More schooling *and* more learning?

Effects of a 3-Year Conditional Cash Transfer Program in Nicaragua after 10 years[1]

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Introduction

Conditional Cash Transfer (CCT) programs have expanded rapidly over the past decade, and currently operate in more than 30 countries worldwide (Fizsbein and Schady 2009; IEG 2011). Most of these programs include substantial schooling components, typically providing transfers conditional on school enrolment and attendance. Numerous evaluations, many based on rigorous experimental designs, leave little doubt that such programs can increase enrolment and grades attained—*in the short term*. But evidence is notably lacking on whether these short-term gains eventually translate into the types of *longer term* educational (and labor market) benefits ultimately needed to fully justify these programs.

In this paper, we make a first step toward filling this gap, assessing the persistent effects of a Nicaraguan CCT, 10 years after it began, on completed years of schooling and learning outcomes measured by a set of achievement tests. By going beyond measuring years of school attainment, we also address an important gap in the literature on short- and longer-term effects of CCTs; while most studies have demonstrated an increase in schooling, evidence of effects on achievement scores is both limited and mixed. This pattern, more schooling without additional learning, has many possible explanations but one that is particularly problematic for CCTs is that the simple conditionality on school attendance may be insufficient to improve learning.

In 2010, we collected data on households and individuals originally interviewed prior to the program start in 2000 as part of a Nicaraguan CCT's randomized evaluation. Given the research questions and the potential (but unknown) relationship between education and migration, it was paramount to our study to minimize attrition. Consequently, migrants were tracked throughout the country, as well as to neighboring Costa Rica, the dominant destination for international migrant labor from Nicaragua. The timing of the survey, in 2010, means that we obtain learning measures approximately seven years after the households of these individuals stopped receiving transfers. As such, it offers a rare opportunity to examine the sustainability of program effects, as we measure whether children who might have received more schooling because of the program learned more and perform better on achievement tests, even after leaving school.

We exploit the experimental design of the original evaluation, based on a randomized phase-in, to estimate intent-to-treat (ITT) effects in 2010. The analysis focuses on a cohort of children ages 9–11 at the start of the program in 2000, since the timing of the program, the age eligibility rules for the education transfer and pre-program school dropout patterns indicate that this age cohort had greater program exposure (up to 3 years) in the early treatment group than in the late treatment group. In particular, households in the early treatment group were eligible for transfers from 2000 to 2003, while those in the late treatment group were eligible from 2003 to 2005. In both groups, only children between 7 and 13 were eligible for education transfers. Hence, the 9–11 year olds in 2000 benefitted more intensively from the education transfers in the early treatment group, than in the late treatment group. Moreover, some children in the late treatment group did not benefit at all from the education transfers. For boys, this cohort encompasses the ages where the risk of school dropout, without the program, is high, further increasing the potential impact of the program. By looking at the achievement outcomes of this group in 2010, we determine whether potential increases in years of schooling were accompanied by increases in longer-term learning outcomes. While we focus on this cohort because of the sharp differentials in access to education transfers and conditionalities, the program effects we estimate reflect all components of the CCT program (as is true for most CCT evaluations), not just the education components.

We find that the short term program effect of a one-half year increase in schooling was sustained, after the end of the program and into early adulthood. In addition, results indicate significant and substantial gains in both math and language achievement scores. Specifically, random exposure to the CCT during critical school years led to a one-quarter standard deviation increase in learning outcomes for young men. Hence in Nicaragua, schooling and achievement gains coincided, implying important long-term returns to CCT programs.[2]

The estimated increases in schooling and achievement are for 7 years *after* the households of these young adults stopped receiving the transfers. As such, they go beyond existing evidence on

[2] We refer to effects estimated using 2010 information as long-term to distinguish them from the more typical program evaluation results, such as those done on *RPS* over its first four years, as well as to underscore that our results are post-program. We do not argue that 10 years is the "long term," though an argument could be made that to the extent some of the outcomes are altered permanently, for example completed schooling, they do represent long-term effects.

the short-term gains of CCTs. They also provide important evidence on the sustainability of program effects. Indeed, in contrast with many other CCT programs, in Nicaragua households stopped receiving transfers after three years. Therefore, the findings are also relevant for policy discussions regarding exit strategies for ongoing CCT projects.

This paper contributes to the limited experimental evidence on the long-term effects of CCTs. The lack of evidence is partly because the first large scale programs only began in the late 1990s. The most related evidence comes from the Mexican CCT *Progresa/Oportunidades*. *Oportunidades* is an ongoing program and analyses 5.5 years after program start exploit the original randomized design with an 18 month difference in length of exposure between the original treatment and control groups. For rural youth 15–21 years old at final measurement, the longer program exposure increased the number of grades attained by 0.2 years, but had no effect on any measures of achievement (including some of the measures used in this study). This raises the possibility that even though schooling increased, on average, learning did not.[3] The study also finds negative effects (approximately 4 percentage points) on male labor force participation, consistent with delayed entry into the labor market associated with additional schooling, but no effects on female labor force participation. Finally, they find negative effects (6 p.p.) on male migration (Behrman, Parker and Todd 2009a, 2011).

The lack of greater significant positive results of differential exposure to *Oportunidades* is surprising in light of the substantial short-term effects attributed to the program. Two aspects of the analyses that may play a role in their findings can be addressed in our study. First, the differential exposure period, 18 months, may have been too short, in particular since eligibility rules in *Oportunidades* enabled virtually all children in the original control eventually to receive education transfers. Second, there was substantial attrition between 1997 and 2003, mainly due to migration—approximately 40 percent for the 15–21 year old age group. A number of pre-program characteristics are correlated with migration (Parker, Rubalcava and Teruel. 2008). Therefore, it is possible that the lack of significant findings in the long-term evaluations of

[3] In discussing the incongruence between the results for schooling and achievement, the authors raise a number of possible reasons including sample selectivity, the potential for pre-program imbalance since achievement tests were not implemented at baseline, and the possibility of low school quality.

Oportunidades, in which they did not follow migrants, is the result of selection bias from migration, despite a reweighting procedure implemented to control for such selectivity.

In addition to the experimental evidence on the long-term effects of CCTs, there is a small body of work examining educational outcomes using non-experimental comparison groups and based on matching and regression discontinuity designs (RDD). For example, Behrman, Parker and Todd (2009a, 2011) use matching estimators and a non-experimental comparison added to the study at the time of their 2003 follow-up survey to estimate the effect of *Oportunidades* after 5.5 years. Estimated impacts on schooling are between 0.5 and 1.0 for all but the oldest girls (19–21 in 2003) they examine. As with the experimental results, however, concerns about selective attrition remain. Selection concerns also affect the non-experimental analysis of longer term effects of the Colombian *Familias en Acción* program, that relies on administrative data from a national exam administered prior to graduation from high school and mandatory for higher education, and thus only available for children still in school (Baez and Camacho, 2011).

