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IMPACT EVALUATION OF A CONDITIONAL CASH TRANSFER PROGRAM: THE NICARAGUAN *RED DE PROTECCIÓN SOCIAL*

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Abstract

This paper presents the main findings of a quantitative evaluation of the *Red de Protección Social* (RPS), a conditional cash transfer program in Nicaragua, against its primary objectives. These included supplementing income to increase household expenditures on food, reducing primary school desertion, and improving the health care and nutritional status of children under age 5. The evaluation design is based on a randomized, community-based intervention with measurements before and after the intervention in both treatment and control communities. Where possible, we erred on the side of assessing effects in conservative manners, for example, in the calculation of standard errors and the treatment of possible control group contamination. Overall, we find that RPS had positive (or favorable) and significant double-difference estimated average effects on a broad range of indicators and outcomes. Where it did not, it was often due to similar, smaller improvements in the control group that appear to have been stimulated indirectly by the program. Most of the estimated effects were larger for the extreme poor. The findings presented here played an important role in the decision to continue this effective program.

Contents

Tables

Appendix B Table

Figures

Acknowledgments

This research began under the formal evaluation of the Nicaraguan *Red de Protección Social* by the International Food Policy Research Institute and in part draws from reports prepared under that project. We thank the *Red de Protección Social* team, particularly Tránsito Gómez, Caroll Herrera, and Mireille Vijil, for continued support during the evaluation. We thank Natàlia Caldés, Oscar Neidecker-Gonzalez, and Jane Rhode for research assistance, and David Coady, Alexis Murphy, Ferdinando Regalía, Marie Ruel, and Lisa Smith for many helpful comments.

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1. Introduction

In recent years, increasing emphasis has been placed on the importance of human capital in stimulating economic growth and social development. Consequently, investing in the human capital of the poor is widely seen as crucial to alleviating poverty, particularly in the long term. There is also growing recognition of the need for social safety nets to protect poorer households from poverty and its consequences during the push for economic growth (World Bank 1997). While at first glance stimulating economic growth and investing in social safety nets are apparently different strategies for economic development, both are important. They are also potentially complementary, as effective social safety nets may directly contribute to economic growth via improved human capital, particularly in the long term (Morley and Coady 2003).

Consistent with this view, several Latin American countries have introduced programs that integrate investing in human capital with access to a social safety net. One reason for the growing popularity of these programs is that by addressing various dimensions of human capital, including nutritional status, health, and education, they are able to influence many of the key indicators highlighted in national poverty reduction strategies. One of the first, and largest, programs was the *Programa Nacional de Educación, Salud y Alimentación* (PROGRESA) in Mexico, begun in 1997. Another large program is the *Programa de Asignación Familiar* (PRAF) in Honduras. This paper examines a third, the Nicaraguan *Red de Protección Social* (RPS) or "Social Safety Net."

The primary objective of these programs is to generate a sustained decrease in poverty in some of the most disadvantaged regions in their respective countries. Their basic premise is that a major cause of the intergenerational transmission of poverty is the inability of poor households to invest in the human capital of their children. Supply-side interventions, which increase the availability and quality of education and health services, are often ineffective in resolving this problem, since the resource constraints facing poor households preclude them from incurring the private costs associated with utilizing these services (e.g., travel costs and the opportunity cost of women's and children's time).

These programs attack this problem by targeting transfers to the poorest communities and households and conditioning the transfers on actions intended to improve children's human capital development. This effectively transforms cash transfers into human capital subsidies for poor households.

Modeled after PROGRESA, RPS is designed to address both current and future poverty via cash transfers targeted to households living in poverty in rural Nicaragua. The transfers are conditional, and households are monitored to ensure that children are, among other things, attending school and making visits to preventive health-care providers. When households fail to fulfill those obligations, they lose their eligibility. By targeting the transfers to poor households, the program alleviates short-term poverty. By linking the transfers to investments in human capital, the program addresses long-run poverty. RPS's specific objectives include

- supplementing household income for up to three years to increase expenditures on food,
- reducing school desertion during the first four years of primary school, and
- increasing the health care and nutritional status of children under age 5.

RPS comprised two phases over five years, starting in 2000. The pilot phase (also known as Phase I) lasted three years and had a budget of $11 million, representing approximately 0.2 percent of GDP or 2 percent of annual recurring government spending on health and education (World Bank 2001, annex 21). As a condition of the Inter-American Development Bank (IADB) loan financing the project, and to assess whether the program merited expansion in the same or in an altered form, the Government of Nicaragua solicited various external evaluations of Phase I. The International Food Policy Research Institute (IFPRI) conducted the quantitative impact evaluation, using a randomized community-based design. In late 2002, based in part on the positive findings of the various evaluations, the Government of Nicaragua and IADB agreed to an expansion of the program for three more years with a budget of $22 million.

This paper presents the principal findings from the impact evaluation of RPS for a broad range of outcomes related to the program's objectives, including (1) household (food) expenditures, (2) child schooling and child labor, (3) preventive health care of children under age 5, and (4) nutritional status of children under age 5. Though they are widely used and have a long history in developed countries, rigorous, large-scale, randomized evaluations of social programs such as the one reported on here remain rare in developing countries (National Research Council 2001; Newman, Rawlings, and Gertler 1994). Such studies have been increasing in popularity recently, however, after the widely cited case of PROGRESA (Skoufias 2003).

2. Design and Implementation of the *Red de Protección Social*

To analyze how a complex program like RPS altered behavior, it is first necessary to describe the program's operation and evolution.

Program Targeting

In the design phase of RPS, rural areas in all 17 departments of Nicaragua were eligible for the program. The focus on rural areas reflects the distribution of poverty in Nicaragua—of the 48 percent of Nicaraguans designated as poor in 1998, 75 percent resided in rural areas. For the pilot, the Government of Nicaragua selected the departments of Madriz and Matagalpa from the northern part of the Central Region, on the basis of poverty as well as on their capacity to implement the program. This region was the only one that showed worsening poverty between 1998 and 2001, a period during which both urban and rural poverty rates declined nationally (World Bank 2003). In 1998, approximately 80 percent of the rural population of Madriz and Matagalpa were poor, and half of those were extremely poor (IFPRI 2002). In addition, these departments had easy physical access and communication (including being less than a one-day drive from the capital, Managua, where RPS is headquartered), relatively strong institutional capacity and local coordination, and reasonably good coverage of health posts and

schools (Arcia 1999). By purposively targeting, RPS avoided 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, all six (out of 20) municipalities that had the participatory development program *Microplanificación Participativa* (Participatory Micro-planning), run by the national *Fondo de Inversión Social de Emergencia* (FISE), were chosen.[1] The goal of that program was to develop the capacity of municipal governments to select, implement, and monitor social infrastructure projects such as school and health post construction, with an emphasis on local participation. It is possible, then, that the selected municipalities had atypical capacity to carry out RPS. Nevertheless, in terms of poverty, the six municipalities were well targeted. Between 36 and 61 percent of the rural population in each of the chosen municipalities were extremely poor and between 78 and 90 percent were extremely poor or poor (IFPRI 2002), compared with national averages of 21 and 45 percent, respectively (World Bank 2003). While not the poorest municipalities in the country (or in the chosen departments for that matter), the proportion of impoverished people living in these areas was still well above the national average.

In the last stage of geographic targeting, a marginality index based on information from the 1995 National Population and Housing Census was constructed, and an index score was calculated for all 59 rural census *comarcas*[2] in the selected municipalities. The index was a weighted average of the following set of poverty indicators (with respective weights in parentheses) known to be highly associated with poverty (Arcia 1999):

1. family size (10 percent),
2. access to potable water (50 percent),

[1] 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.

[2] *Comarcas* are administrative areas within municipalities that include between one and five small communities averaging 100 households each.

3. access to latrines (30 percent), and
4. illiteracy rates (10 percent).

Higher index scores were associated with more impoverished areas. Recognizing that the index could not reliably distinguish between two *comarcas* with similar scores, rather than use the scores directly, the 59 rural *comarcas* were grouped into four priority levels after renormalizing the highest index score to 100: a score of above 85 was given highest priority (priority 1); 70–85, priority 2; 60–70, priority 3; and below 60, lowest priority, 4.[3] The 42 *comarcas* with the priority scores 1 and 2 were eligible for the pilot phase's first stage.

Program Design

RPS has two core components:

1. **Food security, health, and nutrition**. Each eligible household receives a cash transfer known as the *bono alimentario* or "food security transfer,"[4] every other month, contingent on attendance at educational workshops held every other month and on bringing their children under age 5 for scheduled preventive (or well child) health-care appointments. The workshops are held within the communities and typically include about 20 participants. They educate women in household sanitation and hygiene, nutrition, reproductive health, breastfeeding, and related topics.

   To ensure adequate supply, RPS trained and paid private providers to deliver the specific health-care services required by the program. These services, provided free of charge to beneficiary households, include growth and

[3] IFPRI (2002) describes RPS targeting in more detail.

[4] One common definition of *food security* is "when all people at all times have both the physical and economic access sufficient to meet their dietary needs in order to lead a healthy and productive life" (USAID 1992). In this paper, we do not formally assess food security, however, but focus on indicators of food expenditures that are associated with food security.

development monitoring, vaccination, and provision of antiparasites, vitamins, and iron supplements. Children under age 2 are seen monthly and those between 2 and 5, every other month. In practice, mothers bring their children to the local service location (often a community center or house of one of the beneficiaries) to be seen by the doctor working for the private provider. First, the professional nurse measures the child, inquires about the child's health and the caretaker's caring and feeding practices, and checks the vitamin A supplementation record. Then the doctor examines the child, prescribing appropriate antiparasite medicine or iron supplements according to the Ministry of Health protocol for making these prescriptions. If the child is growing well, the doctor congratulates the caretaker. Then the caretaker returns to the nurse to receive individual counseling on how to maintain or improve growth with key messages on breastfeeding, child feeding, illness care, and hygiene, taking into account several factors, such as the age of the child and whether the child gained weight adequately the previous month or had been ill. The RPS adapted the individual counseling material from the *Atención Integral a la Niñez* (Integrated Attention to the Child, or AIN) program in Honduras (Van Roekel et al. 2002).

2. **Education**. Each eligible household receives a cash transfer known as the *bono escolar* or "school attendance transfer" every other month, contingent on enrollment and regular school attendance of children ages 7–13 who have not completed fourth grade of primary school. Additionally, for each eligible child, the household receives an annual cash transfer intended for school supplies (including uniforms and shoes) known as the *mochila escolar* or "school supplies transfer," which is contingent on enrollment. Unlike the school attendance transfer, which is a fixed amount per household regardless of the number of children in school, the school supplies transfer is for each child.

To provide incentives to the teachers, who have some additional reporting duties and were likely to have larger classes after the introduction of RPS, and to

increase resources available to the schools, there is also a small cash transfer, known as the *bono a la oferta* or "teacher transfer."[5] This is given to each beneficiary child, who in turn delivers it to the teacher. The teacher keeps one-half, while the other half is earmarked for the school. The delivery of the funds to the teacher is monitored, but not their ultimate use.

Table 1 summarizes the eligibility requirements and demand and supply-side benefits of RPS. At the outset, nearly all households were eligible for the food security transfer, which is a fixed amount per household, regardless of household size (Appendix A describes the small number of households that were not eligible). 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 (C$) in September 2000, just before RPS began distribution. Table 1 shows the original U.S. dollar annual amounts and their Nicaraguan córdoba equivalents (using the September [2003] average exchange rate of C$12.85 to US$1). The food security transfer was $224 a year, and the school attendance transfer $112.[6] On its own, the food security transfer represents 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

[5] In rural Nicaragua, school's parents' associations often request small monthly contributions from parents to support the teacher and the school; the teacher transfer was, in part, intended to supplant those informal fees.

