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Abstract

CCT programs have become the anti-poverty program of choice in many developing countries. Numerous evaluations, often based on rigorous experimental designs, leave little doubt that such programs can increase enrollment and grades attained—in the short term. But evidence is notably lacking on whether these short-term gains translate into longer-term educational benefits needed to fully justify these programs.

This paper uses the randomized phase-in of the *RPS* CCT program in Nicaragua to estimate the long-term effects on educational attainment and learning for boys, measured 10 years after the start of the program. We focus on a cohort of boys aged 9–12 years at the start of the program in 2000 who, due to the program’s eligibility criteria and prior school dropout patterns, were likely to have benefitted more in the group of localities that were randomly selected to receive the program first.

We find that the short-term program effect of a half grade increase in schooling for boys 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, an approximately one-quarter standard deviation increase in learning outcomes for the now young men. Hence in Nicaragua, schooling and achievement gains coincided, implying important long-term returns to CCT programs.

JEL Codes: I25, I38, I28

Key words: CCT, education, long-term effects, achievement

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1 Introduction

Conditional Cash Transfer (CCT) programs have expanded rapidly over the past decade and been implemented in more than 30 countries worldwide (Fiszbein and Schady, 2009; Independent Evaluation Group, 2011). Most of these programs include substantial schooling components, typically providing transfers conditional on school enrollment and attendance. Numerous evaluations, many based on rigorous experimental designs, leave little doubt that such programs can increase enrollment 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 even labor market) benefits ultimately needed to fully justify these programs.

In this paper, we begin to address this evidence gap by assessing the persistent effects of a Nicaraguan CCT, *Red de Protección Social (RPS)* (Social Protection Network), 10 years after it began, on highest grade attained and learning outcomes, as measured by a set of achievement tests. In going beyond measuring grades attained, we also address another 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. The pattern of 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 students' 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 laborers from Nicaragua. The timing of the survey, in 2010, means that we obtain learning and other measures approximately seven years after households of these individuals in the first experimental group eligible for the program stopped receiving transfers. It therefore 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 *RPS*'s initial evaluation, which was based on a randomized phase-in, to estimate intent-to-treat (ITT) effects in 2010. We focus on specific age groups that, due to the program's eligibility criteria and the country's school dropout patterns, were likely to have benefitted more in the group of localities that were randomly selected to receive the program first.

In particular, our focus is on the cohort of boys aged 9–12 years at the start of the program in 2000. Several factors lead us to this cohort as a focus, including length and timing of exposure to the *RPS*, age-eligibility rules, and preprogram 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, children aged 9–12 years in 2000 benefitted more if they were part of the early rather than late treatment, as they were at the upper age limit for eligibility by the time the late-treatment group began. Moreover, some children in the late-treatment group did not benefit from the education transfers. Among 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 grades attained 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 half grade increase in schooling for boys 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.¹

¹We refer to effects estimated using 2010 information as long-term to distinguish them from more typically available program evaluation results, such as those done on the *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

The estimated increases in schooling and achievement are for seven years *after* the young men's households stopped receiving the transfers. As such, the increases go beyond existing evidence on the short-term gains of CCTs and provide important evidence on the sustainability of program effects. In contrast with many other CCT programs, Nicaraguan households stopped receiving transfers after only three years, so our findings are also relevant for policy discussions on exit strategies for ongoing CCT projects.

This paper therefore contributes to the limited experimental evidence on the long-term effects of CCT, which is lacking in part because the first large scale programs only began in the late 1990s. The most similar evidence comes from the Mexican CCT *Oportunidades* (previously called *Progresá*), which is an ongoing program. Analysis conducted five-and-a-half years after program start exploits the original randomized design, with an 18-month difference in length of exposure between the original treatment and control groups. Figures on rural youth aged 15–21 years at final measurement indicate that longer program exposure increased the number of grades attained by 0.2, but had no effect on any measures of achievement (including some of the measures used in this study). This result raises the possibility that even though schooling increased, on average, learning did not. 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, program participation created negative effects (6 percentage points) 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 of 18 months may have been too short, as the program is ongoing and eligibility rules enabled virtually all children in eligible households from the original control group to receive education transfers eventually. Second, there was substantial attrition between 1997 and 2003, mainly due to migration—approximately 40 percent for the age group of 15–21 years. A number of preprogram characteristics are correlated with migration (Parker, Rubalcava and Teruel, 2008). Therefore, it is possible that the lack of significant findings in the long-term

made that because some outcomes are altered permanently, for example completed schooling, they do represent long-term effects.