Beyond having substantially lower attrition rates and fewer selection concerns, our analysis differs from the other research on CCTs, as RPS is not an ongoing program, so we need not rely on differences in the length of exposure nor on non-experimental variation. Instead, we can identify the long-term effects by analyzing impacts on specific age cohorts that, due to the random differences in timing of the intervention between the two groups, did and did not receive the program during age periods considered critical for educational investments. By focusing on long-term effects on learning, we add a different perspective to the existing short-term evidence on learning in CCTs. In addition to the evidence from Mexico, Filmer and Schady (2009) find no short-term impacts of CCTs on tests of mathematics and language in Cambodia; while Baird, McIntosh and Ozler (2011) show small but significant effects on tests scores in Malawi.

More broadly, our paper relates to a body of research examining long-term effects of CCT programs on outcomes other than schooling and education. In particular for *Oportunidades*, Gertler, Martinez and Rubio (2012) show longer-term impacts on household investment in economic activities. Fernald, Gertler and Neufeld (2009) analyze longer-term effects on early childhood development. Finally, Behrman, Parker and Todd (2009b) use the differential

exposure strategy described above to estimate the impact on schooling for younger cohorts, i.e., those 0 to 8 at the time of the original intervention, finding small reductions in the age of first enrolment for girls.

For a different CCT program in Nicaragua, Macours, Schady and Vakis (2012) find effects on early childhood cognitive outcomes and sustained changes in human capital investment behavior two years after the end of the program, while Macours, Premand and Vakis (2012) show that complementary productive interventions added to that CCT helped households protect themselves against shocks, even after the program ended. As with our paper, these two studies consider impacts of a CCT program after it was discontinued. Our analysis, however, differs in its focus on long-term effects and effects on education, one of the primary objectives of CCT programs, and presumably an important channel for impacts on the next generation.

The paper is organized as follows. In the next section, we outline the design of the program and of the experimental evaluation. Section 3 describes the data and section 4 the methodology. We examine in particular baseline census and administrative data to motivate the age cohort on which we focus. Using data collected during the implementation of the program, and to further motivate the long-term analysis, we establish the short-term effects on educational attainment and school attendance for this cohort in section 5. In section 6, we show that the short-term schooling gains are sustained in the longer run, and establish the gains in learning outcomes. Section 7 concludes.

2. Program and experimental design[4]

Key program design features

Modelled after *Oportunidades*, the *Red de Protección Social* (*RPS*) was designed to address both current and future poverty through cash transfers targeted to poor households in rural Nicaragua. The program was implemented by the Government of Nicaragua with technical assistance and financial support from the Inter-American Development Bank (IADB) and benefited more than 30,000 families. On average, the transfers were approximately 18 percent of pre-program expenditures and delivered every other month. Transfers were paid to a designated female

[4] This section and the appendix draw on Maluccio and Flores (2005); see for additional details.

caregiver in the beneficiary household and came with a strong social marketing message that the money was intended to be used for human capital investments. Separate amounts were transferred for, and different conditions applied to, the health and education components of the program.

All households were eligible for a 'food security transfer,' a fixed amount per household, regardless of household size and regardless of whether the households had children affected by the conditions. Households with children ages 7–13 who had not yet completed the fourth grade of primary school were also eligible for the education component of the program. They received an additional fixed bimonthly cash transfer known as the 'school attendance transfer,' contingent on enrolment and regular school attendance of all children ages 7–13 who had not yet completed fourth grade of primary school. For each eligible child, the household also received an annual cash transfer at the start of the school year, intended for school supplies, and conditional on enrolment. In this paper, we refer to the schooling attendance and school supplies transfers as the 'education transfer.' While the education component relied on the existing education infrastructure (primary schools), a small supply-side transfer was included for each child to provide schools with funds for school materials, and to incentivize teachers. Teachers were required to report enrolment and attendance using forms specifically designed by *RPS* for the verification of the conditions.[5]

In addition to the education conditions, all households were expected to attend bimonthly health educational workshops and for households with children under 5 the food security transfer was conditional on preventive healthcare visits for those children. As this paper does not focus on those age groups, we refer the reader to the appendix for more information on the health components of the program.

The program started in 2000 and comprised two phases over six years. Phase I lasted three years with a budget of $11 million, representing approximately 0.2 percent of GDP (World Bank

[5] While modeled in part after *Progresa/Oportunidades*, this section highlights two differences that are important for our analyses. First, transfers were scheduled for a fixed three year period, without possibility of re-certification. Second, *RPS* emphasized the first four years of primary school, reflecting the realities of the impoverished rural areas the program targeted with early primary dropout rates.

2001). In late 2002, based in part on the positive findings of the various evaluations, the Government of Nicaragua and IADB agreed to a continuation and expansion of the program until 2006 with a budget of $22 million. At this point (Phase II), original beneficiaries were phased out of the program while new beneficiaries were incorporated. (We describe this "cross-over" in further detail below, as it underlies our identification strategy.) The program was discontinued in 2006, and was over in the areas we study by the end of 2005.

Experimental design

The original randomized evaluation design targeted the *RPS* intervention in six rural municipalities in central and northern Nicaragua, chosen based on their low health and education indicators. In these six municipalities, 42 out of 59 *comarcas*[6] (hereafter, localities) were selected based on a marginality index constructed using information from the 1995 National Population and Housing Census (see appendix for further information on the targeting). A new program census was done in these 42 localities in May 2000.

The 42 targeted localities were then randomized into equally sized early treatment and late treatment groups at a public lottery. To improve the likelihood that the selection of localities into the experimental groups would be well balanced by poverty, the 42 localities were divided into seven strata of six localities each, using the marginality index. Randomization was done by randomly selecting from each stratum three localities as early treatment and three as late treatment.

The 21 early-treatment localities became eligible for the program and received their first transfers in November 2000.[7] They were eligible to receive three years worth of cash transfers and received the last transfer in late 2003. Households in the late treatment localities were told that the program would start in their localities at a later date. The 21 late-treatment localities were phased in starting in the beginning of 2003. They were also eligible to receive three years worth of cash transfers. While households in the early treatment group did not receive any

[6] Census *comarcas* are administrative areas within municipalities based on the 1995 National Population and Housing Census that on average included 10 small communities for a total of approximately 250 households.
[7] As the school year in Nicaragua coincides with the calendar year, the timing of the start of the transfer implies that children in the early treatment group could potentially receive education transfers during four distinct school years.

transfers after 2003, and were hence not affected by any conditionalities after that point, they continued to be eligible to use the health supply services, and the small supply-side transfer to teachers also continued.[8] By the end of 2005, all program benefits were discontinued for both groups.