[6] The calculations for the transfer amounts were based on the extreme poverty gap, i.e., the difference between the extreme poverty line and the average level of expenditures of the extreme poor reported in the 1998 LSMS (World Bank 2001). The 1998 daily per capita extreme poverty line (calculated to enable the purchase of a minimum requirement food basket) is $0.58 and the extreme poverty gap, $0.18. For comparison, the 1998 daily per capita poverty line is $1.12. The amount for the school attendance transfer was calculated using an approximation of the opportunity cost of children multiplied by the average number of children ages 7–13 in households in extreme poverty. The sum of the food and school attendance transfers was an estimated average daily transfer of $0.12, an amount that would fill two-thirds of the average extreme poverty gap for extremely poor households.

Table 1—Nicaraguan *Red de Protección Social* (RPS) eligibility and benefits in the pilot phase

	Program components	
	Food security, health, and nutrition	**Education**
ELIGIBILITY		
Geographic targeting	All households[a]	All households[a] with children ages 7-13 who have not yet completed fourth grade of primary school
DEMAND-SIDE BENEFITS		
Monetary transfers	*Bono alimentario* *(food security transfer)* CS2,880 per household per year (US$224)	*Bono escolar* *(school attendance transfer)* C$1,440 per household per year (US$112)
		Mochila escolar *(school supplies transfer)* C$275 per child beginning of school year (US$21)
SUPPLY-SIDE BENEFITS		
Services provided and monetary transfers	Bimonthly health education workshops	
	Child growth and monitoring -Monthly (0-2 year olds) -Bimonthly (2-5 year olds)	*Bono a la oferta* *(teacher transfer)* (C$60 per child per year given to teacher/school (US$5)
	Provision of antiparasites, vitamins, and iron supplements	
	Vaccinations (0-5 year olds)	

[a] As described in Appendix A, a small percentage of households were excluded.

about 8 percent, yielding an average total potential transfer of 21 percent of total annual household expenditures. Over the two years, the actual average monetary transfer (excluding the teacher transfer) was approximately C$3,800 (or 18 percent of total annual household expenditures). This is approximately the same percentage of total annual household expenditures as the average transfer in PROGRESA, but more than five times as large as the transfers given in PRAF. In contrast to PROGRESA, which indexes transfers to inflation, the nominal value of the transfers remained constant for RPS, with the consequence that the real value of the transfers declined by about 8 percent over two years in the pilot phase due to inflation. It is possible that any differences in the effectiveness of RPS between 2001 and 2002 resulted, in part, from a decline in the real value of the transfers, though such effects are likely to be small.

The value of the supply-side services, as measured by how much RPS paid to the providers, was also substantial. On an annual basis, the education workshops cost

approximately $50 per beneficiary and the health services for children under age 5, approximately $110, including the value of the vaccines, antiparasites, vitamins, and iron supplements, all of which were provided by the Ministry of Health.

To enforce compliance with program requirements, beneficiaries did not receive the food or education component of the transfer if they failed to carry out any of the conditions listed in Table 2. The monitoring is done using the management information system (MIS) designed specifically for and by RPS. It comprises a continuously updated, relational database of beneficiaries, health-care providers, and schools. The MIS is also used to (1) select beneficiaries and prepare invitations to program incorporation assemblies, (2) calculate transfer payments, (3) compile requests to the Ministry of Health for vaccines and other materials, and (4) monitor whether service providers are meeting their responsibilities. Decision rules capturing the requirements in Table 2 are programmed directly into the MIS. Substantial time was dedicated to designing data forms for the various program participants that feed into this system (including the household registry or census forms, school forms, and health-care provider forms that are all sent to the main office where they are entered into the computer).

Table 2 shows the four different "types" of beneficiary households in the program, who receive different transfers and have to fulfill different requirements. Households with no children in the targeted age ranges are only eligible for the food security transfer but, at the same time, need only attend the health education workshops to qualify for continued receipt of the transfers. Households with children under age 5 (but without children ages 7–13 who have not completed the fourth grade) are also eligible for the food security transfer only, but have more requirements to fulfill related to their young children. Households with children ages 7–13 who have not completed the fourth grade are eligible for both the food security and education transfers and are required to comply with the school-related conditions. If, in addition, there are children under age 5 in the household, it is eligible for the same transfers, but has more requirements to fulfill, in particular, those related to the health controls for young children.

Table 2—Nicaraguan *Red de Protección Social* (RPS) beneficiary co-responsibilities monitored by Phase I

	Household type			
Program requirement	**Households with no targeted children (A)**	**Households with children ages 0-5 (B)**	**Households with children ages 7-13 who have not completed fourth grade (C)**	**(B) + (C)**
Attend bimonthly health education workshops	✓	✓	✓	✓
Bring children to prescheduled health-care appointments				
Monthly (0-2 years)		✓		✓
Every other month (2-5 years)				
Adequate weight gain for children under 5[a]		✓		✓
Enrollment in Grades 1 to 4 of all targeted children in the household			✓	✓
Regular attendance (85 percent), i.e., no more than five absences every two months without valid excuse) of all targeted children in the household			✓	✓
Promotion at end of school year[b]			✓	✓
Deliver teacher transfer to teacher			✓	✓
Up-to-date vaccination for all children under 5 years[b]		✓		✓

[a] The adequate weight gain requirement was discontinued in Phase II starting in 2003.
[b] Condition was not enforced

RPS allows this latter type of household to receive a partial transfer if it complies with the health-care requirements and not the education requirements or vice versa. During the first two years of transfers, approximately 10 percent of beneficiaries were penalized at least once and therefore did not receive, or received only part of, their transfer. It was also possible for households to be expelled from the program.[7] At the start of the program, about 90 percent of the households in the intervention areas were participating (see Appendix A). Less than 1 percent of households were expelled during the first two years of delivering transfers, though 5 percent voluntarily left the program, e.g., by dropping out or migrating out of the program area.

[7] Causes for expulsion include (1) repeated failure to comply with program requirements, (2) failure to collect the transfer in two consecutive pay periods, (3) more than 27 unexcused school absences during the school year per beneficiary child, (4) failure of a beneficiary child to be promoted to the next grade, and (5) discovery of false reporting of information during any part of data collection, including information about fulfillment of program responsibilities.

When it was learned that some, but not all, schools practiced automatic promotion, enforcement of the grade promotion condition was deemed unfair and therefore was never enforced. Similarly, when there were some delays in the delivery of vaccines, the up-to-date vaccination condition was also deemed unfair and not enforced. A third condition, punishment of children who did not have adequate weight gain, was dropped at the end of the pilot phase because of a concern about the role of measurement error and the finding that the poorest households were more likely to be punished. These changes highlight the importance of careful consideration of the required responsibilities and how they are to be monitored during the design of a conditional cash transfer program. At the same time, they show the importance of flexibility during program implementation.

Only the designated household representative could collect the cash transfers and, where possible, RPS designated the mother as the household representative. This strategy mimics 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 education workshops and they were responsible for ensuring that the requirements for their households were fulfilled. In a small number of multigenerational households, the grandmother was selected as the household representative. Since the workshops at times cover themes such as family planning, flexibility on who attends the sessions might be called for in this area.

Although centrally administered, with its multisectoral approach across education, health, and nutrition, RPS required bureaucratic cooperation at national, municipal, and community levels. Given funding and administrative oversight from 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 volunteer representatives known as *promotoras* (beneficiary women chosen by the community) and local school and health-care service providers, to implement the program. The *promotoras* were charged with keeping beneficiary household representatives informed about upcoming health-care appointments for their children, upcoming payments, and any failures in fulfilling the conditions. Each *promotora* had, on average, 17 beneficiaries in her charge, though this average masked substantial variation ranging from 5 to 30 beneficiaries.

3. Design of the Evaluation and Methodology

The evaluation design is based on a randomized, community-based intervention with measurements before and after the intervention in both treatment and control communities. One-half of the 42 *comarcas* (targeted in the first stage as described in the first portion of Section 2) were randomly selected into the program. Thus, there are 21 *comarcas* in the intervention group and 21 in the control group (IFPRI 2001a). Including a control was ethical because the effectiveness of the intervention was unknown. In addition, there was not sufficient capacity to implement the intervention everywhere. Given the geography of the program area, control and intervention *comarcas* are at times adjacent to one another. The selection was done at a public event with representatives from the *comarcas*, the Government of Nicaragua, IADB, IFPRI, and the media present. The 42 *comarcas* were ordered by their marginality index scores and stratified into seven groups of six each. Within each stratum, randomization was achieved by blindly drawing one of six colored balls (three blue for intervention, three white for control) from a box after the name of each *comarca* was called out. Thus, three *comarcas* from each group were randomly selected for inclusion in the program, while the other three were selected as controls.

The evaluation was designed to last for one year; that is, the control group was meant to be a control for only one year (after which it was expected there would be capacity to implement the intervention everywhere). Due to delays in funding for RPS as

a result of a governmental audit unrelated to the program, however, incorporation of beneficiaries in the control *comarcas* was postponed until 2003, extending the possible length of the treatment-control evaluation by more than a year. In fact, control *comarcas* waited a little over two years before being fully incorporated into the program.

Data Collection

The data collected for the evaluation are from an annual household panel data survey implemented in both intervention and control areas of RPS before the start of the program in 2000, and in 2001 and 2002 after the program began operations.[8] The questionnaire was a comprehensive household questionnaire based on the 1998 Nicaraguan LSMS instrument, expanded in some areas (e.g., child health and education) to ensure that all the necessary program indicators were captured, but cut in other areas (e.g., income from labor and other sources) to minimize respondent burden and ensure collection of high-quality data in a single interview.[9] An anthropometric module for children under age 5 was implemented in 2000[10] and 2002, but not in 2001. In this module, we measured height (or length) and weight; we also measured hemoglobin using portable (Hemocue) machines and following standard international procedures.

The survey sample is a stratified random-sample at the *comarca* level from all 42 *comarcas* described above. The areas represented comprise a relatively poor part of the rural Central Region in Nicaragua, but the sample is not statistically representative of the six municipalities (or other areas of Nicaragua, for that matter). Forty-two households were randomly selected in each *comarca* using a census carried out by RPS three months prior to the survey as the sample frame, yielding an initial target sample of 1,764 households. The sample size calculation was based on assessing the necessary sample sizes for the indicators listed in Appendix B, Table 26. Assuming a random sample, the

[8] Results reported on here are based on the September 2003 release of the RPS evaluation data.

[9] LSMS surveys are typically implemented in two visits to the household (Grosh and Glewwe 2000).

[10] About one-half of the 2000 anthropometry survey had to be completed in early October, one month after the main survey, due to delays in getting all the necessary equipment and supplies for hemoglobin testing.

indicator that required the largest sample size, using a significance level of 5 percent and a power of 80 percent, was enrollment for Grades 1–4 (indicator 5 in Appendix B, Table 26). To detect a minimum, statistically significant difference of eight percentage points between intervention and control groups, a sample size of 549 students for each group was required. Of course, not all households had children in this age group. According to the 2000 RPS population census, 63 percent of households had at least one child between ages 6 and 12. Therefore, to obtain a sample of 549 children (in different households), it was necessary to interview 871 households in each group (549 ÷ 0.63) or 1,742 in total. Thus, we arrived at a target sample of 1,764 households.[11] The first wave of fieldwork was carried out in late August and early September 2000, without replacement—that is, when it was not possible to interview a selected household, another household was not substituted.