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

In addition to the experimental evidence on the long-term effects of CCTs, a small body of work is based on examining educational outcomes using nonexperimental comparison groups based on matching and regression discontinuity designs (RDD). For example, Behrman et al. (2009a, 2011) also examine the 15–21-year-old age group using matching estimators and a nonexperimental comparison added to the study for the 2003 follow-up survey to estimate the effect of *Oportunidades* after five-and-a-half years. Estimated impacts on schooling are between 0.5 and 1.0 grades for all but the oldest girls (aged 19–21 years in 2003) they examine. As with the experimental results, however, concerns about selective attrition remain. Selection concerns also affect the nonexperimental analysis of longer term effects of the Colombian *Familias en Acción* program, which relies on administrative data from a national exam administered prior to graduation from high school that is mandatory for higher education and is thus only available for children still in school (Baez and Camacho, 2011).

Our analysis differs from the other research on CCTs, as the *RPS* is not ongoing, so we need not rely on differences in the length of exposure or on nonexperimental variation. Instead, we can identify long-term effects by analyzing impacts on specific age cohorts that, due to the random differences in timing of the interventions between the early- and late-treatment groups, did or did not receive the program at ages 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 from 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 et al. (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 a specific analysis of *Oportunidades*, Gertler et al. (2012) show longer-term impacts on household investment in economic activities. Fernald et al. (2009) analyze longer-term effects on early childhood development. Finally, Behrman et al. (2009b) use the differential exposure strategy described above to estimate the impact on schooling for younger cohorts, i.e., those children aged 0–8

years at the time of the original intervention, finding small reductions in the age of first enrollment for girls.

For a different CCT program in Nicaragua, Macours, Schady et al. (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 et al. (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, the latter being one of the primary objectives of CCT programs and presumably an important channel for impacts on the next generation.

This paper is organized as follows. The next section outlines the design of the *RPS* and the experimental evaluation. Section 3 describes the data, and section 4 details the methodology. In section 5, *RPS* baseline census and administrative data for the age cohort that forms the focus of our study are examined. Using data collected during the implementation of the program, we determine the short-term effects of the *RPS* on educational attainment and school attendance for our targeted cohort. Section 6 shows that the short-term schooling gains are sustained in the longer run and establish that gains also were made in learning outcomes. Section 7 presents the study's conclusions.

[2 *RPS* program and its experimental design²](#)

2.1 Key program design features of the *RPS*

Modelled after *Oportunidades*, the *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 IDB and benefited more than 30,000 families. On average, the transfers comprised approximately 18 percent of preprogram expenditures and were delivered every other month. Transfers were paid to a designated female caregiver in the beneficiary household and came with a strong social marketing message that the money was intended to be used for human capital

²This section and the appendix draw on Maluccio and Flores (2005); see that report for additional details.

investments. Separate amounts were transferred for, and different conditions applied to, the health and education components of the program.

The *RPS* had multiple types of transfers. All households were eligible for a food security transfer that provided a fixed amount per household regardless of the household's size and composition. Households with children aged 7–13 years 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, which was contingent on enrollment and regular school attendance of those children. For each eligible child, the household also received an annual cash transfer intended for school supplies at the start of the school year that was conditional on enrollment. In this paper, we refer to the combined schooling attendance and school supplies transfers as the education transfer.

While the education component relied on the existing schooling infrastructure (primary schools), a small supply-side transfer also was included for each child to provide schools with funds for school materials and to incentivize teachers. Teachers were required to report enrollment and attendance using forms specifically designed by the *RPS* for the verification of the conditions.³ In addition to the education transfer's conditions stated above, beneficiary households were expected to attend bimonthly health education workshops. Similarly, the food security transfer was conditional on preventive healthcare visits for households with children under the age of five years. As this paper does not focus on children of that age group, we refer the reader to the appendix for more information on *RPS* health components.

The *RPS* started in 2000 and comprised two phases over six years. Phase I lasted three years and had a budget of \$11 million, representing approximately 0.2 percent of GDP (World Bank, 2001). In late 2002, based in part on the positive findings of various program evaluations, the government of Nicaragua and the IDB agreed to continue the program until 2006 and expand it, with a new budget of \$22 million. In the areas we study, the program was completed by the end of 2005.

