

Education and Child Labor: Experimental Evidence from a Nicaraguan Conditional Cash Transfer Program

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Abstract

The *Red de Protección Social* (RPS) is a conditional cash program, modeled after PROGRESA, to reduce both current and future poverty via cash transfers to households living in extreme poverty in rural Nicaragua. This chapter examines the education- and child labor-related effects of the program, presenting results from a randomized community-based trial. The evidence demonstrates that RPS had a significant and substantial effect on schooling matriculation and enrollment during its first year of operation. Moreover, it led to a substantial reduction in child labor for the school age population.

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Introduction

Education levels in Nicaragua are dismal. One-third of adults over the age of 25 have no formal education and another one-third never completed primary school. Although increasing school coverage and stable political conditions in the 1990s have spurred improvements, the net primary enrollment ratio, at 78 percent, remained one of the lowest in Latin America in the late 1990s (World Bank 2001, Annex 16). Unsurprisingly, these poor educational outcomes were accompanied by a high incidence of child labor, particularly for boys. In 1998, 27 percent of boys aged 10–14 in rural areas were working an average of 30 hours a week (World Bank 2001, Annex 25). These initial conditions and continued poor outcomes despite improvements in school supply, are primary concerns for the economic development of Nicaragua and have led the government to consider different approaches, including demand side interventions.

One of these was the *Red de Protección Social* or “Social Safety Net” (hereafter RPS), a government program to reduce both current and future poverty via cash transfers to households living in extreme poverty in rural Nicaragua. The transfers were conditional, requiring evidence that the household had undertaken prescribed actions to improve the human capital development of their children. The program’s stated objectives included:

- Supplementing household income for up to three years to increase expenditures on food,
- Increasing the healthcare and nutritional status of children under age five, and
- Reducing school desertion during the first four years of primary school.

Cash transfer programs similar to RPS are being implemented in several Latin American countries. Examples include the *Programa Nacional de Educación, Salud y Alimentación* (PROGRESA) in Mexico, after which RPS was modeled, and the *Programa de Asignación Familiar* (PRAF) in Honduras.¹ One reason for their popularity is that they take an integrated approach, encompassing various dimensions of human capital, including nutritional status, health, and education. As such, these programs are able to influence many of the key indicators highlighted in national poverty reduction strategies.

The Government of Nicaragua initiated RPS as a two-year pilot in 2000 with a budget of U.S. \$10 million—representing approximately 0.2 percent of the country’s GDP and 2.5 percent of recurrent government spending on health and education. As a condition of the Inter-American Development Bank (IDB) loan financing the project, and to assess whether the program merited expansion in the same or in an altered form, the government solicited an evaluation of the pilot phase of RPS. The International Food Policy Research Institute (IFPRI) conducted the evaluation. In late 2002, the program expansion was approved for three more years with a budget of U.S. \$20 million.

I. The *Red de Protección Social*

Program Design

The pilot phase of RPS was implemented in two stages. In the first, the program benefited all of the approximately 6,000 households in 21 so-called census “comarcas” (hereafter *comarcas*).² The *comarcas* were selected from six municipalities in the northern part of the Central Region of Nicaragua. In the second stage, approximately 4,000 additional beneficiary households from different *comarcas*, but the same six municipalities, were selected using household-level targeting mechanisms. This chapter examines education- and child labor-related effects of the program on beneficiaries during the first year of the first stage of the pilot phase in which only geographic, *comarca*-level targeting was used.

RPS had two main components:

Health, nutrition, and food security: Each eligible household received a bimonthly cash transfer known as the *bono alimentario*, contingent upon attendance at bimonthly educational workshops and on bringing their children under age 5 for scheduled healthcare appointments.

To ensure adequate supply in these poor, rural communities, RPS trained (and paid) nongovernmental organizations to provide the healthcare services. These services were provided free of charge to beneficiary households. In the workshops, mothers were trained in household sanitation and hygiene, nutrition, reproductive health, and breastfeeding. Other services were directed toward children, and included growth monitoring, vaccination and provision of anti-parasites, vitamins, and iron supplements. Children under age 2 were seen monthly while those between 2 and 5 were monitored bimonthly.

Education: RPS gave each beneficiary household a bimonthly cash transfer known as the *bono escolar*, contingent upon matriculation and regular school attendance. Additionally, for each eligible child, the household received an annual cash transfer intended for school supplies (including uniforms and shoes) known as the *mochila escolar*, and contingent upon matriculation. Unlike the *bono escolar*, which was a fixed amount per household (regardless of the number of children in school), the *mochila escolar* was a per-child transfer.

In rural Nicaragua, it is common for schools’ parents’ associations to request contributions to support the teacher and the school. Therefore, there was also a small cash transfer, known as the *bono a la oferta*, to cover this contribution. In practice, this token amount was given to each beneficiary child, who in turn delivered it to the teacher. The teacher could then keep one-half while the other half was earmarked for the purchase of additional school supplies. Only the delivery of the funds to the teacher was monitored.

Table 1 summarizes the eligibility requirements, demand and supply side benefits, and conditions or “co-responsibilities” for the different components of RPS.

In the *comarcas* where there was only geographic targeting, all households were eligible for the *bono alimentario*, and the transfer was a fixed amount per household. 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 amounts for each transfer were initially determined in U.S. dollars and then converted into Nicaraguan Córdobas in September 2000, just before they began distributing the

bonos. Table 1 shows the original U.S. dollar annual amounts and their Córdoba equivalents. On its own, the *bono alimentario* 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 receive additional transfers of about 8 percent, yielding a total transfer of approximately 21 percent of total annual household expenditures.³ This is the same percentage as the average transfer in PROGRESA, but about twice as large as the transfers given in PRAF. In contrast to PROGRESA and PRAF, which index transfers to inflation, the nominal value of the transfer remained constant for RPS, with the consequence that due to inflation the real value of the transfers declined about 7 percent during the first year.