While there was a great deal of progress in getting RPS started throughout 2001, it was not possible to design and implement all the components according to the original timelines. In particular, the health-care component was not initiated until June 2001. This delay occurred because it took longer than originally planned to design the intervention and select, contract, and train the NGO and private health-care providers. There were also delays in the payment of transfers to households due to a governmental audit that effectively froze RPS funds. As a result, the RPS 2001 follow-up survey was delayed until the beginning of October, to allow additional time for the interventions to take root and for five of the scheduled six payments to be effected. Of course, the advantage of the original design, with the scheduled RPS follow-up at exactly the same time of year as in the 2000 baseline, was that it would enable us to control better for possible seasonal variations in consumption and health. With a control group, however, the possible bias introduced by seasonality can be controlled for statistically. This difference in the timing of the survey, then, does not present a serious problem for the

[11] IFPRI (2001a) describes the sample size calculations in more detail and IFPRI (2001b and 2003) describe the baseline and follow-up samples in more detail. Since anthropometric measures were not part of the original indicator list to be evaluated, they were not used in sample size calculations.

estimation of average effects of the program. The delay in the survey work had the advantage of giving the program more time to take effect, thereby providing a more realistic evaluation of program operations (rather than an evaluation of program delays). In October 2001, then, beneficiaries had been receiving transfers, and the educational components of the program had been monitored for 13 months, but they had only received five months of the health and nutrition services, including the health education workshops. This unforeseen change in operations illustrates the importance of having a credible control group—without the control, it would have been very risky to change the timing of the survey and still confidently attribute observed changes to RPS. The 2002 survey was also carried out in October, and in the second year, beneficiaries received all components of the program for a full 12 months.

We now document nonresponse in the 2000 baseline survey and attrition in the follow-up surveys. Overall, 90 percent (1,581) of the stratified random sample was interviewed in the first round (see Table 3). In a handful of *comarcas*, the coverage was 100 percent, but in six, it was under 80 percent. For the follow-up surveys in October 2001 and October 2002, the target sample was limited to these 1,581 first-round interviews. In 2002, just over 90 percent of these were reinterviewed, on a par with surveys of similar magnitude in other developing countries (Alderman et al. 2001; Thomas, Frankenberg, and Smith 2001). Again, however, coverage in six of the *comarcas* was substantially worse, with less than 80 percent successfully reinterviewed (and one of these is one of the six from above with high first-round nonresponse rate). This attrition is unlikely to have been random (a theme we return to later). Because the same target sample was used in 2002 as in 2001, regardless of whether the household was interviewed in 2001, some households that were not interviewed in 2001 were successfully interviewed in 2002. The sample for which there is a complete set of observations (one in each of the three survey rounds) is 1,396, smaller than the 1,434 shown in the first row of the third column of Table 3. The households are about evenly divided between intervention and control groups, indicating that at least the level of attrition was not significantly different between them.

Table 3—Nicaraguan *Red de Protección Social* (RPS) evaluation survey nonresponse and subsequent attrition

	Baseline 2000	Follow-up 2001	Follow-up 2002
Completed interview	1,581	1,490	1,434
	(89.6)	(94.2)	(90.7)
Completed interview in all three rounds	1,396	1,396	1,396
	(79.1)	(88.3)	(88.3)
...of which			
Intervention	706	706	706
(percent intervention)	(80.0)	(87.2)	(87.2)
Control	690	690	690
(percent control)	(78.2)	(89.5)	(89.5)
Not interviewed			
Uninhabited dwelling	60	51	83
Temporary absence	100	28	46
Refusal	16	6	12
Urban (misclassified)	6	0	0
Lost questionnaire	0	6	6
Target sample	1,764	1,581	1,581

Note: Percent of target sample in parentheses.

Issues Related to the Experimental Design

To measure program impact, it is necessary to know what would have happened had the program not been implemented. The fundamental problem, of course, is that an individual, household, or geographic area cannot simultaneously undergo and not undergo an intervention. Therefore, it is necessary to construct a counterfactual measure of what might have happened had the program not been available. The most powerful way to construct a valid counterfactual is to randomly select beneficiaries from a pool of equally eligible candidates. This was done for the evaluation of RPS using a community-based randomized intervention (IFPRI 2001a).

The value of such randomized evaluations is widely recognized. When done well, recipients and nonrecipients will have, on average, the same observed and, more important (since they are more difficult to control for), unobserved characteristics. As a result, they establish a credible basis for comparison, freed from selectivity concerns, and the direction of causality is certain. Nonrandomized approaches, on the other hand, typically rely on assumptions that are often hard to believe and almost always hard to

verify (Burtless 1995). A further advantage to a randomized design is that program impact is easy to calculate and, as a consequence, easier to understand and explain.[12]

However, even a well-implemented randomized evaluation design is not without its weaknesses. First, the usual difficulties of following subjects over time persist, so selection bias due to attrition remains a potential problem; the advantages of randomization are dissipated with attrition if it is nonrandom. Second, such studies can be costly (financially and politically), and often one must wait years for results, making them less useful for making pressing policy decisions. Third, there are important ethical concerns about withholding treatment from the control group of an intervention known to have positive effects. In RPS, the randomized design was justified because it had not been shown to have positive effects and because of the infeasibility, given the fixed budget, of extending the program to all potential beneficiaries in a short period of time. In this case, random selection would appear to be as fair as any other arbitrary criterion for selecting the first set of beneficiaries.

Unfortunately, randomized design evaluations can provide only partial answers to important questions they are not explicitly designed to address. This is often referred to as their "black box" nature. The evaluation only allows us to assess the effect of the program (or program components) it was explicitly designed to assess. In the case of the RPS evaluation, this means that we only evaluate the program as a whole, with all of its components. Without further assumptions, we are in the dark if we want to consider how even slightly changing the program would alter the outcomes under consideration. For example, RPS provides a "package" of services in which all households are eligible for the food security transfer, regardless of whether they also receive the educational transfers. With only the randomized design implemented here, it is not possible to isolate

[12] Heckman and Smith (1995), however, point out that this apparent simplicity can be deceiving, particularly in poorly designed evaluations where there is randomization bias (where the process of randomization itself leads to a different beneficiary pool than would otherwise have been treated) or substitution bias where nonbeneficiaries obtain similar treatments from different sources—a form of "contamination." The former should not be a concern in the RPS evaluation. We discuss the reasons for this later.

the effects of just the educational transfer—all the observed effects are the result of the program as a whole. Nor is it possible, without further assumptions, to assess reliably what the effect of the program would be if the size of the transfers were to change, as they did when expansion of the program began in 2003.

Another limitation of randomized evaluations is that the results pertain specifically to the study population—extrapolating them to other populations requires additional assumptions that may not be easy to verify (Burtless 1995). This is typically referred to as the external validity problem. In the case of RPS, the purposive selection of program areas may have affected program performance; therefore the generalizability of the results is questionable. As described earlier, the selection of municipalities was conditioned on the likelihood of success, so that the observed outcomes might exaggerate the likely outcomes from program expansion to other areas with, for example, weaker institutional capacity to implement the program.

A final problem to bear in mind when interpreting the results in this analysis is that the program was in its pilot phase, and outcomes (and therefore estimated effects) for the pilot may differ from outcomes for an expanded program. Like most pilots, RPS underwent an initial learning period (with attendant setbacks) and undertook a variety of activities that might not need repeating in an expansion (e.g., preparing training materials for beneficiaries, *promotoras*, and health-care providers). Some of these activities could have reduced the program's effectiveness during the pilot (Caldés and Maluccio 2004). Moreover, as with any new program, there was the potential for observed behavioral changes to result, in part from the novelty of the program or the evaluation rather than from permanent behavioral changes—the Hawthorne effect (Krueger 1999). There is some evidence consistent with this phenomenon when we compare the effects after one year (2000–01) with those after two years (2000–02). Performance was slightly lower in 2002 than in 2001 on several outcome indicators. Unfortunately, we cannot directly test whether this is due to a Hawthorne effect, changes in the effectiveness of program implementation, or the slight decline in the real value of transfers. Finally, expansion of the program could introduce new advantages and disadvantages associated with scaling

up and economies of scale. All these factors call for a degree of caution in forecasting what would happen were the program to be extended to other municipalities or departments of Nicaragua.

Double-Difference Methodology

Household- and individual-level data were collected in both the intervention and control *comarcas* before and after RPS was implemented. This enables the use of the double-difference method to calculate "average program impact."[13] The resulting measures can be interpreted as the expected effect of implementing the program in a similar population elsewhere, subject to the caveats discussed above. The method is shown in Table 4. The columns distinguish between groups with and without the program (denoted by *I* for intervention and *C* for control), and the rows distinguish before and after the program (denoted by subscripts 0 and 1). Anticipating one of the analyses presented below, consider the measurement of school enrollment 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 effect under this design and is referred to as the first difference. A more robust measure of the effect, however, would account for any preexisting observable or unobservable differences between the two randomly assigned groups: this is the double difference obtained by subtracting the preexisting differences between the groups, $(I_0 - C_0)$, from the difference after the program has been implemented, $(I_1 - C_1)$.

[13] Ravallion (2001) provides a useful and enjoyable discussion of this and related evaluation tools.

Table 4—Calculation of the double-difference estimate of average program effect

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

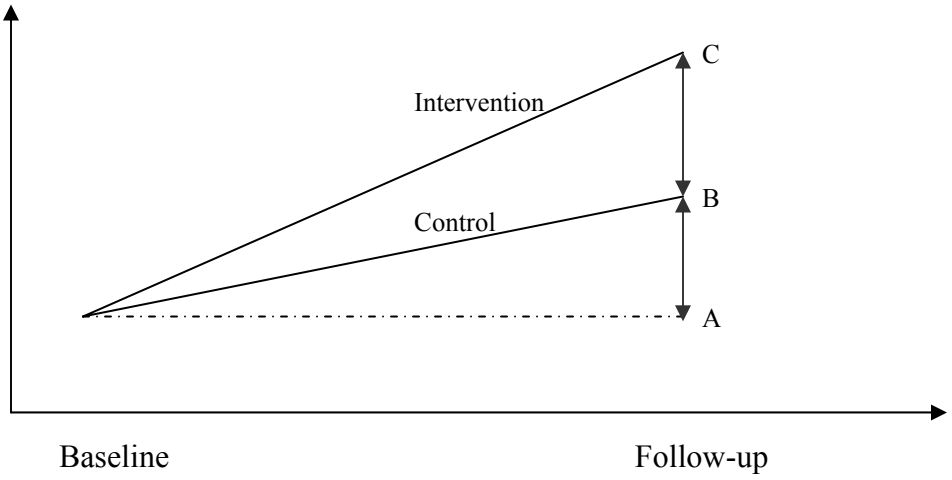
An alternative interpretation of the double-difference estimator emerges if one first considers the differences *within* the (intervention or control) groups. This approach begins with a naive estimator of the program effect, the difference over time for the intervention group, $(I_1 - I_0)$.[14] It is naive because it would include all changes over time in enrollment rates in the intervention group, regardless of what causes them. For example, if increases in public investment nationally were improving school access and leading to changes in enrollment, these effects would show up in the difference over time in the intervention group, in addition to the effects attributable to the program. The obvious measure for the nonprogram-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 effect by first considering the total change over time in the intervention group, and then subtracting from this the change over time in the control group. As above, this yields the double-difference estimator.

