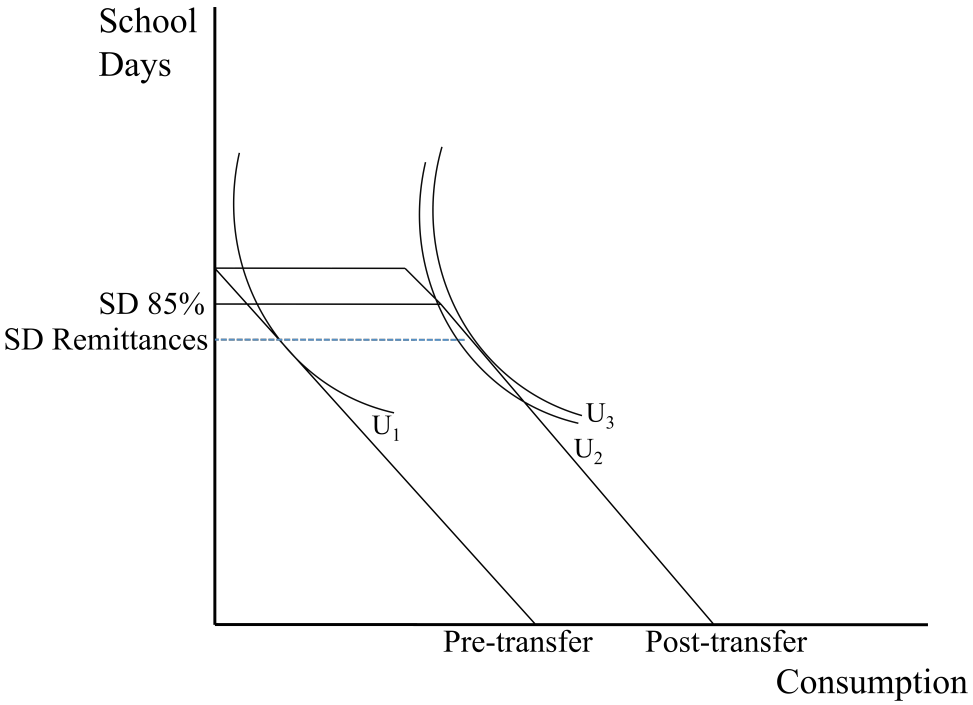


Figure 2: A typical family in a conditional regime

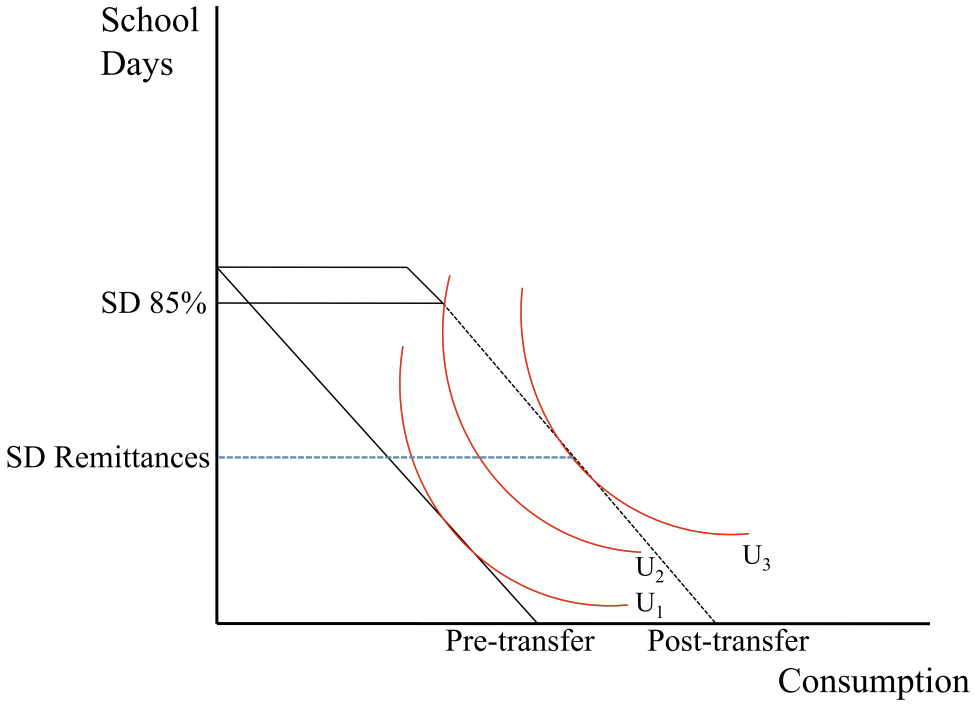


Table 1: Conditions Choice Matrix

Nicaraguan Families:	**Face Constraints**	**Do Not Face Constraints**
Value Education	Conditions Don't Matter	No Program Necessary
Do Not Value Education	Conditions Matter	Laws to require school attendance