To enforce compliance with program requirements, beneficiaries did not receive a transfer when they failed to carry out any of the conditions shown in Table 1. During the first year of operation, about 10 percent of beneficiaries were penalized at least once and therefore did not receive a (full) transfer. The program allowed households to receive a partial transfer if they complied with the health requirement and not the education requirement or vice versa. It was also possible for households to be removed from the program. Causes for expulsion included 1) failure to collect the transfer in two consecutive pay periods, 2) more than 27 unexcused school absences during the school year for a single child, 3) failure of a student in the program to be promoted to the next grade, 4) falsifying information during any part of data collection, or 5) falsely reporting fulfillment of co-responsibilities. Only a handful of households were expelled from the program during the first year of operation. When it was learned that some, but not all, schools practiced automatic promotion, enforcement of the grade promotion condition was deemed unfair and it was not enforced. This change highlights the importance of careful consideration of co-responsibilities and their monitoring in the design of a conditional cash transfer program, as well as flexibility during its implementation.

[TABLE 1 ABOUT HERE]

Only the designated household representative could collect the cash transfers, and where possible, RPS designated the mother as the household representative. This strategy mimicked the design of PROGRESA and PRAF and is based on evidence that resources in the hands of women often lead to better outcomes for child well-being and household food security (Strauss and Thomas 1995). As a result, more than 95 percent of the household representatives were women. These representatives attended the health workshops and were responsible for ensuring that the other co-responsibilities were fulfilled.

With its multisectoral approach, RPS required bureaucratic cooperation at the national, municipal, and community levels. Given funding and administrative direction from the national Emergency Social Investment Fund (FISE), municipal planning and coordination was conducted by committees composed of delegates from the health and education ministries, representatives from civil society, and RPS personnel. This coordination proved important in directing supply-side responses to increased household demand for health and schooling services. At the *comarca* level, RPS representatives worked with local representatives known as *promotoras* (beneficiary women chosen by the community) and local school and healthcare service providers, to implement the program. The volunteer *promotoras* were charged with keeping beneficiary

household representatives informed about upcoming healthcare appointments for their children, upcoming payments, and any failures in fulfilling the conditions.⁴

Program Targeting and Design of the Evaluation

Rural areas in all 17 departments of Nicaragua were eligible for the pilot phase. The focus on rural areas reflects the distribution of poverty in Nicaragua—of the 48 percent of Nicaraguans designated as poor, 75 percent reside in rural areas. The government selected the departments of Madriz and Matagalpa for the pilot on the basis of need and their capacity to implement the program. Approximately 80 percent of the rural population of Madriz and Matagalpa was poor, and half of those extremely poor (IFPRI 2002b). In addition, these departments had easy physical access and communication, relatively strong institutional capacity and local coordination, and reasonably good coverage of health posts and schools in the majority of their poor communities (Arcia 1999). The Nicaraguan education system consists of six years of primary school and four years of secondary school. In rural areas, however, it is common to find schools that provide only four years of primary and in some cases fewer. By purposively targeting, RPS could avoid devoting a disproportionate share of its resources during the pilot to increasing the supply of educational and health services.

In the next stage of geographic targeting, six out of 20 municipalities were chosen within the selected departments for their participation in a FISE-run participatory development program.⁵ The goal of that program was to develop the capacity of municipal governments to select, implement, evaluate, and monitor social infrastructure projects such as school and health-post construction. Therefore, it is possible that the municipalities had atypical capacity to carry out RPS. The municipalities were also selected such that there were three each under the control of the two dominant political parties. Nevertheless, these municipalities were appropriately targeted on the basis of poverty. Between 36 and 61 percent of the rural population in each of the chosen municipalities was extremely poor and between 78 and 90 percent was poor (IFPRI 2002b). Though they were not the poorest municipalities in the chosen departments, the proportion of impoverished people living in these areas was certainly higher than the national average.

In the next stage of geographic targeting, a marginality index based on information from the 1995 National Population and Housing Census was constructed for all 59 rural *comarcas* in the selected municipalities. The index was a weighted average of a set of poverty indicators in which higher index scores were associated with more impoverished areas (World Bank 1995; Arcia, Mendoza, and Iachan 1996). The indicators (and their associated percentage weights) were family size (10 percent), access to potable water (50 percent), access to latrines (30 percent), and illiteracy rates (10 percent) (Arcia 1999). The 42 *comarcas* with the highest scores were selected for the pilot phase's first stage. These *comarcas* were ordered by their marginality index scores and stratified into seven groups of six each. Three *comarcas* from each group were randomly selected for inclusion in the program, leaving the other three as controls for the evaluation. Thus, there were 21 *comarcas* selected in the intervention group and 21 distinct *comarcas* with similar levels of poverty in the control group. IFPRI (2001a) describes the design of this social experiment, known as a community-based randomized trial.

II. Methodology and Data Sources

Methodology

To measure program impact, it is necessary to know what would have happened had the program not been implemented. The problem, of course, is that a single area, household, or individual cannot simultaneously undergo and not undergo the intervention. To measure impact, then, we construct a counterfactual measure of what would have happened if the program had not been available. The most powerful way to construct a valid counterfactual is to randomly select beneficiaries from a pool of equally qualified candidates. Recipients and non-recipients will have both the same observed and unobserved characteristics, on average. A further advantage to such a randomized design is that the program impact is easy to calculate and, as a consequence, easy to understand.