The alternative interpretation is probably best illustrated graphically in Figure 1. For an arbitrary indicator that we measure over time, we assume (for the graph) that as a result of the randomization, both the intervention and control groups start at the same level. No change in the indicator over time would lead to the outcome depicted by point A in 2002; if we were only following the intervention group, we would then naively calculate the effect of the program as $C - A$. However, as the control group makes clear,

[14] This approach is sometimes referred to as a "reflexive comparison."

there was a trend over time that led to an improvement (in this example) of *B* - *A*. Were we to ignore this, we would overstate the effect of the program. Instead, our estimate of the program effect is *C* - *B*; this is the double difference estimate. In the case where the trend line for the control group was declining, ignoring that effect would tend to understate the program effect.[15]

Figure 1—Illustration of the double-difference estimate of average program effect



For this work, the technique just described is extended to account for the three measurements taken in 2000, 2001, and 2002. The basic estimating equation is

$$E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 P_c + \delta_1 A_1 P_c + \delta_2 A_2 P_c + \mu_{ic} + \nu_{ict}, \qquad (1)$$

where

E_{ict} = outcome variable of interest for household (or individual) *i* in *comarca c* at time *t*,

A_1 = (1) if Year 2001,

A_2 = (1) if Year 2002,

[15] Relaxing the (unnecessary) assumption that the two groups start at identical points slightly complicates the graphical exposition, but the logic remains the same.

P_c = (1) if program intervention in *comarca c*,

μ_{ic} = all (observed and unobserved) household- (or individual-) level time-invariant factors,

ν_{ict} = unobserved idiosyncratic household (or individual) and time-varying error, and

all the α and δ are unknown parameters.

The parameters of interest are δ_1 and δ_2; δ_1 is the double-difference estimator of the average program effect for 2001 (relative to 2000), and δ_2 for 2002 (relative to 2000). We emphasize that the program effects are identified by the randomized design; given the randomization of P_c, it (and any interactions involving it) is uncorrelated with all observed or unobserved household- (or *comarca*-) level variables so that the δs can be consistently estimated. Indeed, the main reason to include household variables in a regression like this is to increase the precision of the estimates, not because we are concerned about the consistency of the estimator for δ. As a robustness check on the results (described later), we include household-level effects and find no substantive differences in the estimates of the program effects, other than that they tend to be much more highly significant.[16]

To assess differences in effects for the poor and nonpoor for all the analyses considered below, we also classify households into three poverty groups—extreme poor, poor, and nonpoor—based on their per capita annual total household expenditures (including own-production) measured in 2000 and using 2001 national poverty lines developed by the World Bank (World Bank 2003).[17] Double-difference estimates were

[16] Due to the random allocation of the program, none of the explanatory variables in equation (1) would be correlated with household-level effects, so we can safely treat them as either random or fixed. The results are similar for the two different approaches.

[17] Another approach we considered for calculating the poverty groups was to adjust the 1998 national poverty lines for inflation up to 2000. That methodology yields poverty lines that are similar but 3–4 percent higher than the 2001 national lines, since general inflation tends to outpace increases in the poverty line, given the more limited consumption basket on which the latter is based. Categorizations based on this alternative approach do not substantively change the reported results.

calculated within each poverty group and are discussed in the text (and presented in some of the tables) when they differ across the groups.

Finally, since we do not condition on the individual household-level decision to participate but instead only on whether the program was available in the household's *comarca*, we are estimating what is commonly referred to as the "intent to treat" effect. Recall that about 10 percent of the households in the intervention areas were either excluded by RPS or chose not to participate. Survey sample households in this subgroup are not program beneficiaries, so basing estimates that combine them with beneficiary households dilutes the estimated effects of the program. The intent-to-treat methodology is a conservative one relative to measuring the effect of the treatment on the treated, though given the relatively high participation rates; it is unlikely to underestimate the effect on participating households by much. Furthermore, the intent-to-treat estimate is less subject to selection biases associated with the decision to participate in the program. Finally, the advantages of the randomized design are dissipated if we estimate the effect of the treatment on the treated, since the evaluation was not designed to do that. Rather than estimating the double difference, we would instead have to endogenize the participation decision, most likely by using the random program placement as an instrumental variable.

In the double-difference analyses that follow, all (relevant) individuals or households from each survey round, regardless of whether they were interviewed in one, two, or three of the waves, are included. On the whole, there were few significant differences between households (or individuals) in intervention and control groups at baseline; even when there were, of course, the double-difference estimator controls for them. All standard errors are calculated allowing for heteroscedasticity and for clustering at the *comarca* level (Stata Corporation 2001). We do not control for the fact that the randomization was at the *comarca* level—so-called "community effects." When we do not control for heteroscedasticity and *comarca*-level clustering, or when we do control for community-level effects, the standard errors decrease, leading to stronger statistical

significance. Consequently, the results we present are conservative in terms of their significance.

We also ignore the stratified sample design, which can be corrected for statistically by using *comarca*-level sample weights; correcting for this aspect of the design made no substantive changes to the estimated effects, so we chose not to do so in order to present estimates with the more conservatively estimated standard errors described above. Similarly, when we limit the sample to those interviewed in all three rounds as a partial control for attrition bias, estimated effects change only slightly, with no systematic bias.

Another partial remedy to control for attrition bias is to estimate a household fixed-effects model, particularly if one suspects that unobserved persistent heterogeneity is leading to attrition. However, as with the other robustness checks, when we estimate the models with these controls, the results differ little. Recall that the number of households is about evenly divided between intervention and control groups, suggesting that attrition was not significantly different between intervention and control groups. Combining this with the evidence from the robustness checks just described, we conclude that attrition bias is not driving the results presented here.

4. The Effects of Conditional Cash Transfers: The *Red de Protección Social*

As a condition for approval of the second tranche of the IADB loan underwriting RPS, an evaluation was carried out by IFPRI assessing the main objectives of the program. The IADB loan document contained a set of specific indicators and numerical goals for each objective (see Appendix B, Table 26), and these were used in the sample size calculations described in the first portion of Section 3. While the program achieved most of these goals and the loan extension was approved, we do not emphasize these specific indicators in our analysis. This is because (1) they represent only a subset of the possible effects of the program, (2) at the time, the program designers had little information on which to base the numerical goals, and so they are somewhat arbitrary,

and (3) they largely measure process or inputs and did not capture the underlying objectives of the program such as improved human capital. While we are in favor of the trend by development institutions to embed project evaluations in projects, we would caution against overspecifying the goals or holding to them too rigidly, particularly when little evidence of similar programs exists. Hence, while we present results for the contractually agreed-to indicators, we do not compare the results with the numerical goals. Moreover, we present results on a large number of additional indicators including direct measures of one form of human capital, the nutritional status of children under age 5.

Household Expenditures

At the outset, we indicated that 36–61 percent of the rural population in each of the RPS municipalities was extremely poor, and 78–90 percent was extremely poor or poor, compared with national averages in 1998 of 21 and 45 percent, respectively. Though not the poorest municipalities in the country, the proportion of impoverished people living in these areas was well above the national average. Within the 42 *comarcas* selected for the program evaluation, 42 percent of the population was extremely poor before the program—that is to say, their total expenditures were less than the amount necessary to purchase a food basket providing minimum caloric requirements (World Bank 2003)—and 80 percent extremely poor or poor.[18] Moreover, the majority of the remaining households, or "nonpoor" in the sample, was in the bottom two-thirds of the national Nicaraguan per capita expenditure distribution and so was near-poor. Clearly there was substantial need, and hence scope, for alleviating current poverty in this population.

[18] These and other descriptions of poverty in the RPS sample are calculated based on 2001 per capita annual expenditure poverty lines of $C2,691 ($202) for extreme poor (calculated as the amount required to purchase a minimum requirement food basket) and $C5,157 ($386) for poor, which adds nonfood requirements (World Bank 2003). Households are classified into poverty groups based on their initially measured (in 2000) per capita annual total household expenditures (including own-production) using these 2001 Nicaraguan poverty lines.

Table 5 shows the average effect of RPS on nominal annual total household expenditures, as measured by the double-difference methodology.[19] The control column shows that in 2000, before the program began, average annual total household expenditures in the control areas were C$20,695. A year later, expenditures had declined by C$2,626 to C$17,970, but by 2002, they had recovered somewhat, reaching C$18,856. The intervention column shows preprogram annual total household expenditures of C$20,903 in the beneficiary areas. After one year of operation of RPS, annual total household expenditures had risen by more than C$1,000 to C$22,142, but then fell back to C$21,307 in 2002, only C$404 (less than 2 percent) above the level reported in the 2000 baseline.

Table 5—*Red de Protección Social* (RPS) average effect on annual total household expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	21,307	18,856	2,451
	[722]	[712]	(1,649)
Follow-up 2001	22,142	17,970	**4,172*****
	[766]	[724]	(1,300)
Baseline 2000	20,903	20,695	307
	[811]	[771]	(1,215)
Difference 2001-2000	1,239	**-2,626*****	**3,864*****
	(808)	(1,053)	(1,311)
Difference 2002-2000	404	**-1,739***	**2,144***
	(812)	(993)	(1,268)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets.
*** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

[19] The construction of the expenditure measures is detailed in IFPRI (2001b). We present nominal figures rather than real inflation-adjusted figures to enable a more direct comparison with the fixed nominal transfer levels.

As shown in the right-hand "Difference" column, before the program began, annual total household expenditures in 2000 were very similar in intervention and control areas (differing by only C$307), indicating that on this measure, the randomization into two groups was effective. One year later, however, that small initial difference had grown to C$4,172, and the net average increase, or double-difference estimate of the effect of the program between 2000 and 2001, was C$3,864 (statistically significant at the 1 percent level). With the partial recovery of expenditures in the control group in the second year, however, the estimated effect of the program declined to C$2,144 (marginally statistically significant at the 10 percent level).[20] For comparison, the average value of cash transfers for beneficiary households in the evaluation survey was C$3,500 in the first 12 months of operation and C$3,800 in the second 12 months (as only five of the scheduled six payments were made in each year). Beneficiary households are, on average, spending a large proportion of their transfers on current expenditures (rather than increasing savings, for example), though the fraction spent appears to have been smaller in the second year, perhaps, in part, because it was less necessary as the area underwent a partial recovery compared to 2001. Comparing across the extreme poor, poor, and nonpoor in the sample, we find that the largest estimated double-difference effect was for extremely poor households (over C$3,000 in 2002).

The drop in expenditures in the control group seems to have been due in part to an economic downturn in the areas where RPS was operating and in Nicaragua more generally. Within the control group, expenditures fell among the poor and nonpoor but

[20] This effect is not statistically significant at the 5 percent level, in large part because of the asymmetric distribution of total expenditures. When we examine the double-difference estimate on the natural logarithm of annual total household expenditures (so that they more closely approximate a normal distribution), it is significant at the 1 percent level. In the text, we present absolute measures to facilitate comparison with the nominal transfer amounts.

held steady for the extremely poor.[21] Two events affecting the area included a severe drought in 2001 and a sharp, persistent, drop in international coffee prices, which affected many of the agricultural laborers in that industry (Varangis et al. 2003). The rural Central Region of Nicaragua was the most affected by these events and was the only region showing an increase in poverty rates between 1998 and 2001 (World Bank 2003). The transfers provided by RPS apparently compensated for income losses during this downturn. While not designed as a traditional safety net program in the sense of reacting or adjusting to crises or shocks, the economic difficulties experienced by these communities allowed RPS to perform like one, as it enabled households to maintain expenditures during a downturn.

The substantial decline in expenditures in the control areas demonstrates the importance of having a control group in this, or any, evaluation. Control groups help isolate the effects attributable to the program and keep them from being confounded with other, nonprogram factors. Without a control group, the analysis would have mistakenly concluded that the RPS had no effect on annual total household expenditures in 2002 (see C$404 difference over time in the intervention group in the second to bottom row of the first column of Table 5).