2.2 Experimental design of the *RPS*

³While modeled in part after *Progres/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 recertification. Second, the *RPS* emphasized the first four years of primary school, to address the high early-primary-school dropout rates in these impoverished rural areas.

The original randomized evaluation design targeted the *RPS* intervention in six rural municipalities in central and northern Nicaragua that were chosen based on their low health and education indicators. In these six municipalities, 42 out of 59 *comarcas*⁴ (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 special census was done by the *RPS* in these localities in May 2000.

The targeted localities were then randomized into one of two equally sized treatment groups, the early- or late-treatment group, at a public lottery. To improve the likelihood that the localities in the experimental groups would be well balanced in terms of poverty level, 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 2000.⁵ 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 informed that the program would start in their localities at a later date. The 21 late-treatment localities were phased in at 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 transfers after 2003, and were hence not affected by any conditionalities after that date, they continued to be eligible to use the health supply services. The small supply-side transfer to teachers also continued.⁶ 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

⁴Census *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.

⁵As 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.

⁶Initially, *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 IDB 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.

in 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-year-olds and should not have been eligible. This leakage to older children was corrected when the late-treatment group became eligible (and we examine whether this minimal contamination affects our findings).

3 Data

Baseline census-level data were collected by the *RPS* in May 2000 on all households living in the early- and late-treatment localities. The census data includes information on primary school enrollment and highest grade attained 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 the purpose of household targeting (based on the 1998 *Nicaraguan Living Standards Measurement Survey*; LSMS).

From the census roster, a random sample of households (42 households in each of the 42 early- and late-treatment localities) was selected for inclusion in a short-term-evaluation panel survey, as discussed in more detail in Maluccio and Flores (2005). The first round of the survey was conducted in September 2000, with subsequent rounds in 2001, 2002, and 2004, and attrition between rounds was approximately 10 percent per round. This household-level survey was modeled after the LSMS, with modules on education, health, detailed household expenditures, and fertility, among others.⁷

Between November 2009 and November 2011, i.e., between 9 and 11 years after the start of the program for 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. In 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

⁷In 2002, a nonexperimental comparison group was added from neighboring municipalities, and resurveyed in 2004. This sample was also included in the 2010 sample. This additional sample will be used in future work to obtain nonexperimental estimates of the absolute impacts for the late treatment group.

with children born between January and June 1989.⁸ We do not have the detailed information from the earlier short-term evaluation surveys for the oversampled households, but we have baseline information from the 2000 census. The target sample has a total of 1,330 households from the early-treatment group and 1,379 households from 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 individual cognition and achievement levels of each child and young adult born after January 1, 1988.

A great deal of effort went into minimizing attrition, both for the household and individual surveys. Respondents who could not be found in their original locations were tracked to new locations 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, multiple return visits by the survey team to the original localities were organized to interview 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 12 percent for variables included in the household survey (highest grade attained), and 19 percent for variables included in the individual survey (test scores). Attrition is logically higher in the individual survey as it required direct, in-person interviews with young-adult respondents between the ages of 15 and 22 years (who are mobile), while information in the household survey on highest grade attained could be obtained from another respondent, typically a parent. Importantly, there is no significant difference in attrition (coefficient 0.009 with P value of 0.771 for the individual survey) between early- and late-treatment groups.

The individual survey includes a number of tests to assess cognition and learning achievement. For young adults between the ages of 15 and 22 years, three tests were administered on the Spanish language and two on math. Specifically, we used one grade-level appropriate test on these subjects: word identification, spelling, reading fluency, and math fluency. Also, a second math test measures math problem solving at various levels of difficulty, similar to the *Peabody Individual Achievement Test* (Markwardt, 1989). In addition, we administered two tests likely to capture both achievement and cognitive development: the

⁸We also oversampled children born between November 2000 and mid-May 2001, but these children's data are not included in this analysis.

Test de Vocabulario en Imágenes Peabody (TVIP; the Spanish version of the *Peabody Picture Vocabulary Test*; Dunn, Lugo and Padilla, 1986), and a forward and backward digit-span test, in which the respondent is asked to repeat series of numbers read to him.⁹ The last test, the Raven colored matrices test (the 36-item version with sets A, AB, and B) likely captures mostly cognition, and has been used in many other studies for this purpose.