Household and individual level data were collected before and after RPS was implemented in both the intervention and control *comarcas*. This enables the use of the double-difference method to calculate “average program impact.” The resulting measures can be interpreted as the expected effect of implementing the program in a similar population elsewhere. The method is illustrated in Table 2. The columns distinguish between groups with and without the program (denoted by I for intervention and C for control) and the rows distinguish between before and after the program (denoted by subscripts 0 and 1). Anticipating the analyses presented below, consider the measurement of school matriculation rates for children. Before the program, we would expect the average percentage enrolled to be similar for the two groups, so that the quantity $(I_0 - C_0)$ would be close to zero. After the program has been implemented, however, we would expect differences between the groups as a result of the program. Furthermore, because of the random assignment, we expect the difference $(I_1 - C_1)$ to measure the effect directly attributable to the program. Indeed, $(I_1 - C_1)$ is a valid measure of the average program impact under this experimental design. A more robust measure of the effect, however, would account for any preexisting differences between the two randomly assigned groups: this is the double difference obtained by subtracting any preexisting differences between the groups, $(I_0 - C_0)$, from the difference observed after the program has been implemented, $(I_1 - C_1)$.

[TABLE 2 ABOUT HERE]

An alternative interpretation of the double-difference estimator emerges if one first considers the differences *within* the (intervention or control) groups. This approach begins with a naïve estimator of the program effect, the difference over time for the intervention group, $(I_1 - I_0)$. This is naïve because it would include all changes over time in matriculation rates, regardless of what is causing them. For example, if increases in public investment improving school access were leading to changes in matriculation, these changes should not be attributed to the program. The obvious measure for the non-program related change over time in the intervention group is the change over time in the control group, $(C_1 - C_0)$. Thus we estimate the average program impact by first considering the overall change over time in the intervention group, and then subtract from this the change over time in the control group. As above, this yields the double-difference estimator.⁶

Before presenting the estimated effects of the program, there are two important aspects of RPS that need to be taken into account for their interpretation. The first is that the program was in its pilot phase, and as such the outcomes for the pilot may differ from the outcomes for the expanded program. Like most pilots, RPS underwent an initial learning period (with its attendant setbacks) and undertook a variety of activities that would not need to be repeated in an expansion (e.g., preparing training materials) possibly reducing its effectiveness. At the same time, the selection of municipalities was conditioned on the likelihood of success as described above, so that the observed outcomes might exaggerate the likely outcomes from program expansion to other areas. Moreover, as with any new program, there was the potential for observed behavioral changes to result, in part, from the novelty of the program—the Hawthorne effect. Finally, expansion of the program could introduce new advantages and disadvantages associated with scaling up and economies of scale. All these factors suggest a degree of caution in forecasting what would happen were the program to be extended to other municipalities and departments.

A second feature important for the interpretation of the results is the design of RPS, which provided a “package” of services in which all households were eligible for the *bono alimentario* regardless of whether they also benefited from the educational transfers. Therefore, it is not possible to isolate the effects of the education component of the program; all the observed effects, even those that pertain specifically to educational outcomes, are the result of the program as a whole.

Data Sources

There are two household surveys used in this analysis: the RPS baseline (2000) and the RPS follow-up (2001). Both were conducted in all 42 intervention and control *comarcas*. The baseline survey was carried out before the start of the program, in August and early September 2000. It was a stratified random sample of 1,585 households, approximately 13 percent of the household population in the study area. In October 2001, the follow-up survey revisited all the original baseline households, successfully re-interviewing 1,494 households (94 percent).⁷ In the double-difference analyses that follow, all (relevant) households from each survey round, regardless of whether they were interviewed in both waves, were included. Furthermore, the stratified sample design, which can be corrected for statistically using sample weights, was ignored; correcting for these and other statistical concerns having to do with the randomization procedure yielded no substantive differences in the results.

III. Results

Schooling and Child Labor at Baseline

Before the start of RPS, the matriculation rate in the program area for the target group, those aged 7–13 who had not yet completed fourth grade of primary school, was 71 percent. This overall average, while demonstrating a large potential for improved outcomes, masked important differences by age of the child and level of household well-being. Figure 1a shows matriculation rates by age in the completely shaded portion of the bars.⁸ (The dotted areas represent the impact of RPS and will be discussed in the next subsection.) For the targeted children, matriculation peaks at 82 percent for nine-year olds but declines to 51 percent by age 13. Thus, even at its

peak, there was substantial room for improvement. In addition, the (initially rising) age pattern indicated that, of those children who eventually attend school, many start late. A possible effect of the program would be not only to increase overall attendance but also to improve appropriate-age starts.

[FIGURE 1 ABOUT HERE]

Figure 1b shows the matriculation rates for the same children by household expenditure group (extremely poor, poor, and nonpoor)⁹ and by gender. These simple comparisons indicate that resources play a role in the decision to enroll children. Indeed, children living in households in the lowest per capita expenditure decile in the sample were more than one-third less likely to have matriculated than those living in the wealthiest decile (not shown). Although not controlling for the many other factors that affect enrollment, this evidence still suggests there was potential for a cash transfer program to influence matriculation rates. There were no differences between the matriculation rates for boys and girls.

Figures 2a and 2b show very similar patterns for continued enrollment in the baseline survey, collected approximately three months before the end of the academic year. A child is defined to be continuing enrollment if he indicated he was still enrolled and had either missed three or fewer days in the past month or had missed more, but due to illness. As with matriculation, continued enrollment rises to age nine and declines thereafter. The percentage of children still in school toward the end of the academic year was on average 12 percentage points lower than the percentage matriculating, indicating that dropout was common. Once again, it is evident that there was substantial room for improvement in this education indicator. Finally, the large advantage in matriculation for children in nonpoor households did not seem to carry through to continued enrollment (Figure 2b). Hence, even children from wealthier households stood to benefit from the program.¹⁰

[FIGURE 2 ABOUT HERE]

Figure 3 shows the extent to which children were working before the start of RPS as the entire bar with the total percent indicated above the bar. All individuals were asked whether work was their primary activity in the previous week and, if not, why it was that they did not work. The possible reasons for not working most relevant for children were that they were in school or that they were disabled. If the primary activity was not work, the child was further prompted about other activities in the previous week. The child was considered to be working if work was a primary activity or secondary activity, with positive hours worked. The vast majority of child workers were agricultural laborers or unskilled helpers, and typically worked without pay.