The RPS effects on per capita annual total household expenditures are shown in Table 6. Average per capita expenditures in 2000 were just over $300 compared to a national average of nearly $500 in 1999, again emphasizing the relative deprivation of the program areas. Reflecting the pattern in total expenditures just described, combined with no significant changes in household size, the results show a small but insignificant

[21] The drop in expenditures in the control group was not due to changes in household size or family composition (see Table 6 on per capita expenditures). Another possibility is that there were biases in the reporting of expenditures. For example, in control areas, it is possible that nonbeneficiaries who had learned about the program understated expenditures to appear more in need of the program. However, at this stage, the program was being implemented using only geographical targeting, and being more or less poor would not have affected eligibility. At the same time, beneficiaries may be overstating food expenditures knowing that increased expenditures on food was one of the objectives of RPS. The fact that the net change in average expenditures is similar in magnitude to the amount of cash transfers suggests these sorts of reporting biases are not substantially altering the findings. There would be more concern if, for example, changes in expenditures were substantially larger than the transfer.

increase in per capita annual total household expenditures within the intervention group, but significant declines for the control group. Taken together, these changes produce average program effects of C$850 in 2001 and C$521 in 2002. In relative terms, in 2002 the average effect of the RPS was about 13 percent of initial per capita annual household expenditures. This average, however, masks substantial differences among the extreme poor, poor, and nonpoor in the sample. For the extreme poor, the estimated average effect is C$781 for 2002 and represents over 40 percent of initial per capita expenditures, whereas for the nonpoor, the estimated effect was only 6 percent of initial per capita expenditures.

Table 6—*Red de Protección Social* (RPS) average effect on per capita annual total household expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	4,356	3,489	**867****
	[722]	[712]	(381)
Follow-up 2001	4,461	3,266	**1,195*****
	[766]	[724]	(291)
Baseline 2000	4,190	3,845	345
	[811]	[771]	(288)
Difference 2001-2000	271	**-579*****	**850*****
	(163)	(206)	(251)
Difference 2002-2000	165	**-356***	**521****
	(173)	(186)	(251)

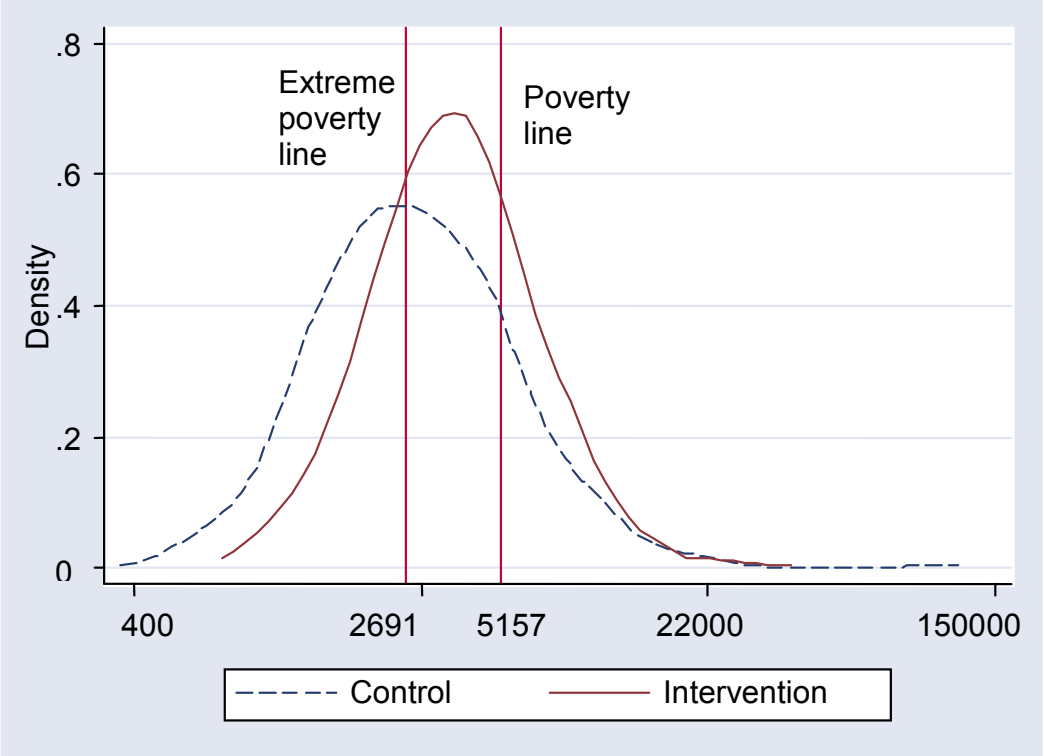
Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Since the changes in expenditures for poorer households were relatively large, RPS had a substantial effect on the percentage of extremely poor households in the program areas. The average effect on the extreme poverty rate in 2001 was -21 percentage points; in 2002, it was -15 percentage points. Declines in the overall poverty rate were smaller, -10 and -5 percentage points, respectively. Unsurprisingly, the income

supplementation has a significant effect on expenditure-based poverty measures of "current" poverty. In Figure 2, we show this in an alternative fashion by graphing the empirical density functions of the logarithm of per capita annual total household expenditures in the year 2002 for households in the control and intervention groups separately. This makes it clear that the program has shifted the density function to the right (and decreased its spread) and that the percentages of households that are extremely poor (to the left of the extreme poverty line) or poor (to the left of the poverty line) are substantially lower in the intervention *comarcas*. Nevertheless, even in the intervention *comarcas*, a large proportion of households remain poor.

Figure 2—Density functions of per capita annual total expenditures in 2002: Control versus intervention



Showing a pattern similar to per capita annual total household expenditures, RPS produced significant net average increases in per capita annual food expenditures of C$774 in 2001 and C$556 in 2002 (Table 7), and these averages again mask larger

effects for the extreme poor and poor. The average increases in the per capita annual food expenditures are approximately equal to the average changes in per capita annual total household expenditures in each year. Consistent with the program's goals, additional expenditures as a result of the transfers were spent predominantly on food. During the incorporation assemblies and some of the health education workshops, an informal "requirement" that the income supplements are primarily intended for food purchases is emphasized; in addition, anecdotal evidence suggests that some *promotoras* take this aspect of the program very seriously, asking to see receipts after transfers have been made (though it is not possible to gauge whether this practice is widespread). Regardless, RPS was very important in preventing the deterioration of the food security situation in the intervention group, offsetting the decline seen in the control group. Though we are unable to directly test this hypothesis, these results point to a likely increase in calories consumed (at the household level), as has been shown for PROGRESA (Hoddinott and Skoufias 2003).

Table 7—*Red de Protección Social* (RPS) average effect on per capita annual food expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	3,027	2,246	**781*****
	[722]	[712]	(253)
Follow-up 2001	3,129	2,141	**988*****
	[766]	[724]	(217)
Baseline 2000	2,812	2,598	214
	[811]	[771]	(181)
Difference 2001-2000	**316***	**-457*****	**774*****
	(138)	(129)	(187)
Difference 2002-2000	**214***	**-352****	**566*****
	(123)	(142)	(186)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Table 8 shows how the significant changes in per capita food expenditures affected the share of food expenditures in the household budget—RPS produced a significant net increase of 4.1 percentage points in the food share, and this change was roughly the same across poverty groups. The percentage of the budget spent on food did not change for households in the intervention group after implementation (remaining at 70 percent, consistent with the high rates of poverty for this group compared to the national average of 60 percent in 1998).

Table 8—*Red de Protección Social* (RPS) average effect on food shares (percent)

Survey round	Intervention	Control	Difference
Follow-up 2002	70.0	66.4	**3.7*****
	[722]	[712]	(0.9)
Follow-up 2001	70.0	66.5	**3.4****
	[766]	[724]	(1.4)
Baseline 2000	69.8	70.2	-0.5
	[811]	[771]	(1.3)
Difference 2001-2000	0.2	**-3.7*****	**3.9*****
	(1.2)	(1.2)	(1.7)
Difference 2002-2000	0.3	**-3.9*****	**4.1*****
	(0.9)	(0.9)	(1.3)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

This net average effect on the food share is consistent with the program's success in promoting increased food expenditures. The estimated effect comes not from an increase in the food share for beneficiaries, however, but from a decline in the food share of the control group. What is somewhat puzzling is why—during an economic downturn marked by decreased expenditures—the food share would be declining for control households. For a relatively poor population such as this, we would have expected the food share to increase as total expenditures declined, since in this and most samples,

households with greater per capita expenditures typically have lower food shares. It turns out that much of the decline in the food share for the control group can be attributed to implementing the 2001 and 2002 surveys in October rather than in August and September, when the baseline 2000 survey was carried out. In a quality-control survey carried out in October 2000 on a randomly selected subset of baseline households, food shares are substantially lower than they were in baseline interviews (IFPRI 2001b). Seasonality seems to play an important role in the absolute levels we observe, though, as emphasized above, it is not biasing the estimated average effects of the program, which show a net increase in the food share for beneficiary households relative to nonbeneficiary households.

In addition to enabling more spending on food, one of the objectives of RPS is to improve the food security and nutrition of beneficiary households. We begin to explore how well this was achieved by decomposing the increase in food expenditures into its component parts. The RPS baseline and RPS follow-up surveys list 60 types of food purchased or obtained by home production in the households (IFPRI 2001a). It appears that the transfers have had a significant effect on dietary diversity, a correlate of food security (Hoddinott and Yohannes 2002). A double-difference estimate of the number of different types of purchases reported shows that the program appears to be promoting a more diverse diet. At the outset in 2000, households reported consuming an average of 12 (SD 5.4) different items, excluding alcohol and tobacco. On average, households in the intervention areas report that they are buying four additional types of food from the listed 60 types in 2002, a result that was consistent across the poverty groups (results not shown).

Not only did the number of foods purchased increase, but the nutritional value or quality of food items did as well. By organizing the types of food into different categories, two conclusions can be reached (see Table 9). First, in absolute terms,

expenditure on nearly all food groups increased with the program.[22] Second, nutrient-dense foods, including meats and fruits and vegetables, which are associated with a better quality diet, increased not only in terms of absolute expenditures but also as a percentage of total food expenditures. These relative improvements were accompanied by an increase in fats (including oils) as well, and were made possible by declines in two staples (grains and beans) that represented over 60 percent of the preintervention budgets of beneficiary households. Moreover, extremely poor households show the largest changes in the nutritional quality of the food purchased, indicating those most in need were benefiting the most. Exploring whether this improved diet was associated with the nutritional improvements for children within these households is the subject of the section on Child Nutrition Status below.

Table 9—*Red de Protección Social* (RPS) average effect the composition of food expenditures

Food category	2000 average	2000-2002 average effect on expenditures	2000 average	2000-2002 average effect on food share
	(C$)	(C$)	(percent)	(percent)
Grains, potatoes, bread	6,987	504	48.3	**-3.2*****
Beans	1,454	-138	11.3	**-3.1*****
Meat	1,132	**656*****	7.0	**1.9*****
Milk	546	122	3.4	0.4
Fats	1,288	**663*****	9.2	**1.9****
Fruits and vegetables	761	**494*****	5.2	**2.3*****
Alcohol and tobacco	104	18	0.6	0.0
Sweets	996	**321*****	7.7	0.5
Other	865	-129	6.6	**-1.1***

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

The majority of the additional expenditures induced by RPS were spent on food. A second key component of the program is education. In 2002, the estimated average

[22] Information about alcohol and tobacco expenditures in these types of surveys is not very reliable; it is presented separately, and we draw no conclusions from the reported information.

effect of RPS on educational expenditures was C$302 (significant at 1 percent), slightly larger than the value of the educational supplies transfer. These gains were concentrated in the extreme poor and poor households. Health-care expenditures actually decreased with RPS (–C$231), though this effect is only significant when we consider separately the extreme poor. This is consistent with the fact that the program provides many health services free of charge, which were possibly substituting for others that beneficiaries previously had to pay for directly or via other related costs, such as travel expenses.