All of the tests were piloted extensively, with adjustments made for the local context as necessary, and questions were rephrased as needed for maximum understanding in the study population. Very similar tests have been applied in other 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, 2009a; 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, therein substantially reducing concerns about reporting biases.

All tests were conducted in the young-adult respondents' homes by a specially trained team of test administrators. The results were therefore obtained independent of whether the respondent was in school, avoiding potential selection concerns.¹⁰ During the test administrators' 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. Data collection and test administration was also organized in such a way that test administrators would maintain a balance between the number of children visited in early- and late-treatment localities. Visits to early- and late-treatment localities were also balanced over time to avoid seasonal differences in measurement between the experimental groups. Consistent with these field protocols, results are robust to controls for the identity of the test administrator.

⁹We 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.

¹⁰The test administrators were trained to motivate the children and young adults to participate in the tests and revisit the youths who refused to participate, therein 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 for their background (trained as psychologists, social workers, or similar fields) and for their ability to establish a strong rapport with children and young adults.

The household- and individual-level survey data is complemented by *RPS* administrative data that was originally gathered to register beneficiaries. It contains detailed information on school enrollment and attendance for both early- and late-treatment groups, as well as information on all 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. It also helps isolate the age group for which we can identify clear program impacts (in the short and long terms) on education.

4 Methodology

4.1 Identification strategy for 2010 impact estimates

To determine the long-term effects of the *RPS*, we use the exogenous variation in the early-versus late-treatment assignment provided by the randomized phase-in of the program. As described earlier, education transfers were provided and conditionalities were applied to the early-treatment group from late-2000 to late-2003 and the late-treatment group from the beginning of 2003 to the end of 2005. Despite each group having three years of program exposure, the number of school years affected by the early versus late treatment was not equal. The school year in Nicaragua corresponds to the calendar year, so the early-treatment group's program start near the end of the year meant that the three full years of transfers and conditionalities could (at least partly) influence up to four distinct school years. In contrast, the late-treatment group's program start time also included three full years of transfers and conditionalities, but the timing allowed at most three school years to be affected.¹¹

To determine which children likely benefited the most from the program, it is first important to consider the age at which children commonly drop out of school in rural Nicaragua. Arguably, at least with respect to maintaining enrollment 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 enrollment rates before the program for boys in the 2000 census (gray line, right vertical-axis scale). Enrollment rates peak

¹¹For clarity, age in 2000 refers to the age at the start of transfers in the early-treatment group, that is, in November 2000.

¹²Of 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.

around ages 10 and 11, after which they decline sharply, indicating increased risk of dropout for boys beginning at age 11.¹³ So, boys aged 9–12 years in 2000 who were in the early-treatment group were at risk of dropping out, and it is possible they would have stayed in school longer by receiving program benefits between 2000 and 2003. On the other hand, children in this age bracket from the late-treatment group would have been between the ages of 12 and 15 by the time their households became eligible for the program. By then, many may already have dropped out of school, so it would have been harder for the program to have affected 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. Setting the grade requirement aside 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 seven years old in 2000, the early-treatment child would be eligible across four calendar years, from 2000 to 2003. The late-treatment child would be eligible for three calendar years, from 2003 to 2005, since he still would be under 13 years old this entire period. Consequently, in addition to exposure at different ages, the early-treatment child has the potential for an additional calendar year of exposure compared to his age-mate in the late-treatment group. This potential differential, however, changes with starting age. For example, a boy who is 10 in 2000 again would be eligible for four years in the early treatment. A boy of the same age but in the late-treatment group, however, would be eligible for only the first year of his cohort's benefits, 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.

Reincorporating the grade-requirement component for eligibility into the discussion, it is clear that as older children are more likely to have completed grade four, measures of potential maximum school years affected by the program based on age eligibility alone will not reflect

¹³Enrollment 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.

¹⁴Exposure 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.

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 vertical-axis scale) shows the difference between the average number of school years during which boys actually received transfers in the early treatment compared with boys in the late treatment.¹⁶ Actual exposure differences are the highest between about ages 9 and 11 years and peak at two school years for 10-year-olds.

Based on this examination of dropout and exposure, we focus the analysis of the long-term effects on children aged 9–12 years, as this is the age group for which we expect the largest differential impact as a result of being randomly allocated into early versus late treatment. Although the length of exposure for the eight-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 include the 12-year-olds in our principal results, but also examine results without them because the upper age limit for the education transfer was not strictly implemented in the initial years, and a small percentage of 12-year-olds (in 2000) in the early-treatment group received the education transfers for more than two years.