Figure 3 Attendance

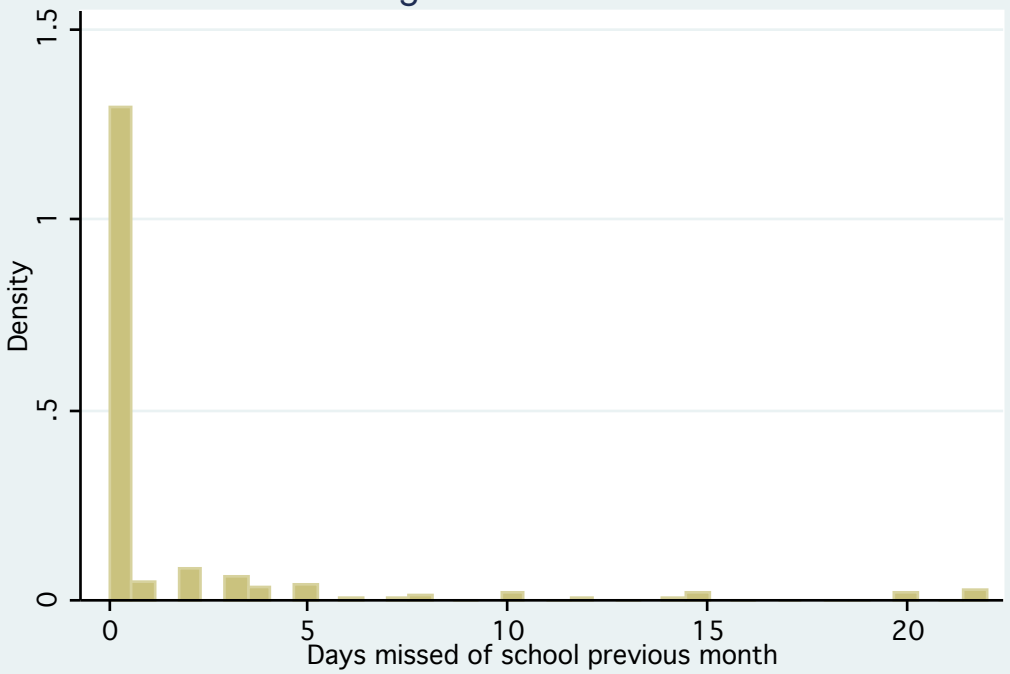
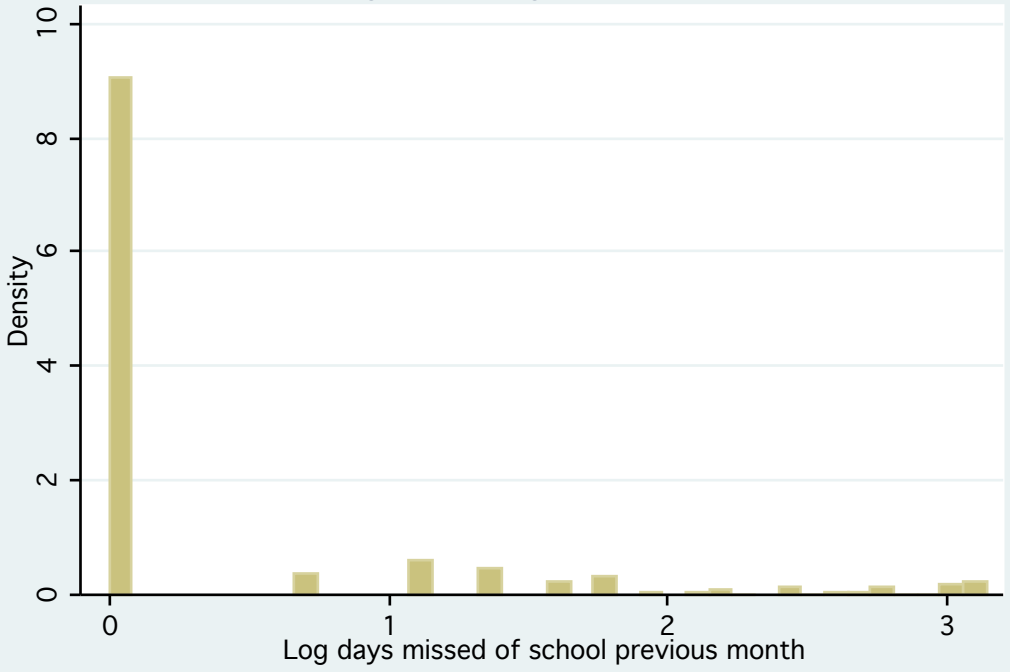