Finally, the study asked about other forms of expenditures related to investments at the household level, such as on household improvements, durable goods, etc.; none of these showed significant changes. Naturally, since total expenditures were flat while the percentage spent on food remained the same, it was unlikely that investments of this sort would have changed very much. It is important to emphasize that the evidence indicates that households are indeed following the recommendations of the program, i.e., they are spending most of their income from the program on current (food and education) expenditures. This pattern will need to be considered as RPS plans its exit strategy and may have implications for the sustainability of the effects described in this paper.

Another possible effect of the program on the household economy is on labor participation; cash transfers may be a disincentive to work. Examining separately women and men ages 15–50, healthy, and not in school, the results show that there were no significant changes in labor participation in the previous week or in the number of laborers per household. Nearly all men report having worked the week before, and there are no differences between the two periods or between the intervention and control areas. For women, about 70 percent reported working in the previous week, but as with the men, there were no program effects on the probability of working. The program does appear to have lead to a slight decrease in the intensity of work (conditional on working), as measured by the average hours worked last week, which declined, on average, about two hours per worker.

Schooling and Child Labor[23]

Education levels in Nicaragua are dismal. One-third of adults over age 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, at 78 percent the net primary enrollment ratio remained one of the lowest in Latin America in the late 1990s (World Bank 2001, Annex 16). Unsurprisingly, these low enrollment rates are accompanied by a high incidence of child labor, particularly for boys. In 1998, 27 percent of boys ages 10-14 in rural areas were working an average 30 hours a week (World Bank 2001, Annex 25). These poor outcomes, despite improvements in school supply, are primary concerns for the economic development of Nicaragua; at the same time, they suggest a potentially large role for demand-side interventions such as RPS.

Before the start of RPS, the enrollment rate in the program area for the target group, those ages 7-13 who had not yet completed fourth grade of primary school, was 71 percent. This overall average demonstrated a large potential for improved outcomes but at the same time masked important differences by age of the child and level of household well-being. Figure 3a shows enrollment (or matriculation) rates by age for the sample in 2000. For the targeted children, enrollment peaked at 81 percent for 9-year olds but declined to 46 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; the legal starting age for first grade is 7. A possible effect of the program may be not only to increase overall attendance but also to improve appropriate-age starts.

Figure 3b shows the enrollment rates for the same children by gender and by household expenditure group: extremely poor, poor (though not extremely poor), and nonpoor. These simple comparisons indicate that resources play a role in the decision to

[23] This section updates and extends to 2002 results from 2001 first reported in Maluccio (2004).

Figure 3a—Enrollment in 2000 for 7-to-13-year-olds who have not completed fourth grade, by age

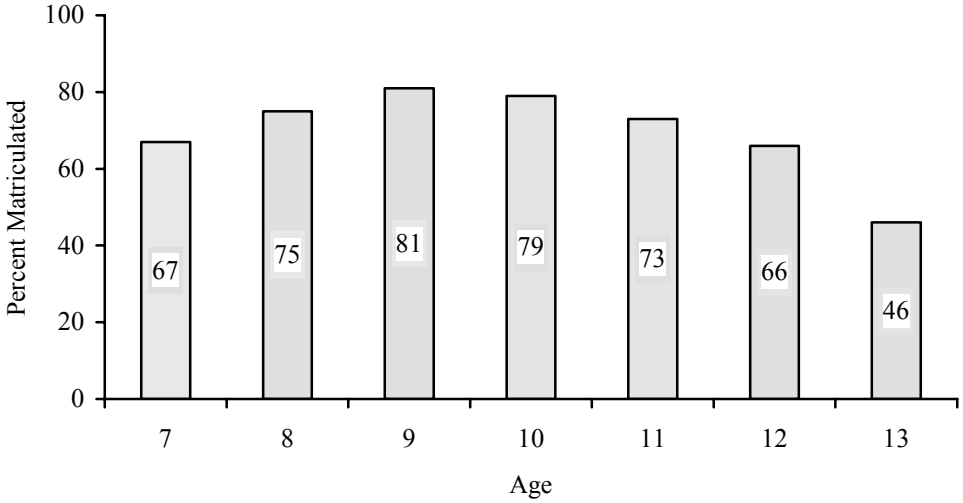
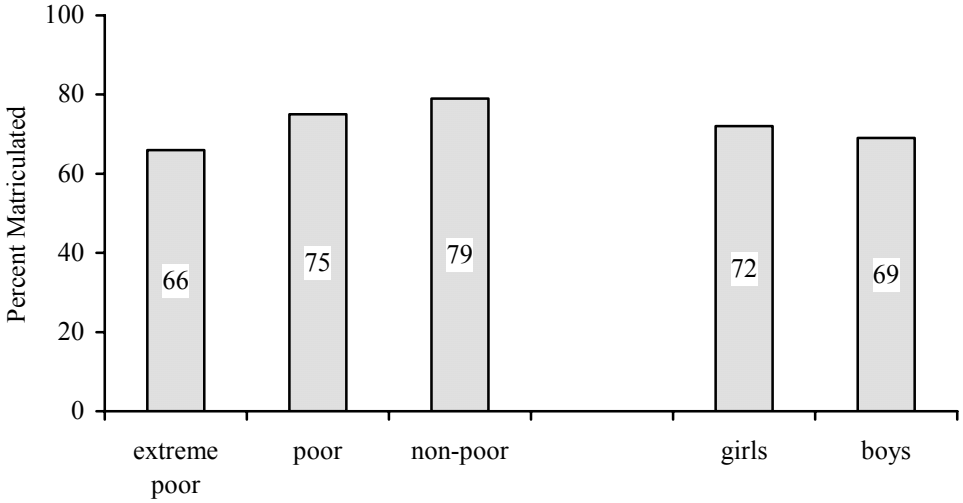


Figure 3b—Enrollment in 2000 for 7-to-13-year-olds who have not completed fourth grade, by expenditure group and by gender



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 enrolled than those living in the wealthiest decile (not shown). There was little difference between the enrollment rates for boys and girls. Although not controlling for the many other factors

that affect enrollment, this evidence still suggests that there was potential for a cash transfer program to influence enrollment rates, for boys and girls alike. Similar patterns emerge for current attendance in school in 2000, collected approximately three months before the end of the academic year and shown in Figure 4. A child is defined to be currently attending 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 enrollment at the start of the year, current attendance rises to age 9 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 enrolled at the outset of the year, indicating that dropout was common. Once again, it is evident that there was substantial room for improvement. Girls were five percentage points more likely to be currently attending school than boys, foreshadowing the gender differences in labor participation we consider next.

All individuals over age 6 were asked whether work was their primary activity in the previous week and, if not, why they did not work. Work included working for pay or other remuneration outside the home, as well as unpaid labor in household enterprises such as agriculture or small business. Possible reasons for not working most relevant to children were that they were in school or 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 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. Average hours worked also increased with age. There was no obvious monotonic relationship, however, between working and the economic well-being of the household. This undoubtedly reflects the likelihood that child labor increases household expenditures, our measure of well-being. Boys were substantially more likely to report

Figure 4a—Current attendance in 2000 for 7-to-13-year-olds who have not completed fourth grade, by age

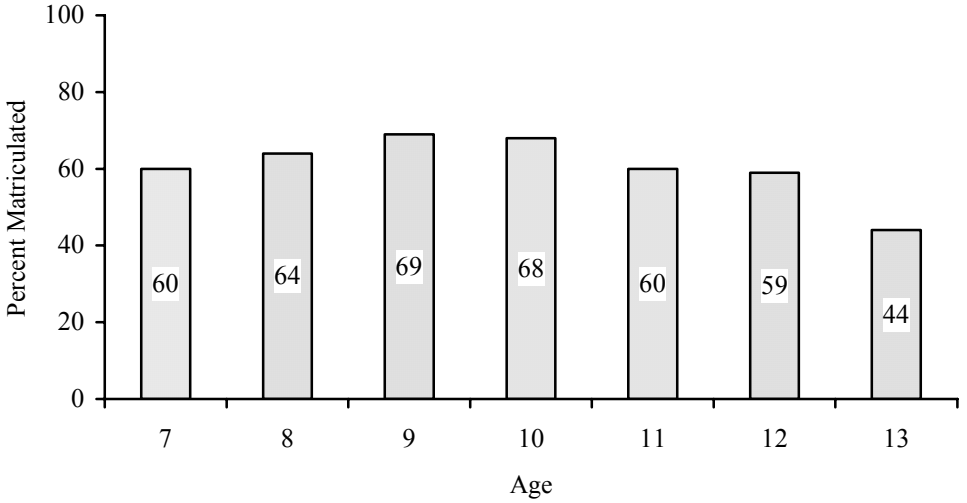
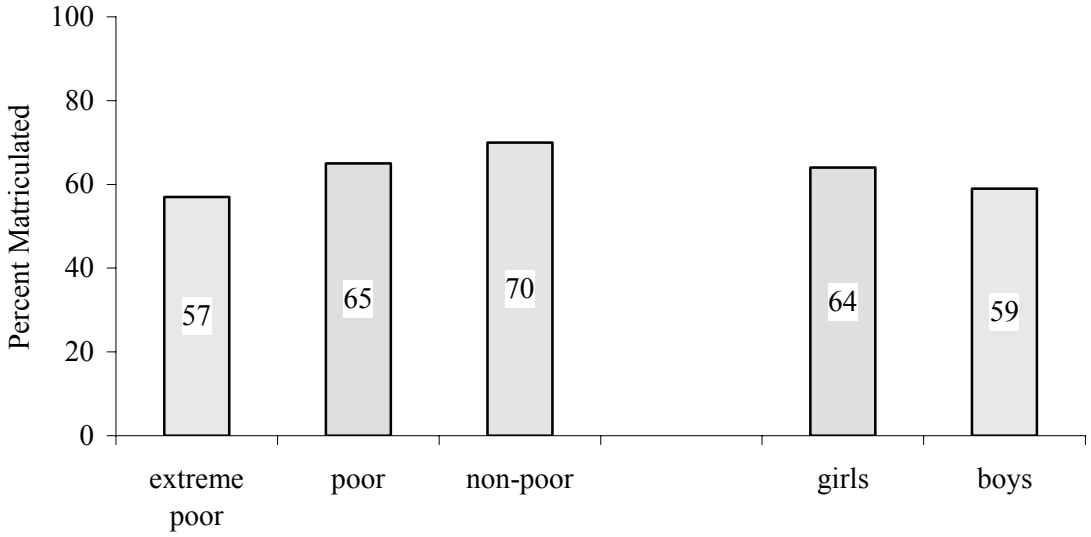


Figure 4b—Current attendance in 2000 for 7-to-13-year-olds who have not completed fourth grade, by expenditure group and by gender



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 aside from housework, the

difference between boys and girls' reported work might reflect in part the underreporting of girls' domestic responsibilities.

With these basic facts describing the pre-RPS conditions in the study areas, we now examine how the program has changed the landscape, considering first the overall average effects and then the effects by age group.