Overall, compliance with the experimental design was high. Past analysis on the program has shown that the sample was balanced at baseline and that there was very little contamination of the late treatment group (Maluccio and Flores 2005). At the household level, program take-up between early and late treatment localities was approximately 85 percent. At the individual level, take-up of the education transfer was approximately 93 percent in the early treatment group and 77 percent in the late treatment group. However, in the early treatment the individual education transfer also was given to approximately 10 percent of children who were 14, and therefore should not have been eligible. This leakage to older children was corrected when the late treatment group became eligible.

3. Data

Baseline census-level data were collected in May 2000 on all households living in the early and late treatment localities. The census includes information on primary school enrolment and years of education for all household members. It also includes information on household demographics and basic assets, and can be used to obtain an estimate of household expenditure levels, using the proxy means method developed by the government of Nicaragua for targeting purposes (based on the 1998 Nicaraguan Living Standards Measurement Survey, or LSMS). From the census roster, a random sample of households (42 in each of the 42 early and late treatment localities) was selected for inclusion in the short-term evaluation survey. The short-term evaluation survey, discussed in more detail in Maluccio and Flores (2005), constitutes a panel with the first round in September 2000, and subsequent rounds in 2001, 2002 and 2004. In the short-run panel, attrition between rounds was around 10 percent. The survey was a household level survey, modeled after

[8] Initially, *RPS* planned to provide transfers and related supply-side services for a period of up to three years. During negotiations for Phase II, however, the Government of Nicaragua and IADB agreed to extend the supply-side health and education (which included a small transfer to the schools) components for an additional two years, but not the demand-side transfers.

the LSMS, with modules on education, health, detailed household expenditures, and fertility among others.[9]

Between November 2009 and November 2011, i.e., between 9 and 11 years after the start of the program in the early treatment group, we conducted a long-term follow-up survey. For convenience, and since the majority of data was collected in 2010, we refer to this as the 2010 survey. For this survey, we included all households in the original short-term evaluation survey, as well as a sample of additional households who, according to the 2000 census, had children of ages critical to the long-term evaluation. Specifically, we oversampled households with children born between January and June 89 (used for this analysis, see below) and children born between November 2000 and mid-May 2001 (for a different analysis that will focus on critical ages during early childhood). For these oversample households, we do not have the detailed information from the earlier short-term evaluation surveys, but we do have baseline information from the 2000 census. The target sample totals 1330 households in the early treatment group and 1379 households in the late treatment group. In 2010, data was collected using an expanded household survey instrument, including new modules on labor market history and economic activities. In addition, a separate, individual-level instrument was designed to measure cognition and achievement of all children and young adults born after January 1st, 1988.

A great deal of effort went into minimizing attrition, both for the household and for the individual surveys. Respondents who could not be found in their original locations were tracked to their new locations, anywhere they went in Nicaragua. Migrants to Costa Rica (the destination of 95 percent of international migrants from the sample) also were tracked. As migration is often temporary in nature, multiple return visits to the original localities also were organized to incorporate temporary migrants after they returned. As a result, attrition at the household level is below 7 percent. At the individual level for the specific cohort studied in this paper, attrition is 9.5 percent for variables included in the household survey (years of education), and 17 percent for variables included in the individual survey (test scores). Attrition is logically higher in the individual survey as that required directly interviewing the young adults themselves (and they

[9] In 2002, a non-experimental comparison group was added from neighboring municipalities, and re-surveyed in 2004. This sample was also included in the 2010 sample. This additional sample will be used in future work to obtain non-experimental estimates of the absolute impacts for the late treatment group.

are a mobile population), while the information on years of education in the household survey could be obtained from another respondent, typically a parent. Importantly, there is no significant difference between early and late treatment groups in attrition (coefficient -0.003 with P-value of .931 for the individual survey).

The individual survey includes a number of tests to assess cognition and learning achievement. For young adults between 15 and 22, two Spanish language and two math tests were administered. From the Woodcock-Muñoz Spanish language achievement battery, we used a spelling test, a test of reading fluency and a test of math fluency (Woodcock and Muñoz 1996; Schrank et al. 2005). The second math test is the Peabody Individual Achievement Test of math problems (PIAT, Markwardt 1989). In addition, we administered two tests likely to capture in part achievement and in part cognitive development: the TVIP (Spanish version of the Peabody Picture Vocabulary Test, Dunn et al. 1986), and the McCarthy memory test (consisting of the forward and backward digit span).[10] The last test, the Raven colored matrices test (36 item version) likely captures mostly cognition, and has been used in many other studies for this purpose.

All of the tests were piloted extensively and adjustments made, as necessary. Math questions in the PIAT, originally phrased in English, were translated into Spanish and then carefully piloted and reworked for maximum understanding in the study population. The other tests are either non-verbal or already developed for Spanish speaking respondents, so that they could be used in their original versions. Many of these tests have been applied in similar populations in Latin America, including in the evaluations of cash transfer programs in Ecuador and Mexico, and a different CCT program in Nicaragua (Paxson and Schady 2010; Fernald, Gertler and Neufeld 2008; Behrman, Parker and Todd 2009; Macours, Schady and Vakis 2012). An important advantage of all of the tests is that they provide observed, as opposed to self-reported, measures of learning and cognition. This substantially reduces concerns about reporting biases.

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[10] We use the combined measure of the forward and backward digit span, but separate point estimates for the forward and backward components are similar to the combined effect.

All tests were conducted in the homes of the young adults, by a specially trained team of test administrators. They are therefore obtained independent of whether the respondent was in school or not, avoiding potential selection concerns.[11] During the training, great emphasis was placed both on gaining the confidence of the respondents before starting the tests and on the standardized application of each of the tests. The quality and standardized application of the tests was closely monitored in the field. Furthermore, given the long survey period, several re-standardization trainings were organized. Moreover, data collection and test administration was organized in such a way that the test administrators would maintain a balance between the number of children visited in early and late treatment localities, and visits to early and late treatment localities were also balanced in time, to avoid seasonality differences between the experimental groups. Consistent with these field protocols, results are robust to controls for the identity of the test administrator.

The household and individual level survey data is complemented with the *RPS* program's administrative data, originally obtained for the purpose of registering beneficiaries, monitoring conditionalities, and determining transfers. It contains detailed information on school enrolment and attendance for both early and late treatment groups, as well as information on transfers, and household eligibility for the education transfer. This data is used to examine the actual transfers received by children of different age groups and their consistency with the experimental design and with the program rules. This helps isolate the age group for which we can identify clear program impacts (in the short- and long-term) on education.