In sum, our main focus is on the cohort aged 9–12 years in 2000, for which there is multiyear 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 in the 2010 survey is on individuals who were 11 years old when they were assigned into early- or late-treatment groups, as they were exposed to the education 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 explained earlier, 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

¹⁵The actual differences are also lower than potential because take-up was not 100 percent.

¹⁶We 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.

¹⁷Moreover, for some children in late treatment there would be up to three full years of exposure around the ages where dropout increases.

analyses are carried out on an ITT basis, and use all children from both treatment groups in the 9–12-year-olds age cohort, regardless of initial levels of completed schooling.

4.2 Empirical specifications

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

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

where Y_k is the k th outcome (of three in the 2002 and 2004 follow-up surveys, and nine in the 2010 follow-up survey), T is an ITT 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 \mathbf{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 can be examined for a groups of outcomes together.¹⁸ 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 (in three-month intervals) when the transfers started and dummy variables for the stratification groups. The second specification includes in addition controls for pre-intervention characteristics from the 2000 census.¹⁹ These include the log of per capita expenditures (as estimated by the proxy means), distance to school, grade attained, and indicator variables for whether: the household is active in agriculture, the child had no completed grades, the child was working, the mother and father are in the household, and the respondent was the child of the household head. For the short-term analysis, we also control for the outcome variable at baseline.²⁰ Inclusion of these controls adjusts for possible 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.

First, we examine the short-term effects of the program on education for the children aged 9–12 years to understand what the possible trajectories could be for long-term effects of the

¹⁸The 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.

¹⁹For the short-term analysis, missing values in the census data were filled in with the 2000 baseline data.

²⁰No 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.

program. We use the 2002 and 2004 surveys and examine the short-term effects on three educational outcomes: maximum grade 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.²¹ 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 on children aged 9–12 years 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 highest grade attained 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 nine 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 (word identification, spelling, reading and math fluency, and math problems), outcomes that are likely to capture both cognition and achievement (receptive vocabulary and memory test), and cognition (Raven colored matrices). When test scores are grouped together the average ITT effect is given by

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

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

²¹This sample includes all of the 9–12-year-olds, except those who were oversampled in 2010, or who were lost to attrition between 2000 and 2002 or 2004. Consequently, it 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 from the analysis children who were not found in 2010.

5 Age cohorts and short-term results

Short-term results on educational outcomes for 2002 and 2004 are presented in table 1 for boys aged 9–12 years. Column 1 confirms previous research—by 2002 the program had led to a statistically significant half grade (or 22 percent) increase in the highest grade attained, a 14.2 percentage point (or 18 percent) increase in the enrollment rate, and a 4.0 day (62 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 highest grade attained decreases from 0.54 to 0.40.

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 9–12-year-old cohort in the late-treatment group were no longer eligible for the education transfer as they were by then too old (see section 4), though their households still potentially benefited from other components of the program, which possibly influenced the child’s education. In particular, beneficiary households of children in the 9–12-year-old 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 with baseline controls the early-treatment group still had 0.49 grades attained more than the late-treatment group, despite no longer receiving program benefits. This indicates that, at least by 2004, the program had led to a sustained increase in the highest grade attained for the early-treatment group. However, columns 4 and 6 indicate that enrollment rates for the early-treatment group were 10 percentage points lower than for the late-treatment group, and the early-treatment group had missed 2.5 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 enrollment rate was higher in the late-treatment group than the early-treatment group, we need to examine the differences in the highest grade attained in 2010 to determine if completed schooling 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 aged 19–22 years) had left school and were engaged in economic or domestic activities. This means that we can observe the

nearly final difference in grade attainment between the early- and late-treatment groups. Table 2 presents the differences in 2010 for highest grade attained in the first two columns, and for the standardized test scores (z-scores) in columns 3 to 10. Results are presented with and without baseline controls for boys who were aged 9–12 years at the time transfers began in the early treatment. In the third specification (the bottom two rows of the table), the sample is limited to the 11-year-olds, the oversample group for whom we hypothesized differences between early- and late-treatment group to be the largest (section 4).