RPS induced a significant average net increase in school enrollment at the start of the school year of 17.7 percentage points for the target population of children ages 7–13 who had not yet completed the fourth grade of primary school (Table 10). With the program, enrollment rose to 92.7 percent. Before the program, enrollment rates in intervention and control areas for this age group were very similar, with approximately 70 percent of eligible children enrolling. Enrollment in the control group increased only 6.8 percentage points, indicating that the transfers proved to be a huge stimulus.[24] For

Table 10—*Red de Protección Social* (RPS) average effect on enrollment, children 7-to-13 years old in first through fourth grades

Survey round	Intervention	Control	Difference
Follow-up 2002	92.7	79.2	**13.4*****
	[806]	[872]	(2.6)
Follow-up 2001	93.0	75.1	**17.8*****
	[896]	[869]	(3.0)
Baseline 2000	68.3	72.5	-4.2
	[978]	[892]	(4.9)
Difference 2001-2000	**24.7*****	2.6	**22.1*****
	(3.6)	(1.9)	(4.0)
Difference 2002-2000	**24.4*****	**6.7****	**17.7*****
	(4.0)	(2.5)	(4.7)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets.
*** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

[24] In four cases, individual schools appear to serve students in both intervention and control *comarcas*. This is a problem for interpretation of the estimated effects since, unlike the arguments we make regarding this form of contamination with respect to health post, crowding in schools may lead to individuals in the control group being discouraged from attending. The rate of increase in the control group, however, is similar to municipality level trends (taking out RPS schools), so we expect any such contamination bias to be small.

comparison, double-difference estimates of changes in enrollment due to PROGRESA were under 5 percent for primary school students (largely because enrollment in primary school in Mexico was already high) and around 12 percentage points for Grades 6–8 (Schultz 2000).

To examine what underlies the average effect of 17.7 percentage points on enrollment, we consider the effect of the program by age. The results are shown in Figure 5a, in which the bottom, dotted portion of each column shows the initial situation described earlier (Figure 3a), and the top, white, portion is the double-difference estimated average program effect. In all cases, this effect was positive (though it is not statistically significant for ages 10–12). Enrollment rates in the intervention areas are now 90–100 percent for those ages 7–12, and no longer vary by age.[25]

Figure 5a shows that gains were made in enrollment by reaching both younger children, who are now more likely to enroll (no longer delaying entry into school) and, at the same time, older children who had completed some schooling and left but now are returning. A potential concern for the latter group is 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 enrollment 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 5b presents results for enrollment by household expenditure group and by gender. Clearly, the extreme poor and poor are benefiting most—the relationship between enrollment and per capita expenditures has largely been erased. The effects for boys and girls were the same.

[25] Note that while these bar charts are a convenient way of summarizing program effects, it is not possible to interpret the sum of the two parts of each column as the enrollment rate with the program. This fact becomes evident, for example, in the subgroup of 9-year olds, for whom the sum is higher than 100 percent.

Figure 5a—*Red de Protección Social* (RPS) average effect on enrollment for 7-to-13-year-olds who have not completed fourth grade, by age

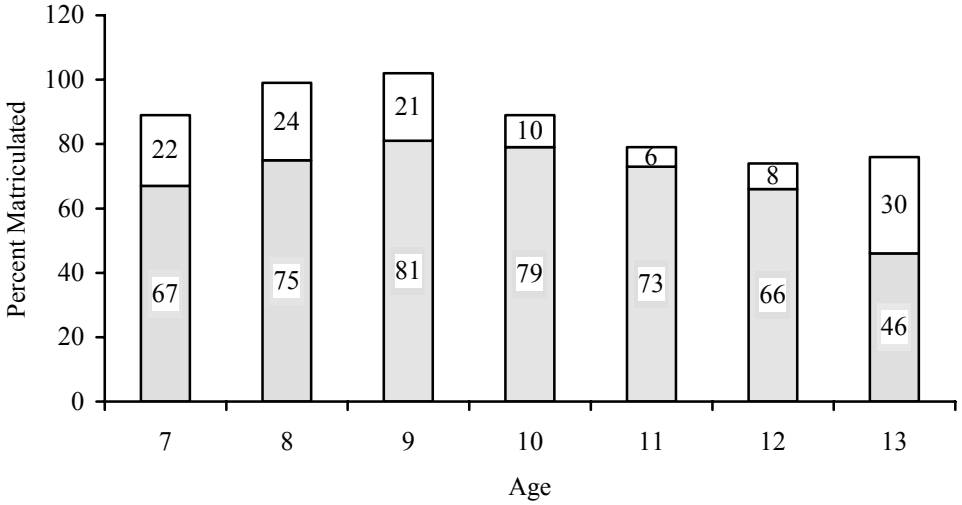
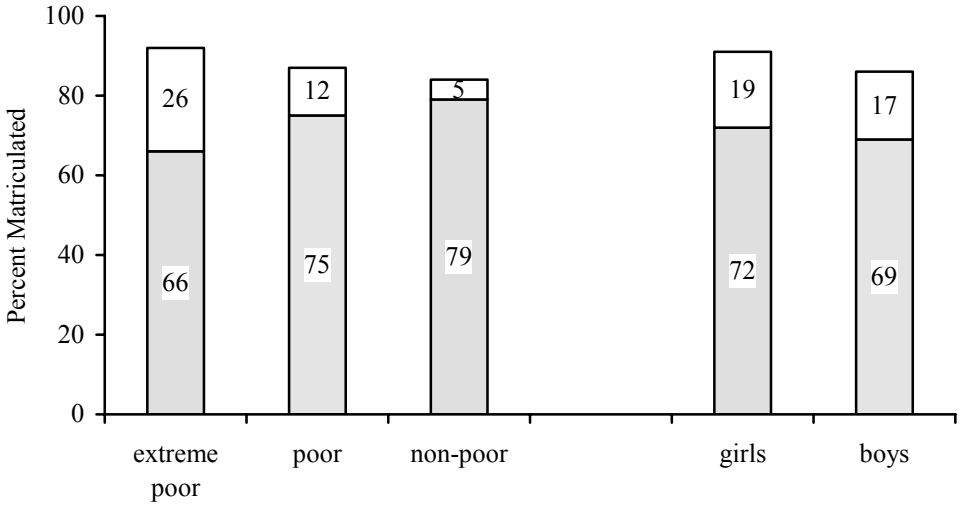


Figure 5b—*Red de Protección Social* (RPS) average effect on enrollment for 7-to-13-year-olds who have not completed fourth grade, by expenditure group and by gender



Of course, enrollment does not guarantee that a child will continue in school throughout the school year, nor does it mean that he attends school regularly. In order to continue receiving the education transfers, the program requires that no enrolled student have six or more unjustified absences in a two-month period (see Table 2). The effect of

the program on current attendance was even larger than that on enrollment, with an average program effect of 23 percentage points for children ages 7–13. The effect was significant for all age groups except 12-year olds (Figure 6a). As with enrollment, the extreme poor and poor benefited the most (Figure 6b). Nonetheless, the nonpoor also experienced significant gains in current attendance. Boys benefited more than girls, in part because of their lower starting point. These findings indicate that there have been positive effects, even for those children who were attending school prior to the program as they are now attending more regularly.

The final aspect of the effect of RPS on education we consider is grade advancement or continuation rates—the percentage of students in each grade who progressed two grades during the two years of RPS operation. Unlike the enrollment and current attendance results just presented, the effect of RPS on continuation rates is measured as a first difference, since information at two points in time is required to calculate progression. The estimated effect is the difference between the percentage of students continuing in the intervention areas and the percentage in the control areas. Overall, the effect is significant and shows an average improved retention rate of 6.5 percent. Table 11 shows the results by starting grade. Though not very precisely estimated for these subgroups, we still find significant effects for those starting in Grades 1 and 4 (in 2000). An unanticipated benefit of the program was the large effect on those making the transition from fourth to fifth and sixth grades. This effect was surprising, because enrollment in the fifth grade or higher was not one of the conditions for receiving the education transfer. Unfortunately, it was not possible to determine exactly why this is occurring. It may be simply an income effect of the program. It could also be due to potentially long-lasting changes in attitudes toward education. Finally, it may merely reflect confusion about the program requirements. Examining continuation rates for all four grades at once for the different expenditure groups (Table 12) shows that, as with the other measures, there is a tendency for the largest effects of the program to be concentrated among poorer households. The effects were similar for boys and girls.

Figure 6a—*Red de Protección Social* (RPS) average effect on current attendance for 7-to-13-year-olds who have not completed fourth grade, by age

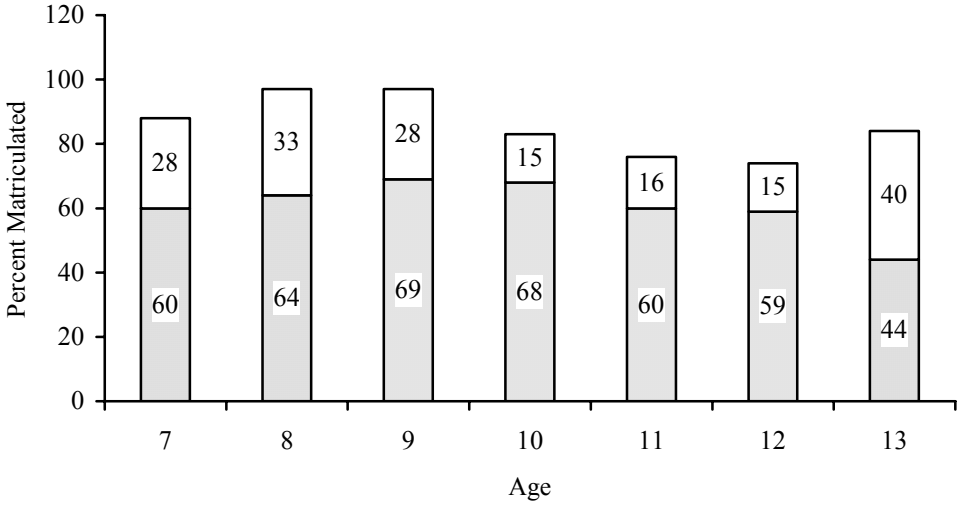


Figure 6b—*Red de Protección Social* (RPS) average effect on current attendance for 7-to-13-year-olds who have not completed fourth grade, by expenditure group and by gender

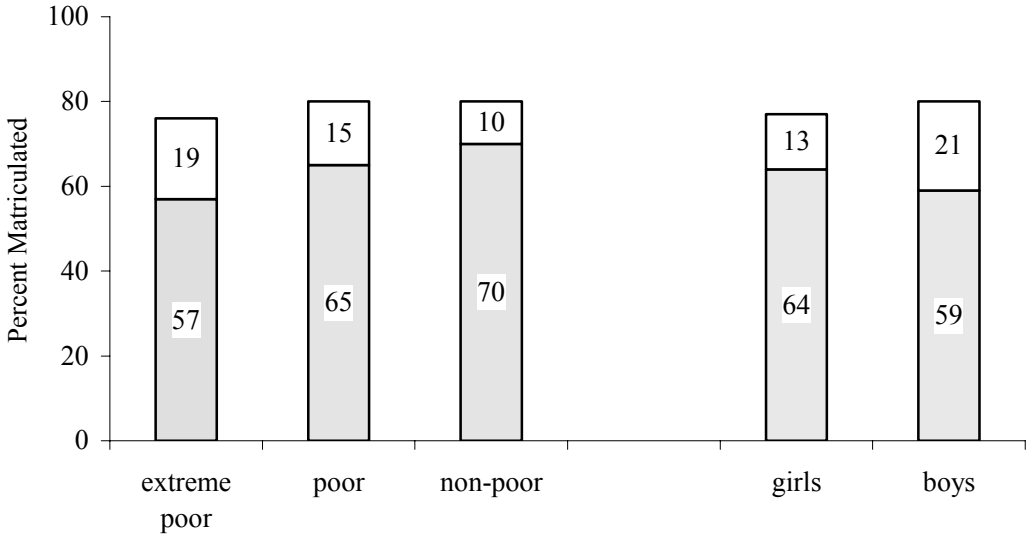


Table 11—*Red de Protección Social* (RPS) average effect on school advancement, children 7-to-13 years old in first through fourth grades (2000-2002), by starting grade

Grade	Grade 1	Grade 2	Grade 3	Grade 4
Intervention	93.3	90.0	82.7	84.7
	[328]	[150]	[139]	[118]
Control	85.6	85.8	77.0	73.7
	[340]	[190]	[122]	[95]
Difference	**7.7*****	4.2	5.7	**11.1****
	(2.9)	(3.6)	(6.0)	(4.4)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Table 12—*Red de Protección Social* (RPS) average effect on school advancement, children 7-to-13 years old in first through fourth grades (2000-2002), by poverty group

Group	Extreme poor	Poor	Nonpoor
Intervention	87.5	91.9	86.8
	[320]	[309]	[106]
Control	82.3	82.9	84.5
	[423]	[240]	[84]
Difference	**5.2***	**9.0*****	2.3
	(3.0)	(3.2)	(5.7)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

RPS had a massive effect on enrollment and current attendance 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 enrollment 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 enrollment in Grades 1–4 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 during that period.