4. Methodology

Identification strategy for 2010 impact estimates

To determine the long-term effects of RPS, we use the exogenous variation in early versus late treatment assignment provided by the randomized phase-in of the program. We select a cohort of children who were likely to benefit more from the educational transfers in the early compared to the late treatment group by exploiting 1) the difference in timing of the intervention between

[11] The test administrators were trained to motivate young adults to participate in the tests, and to return in case of refusals, keeping final non-response to a minimum. Tests were administered inside the home (or in the yard) in a manner to assure the privacy of the test-taker and the confidentiality of the results. Test administrators were all women and selected on their background (trained as psychologists, social workers, or in similar fields) and for their ability to establish good rapport with children and young adults.

experimental groups; 2) the fixed 3-year duration of the transfers; and 3) the age-specific conditionalities of the education transfer, together with pre-program patterns of school dropout. The combination of these elements points to a cohort of children, between 9 and 11 years old in 2000, who were more intensively exposed to the education transfers and conditionalities in the early compared to the late treatment, and at a critical age for their schooling, an age when risk of dropout was high.

As described earlier, education transfers were provided and conditionalities were applied to the early treatment group from late 2000 to late 2003, and to the late treatment group from the beginning of 2003 to 2005. The school year in Nicaragua corresponds to the calendar year. A consequence of the near end-year start in the early treatment group, then, was that three full years of transfers and conditionalities could (at least partly) influence up to four distinct school years. In contrast, three full years of transfers and conditionalities in the late treatment could influence at most three school years. For clarity, age in 2000 refers to the age at the start of transfers in the early treatment group, that is, in November 2000.

To determine the cohort of children that likely benefited the most from the program, it is first important to consider at what age children leave school in rural Nicaragua. Arguably, at least with respect to maintaining enrolment and attendance, transfers provided at ages when most children are already in school, or when most have already left school, will be less effective than transfers provided at ages when children are at higher risk of dropping out (de Janvry and Sadoulet, 2006).[12] In Figure 1, we present average enrolment rates before the program for boys in the 2000 census (gray line, right-hand scale). Enrolment rates peak around ages 10 and 11, after which they decline sharply, indicating increased risk of dropout for boys beginning at age 11.[13] So, 9–11 year olds in 2000 in the early treatment group, at risk of dropping out, may stay longer in school if they receive the benefits of the program between 2000 and 2003. On the other hand, children in this age cohort in the late treatment group will be between the ages of 12–14

[12] Of course, children who would have been in school even in the absence of the transfer are likely to benefit in other ways, due to the requirement on number of days of attendance and the increase in household resources, some of which were designated for school uniforms and materials.
[13] Enrollment patterns for girls, on the other hand, are somewhat different, and fairly high until age 13, after which there is a sharp decline (not shown). This is one reason we do not analyze girls in this paper.

when they become eligible for the program. By then, many of these children may have already dropped out of school so it will be harder for the program to affect their educational attainment.

Second, it is important to consider the program rules. Children between 7 and 13 years of age were eligible for the education transfer as long as they had not completed fourth grade. Abstracting from the grade requirement (for the moment), consider the potential differential exposure to the educational transfer for two children of the same age, one living in an early treatment locality and the other in a late treatment locality. [14] If the children are both 7 in 2000 the early treatment child would be eligible for all four calendar years, from 2000 to 2003. The late treatment child would be eligible for all three calendar years, from 2003 to 2005, since he still would be under 13. Consequently, the early treatment child has the potential for an additional calendar year of exposure compared to his age-mate in late treatment group. This potential differential changes with starting age. For example, a child who is 10 in 2000 again would be eligible for four years in early treatment. A child of the same age but in the late treatment group, however, would be eligible for only one year in 2003, when he was 13. Similar comparisons for all eligible ages in 2000 make clear that the potential difference in exposure is highest at three years for 10 and 11 year olds, is two years for 9 and 12 year olds, and so on.

Incorporating the grade requirement component for eligibility into the discussion, it is clear that as older children are more likely to have completed grade 4, measures of potential maximum school years affected by the program based on age eligibility alone will not reflect actual number of school years affected. Because some fraction of children reach the maximum grade of eligibility prior to the end of the program in their localities the actual exposure differences are lower, on average, than the potential exposure differences discussed above.[15] Figure 1 (black line, left-hand scale) shows the difference between the average number of school years during which boys actually received transfers in early treatment compared with boys in late treatment.[16]

[14] Exposure in this section refers specifically to child-specific exposure to the educational components of the *RPS* program. It is not to be confused with the exposure to other program components that are independent of the education eligibility rules.
[15] The actual differences are also lower than potential because take-up was not 100 percent.
[16] We show the actual transfers, as the administrative data do not allow us to observe grade progression in the absence of the education transfer. It is therefore impossible to estimate how the grade component of eligibility affects the potential exposure without making several strong assumptions.

Actual exposure differences are the highest between about age 9 and 11, and peak at two for 10 year-olds.

Based on this examination of dropout and exposure, we focus the analysis of the long-term effects on children 9–11, as this is the age group for whom we expect the largest differential impact as a result of being randomly allocated into early versus late treatment. While the length of exposure for the 8 year olds is also high, we do not include them because their risk of dropout is low during the program years in the early treatment group.[17] We also do not include the 12 year olds in our principal results, because the upper age limit for the education transfer was not rigorously implemented in the initial years, and some 12-year olds (in 2000) in the early treatment group received the education transfers for more than two years. While it is possible that application of program rules were equally lax for all 12-year olds, it is not certain and, therefore, possible that the 12-year olds with higher program exposure are a selected group.[18] We therefore exclude 12-year olds from the main analyses, but consider them in some robustness results.

In sum, our main analyses focus on the cohort aged 9 to 11 in 2000. For this group, there is multi-year exposure to the education transfers in the early treatment group, high differential exposure in early versus late treatment, and exposure at ages for which the risk of dropout increase in early treatment. The oversample is on the 11 year olds, as they were exposed to the educational transfers for up to three years and during ages with high risk of dropout. While we focus on this cohort because of the sharp differentials in access to education transfers and conditionalities, it is important to emphasize that the program effect we estimate reflects all components of the program, not just the education components. All of the analyses are carried out on an intent-to-treat basis, and using all children in the 9–11 age cohort, regardless of initial levels of completed schooling.

Empirical specifications

[17] Moreover, for some children in late treatment there would be up to three full years of exposure around the ages where dropout increases.
[18] It is for the same reason that actual transfers are not zero for those 14 and older in 2000.

To analyze both short- and long-term results on schooling outcomes, we estimate child-level intent-to-treat regressions of the following form:

(1) $Y_k = \alpha_k T + \beta_k \mathbf{X} + \varepsilon_k,\ k=1\ldots K,$

where Y_k is the kth outcome (of three in the 2002 and 2004 follow-up surveys, and eight in the 2010 follow-up survey), T is an intent-to-treat indicator that takes on the value of one for children in localities that were randomly assigned to the early treatment and zero for those in late treatment, and **X** is a set of controls. The 2010 cognitive and achievement test outcomes are presented as within-sample z-scores so that magnitudes of the program effects are easier to compare across outcomes and total effects for a group of outcomes together can be examined.[19] The coefficient on the treatment indicator, T, therefore measures the effect size in standard deviations.