While children under age 10 rarely reported working, from age 10 upward they were increasingly likely to work; 45 percent of 13 year olds in the sample reported working (Figure 3a). Average hours worked also increased with age (not shown). There was no obvious relationship between working and the economic well-being of the household, however (Figure 3b). This undoubtedly reflects the likelihood that child labor increases household expenditures, our measure of well-being. Boys were substantially more likely to report working. By age 13,

only one-quarter of the girls reported working compared to nearly 60 percent of the boys. Conditional on working, boys also worked longer hours, averaging 25 hours per week compared to 16 hours for girls. Given the questionnaire's orientation toward economically productive activities outside the home, the difference between boys' and girls' reported work might reflect in part the underreporting of girls' domestic activities within the home.

[FIGURE 3 ABOUT HERE]

With these basic facts describing the pre-RPS conditions in the study areas, I now examine how the program has changed the landscape.

Double-Difference Results on Schooling and Child Labor

RPS induced an average net increase in matriculation of 22 percentage points for the target population of children ages 7–13 who had not yet completed the fourth grade of primary school (Table 3). Before the program, matriculation rates in intervention and control areas for this age group were very similar with approximately 70 percent of eligible children matriculating. With the program, matriculation rose to nearly 95 percent.

[TABLE 3 ABOUT HERE]

As a first step toward examining what underlies the average impact of 22 percentage points, I consider the effect of the program by age. The results are shown in Figure 1a, in which the bottom, completely shaded portion of each column is the initial situation described earlier, and the dotted top portion is the double-difference estimated average program impact. In all cases, this impact was positive.¹¹ With the exception of those aged 11, the program impact was statistically significant for each year and tended to be larger where there was more potential at the outset, i.e., where the initial matriculation levels were lower.¹² Matriculation rates in the intervention areas are now between 90 and 100 percent and no longer vary by age.

Figure 1a shows that gains were made in matriculation by reaching both younger children, who for the most part had not yet attended school, as well as older children, who had completed some schooling but had abandoned it before the program started. A potential concern for the latter group of older children was that they were returning to the first two grades. If so, this would lead to more mixing of younger and older children in the same grade with classroom disruption a possible consequence. Nearly all (80 percent) of the overall improvement in matriculation came from younger children, however, and most of the older children who returned to school were returning to the third and fourth grades. Moreover, both the average and standard deviation of child age-by-grade remained constant before and after the program, indicating little change in overall classroom composition. Figure 1b presents results for matriculation and current enrollment by household expenditure group and gender. Clearly the extreme poor and poor are benefiting most. The effects for boys and girls were identical.

The effect of the program on continued enrollment was even larger than that on matriculation, with an average program impact of 29 percentage points. The impact was again

significant for all ages except 11, and the effects were generally larger for those age groups with lower initial current enrollment rates (Figure 2a). As with matriculation, the extreme poor and poor benefited the most (Figure 2b). Nonetheless, the nonpoor also experienced significant gains in current enrollment.

The final aspect of the effect of RPS on education I consider is grade progression—the percentage of students in each grade who continue in school from one year to the next. Unlike the matriculation and enrollment results just presented, the effect of RPS on grade progression is measured as a first difference, because two years of information are required to calculate progression. The estimated impact is thus the difference between the percent of students continuing in the intervention areas minus the percent in the control areas. Overall, and by grade (except for third to fourth), the effects are significant and show an average improved retention rate of 8 percent (Table 4). An unanticipated additional benefit of the program was the large effect on those making the transition from fourth to fifth grade. This was in spite of the fact that matriculation in the fifth grade was not one of the conditions for receiving the education transfer. Unfortunately, it is not presently possible to determine why this is occurring. On the one hand, it may be due to potentially long-lasting changes in attitudes toward education. On the other hand, it may merely reflect confusion about the program requirements on the part of beneficiaries. Examining grade progression for all four grades at once for the different expenditure groups (Table 5) shows that as with the other measures, the largest effects of the program were concentrated among the extreme poor.

[TABLES 4 & 5 ABOUT HERE]

RPS had a massive effect on matriculation and enrollment in the intervention areas. Even though only about one-third of the rural *comarcas* in each municipality were included in the pilot phase, increases in matriculation could be seen even in the aggregate municipal-level data compiled by the Ministry of Education. In the six municipalities combined, there was an increase of about 5 percent in matriculation in grades one to four between 1999 and 2000 before the program. The increase was nearly 18 percent between 2000 and 2001, far higher than what occurred in the rest of the country for that period.

While schools were generally available in the program area due to the targeting described above, a number of steps were taken to accommodate the large changes in matriculation as the program developed. The two principal ones were increasing the number of sessions per day and increasing the number of teachers. RPS supported local communities in their efforts to solicit additional teachers from the Ministry of Education. For most rural schools, this was a straightforward process, because they operate under an autonomous system with substantial local control.¹³ In one RPS municipality with a smaller proportion of autonomous schools, however, it was more difficult to increase the number of teachers. In some cases, this problem was resolved when beneficiary parents agreed, on the suggestion of RPS, to contribute part of their transfers to help pay for a new teacher. In other cases, staffing problems were not resolved. Probably reflecting these problems, matriculation rates were the lowest in this municipality, though they were still 90 percent on average. In sum, the overall level of matriculation left little room for improvement, and supply does not appear to have been a major constraint. This achievement, however, required active intervention and coordination on the part of RPS.

Among those not matriculating, economic reasons were cited in nearly half the cases, and work was specifically cited for about 10 percent of cases. For those who dropped out during the year, work was cited as the main cause 20 percent of the time. The need to work plays a role in schooling decisions, though apparently not the dominant one.