For comparison with table 1, the gain in actual grades attained (without standardization) is shown in column 1. The impact is statistically significant and the magnitude of the 2010 difference in highest grade attained is similar to those found in 2002 and 2004. Seven years after the early-treatment group stopped receiving the transfers, they continued to have nearly half a grade’s 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 enrollment. 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 suggests that it was “too late” for these boys; they had likely dropped out before the program had begun and did not reenroll. 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. If the decisions regarding schooling in the early-treatment group continued to be affected positively, this offset would occur even after program benefits ended for this group.²²

With the finding that the CCT for this group of boys led to a significant increase in the final grade attained of half a grade, we next explore whether affected children also learned more. The results for the achievement tests are presented in columns 3 through 7, and show large and significant effects on each of the Spanish language and math tests. The results are robust to the inclusion of baseline controls, and language and math test scores are slightly larger for children who were 11 years old at the start of the program. In contrast, the Raven test (column 10), a

²²Comparisons of the magnitude of the experimental differential impact with matching estimates using the nonexperimental comparison group might help to separate out these potential mechanisms (work in progress).

general test of cognition that does not capture specific skills learned in school, shows no significant differences between the groups, and point estimates are 0.1 standard deviation or below in all three specifications. Results for the memory test (column 9) are smaller and less significant, but those for receptive vocabulary (column 8) are significant in all three specifications.

To account for potential inference errors from individual estimates on nine different outcomes, in table 4 we present the SURE results and show impacts for families of outcomes. We group the five achievement tests together (but also show the three language and two math tests separately). We also combine the receptive vocabulary and memory math 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 excluding 12-year-olds.

Across specifications, there is clear evidence of a differential impact of the program on achievement, ranging from 0.20 standard deviations for the 9–12-year-olds, to 0.28 standard deviations when we focus on boys who were 11 years old 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 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, at 0.13, 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 grades attained. 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. The sample size is therefore reduced since it eliminates the children who were oversampled in 2010; however, impacts are similar. In the final specification, we then include additional household controls, capturing in particular actual baseline consumption levels, the

share of food in total consumption, and baseline educational aspirations for each individual. The results are also robust to addition of those controls.²³

Attrition in our sample is both relatively low and balanced across groups. And as the estimates in table 3 are precise, any attrition correction likely will lead to similar qualitative findings.²⁴

To interpret the 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 grades attained through the education transfer and the conditions, we cannot exclude potential additional channels through which the CCT may have affected schooling or learning. But these results do allow us to conclude confidently that boys' exposure to the CCT at critical ages during primary school resulted both in higher grades attained and in significant and substantial learning gains, seven years after the transfers had ended in the treatment localities.

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 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 is little effect on memory math, evidence against the possibility that early-treatment individuals did not necessarily learn more,

²³All 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.

²⁴Preliminary results using Lee (2009) bounds for attrition show that the lower bounds remain significant for the achievement outcomes.

but rather 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.

[7 Conclusions](#)

CCT programs have become the anti-poverty program of choice in many developing countries. Their approach of 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 for programs, there is a demand from policymakers to establish whether such longer-term gains exist.

This paper estimates the long-term effects of the *RPS* CCT program in Nicaragua on educational attainment and learning for boys. Taking advantage of the program's 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 the country's 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 grades attained 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 to be due to improved ability. As these estimates are obtained for children of households that stopped receiving transfers seven 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 such as the labor market, which is 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 of CCTs for girls.