We present two specifications. The first specification only includes controls for the child's age when the transfers started (in three-month intervals) and dummy variables for the stratification groups. The second specification includes in addition controls for pre-intervention characteristics from the 2000 census.[20] These include the log of expected per capita expenditures (as estimated by the proxy means), distance to school, child's years of completed schooling, and indicator variables for whether the household is active in agriculture, whether the child had no education, whether the child was working, presence of mother and father in the household, and whether the respondent was the child of the household head. For the short-term analysis, we also control for the outcome variable at baseline.[21] Including these controls adjust for baseline differences between early and late treatment groups, and may also improve the precision of the estimated program effects. Standard errors are adjusted for clustering at the locality level.

We first examine the short-term effects of the program on education for the 9–11 year olds to understand what the possible trajectories could be for long-term effects of the program. We use the 2002 and 2004 surveys and examine the short-term effects on three educational outcomes:

[19] The z-score is calculated by subtracting the mean and dividing by the standard deviation of the late treatment group. The late treatment group is used to standardize as it is less likely to be affected by the program for this age group. Results are qualitatively similar when using the mean and standard deviation of the whole population.

[20] For the short-term analysis, missing values in the census data were filled in with the 2000 baseline data.

[21] No cognitive or achievement tests were collected in 2000. For the 2010 test outcomes, then, we cannot control for the baseline outcome variable and instead control for baseline years of education.

years of completed education attained, a dummy variable indicating if the child was enrolled in school, and the number of days a child missed school in the past month.[22] The comparison between early and late treatments in 2002 provides an estimate of the impact of the program (in the early treatment group) approximately two years after the transfers started and before the late treatment group was eligible for the program. By 2004, the late treatment group had started receiving transfers, while the early treatment group had been phased out. So, the estimates for 2004 for the 9–11 year olds provide a first indication of the potential sustained differences between the early and late treatment groups. If, indeed, the early treatment group in this age cohort benefited more from the program as previously hypothesized, we would expect to see sustained program impacts for the early treatment group at least in terms of the number of years of schooling completed in 2004. While these results build on the detailed evidence of the *RPS* program on education (Maluccio and Flores 2005; Maluccio, Murphy and Regalia 2010), they focus on the specific age group directly relevant to the analyses of the 2010 program effects.

For the 2010 outcomes, we estimate the effects for each of the eight outcomes individually and also estimate the average effect on three groups, or families, of similar outcomes, to determine the effect of the program on achievement (reading and math fluency, math problems and spelling), outcomes that are likely to capture a mix of cognition and achievement (receptive vocabulary and McCarthy memory test), and cognition (Raven). When outcomes are grouped together the average intent-to-treat effect is given by

$$(2) \qquad \overline{\alpha} = \frac{1}{K}\sum_{k=1}^{K}\hat{\alpha}_k$$

We estimate (1) and (2) by carrying out seemingly unrelated regressions (SURE) for all of outcomes, and use the estimated variance-covariance matrix of the estimates to calculate the standard error of $\overline{\alpha}$ (see Kling, Liebman and Katz 2007; Duflo, Glennerster and Kremer 2008).

5. Age cohorts and short-term results

[22] This sample includes all of the 9–11 year olds, except those who were oversampled in 2010, or who were lost to attrition between 2000 and 2002 or 2004. The sample includes children in this age group that were not found in 2010, but found in earlier rounds. Robustness analysis shows the short-term results are similar when we exclude children who were not found back in 2010 from the analysis.

Short-term results on educational outcomes for 2002 and 2004 are presented in Table 1 for 9 to 11 year old boys. Column 1 confirms previous research, by 2002 the program had led to a statistically significant half year (or 21 percent) increase in years of schooling completed, a 13.5 percentage point (or 16 percent) increase in the enrolment rate, and a 3.7 day (168 percent) reduction in the number of days missed of school in the past month. These results are robust to the inclusion of baseline controls though the point estimate of the treatment effect for years of schooling decreases from 0.48 to 0.32.

By 2004, the early treatment group had been phased out and the late treatment group was receiving program benefits. Many of the children in the 9–11 year old cohort in the late treatment group were no longer eligible for the education transfer as they were then too old (see Section 4), though their households still potentially benefited from other components of the program, possibly influencing the child's education. In particular, beneficiary households of children in the 9–11 year cohort were eligible for the food transfer, were exposed to social marketing, and possibly received the education transfer for younger, eligible children. The results from 2004 in Table 1 column 2 demonstrate that the early treatment group still had 0.62 years of education more than the late treatment group, despite no longer receiving program benefits. This indicates that, at least by 2004, the program had lead to a sustained increase in the number of years of completed education for the early treatment group. However, columns (4) and (6) indicate that enrolment rates for the early treatment group were 13 percentage points lower than for the late treatment group, and the early treatment group had missed 3.2 more school days in the previous month. Since children in the late treatment group still had approximately one more year of program benefits after they were surveyed in 2004, and the 2004 enrolment rate was higher in the late treatment group than the early treatment group, we need to examine the differences in years of education completed in 2010 to determine if completed education indeed remained higher in the early treatment group.

6. Longer term gains in education and learning for boys

By 2010, the program had been over in both experimental groups for at least four years. Moreover, the vast majority of the young adults in the cohort (now ages 19–21) had left school and were engaged in economic or domestic activities. This means that we can observe the close

to final difference in years of schooling between the early and late treatment groups. Table 2 presents the differences in 2010 for years of education completed in the first two columns, and for the standardized test scores (z-scores) in columns 3 to 9. Results are presented with and without baseline controls for boys who were 9 to11 at the time transfers began in the early treatment. In the third specification (at the bottom of the table), the sample is limited to the 11 year olds, the oversample group for whom we hypothesize differences between early and late treatment group to be the largest (section 4).

For comparison with Table 1, the gain in actual years of education (without standardization) is shown in column 1. The impact is statistically significant and the magnitude of the 2010 difference in final years of education remarkably similar to that found in 2004. Seven years after the early treatment group stopped receiving the transfers, they continued to have nearly half a school year advantage. This result is interesting in itself, as it demonstrates that the boys in the late treatment group did not catch up after 2004, as seemed possible given the short-term results on enrolment. The persistent gain is consistent with the dropout patterns observed at baseline and age of eligibility of the program; many of the boys in this cohort in the late treatment group would have left school or been too old for the education transfers by the time their localities became eligible for the program. While their households would have been eligible for program benefits, the similarity of the estimated magnitudes suggest that it was "too late" for these boys, they had likely dropped out before the program had come and did not re-enroll. The possibility remains, however, that there may have been a positive absolute effect on this late treatment group that is offset by a similar effect in the early treatment. This would be the case if the decisions regarding schooling in the early treatment group continued to be affected positively, even after program benefits ended for this group.[23]

With the finding that the CCT for this group of boys led to a significant increase in the final years of schooling of nearly half a grade, we next explore whether children also learned more. The results for the achievement tests are presented in columns 3 through 6, and show large and significant effects on each of the Spanish language and math tests. The results are robust to

[23] Comparisons of the magnitude of the experimental differential impact with matching estimates using the non-experimental comparison group might help to separate out these potential mechanisms (work in progress).

including baseline controls, and appear to be somewhat larger for children who were 11 years old at the start of the program (and thus 21 when the tests were administered). In contrast, the Raven test (column 9), a general test of cognition that does not capture specific skills learned in school, shows no significant differences between the groups and point estimates are below 0.1 SD in all three specifications. Results for the memory test (column 8) are also small and insignificant but those for receptive vocabulary (column 7) are significant in all three specifications.