I now examine whether the implementation of RPS reduced child labor for the target population children. It has. Figure 3a shows the effect of RPS on the percentage of children working by age in the intervention areas.¹⁴ The dotted portion of each column represents the *reduction* in those reporting work after one year of the program. For every age group, the percentage of children working was lower after the program. Only in the case of 12 and 13 year olds did the program significantly decrease the percentage of children working, however. This is not surprising since for the younger age groups, very few were working to begin with, so that while there appear to have been changes of 50 percent or more, it was not possible to estimate them precisely. The double difference estimator shows a significant 9 percentage-point decrease in the number of children working when restricted to ages 10–13 (Table 6). Note that in both the intervention and control areas, the percentage of children working declined significantly. This likely reflects a general economic downturn in the program area as the result of severe drought and depressed international coffee prices, both of which led to substantial declines in well-being within the control group (IFPRI 2002a). The percentage of children working declined about the same amount within each of the three expenditure groups (Figure 3b). Finally, while the impact on education outcomes was the same for boys and girls, the impact on reported work for boys was three times as large as for girls (Figure 3b).¹⁵

[TABLE 6 ABOUT HERE]

Not only did the percentage of children reporting work decrease, but also for those who did work, hours worked declined substantially. The double-difference calculation (not shown) indicates a significant average decrease of about 10 hours of work in the previous week (for both boys and girls) yielding an average of 15 hours of work per week. For those who continued to work, they did so less intensively, permitting time for schooling.

Another way to examine changes induced by the program is consider the percentages of children who were 1) schooling exclusively, 2) working exclusively, 3) both schooling and working, or 4) neither schooling nor working, before and after the program. Figure 4 shows the distribution of children among these categories before and after the program (for the intervention areas only). For comparison, among children between 10 and 14 in rural Nicaragua in 1998, 69 percent were exclusively schooling, 7 percent exclusively working, 9 percent doing both, and 15 percent doing neither (World Bank 2001, Annex 25). Exclusive schooling increased substantially (from 59 to 84 percent) with RPS at the expense of the other categories, in particular at the expense of doing neither, i.e., children who were not economically active nor in school before the program (Figure 4b). This finding is consistent with these children having lower opportunity costs of time than those who had been working. Lastly, while schooling in general rises with the program, it is exclusive schooling that saw the largest gains.

[FIGURE 4 ABOUT HERE]

IV. Conclusion

There is no doubt that RPS had a significant and substantial effect on schooling matriculation and enrollment during its first year of operation in 2001. The evidence presented in this chapter, based on a randomized community-based trial carried out to evaluate the program, provides overwhelming support for the effectiveness of the program. Moreover, all signs are that at the same time the program also led to a substantial reduction in child labor within the target population. It is therefore possible for a cash transfer program aimed at education to reduce child labor, even during a general economic downturn such as occurred during this time.

The preponderance of evidence and the experimental design also suggest that if the program were expanded elsewhere in Nicaragua, it would be equally successful. It would be unwise, however, to presume that the changes observed in the pilot would be replicated exactly as presented here. Scaling up from a pilot program could introduce a variety of potential differences in the outcomes. In particular, the heavy involvement of RPS in facilitating the supply-side response in education would need to be continued or made unnecessary, perhaps by improving coordination between sectors.

At this writing, the program, while remaining small-scale, is about to expand in a somewhat altered form and with smaller transfers. Despite initial reluctance, the newly elected Nicaraguan government supported this expansion and is making RPS a more integral part of its poverty reduction program. Arguably a key factor behind this support has been the fact that RPS carried out a formal evaluation and was able to clearly demonstrate the success of the program. Another reason for this support is that the program influences several of the key indicators embedded in Nicaragua's Poverty Reduction Strategy.

There remain a number of questions regarding the efficacy of the program, two of which I mention here. The first has to do with the medium- to long-term effects of the program. Will the outstanding results be upheld over time during the life of the program and, perhaps more importantly, after the program exits? Currently the program is designed to last three years in a beneficiary community, after which the demand-side incentives cease. Second, a careful consideration of the cost effectiveness of the program—enabling comparison with other approaches—will be a necessary next step.

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Table 1 – RPS eligibility, benefits, and co-responsibilities

	Health, Nutrition, and Food Security (<i>bono alimentario</i>)	Education (<i>bono escolar</i>)	
			(<i>mochila escolar</i>)
Eligibility	All households	All households with children ages 7–13 who have not completed fourth grade of primary school	Each child aged 7–13 who has not completed fourth grade of primary school
Transfers (demand side)			
Scheduled transfer	C\$480 bimonthly per household all year (US\$37)	C\$240 bimonthly per household all year (US\$19)	C\$275 per child at beginning of school year (US\$21)
Expected annual transfer	C\$2,880 (US\$224)	C\$1,440 (US\$112)	C\$275 (US\$21)
Transfers and services provided (supply side)			
Teacher/school transfer (<i>bono a la oferta</i>)	-	C\$10 bimonthly per beneficiary student delivered by student to teacher. Teacher keeps half and remainder purchases of school supplies (C\$60 or US\$4.75 annually)	-
Health education workshops	Bimonthly	-	-
Child growth monitoring	Monthly (0–2 years) Bimonthly (2–5 years)	-	-
Vaccinations	According to Minsa guidelines	-	-
Co-responsibilities (conditions) for receiving transfer			
	1. Attend bimonthly health education workshops	1. Matriculation	1. Matriculation
	2. Bring children to prescheduled healthcare appointments	2. Regular attendance (85 percent, i.e., no more than five absences every two months without valid health reason)	-
	3. Adequate weight gain for children under 5	3. Deliver <i>bono a la oferta</i> to teacher	-

Table 2 – Calculation of the double-difference estimate of average program effect

	Intervention group with RPS program	Control group without RPS program	Difference across groups
Follow-up (2001)	I_1	C_1	$I_1 - C_1$
Baseline (2000)	I_0	C_0	$I_0 - C_0$
Difference across time	$I_1 - I_0$	$C_1 - C_0$	Double-difference $(I_1 - C_1) - (I_0 - C_0)$

Table 3 – RPS average impact on matriculation for 7- to 13-year-olds who have not completed fourth grade

	RPS	Control	Difference
Follow-up (2001)	94.5 [880]	76.4 [852]	18.1*** (3.1)
Baseline (2000)	69.2 [967]	73.0 [886]	-3.8 (5.2)
Difference	25.4*** (3.4)	3.4 (1.9)	22.0*** (3.9)

Source: RPS baseline (2000) and Follow-up (2001). Standard errors correcting for heteroskedasticity are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level and ** at the 5 percent level.