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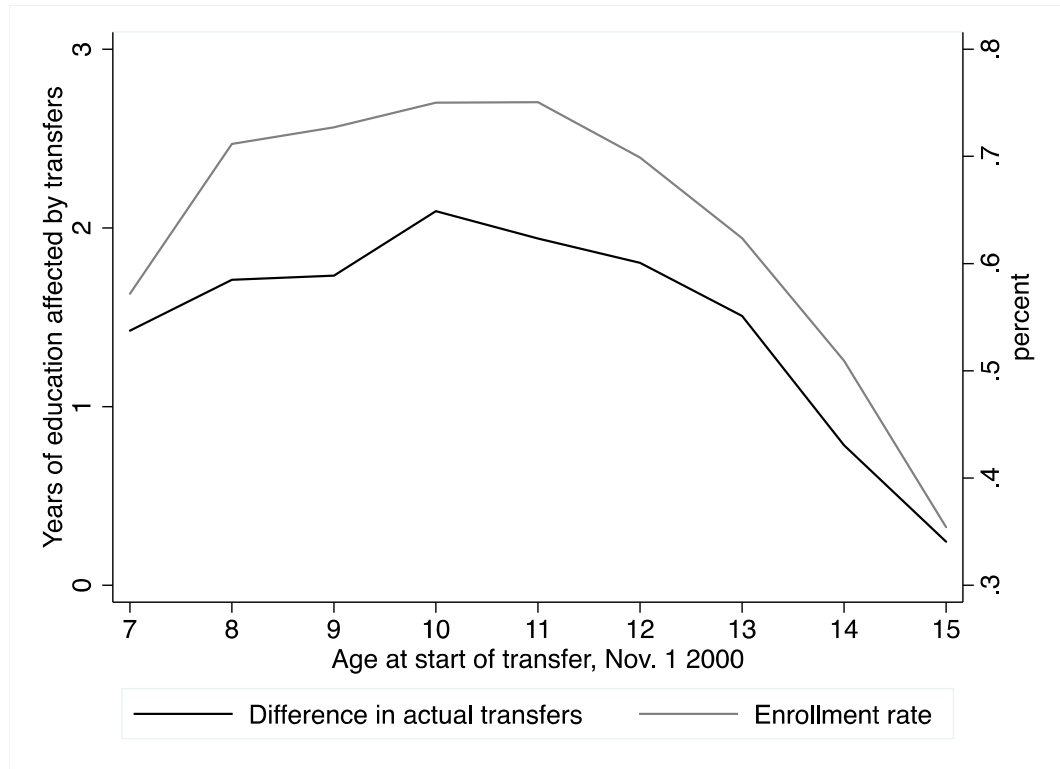
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Figures and Tables

Figure 1: Difference in Mean Years of Transfers Received Between Early- and Late-Treatment Groups and Mean Enrollment Rate, by Boys' Age at Start of Transfers



Source: Author's calculations.

Notes: The difference in mean years of transfers refers to the mean difference in the total number of school years that children in the early- and late-treatment groups received transfers (left vertical-axis scale). The data on transfers is taken from the *RPS* administrative records and the enrollment rate (right vertical-axis scale) is from the 2000 census data. The means of the actual transfers are based on data for the entire early- and late-treatment localities, not just data for the children surveyed in 2000–2004.

**Table 1: 2002 and 2004 ITT on Schooling Outcomes,
Boys Aged 9–12 Years at Start of Program**

	Grades attained		Enrollment (=1)		Days of school missed	
	2002 (1)	2004 (2)	2002 (3)	2004 (4)	2002 (5)	2004 (6)
Age and stratification controls	0.544** (0.237)	0.703*** (0.255)	0.142*** (0.036)	-0.099* (0.057)	-4.020*** (0.920)	2.676** (1.149)
N	476	459	476	459	476	459
Mean dependent variable	2.44	3.63	0.78	0.67	6.46	8.27
Baseline controls	0.403*** (0.085)	0.485*** (0.097)	0.126*** (0.034)	-0.102* (0.050)	-4.022*** (0.824)	2.499** (1.081)
N	475	458	475	458	475	458
Mean dependent variable	2.45	3.65	0.78	0.66	6.49	8.3

Source: Author's calculations.

Notes: Table reports coefficient on T in equation 1. Standard errors are clustered at the locality level and are given in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All specifications include dummies for stratification groups and three monthly age dummies. Baseline controls include: respondent's highest grade attained and a dummy variable for no grades attained, log of per capita expenditures, distance to school, whether the household is active in agriculture, whether respondent is child of household head, whether respondent reported working in the previous week, whether mother and father live in the house, and the outcome variable (measured at baseline). Mean of the dependent variable is for the late-treatment group.

Table 2: 2010 ITT on Schooling Outcomes and Test Scores, Boys Aged 9–12 Years at Start of Program

	Grades attained		Standardized test scores (Z-score)							
	Absolute	z-score	Reading fluency	Spelling	Word identification	Math fluency	Math problems	Receptive vocabulary	Memory math	Cognition (Raven)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age and stratification controls	0.501*	0.159*	0.320***	0.252***	0.228***	0.259***	0.175**	0.176**	0.124*	0.073
	(0.274)	(0.087)	(0.082)	(0.077)	(0.075)	(0.082)	(0.065)	(0.078)	(0.065)	(0.077)
N	980	980	901	908	902	907	907	909	905	909
Baseline controls	0.382**	0.121**	0.271***	0.213***	0.193***	0.211**	0.129*	0.147**	0.083	0.030
	(0.157)	(0.050)	(0.076)	(0.072)	(0.063)	(0.090)	(0.067)	(0.068)	(0.065)	(0.081)
N	977	977	898	905	899	904	904	906	902	906
11-year-olds (baseline controls)	0.453*	0.144*	0.304***	0.281***	0.242***	0.337***	0.276***	0.209**	0.0491	0.101
	(0.239)	(0.076)	(0.100)	(0.09)	(0.072)	(0.099)	(0.094)	(0.081)	(0.079)	(0.094)
N	348	348	319	320	318	319	319	320	318	320

Source: Author's calculations.