To correct for potential inference errors from individual estimates on eight different test outcomes, in Table 4 we present the SURE results, and show impacts for families of outcomes. We group the four achievement tests together (but also show them separately for the two language and two math tests). We also combine the receptive vocabulary and memory tests into a mixed cognitive/achievement category, but leave the Raven test separate as the only measure of cognition. The first row in Table 3 shows the specification with baseline controls, the second results for the 11 year olds only, and the third results including 12 year olds.

Across specifications, there is clear evidence of a differential impact of the program on achievement, ranging from 0.21 standard deviations for the 9–11 year olds, and rising to 0.28 standard deviations when we focus on boys who were 11 at the start of the program. The estimated impacts are not only highly statistically significant, but are also substantial in size. Gains in achievement are observed both in language and in math. On the other hand, and as expected, there are no significant gains (and comparatively small point estimates) on cognition, as measured by the Raven. Results for the tests that are likely to capture aspects of both general cognition and skills learned in school are somewhat smaller and less significant. Hence, program impacts are concentrated on the specific types of skills learned in school.

In the remainder of Table 3 we present evidence that the results are robust to the addition of further controls. In particular, controls for the identity of the test administrator have almost no effect on the results, consistent with the standardized training and application of the tests in the field. Results are also robust to adding controls for mother's education. Finally, as the number of controls available from the census is somewhat limited, we consider estimates using only observations in the short-term household survey sample, allowing a richer set of baseline

controls. This reduces the share of 11 year olds in the sample (as it eliminates the children who were oversampled in 2010), and consistently, the ensuing points estimates are reduced across the board. Impacts on achievement, however, remain large and significant. In the final specification, we then include additional household controls, capturing in particular baseline consumption levels, the share of food in total consumption, and baseline educational aspirations for each individual. The results are also robust to additions of those controls.[24]

Attrition in our sample is both low and balanced across groups. Combined with the fact that the estimates are precise, this strongly suggests that the analysis is unlikely to be substantially affected by attrition bias. [In a future version of this paper, we will more formally correct for possible bias due to the remaining attrition, for example, using bounds methods.]

To interpret our results, it is important to remember that households were eligible to receive the food transfer, independently of whether their children were eligible for the education transfer. By relaxing liquidity constraints, food transfers might have enabled children to stay in school even beyond the ages and grade levels the education transfers were targeting, in either treatment group. In addition, older children, or children in higher grades, might have been in households eligible for education transfers if there were (younger) resident children, further reducing liquidity constraints. Hence, while it might be tempting to conclude that the achievement gains are the direct result of the additional years of schooling obtained through the educational transfer and the conditions, we cannot exclude potential additional channels through which the CCT may have affected schooling or learning. But what these results do allow us to conclude confidently is that boy's exposure to the CCT at critical ages during primary school resulted both in more years of final education, and in significant and substantial learning gains, seven years after the transfers had ended.

In addition, the pattern of findings is suggestive regarding the role of schooling in explaining these results. First, program effects are concentrated on the very skills learned in school (such as spelling or math problems) and there are similarly sized effects on both the math and Spanish

[24] All results are also robust to alternative ways of standardizing (including using means and standard deviations of a wider age group or using observations from both experimental groups). They are also robust to including an even wider set of controls, such as baseline assets, and to removing the 1 percent highest and lowest outliers.

tests. It seems unlikely that such a finding would come from post-schooling experiences, for example, such as differential employment across the treatment groups. Second, there are no effects on the Raven, our best measure of cognition, indicating the gains are unlikely to be due to improved ability or cognition. And third, there are no significant effects on the memory test, evidence against the possibility that early treatment individuals did not necessarily learn more, but retained more due to improved memory. Hence, it seems plausible that the learning achievements stem from the overall increase in schooling, even if they cannot be directly linked to the education transfers and conditionality.

The short-term results further indicate that the increase in schooling due to the CCT came not only from higher enrolment rates, but also from higher attendance (less days missed of school). As a consequence, increases in learning may not have only been experienced by children that enrolled in school earlier, or dropped out later as a result of the CCT, but may also have been experienced by children who missed fewer days of classes.

Conclusions

Conditional cash transfer programs have become the anti-poverty program of choice in many developing countries. Their approach, combining short-term poverty reduction with enhanced investment in human capital, has widespread policy appeal. A number of rigorous empirical studies have established that these programs are effective at reducing short-term poverty and increasing health and children's school attainment. There is little evidence on whether these children are actually learning, however, and the evidence that does exist is mixed. There is even less evidence on whether any gains in learning are sustainable in the longer run. Answering this question is key to understanding the longer-term impact of these programs, and the extent to which their investments in human capital can improve the welfare of the next generation. As the policy discussion on CCTs shifts towards designing exit strategies, there is a demand from policy makers for establishing whether such longer term gains exist.

This paper estimates the long-term effects of a CCT program in Nicaragua on educational attainment and learning for boys. Taking advantage of the randomized phase-in, and measuring learning outcomes 10 years after the start of the program, we find program effects on completed

education and language and math achievement. We focus on specific age groups that, due to the program's eligibility criteria and school dropout patterns, were likely to have benefitted more in the group of localities that were randomly selected to receive the program first. This allows us to show that the increase in the number of years of education is accompanied by gains in learning. In particular, we estimate a statistically significant and substantial average achievement gains of a quarter of a standard deviation. We find no effect on cognition, an indication that the achievement gains are unlikely due to improved ability. As these estimates are obtained for children of households that stopped receiving transfer 7 years earlier, they provide unique evidence on the sustainability of CCT impacts.

The findings on sustained learning outcomes are important in their own right. They are also indicative of other potential benefits, for example in the labor market, a focus for ongoing research. The findings in this paper also point to a number of additional questions that will be addressed in the future. In particular, this paper focuses on boys, for whom the experiment allowed us to select an age group with a clear effect on education based on dropout patterns and eligibility rules. Future analysis will turn to understanding the longer-term effects for girls.