Table 4 – RPS average impact on percentage of students ages 7–13 continuing in school, by grade

	Grade 1 to grade 2	Grade 2 to grade 3	Grade 3 to grade 4	Grade 4 to grade 5
RPS	96.0 [346]	95.6 [159]	95.0 [141]	91.7 [121]
Control	87.8 [336]	88.3 [197]	88.8 [125]	79.6 [98]
Difference	8.2*** (2.1)	7.3*** (2.8)	6.2 (3.4)	12.1** (4.8)

Source: RPS baseline (2000) and Follow-up (2001). Standard errors correcting for heteroskedasticity are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level and ** at the 5 percent level.

Table 5 – RPS average impact on percent of students ages 7–13 continuing in school, by expenditure group

	Extreme poor	Poor	Nonpoor
RPS	94.2 [326]	95.4 [328]	96.5 [113]
Control	84.9 [410]	84.4 [251]	92.6 [95]
Difference	9.3*** (2.2)	6.9** (2.3)	3.8 (3.2)

Source: RPS baseline (2000) and Follow-up (2001). Standard errors correcting for heteroskedasticity are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level and ** at the 5 percent level.

Table 6 – RPS average impact on percent working of 10- to 13-year-olds who have not completed fourth grade

	RPS	Control	Difference
Follow-up (2001)	9.3 [397]	17.8 [411]	-8.4*** (2.6)
Baseline (2000)	27.1 [480]	27.8 [443]	-0.6 (3.9)
Difference	-17.8*** (2.7)	-10.0*** (2.6)	-8.8** (3.7)

Source: RPS baseline (2000) and Follow-up (2001). Standard errors correcting for heteroskedasticity are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level and ** at the 5 percent level.

Figure 1a – RPS average impact on matriculation for 7- to 13-year-olds who have not completed fourth grade, by age

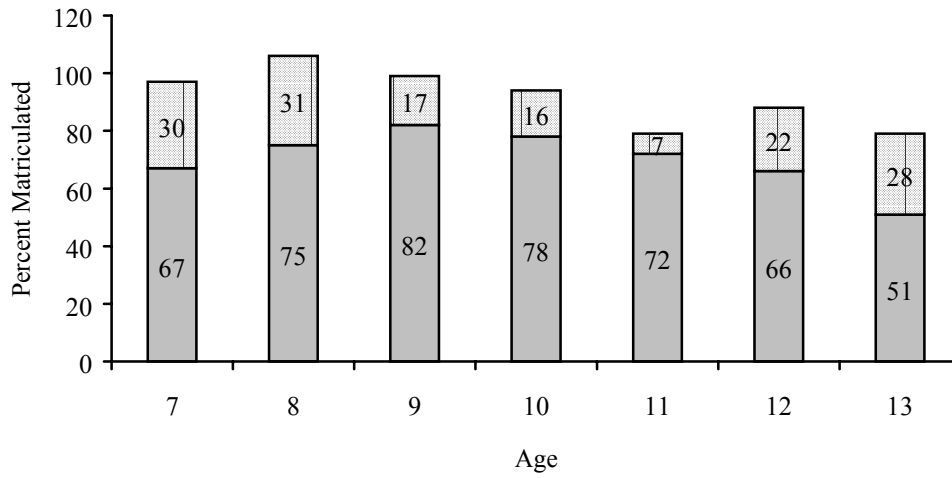


Figure 1b – RPS average impact on matriculation for 7- to 13-year-olds who have not completed fourth grade, by expenditure group and by gender

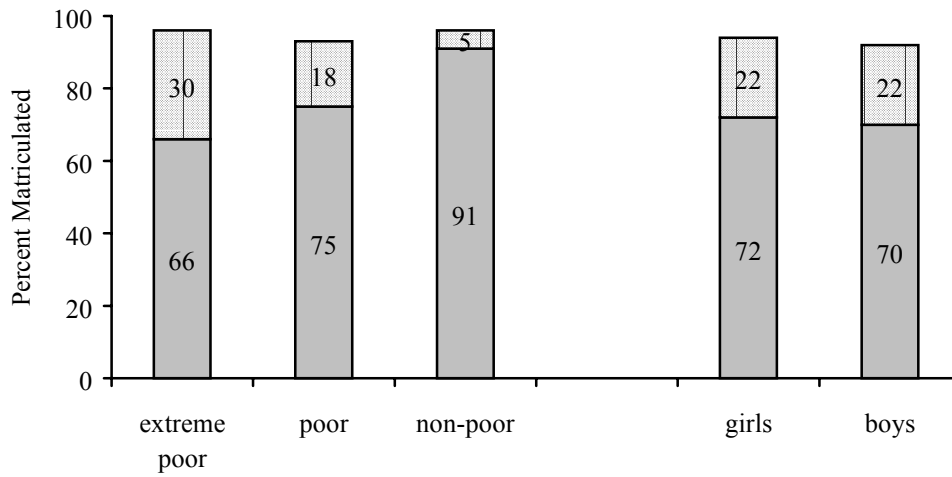


Figure 2a – RPS average impact on current enrollment for 7- to 13-year-olds who have not completed fourth grade, by age