Notes: Table reports coefficient on T in equation 1. Standard errors are clustered at the locality level and are given in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All specifications include dummies for stratification groups and three monthly age dummies. Baseline controls include: respondent's highest grade attained and a dummy variable for no grades attained, log of per capita expenditures, distance to school, whether the household is active in agriculture, whether respondent is child of household head, whether respondent reported working in the previous week, and whether mother and father live in the house.

**Table 3: SURE ITT for Families of Tests,
for Boys Aged 9–12 Years at Start of Program**

	Achievement			Mixed cognition and achievement	Cognition (Raven)
	All	Language	Math		
<u>Base specification</u>	0.20*** (0.07)	0.23*** (0.07)	0.17** (0.07)	0.11* (0.06)	0.03 (0.08)
<u>Age variations</u>					
11 at start of program	0.28*** (0.08)	0.28*** (0.08)	0.31*** (0.08)	0.13* (0.07)	0.10 (0.09)
9–11 at start of program	0.22*** (0.07)	0.24*** (0.07)	0.18** (0.08)	0.13** (0.06)	0.04 (0.08)
<u>Robustness</u>					
Test administrator control	0.21*** (0.06)	0.23*** (0.06)	0.17** (0.07)	0.12* (0.06)	0.03 (0.08)
Mother's education controls	0.22*** (0.06)	0.25*** (0.06)	0.19*** (0.07)	0.12** (0.06)	0.05 (0.08)
Short-term survey sample	0.23*** (0.08)	0.27*** (0.07)	0.18* (0.10)	0.12 (0.08)	0.05 (0.12)
Short-term survey sample with additional household controls	0.24*** (0.07)	0.27*** (0.07)	0.19** (0.09)	0.11 (0.07)	0.06 (0.11)

Source: Author's calculations.

Notes: Table reports SURE results from equation 2. Standard errors are clustered at the locality level and are given in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results for achievement include three language tests (word identification, spelling, and reading fluency) and two math tests (math fluency and math problems). The mixed cognitive-achievement category combines receptive vocabulary and memory. All specifications include baseline controls listed in the notes for table 2. The final row also includes: actual (instead of predicted) expenditures, indicators of whether the household was poor or extremely poor, food share, highest grade attained by the household head, highest grade attained by the designated caregiver for the *RPS* and indicators of educational aspirations for the individual, all measured in 2000.

[Appendix: Details on the *RPS*'s program design](#)

Program targeting

The *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 the targeted municipalities were not the poorest 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 preprogram targeting, a marginality index was constructed for all 59 rural census 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 program areas, as do three-quarters of the approximately 1,000 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 percent, 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, which suggests that those chosen were not atypical.

Program components and conditionalities

The transfers were conditional on a household's health and education behaviours, and education and health conditionalities were monitored by teachers and health providers. The *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 the age of five years.

The *RPS* had two core components. The first was 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 preventive health care visits, children under age two were seen monthly and those aged between two and five were seen 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, the *RPS* contracted and trained private health providers (Regalia and Castro, 2007), and 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 childbearing age, prenatal care consultations, and a health educational workshop for adolescents and household representatives.

The second core component was education. Each eligible household also received a bimonthly cash transfer known as the "school attendance transfer," contingent on enrollment and regular school attendance of children aged 7–13 years who had not yet completed the 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 enrollment. 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 the *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 half, while the other half was earmarked for the school. Although the delivery of the funds to the teacher was a monitored condition of the program, the funds’ ultimate use was neither a program condition nor monitored.

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 a household had children affected by the conditionalities. Households with children aged 7–13 years 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 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.

²⁵In 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 for any such fees.

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, the *RPS* appointed the mother or another female caregiver to this role. As a result, more than 95 percent of the household representatives were women. The *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.