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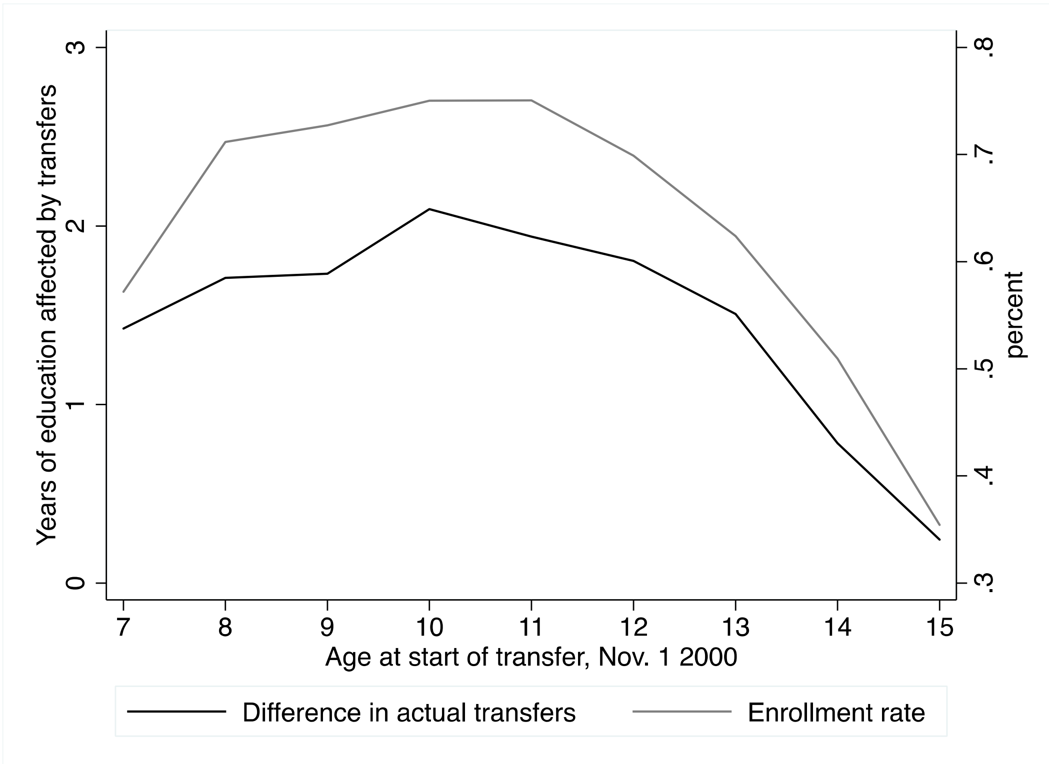
Figures and Tables

Figure 1: Difference in Mean Years of Transfers Received Between Early and Late Treatment Groups and Mean Enrolment rate, by age at start of transfers for boys



Notes: The difference in actual transfers refers to the mean difference in the total number of school years a children received transfers between the early and late treatment groups. The transfer data is taken from the RPS administrative records and the enrolment rate is from the 2000 census data. The means actual transfers are based on data for the entire early and late treatment localities, not just those children who were surveyed in 2000-2004.

Table 1: 2002 and 2004 ITT results on individual schooling outcomes for boys 9 to 11 at start program

	Years of education		**Enrolment (=1)**		**Days of School Missed**	
	2002	2004	2002	2004	2002	2004
	(1)	(2)	(3)	(4)	(5)	(6)
Age and stratification controls	0.477**	0.620***	0.135***	-0.131*	-3.684***	3.197**
	(0.224)	(0.212)	(0.036)	(0.067)	(0.927)	(1.301)
N	364	358	364	358	364	358
Mean depend variable, control group	2.23	3.56	0.82	0.74	5.40	6.74
Baseline controls	0.323***	0.440***	0.140***	-0.110*	-3.824***	2.671**
	(0.092)	(0.078)	(0.038)	(0.061)	(0.908)	(1.209)
N	363	357	363	357	363	357
Mean depend variable, control group	2.25	3.57	0.82	0.74	5.47	6.77

Notes:. Standard errors are clustered at the locality level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All specification include dummies for stratification group and 3-monthly age dummies. Baseline controls include: log of per capita expenditures, distance to school, whether the household is active in agriculture, whether respondent is child of household head, whether mother and father live in the house, years of completed education, and a dummy for no education, and the outcome variable at baseline.

Table 2: 2010 ITT results on individual schooling outcomes for boys 9 to 11 at start program

	Years of education		Standardize Test scores						
	Absolute	z-score	Reading fluency	Spelling	Math fluency	Math problems	Receptive vocabulary	Memory math	Cognition (Raven)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age and stratification controls	0.533*	0.178*	0.251**	0.266***	0.280***	0.168**	0.192**	0.103	0.066
	(0.29)	(0.096)	(0.098)	(0.082)	(0.087)	(0.068)	(0.079)	(0.066)	(0.086)
N	817	817	709	756	753	755	757	752	757
Baseline controls	0.444**	0.148**	0.238**	0.230***	0.240**	0.135*	0.178**	0.070	0.038
	(0.18)	(0.061)	(0.10)	(0.078)	(0.096)	(0.074)	(0.067)	(0.069)	(0.081)
N	814	814	706	753	750	752	754	749	754
Only 11 year olds (with controls)	0.433*	0.145*	0.243**	0.269***	0.350***	0.266***	0.182**	0.041	0.100
	(0.25)	(0.084)	(0.11)	(0.094)	(0.11)	(0.097)	(0.085)	(0.079)	(0.097)
N	348	348	303	321	319	320	321	318	321

Notes:. Standard errors are clustered at the locality level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All specification include dummies for stratification group and 3-monthly age dummies. Baseline controls include: log of per capita expenditures, distance to school, whether the household is active in agriculture, whether respondent is child of household head, whether mother and father live in the house, years of completed education, and a dummy for no education.

Table 3: Impacts on test scores: SURE estimates for family of outcomes

	Achievement			**Mixed Cognition and Achievement**	**Cognition (Raven)**
	All	Language	Math		
Base specification	0.21***	0.23***	0.19**	0.12**	0.04
	(0.08)	(0.08)	(0.08)	(0.06)	(0.08)
Age variations					
Only 11 year olds	0.28***	0.26***	0.31***	0.11*	0.10
	(0.09)	(0.09)	(0.09)	(0.07)	(0.09)
9 to 12 year olds	0.19***	0.21***	0.17**	0.11*	0.03
	(0.07)	(0.07)	(0.08)	(0.06)	(0.08)
Robustness					
Test administrator control	0.21***	0.24***	0.18**	0.13**	0.04
	(0.08)	(0.08)	(0.08)	(0.06)	(0.08)
Additional controls	0.23***	0.25***	0.21***	0.14**	0.06
for mother's education	(0.08)	(0.08)	(0.08)	(0.06)	(0.08)
Only short-term survey sample	0.19**	0.19**	0.19*	0.10	0.04
	(0.09)	(0.08)	(0.11)	(0.08)	(0.13)
Only short-term survey sample	0.20**	0.19**	0.22**	0.10	0.02
with add. household controls	(0.08)	(0.08)	(0.10)	(0.07)	(0.12)

Notes: results for SURE with 8 outcomes (following Kling et al., 2007). Standard errors are clustered at the locality level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Results for achievement include 2 language tests (spelling and reading fluency) and 2 math tests (math fluency and math problems). The mixed cognitive-achievement category combines receptive vocabulary and memory. Controls are the same as those listed in the notes in Table 3.