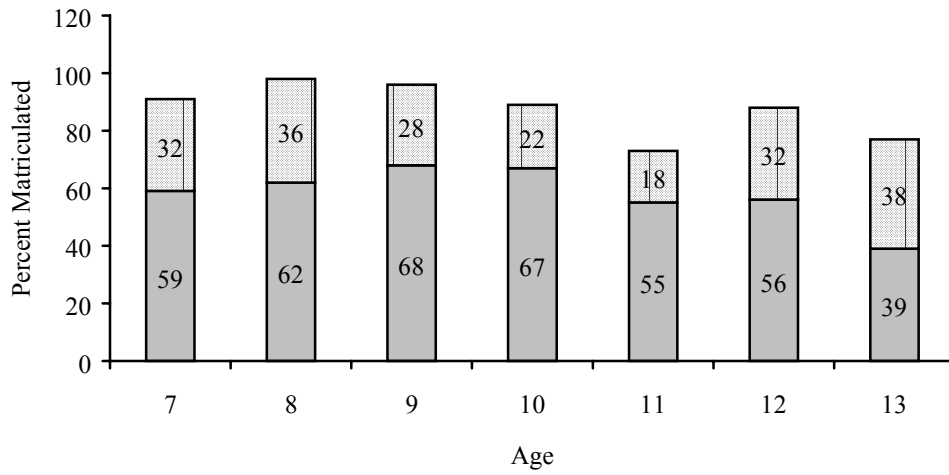


Figure 2b – RPS average impact on enrollment for 7- to 13-year-olds who have not completed fourth grade, by expenditure group and by gender

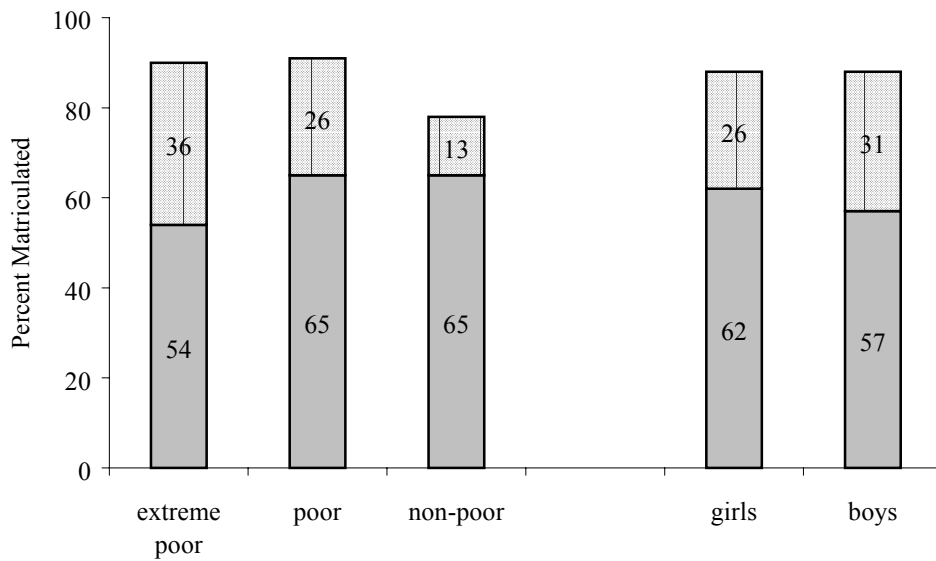


Figure 3a – RPS average impact on work for 7- to 13-year-olds who have not completed fourth grade, by age

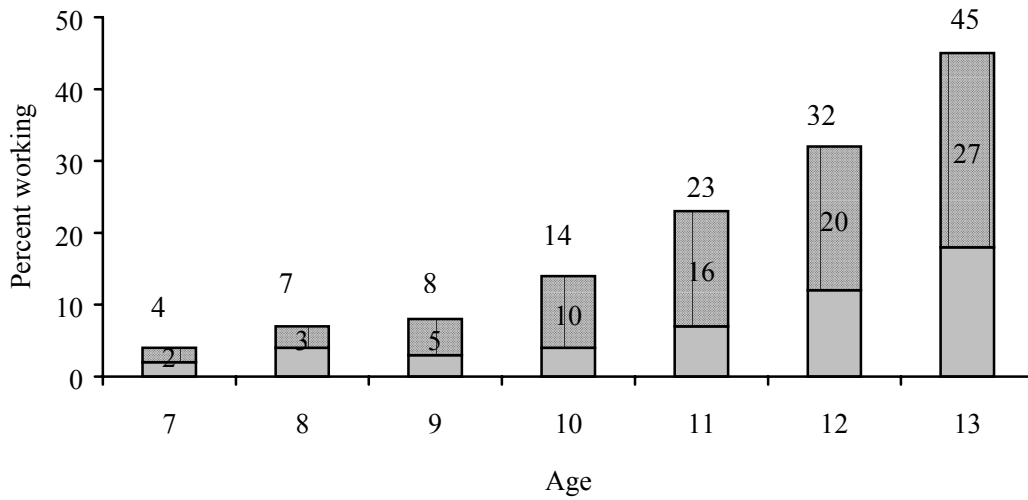


Figure 3b – RPS average impact on work for 7- to 13-year-olds who have not completed fourth grade, by expenditure group and by gender