Figure 4 Log Attendance



Tables

Table 2: Summary Statistics 2000

Variable	Obs	Mean	Std. Dev.	Min	Max
RPS	9747	0.5	0.5	0	1
Enrollment	3444	0.60	0.49	0	1
Attendance	2175	2.92	5.55	0	22
Remittance	9747	8.44	93.39	0	4000
Expense	9747	1731.64	1032.48	133.74	6745.34
Age	9736	21.14	18.13	0	108
Gender	9747	0.51	0.50	0	1
Distance	1777	1.02	3.41	0	72
Work	7635	0.40	0.49	0	1
Farmer	8731	0.89	0.31	0	1
Parent Ed.	7602	4.00	1.56	1	6

Table 3: Summary Statistics 2001

Variable	Obs	Mean	Std. Dev.	Min	Max
RPS	9463	0.52	0.49	0	1
Enrollment	3282	0.71	0.45	0	1
Attendance	2425	1.31	3.88	0	22
Remittance	9463	9.81	139.67	0	4200
Expense	9463	1623.53	940.27	156.34	7448.45
Age	9405	21.57	18.17	0	110
Gender	9463	0.51	0.50	0	1
Distance	2071	0.96	2.18	0	49
Work	7221	0.36	0.48	0	1
Farmer	7951	0.89	0.32	0	1
Parent Ed.	7824	4.03	1.56	1	6

Table 4: Summary Statistics 2002

Variable	Obs	Mean	Std. Dev.	Min	Max
RPS	9482	0.5	0.5	0	1
Enrollment	3195	0.71	0.45	0	1
Attendance	2380	1.15	3.87	0	22
Remittance	9482	9.62	79.19	0	1200
Expense	9482	1562.88	900.28	147.31	7142.98
Age	9429	22.14	18.09	0	111
Gender	9482	0.51	0.50	0	1
Distance	2017	0.90	2.43	0	56
Work	7003	0.40	0.49	0	1
Farmer	8141	0.91	0.29	0	1
Parent Ed.	8283	4.02	1.57	1	6

Table 5: Correlation Coefficients

	RPS	Expense	Remittance	Age	Gender	Distance	Work	Farmer	Parent Ed.
RPS	1.00	-	-	-	-	-	-	-	-
Expense	0.15	1.00	-	-	-	-	-	-	-
Remittance	0.01	0.01	1.00	-	-	-	-	-	-
Age	0.01	0.00	0.01	1.00	-	-	-	-	-
Gender	0.01	0.00	0.00	0.01	1.00	-	-	-	-
Distance	0.02	-0.01	0.00	0.05	-0.02	1.00	-	-	-
Work	-0.03	-0.01	-0.01	0.27	0.54	0.00	1.00	-	-
Farmer	-0.03	-0.09	-0.02	-0.01	0.02	-0.01	-0.02	1.00	-
Parent Ed.	0.00	0.22	0.03	0.02	0.00	0.02	0.00	-0.09	1.00

Table 6: Variance Inflation Factors

Variable	VIF	1/VIF
work	1.15	0.87
age	1.1	0.91
lnexpense	1.07	0.93
gender	1.06	0.95
edparent	1.05	0.95
RPS	1.04	0.96
farmer	1.01	0.99
lnremittance	1.01	0.99
distancekm	1.00	1.00
Mean VIF	1.05	

Table 7: Instrumented Remittances

Expenditures	0.036
	(0.011)***
Age	0.001
	(0.00)
Male	-0.006
	(0.02)
Work	-0.004
	(0.02)
Farmer	-0.026
	(0.02)
Parent Education	0.027
	(0.004)***
Percent Community Migrant	4.979
	(0.151)***
Constant	-0.477
	(0.113)***
Observations	15889
R-squared	0.07

Notes: Standard errors in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Covariate Effect on Enrollment