Appendix: Details on program design

Program targeting

RPS first targeted rural areas in six municipalities of the Central Region of Nicaragua, on the basis of poverty as well as on their capacity to implement the program. The focus on rural areas reflected the distribution of poverty in Nicaragua—of the 48 percent of Nicaraguans designated as poor in 1998, 75 percent resided in rural areas (World Bank, 2001). While not the poorest municipalities in the country, or in the Central Region for that matter, the proportion of impoverished people living in these areas was still well above the national average (World Bank, 2003). In addition, these areas had easy physical access and communication (for example, less than a day's drive to Managua, where the central office was located), relatively strong institutional capacity and local coordination, and relatively good coverage of health posts and schools.

In the next stage of pre-program targeting, a marginality index was constructed for all 59 rural census *comarcas* (localities) in the selected municipalities. The index was the weighted average of a set of locality-level indicators (including family size, access to potable water, access to latrines, and illiteracy rates, all calculated from the 1995 National Population and Housing Census) in which higher index scores are considered to be more impoverished areas. The 42 localities with the highest calculated scores were selected. Although the initial program design called only for geographic-level targeting in these 42 localities (that is, with all resident households eligible), about 6 percent of households, deemed to have substantial resources, were excluded ex ante from the program (Maluccio, 2009).

While it is not possible to claim that the 42 selected localities are statistically representative of rural Nicaragua, there is evidence that they are similar to a large number of other rural areas in the Central Region and elsewhere in the country on a number of dimensions. First, three-quarters of the approximately 150 rural localities in the Nicaraguan departments of Madriz and Matagalpa have marginality index scores in the same range as the programme areas, as do three-quarters of the approximately 1000 rural localities in the country as a whole. If instead one considers levels

of extreme poverty, there are more than 350 localities in the country with extreme poverty at or above 42 per cent, the average level in the targeted areas (Maluccio, 2009). On these broad indicators used for geographical targeting, then, there are a large number of similar localities, suggesting those chosen were not atypical.

Program components and conditionalities

The transfers were conditional on health and education behaviours, and education and health conditionalities were monitored by teachers and health providers. RPS's specific stated objectives included: i) supplementing household income for up to three years to increase expenditures on food; ii) reducing dropout rates during the first four years of primary school, and iii) increasing the healthcare and nutritional status of children under age five.

RPS had two core components:

Food security, health, and nutrition. Each eligible household received a bimonthly (every two months) cash transfer known as the 'food security transfer,' contingent upon the designated household representative attending bimonthly health educational workshops and bringing children under age five for scheduled preventive healthcare appointments with specially trained and contracted providers. The workshops were held within the communities and covered household sanitation and hygiene, nutrition, and other related topics. For the preventive health care visits, children under age two were seen monthly and those between two and five, bimonthly. Health services at the scheduled visits included growth monitoring, vaccination, iron supplementation and provision of anti-parasite medicine. The supply of health care was increased to ensure the program could meet the increased demands for health care without reducing quality. In particular, *RPS* contracted and trained private health providers (Regalia & Castro, 2007), beneficiaries were required to use those providers for fulfillment of the conditions and all services were free of charge. Providers visited program areas on scheduled dates and delivered services in existing health facilities, community centers, or private homes. In Phase II, additional services (and corresponding conditions) were added, including vaccination for school age children, family planning services for women of child bearing age, pre-natal care consultations, and a health educational workshop for adolescents and household representatives.

Education: Each eligible household also received a bimonthly cash transfer known as the 'school attendance transfer,' contingent on enrolment and regular school attendance of children ages 7–13 who had not yet completed fourth grade of primary school. Additionally, for each eligible child, the household received an annual cash transfer at the start of the school year, intended for school supplies (including uniforms and shoes) known as the 'school supplies transfer,' which was contingent on enrolment. Unlike the school attendance transfer, which was a fixed amount per household regardless of the number of children in school, the school supplies transfer was per child.

To provide incentives to the teachers, who had some additional reporting duties and were likely to have larger classes after the introduction of RPS, and to increase resources available to the schools, there was also a small cash transfer, known as the "teacher transfer."[25] This was given to each beneficiary child, who in turn delivered it to the teacher. The teacher was meant to keep one-half, while the other half was earmarked for the school. The delivery of the funds to the teacher was monitored and a condition of the program, but not their ultimate use.

At the outset, nearly all households were eligible for the food security transfer, which was a fixed amount per household, regardless of household size and regardless of whether the households had children affected by the conditionalities. Households with children ages 7–13 who had not yet completed the fourth grade of primary school were also eligible for the education component of the program. The initial US dollar annual amounts and their Nicaraguan Córdoba (C$) equivalents (using the September 2000 average exchange rate of C$ 12.85 to US$ 1.00) were as follows: the food security transfer was $224 a year, the school attendance transfer $112, the school supply transfer $21 and the teacher transfer 5$. On its own, the food security transfer represented about 13 percent of total annual household expenditures in beneficiary households before the program. A household with one child benefiting from the education component would have received additional transfers of about 8 percent, yielding an average total potential transfer of 21 percent of total annual household expenditures. The nominal value of the transfers remained constant, with the consequence that the real value of the transfers declined by about 8

[25] In rural Nicaragua, school's parents' associations often request small monthly contributions from parents to support the teacher and the school; the teacher transfer was, in part, intended to substitute any such fees.

percent due to inflation during Phase I. In Phase II, which began in 2003 and incorporated new beneficiaries, the size of the demand-side transfers was reduced. The food security transfer started at $168 for the first year of program participation and then declined to $145 and $126 in the second and third years. The school attendance transfer also declined slightly, to $90 per year but the school supplies transfer rose to $25 per student. These figures represent potential transfers.

To enforce compliance with program requirements, beneficiaries did not receive the food or education component(s) of the transfer when they failed to carry out any of the relevant conditions described above. Repeated violation led to households losing their eligibility. Only the designated household representative was allowed to collect the transfers and, where possible, RPS appointed the mother or another female caregiver to this role. As a result, more than 95 percent of the household representatives were women. RPS also worked with local volunteer coordinators (beneficiary women chosen by the community) to implement the program. The coordinators were charged with keeping beneficiary household representatives informed about upcoming healthcare appointments for their children, upcoming transfers, and any failures in fulfilling conditions.