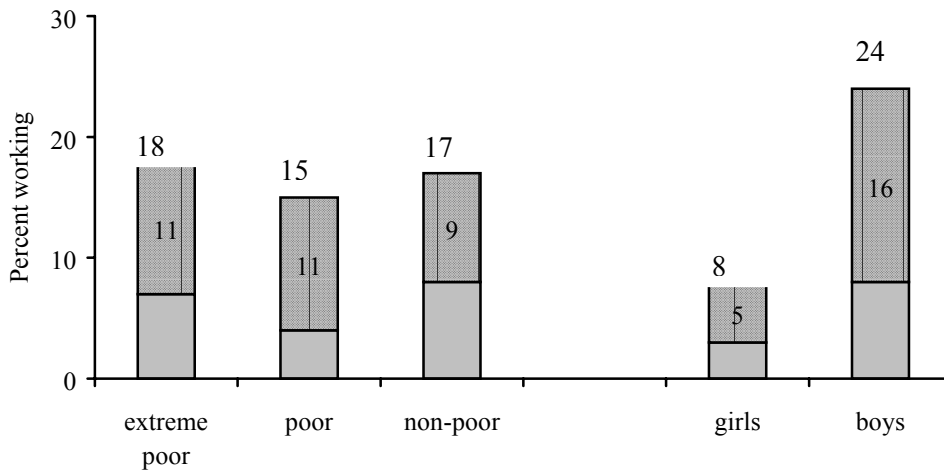


Figure 4a – Schooling and work for 7- to 13-year-olds, RPS baseline intervention areas only

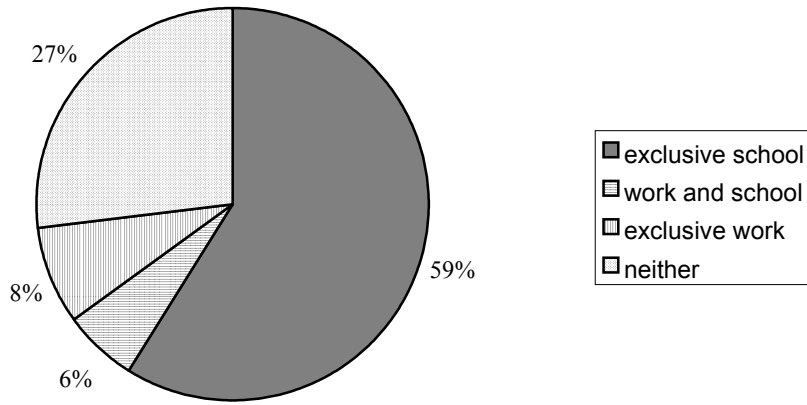
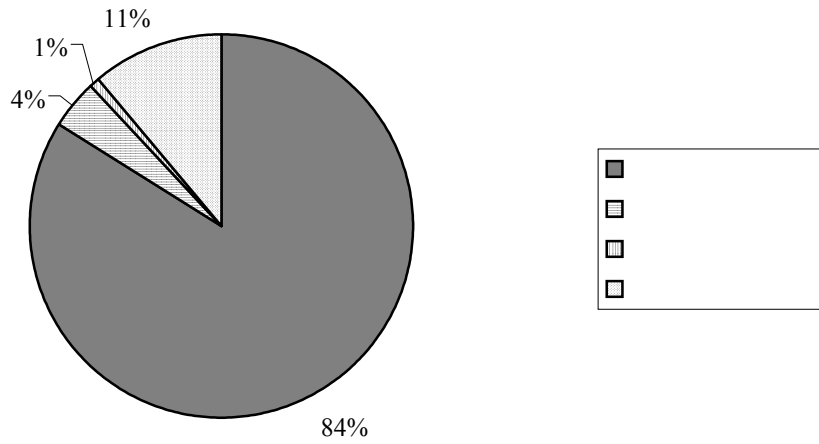


Figure 4b – Schooling and work for 7- to 13-year-olds, RPS follow-up intervention areas only



¹ Skoufias and McClafferty (2001) present an overview of PROGRESA and its impacts and IFPRI (2000) provides background on PRAF.

² Loosely speaking, census comarcas are administrative areas within municipalities that include between one and five small communities averaging 100 households each.

³ The percentage of total annual household expenditures is lower, however, when one considers the national average (13 percent) or the national rural average (18 percent) because of the targeting of the program to poorer areas, described later in the text.

⁴ The loan proposal (IDB 2000) outlines in more detail each component as well as the program's operating structure.

⁵ The six were Totogalpa and Yalagüina municipalities in the department of Madriz, and Terrabona, Esquipulas, El Tuma-La Dalia, and Ciudad Darío municipalities in the department of Matagalpa.

⁶ Ravallion (2001) provides a useful, and enjoyable, discussion on this and related evaluation tools.

⁷ IFPRI (2001a) describes the sample size calculation and IFPRI (2001b and 2002a) describe the baseline and follow-up samples in more detail.

⁸ That is, the (weighted) average of I_0 and C_0 .

⁹ Households are classified into poverty groups based on the per capita annual total expenditures (including own-production) and using the 2001 updated Nicaraguan poverty lines. The extreme poverty line is C\$2,691 per capita per annum (US\$202) and the poverty line is C\$5,157 (US\$386). IFPRI (2001b and 2002b) provide details.

¹⁰ An important consideration is that even the nonpoor children in this sample are generally in the bottom two-thirds of the Nicaraguan income distribution and so are near-poor.

¹¹ Note that while a convenient way of summarizing the program effects, it is not possible to interpret the sum of the two parts of each column as the matriculation rate after the program. This fact becomes evident, for example, in the sub-group of eight year olds, for whom the sum is higher than 100 percent.

¹² The smaller estimated effect for age 11 (and subsequent lack of significance) is due to an imbalance at the outset in which the intervention areas had a somewhat larger matriculation rate that then translates into a smaller estimated effect in the double-difference. It is therefore unlikely that it reflects any behavioral responses to the program.

¹³ In the early 1990s, a school reform was undertaken to devolve control from the central government to local schools or, in some rural areas, clusters of schools (King, Ozler, Rawlings 1999).

¹⁴ The estimated effects on work by age are first differences ($I_1 - C_1$). Because of the small percentages of young children who are working, precise estimation of double differences was not possible for all but the higher ages, as described later in the text.

¹⁵ This finding, like the finding that boys were more likely to work than girls described earlier, surely reflects in part how the questions about work were designed.