	Probit	IV Probit	IV Probit	Probit
RPS	0.025	0.026	-	0.025
	(0.015)***	(0.015)***	-	(0.015)***
Log Remittances	0.036	0.151	0.15	-
	(0.009)***	(0.030)***	(0.031)***	-
Expenditures	0.09	0.083	0.086	0.091
	(0.012)***	(0.012)***	(0.012)***	(0.012)***
Age	-0.005	-0.005	-0.005	-0.005
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Male	0.091	0.091	0.092	0.09
	(0.015)***	(0.015)***	(0.015)***	(0.016)***
Work	-0.536	-0.533	-0.536	-0.532
	(0.030)***	(0.030)***	(0.030)***	(0.030)***
Farmer	0	0.008	0.003	-0.002
	(-0.020)	(-0.021)	-0.02	-0.021
Parent Education	0.014	0.01	0.01	0.015
	(0.005)***	(0.005)**	(0.005)**	(0.005)***
Constant	-3.028	-2.708	-2.785	-3.051
	(0.530)***	(0.530)***	(0.529)***	(-0.528)***
Observations	6457	6457	6457	6457
Log-likelihood	-2950.721	-2946.719	-2948.294	-2959.498

Notes: All coefficients are marginal effects estimates across students and through time. Standard Errors in parentheses. *Significant at 10%. ** Significant at 5%. *** Significant at 1%. The non-instrumented regressions have psuedo R^2s of 0.116 and 0.114, respectively. RPS is 1 for treatment; 0 for control. Remittances are in log form. Distance does not vary sufficiently across the dependent variable for regression. Total yearly expenditures proxy family income. Work records whether the student worked in the previous week. Farmer indicates if the student's family is primarily in agriculture.

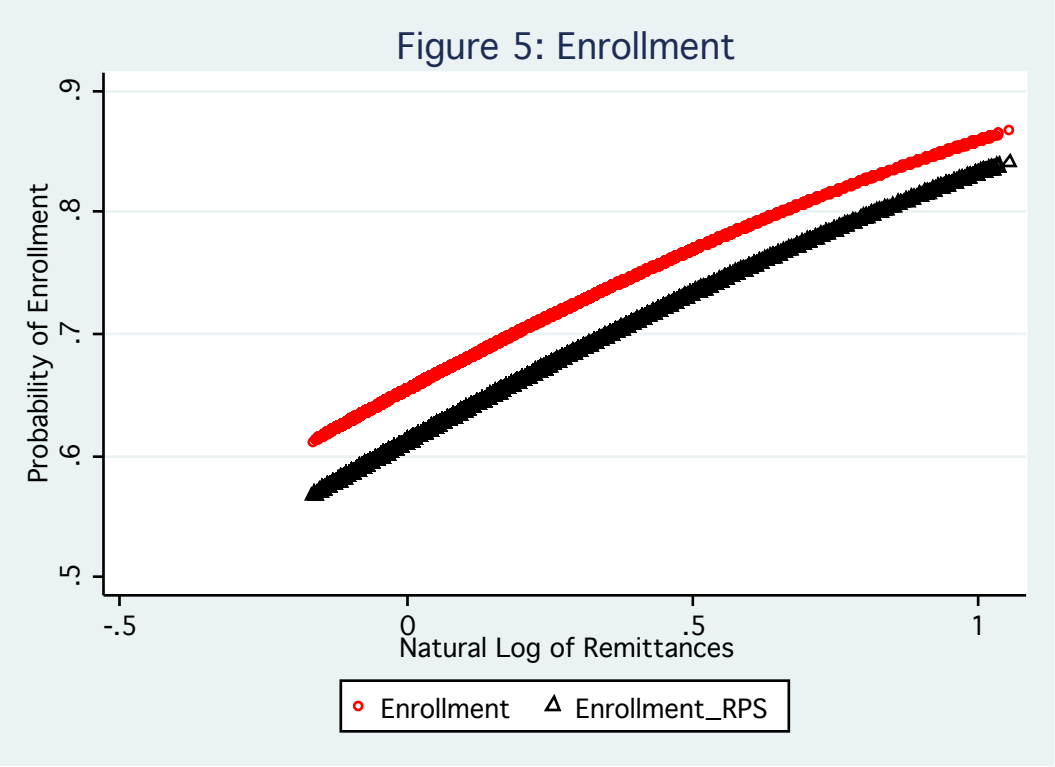


Figure 5: Enrollment
Probability of Enrollment
.5
.6
.7
.8
.9
-.5
0
.5
1
Natural Log of Remittances
Enrollment
Enrollment_RPS

Table 9: Covariate Effect on Attendance

	Tobit	Tobit	Tobit IV	Tobit IV
RPS	-2.500	-2.504	-2.510	-
	(0.469)***	(0.468)***	(0.468)***	-
Log Remittances	-	0.247	-1.447	-1.319
	-	(0.260)	(1.063)	(1.058)
Expenditures	0.072	0.067	0.114	-0.244
	(0.439)	(0.439)	(0.440)	-(0.439)
Age	0.082	0.083	0.074	0.075
	(0.082)	(0.082)	(0.082)	(0.08)
Male	0.446	0.454	0.418	0.304
	(0.474)	(0.474)	(0.474)	(0.477)
Distance to School	0.153	0.152	0.159	0.155
	(0.077)*	(0.077)*	(0.077)**	(0.078)*
Work	5.096	5.089	5.165	5.326
	(0.816)***	(0.815)***	(0.817)***	(0.821)***
Farmer	-2.991	-2.992	-2.994	-2.872
	(0.707)***	(0.707)***	(0.707)***	(0.710)***
Parent Education	-0.314	-0.324	-0.259	-0.23
	(0.151)**	(0.152)**	(0.156)*	0.16
Constant	-5.243	-5.19	-5.567	-3.532
	(4.387)	(4.386)	(4.388)	(4.411)
Observations	4511	4511	4511	4511

Notes: All regressions are indexed across observations and through time. Standard Errors in parentheses. *Significant at 10%. ** Significant at 5%. *** Significant at 1%. RPS is 1 for treatment; 0 for control. Total yearly expenses proxy family income. Work records whether the student worked in the previous week. Farmer indicates if the student's family is primarily in agriculture.

Table 10: Lincom Tests Between Remittances and RPS

	Coefficient	Standard Error	95% Conf. Int
Probit	0.05	(0.078)	(-0.104)- (0.203)
IV Probit	0.656	(0.151)**	(.269) - (.862)
Tobit	2.750	(0.538)**	(1.69) - (3.806)
IV Tobit	1.063	(1.151)	(-1.193) - (3.320)

Notes: The above statistics are point estimates, standard errors and confidence intervals of the difference between remittance and RPS coefficients for each type of regression. Starred standard errors represent coefficients that are statistically different from each other at the 5% confidence level.

Table 11: "Pure" Effects Regressions

	Enrollment			Attendance		
	Unconditional Transfers Only	Conditional Transfers Only	Conditional and Unconditional Only	Unconditional Transfers Only	Conditional Transfers Only	Conditional and Unconditional Transfers
RPS	-	0.028	0.000	-	-2.627	-
	-	(0.015)*	(0.00)	-	(0.469)***	-
Remittances	0.219	-	0.000	-0.069	-	-
	(0.054)***	-	(0.00)	(0.1.649)	-	-
Expenditures	0.063	0.094	0.000	1.183	0.156	-
	(0.018)***	(0.012)***	(0.00)	(0.576)**	(0.44)	-
Age	-0.01	-0.005	0.000	0.183	0.039	-
	(0.004)***	(0.002)***	(0.00)	(0.11)	(0.08)	-
Male	0.087	0.096	0.000	0.344	0.607	-
	(0.026)***	(0.016)***	(0.00)	(0.68)	(0.47)	-
Distance	-	-	-	0.049	0.165	-
	-	-	-	(0.107)**	(0.076)**	-
Work	-0.55	-0.548	-0.007	3.52	5.619	-
	(0.043)***	(0.030)***	(0.02)	(1.113)***	(0.818)***	-
Farmer	-0.012	0.004	0.000	-1.655	-3.21	-
	-(0.03)	-(0.02)	(0.00)	-(1.07)	(0.710)***	-
Parent Ed.	0.015	0.015	0.000	-0.484	-0.358	-
	(0.034)*	(0.005)***	(0.00)	(-0.226)**	(0.151)*	-
Constant	-1.321	-3.134	-4.017	-17.42	-5.188	-
	(0.749)*	(0.534)***	(7.27)	(5.711)***	-4.439	-
Observations	3211	6288	169	2116	4377	-

Notes: All regressions are across observations and through time. Probit regressions present marginal effects. Standard Errors in parentheses. *Significant at 10%. ** Significant at 5%. *** Significant at 1%. RPS is 1 for treatment; 0 for control. Total yearly expenses proxy family income. Work records whether the student worked in the previous week. Farmer indicates if the student's family is primarily in agriculture. There were insufficient observations to estimate the effects of both types of transfers only on attendance.