While schools were generally available in RPS program areas as a result of the targeting described above, steps were taken to accommodate the large changes in enrollment as the program developed. The two principal steps 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.[26] 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 for the first year. In other cases, staffing problems were not resolved. Possibly reflecting these problems, enrollment rates were the lowest in this municipality, though they were still 90 percent, on average. In sum, the overall level of enrollment 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 children not enrolled, 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. We now examine whether the implementation of RPS reduced child labor for the target population children. For every age group, the percentage of children working was lower after the program (though in no case significantly so, given the relatively small sample sizes and low percentages reporting work). When we combine those children ages 7–13 who have not completed the fourth grade, the double-difference estimator shows a 4.9 percentage point decrease in the number of children working, significant at a

[26] 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, and Rawlings 1999).

7 percent level (Table 13). In both the intervention and control areas, the percentage of children working declined significantly. This likely reflects the general economic downturn in the program area and seasonal fluctuations in work patterns. Finally, while the effect on educational outcomes was the same for boys and girls (as described earlier), the effect on reported work for boys was twice as large as for girls.[27]

Table 13—*Red de Protección Social* (RPS) average effect on working, children 7-to-13 years old in first through fourth grades

Survey round	Intervention	Control	Difference
Follow-up 2002	5.9	12.9	**-6.9*****
	[807]	[883]	(2.3)
Follow-up 2001	5.7	10.3	**-4.5*****
	[895]	[868]	(1.4)
Baseline 2000	16.1	18.1	-2.0
	[1,028]	[930]	(2.8)
Difference 2001-2000	**-10.4*****	**-7.8*****	-2.5
	(1.9)	(2.0)	(2.7)
Difference 2002-2000	**-10.1*****	**-5.2*****	**-4.9***
	(1.8)	(1.9)	(2.6)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Child Health Care

Growth Monitoring and Development Program Participation

A necessary and central feature of the growth monitoring and development program (*Programa Vigilancia y Promoción del Crecimiento y Desarrollo*, hereafter VPCD) for children is the monthly visit (for children under age 2) and visits every other month (for children ages 2–5) to a health-care provider. Before the program, about 70 percent of children under age 3 had been taken for a well-child checkup in the previous

[27] This finding, like the finding that boys were more likely to work than girls, surely reflects in part how the questions about work were designed, focusing on work outside the home.

six months. In 2001, RPS produced a significant average increase of 19.5 percentage points in the percent of children under age 3 whose parents had taken them for a well-child visit in the past six months, but only an increase of 11.0 percentage points in 2002 (Table 14).[28] This deterioration in the effect from 2001 to 2002 was largely due to continued improvement in this indicator in the control group and reflects only a slight decline in intervention areas. Indeed, during the period 2000–02, the percentage taken to a health-care visit in the control group increased by 12.0 percentage points.

Table 14—*Red de Protección Social* (RPS) average effect on percent of children age 0-3 taken to health control in past six months

Survey round	Intervention	Control	Difference
Follow-up 2002	92.7	84.9	**7.9****
	[276]	[350]	(3.4)
Follow-up 2001	95.5	79.0	**16.5*****
	[377]	[391]	(5.1)
Baseline 2000	69.8	72.9	-3.1
	[437]	[435]	(6.2)
Difference 2001-2000	**25.7*****	6.2	**19.5*****
	(5.3)	(4.7)	(7.0)
Difference 2002-2000	**23.0*****	**12.0****	**11.0***
	(4.8)	(3.5)	(5.9)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

The RPS effect on the percentage taken to a health-care provider *and weighed* during the visit in the last six months, another key aspect of VPCD, was even larger (Table 15). As above, there was an increase in the control group over the period of 16.2 percentage points, but this change was swamped by a 33.7 percentage point effect in the intervention group, leading to an estimated average effect of 17.5 percentage points.

[28] We consider children ages 0–3 to cover the most vulnerable period in a child's development, as discussed below.

Here again, an increase in the control group explains in large part why the estimated effect of the program dipped from 2001 to 2002.

Table 15—*Red de Protección Social* (RPS) average effect on percent of children age 0-3 taken to health control and weighed in past six months

Survey round	Intervention	Control	Difference
Follow-up 2002	89.1	76.0	**13.1*****
	[276]	[350]	(4.2)
Follow-up 2001	91.5	67.0	**24.5*****
	[377]	[391]	(5.8)
Baseline 2000	55.4	59.8	-4.4
	[437]	[435]	(8.3)
Difference 2001-2000	**36.1*****	**7.2***	**28.9*****
	(6.1)	(3.6)	(7.0)
Difference 2002-2000	**33.7*****	**16.2*****	**17.5****
	(6.0)	(4.4)	(7.3)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

As with the effects for expenditures and schooling, average program effects for VPCD measures are larger among poorer households. Table 16 presents results by poverty group for the two indicators just discussed. It shows that average effects were much larger among the extreme poor and, at least in 2002, were not significantly positive for the poor or nonpoor. Most of the nonpoor already regularly took their children to the health centers before the program, and as a result, the estimated effects for this group were small and insignificant. An analysis contrasting the effects for girls and boys shows that they were no differences by gender (IFPRI 2003).

In addition to these measures, a variety of other process indicators related to VPCD visits for children under age 3 were examined, including whether the child had a health card, the child's weight was graphed on the card, the child's health card was up-to-date, and the child was given vitamins in the last six months. All of these showed patterns similar to the ones just described (IFPRI 2003).

Table 16—*Red de Protección Social* (RPS) average effect on percent of children age 0-3 taken to health control and weighed in past six months, by poverty group

Group	Extreme poor	Poor	Nonpoor
Taken to health control			
DD 2001-2000	**21.3****	**22.0****	10.8
	(8.6)	(8.3)	(8.7)
DD 2002-2000	**17.0****	9.6	-5.4
	(8.4)	(7.1)	(8.0)
Weighed			
DD 2001-2000	**34.9*****	**27.7*****	15.9
	(9.3)	(8.6)	(11.5)
DD 2002-2000	**23.6****	12.4	9.6
	(9.3)	(8.9)	(12.2)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Even though the indicators agreed upon by RPS and IADB (Appendix B, Table 26) did not include effects on children ages 3–5, the program and program requirements do include these children. On the whole, the results for all the indicators described for this older age group show an even greater effect than for their siblings under age 3 (IFPRI 2003).

It is important to emphasize that for most of the indicators considered, the control group also showed large improvements over the period, although on a much smaller scale. A possible explanation for this increase is that other providers are bringing health services into the areas not covered by the program (program providers do not offer or deliver any services to nonbeneficiaries). If this is the case, it could be considered as a kind of "contamination" of the randomized design of the evaluation. The implication of this is that the effect of the program as measured by the estimated double difference may actually underestimate the true effect—another indication of the conservative methodology used in this evaluation.

A second possibility explaining the increase in these indicators in the control group is that previous to the program, these services would have been offered by the existing (mostly government run) health posts and health centers. With the program,

demand for such services has dropped substantially. As a result, it is likely that waiting time diminished for those services, at least at the outset—increasing demand for them within the control group. Higher utilization within the control group would be the result. A final possibility is that RPS had a demonstration effect. Control and intervention *comarcas* are at times adjacent to one another. A household may be a beneficiary while its neighbor is a nonbeneficiary, particularly in a few cases where boundaries such as roads divide two *comarcas*. Seeing the activity and the emphasis placed on the RPS objectives may lead nonbeneficiaries to undertake behavior they would not have otherwise. Reasons for such actions could be many—including the possibility that the individuals thought this was a way to become eligible.

Vaccination for Children Ages 12–23 Months

There are a variety of indicators that can be used to evaluate the effect of RPS on vaccinations. One common indicator that summarizes the overall situation is up-to-date vaccinations for children ages 12–23 months.[29] For this measure, the 1998 Demographic and Health Survey shows the average coverage in rural areas of Nicaragua was 68 percent; in the RPS baseline 2000, it was 38 percent, reflecting the relative poverty of the program areas.

RPS produced an insignificant average net increase of 6.1 percentage points in up-to-date vaccination levels between 2000 and 2002 (see Table 17). There was little difference across poverty groups. Both intervention and control groups improved tremendously between 2000 and 2001 but saw setbacks between 2001 and 2002. Control areas increased 29.3 percentage points over the period, offsetting nearly all of the 35.4 percentage point gain in the intervention areas in the calculation of the double difference.

[29] According to the Nicaraguan Ministry of Health guidelines, a child age 12–23 months should have (at least) (1) one dose of BCG, (2) three doses of polio, (3) three doses of either Pentavalente or DPT, and (4) one dose of MMR (or possibly just measles if vaccinated before MINSA changed to MMR in 1998). We calculate whether they have completed the vaccine schedule to date, that is, given their current age in months, calling this up-to-date vaccination. For children under 18 months, the DPT booster is not required and is therefore not needed to be up-to-date; for those 18 months or older, it is.

These substantial gains are even more impressive when contrasted with national figures that show coverage in rural areas declining from 68 percent in 1998 to 60 percent in 2001. It appears that the coordination with the Ministry of Health in the distribution of vaccines to RPS health providers in these municipalities had an indirect effect—possibly strengthening the actions of the Ministry of Health or providing additional vaccines to the health posts serving the control group. Similar results are found for the complete vaccination at 24 months indicator (IFPRI 2003).[30]

Table 17—*Red de Protección Social* (RPS) average effect on percent of children age 12-23 months with updated vaccination

Survey round	Intervention	Control	Difference
Follow-up 2002	71.7	69.5	2.3
	[92]	[131]	(6.0)
Follow-up 2001	81.9	71.4	10.5
	[116]	[126]	(6.5)
Baseline 2000	36.4	40.1	-3.8
	[165]	[142]	(8.3)
Difference 2001-2000	**45.6*****	**31.3*****	14.2
	(6.2)	(6.5)	(8.9)
Difference 2002-2000	**35.4*****	**29.3*****	6.1
	(6.8)	(7.8)	(10.2)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

[30] When contrasted with RPS administrative data on vaccinations for beneficiary children, which show coverage of nearly 100 percent, the coverage reported in Table 17 and calculated for other vaccine measures are substantially lower. Reasons for the discrepancy include (1) the analyses in this paper do not condition on being a beneficiary but only on living in an intervention community; (2) recall may not be complete and accurate when the health card is unavailable during the interview; (3) even when provided, the health card may be incorrect; and (4) due to the logistics of visiting communities, there is a small possibility of interviewing a child just after she turns old enough for a certain vaccine but has not had another visit to the health-care provider (it is prohibited to administer vaccines before the minimum age set by the Ministry of Health). When one considers vaccine coverage for this group vaccine by vaccine, most are covered at 90 percent or above (IFPRI 2003); only when all are considered together do the coverage rates dip. This could be consistent with minor errors as described above. Finally, in addition to simply having been vaccinated, being vaccinated at the correct time is important—late vaccinations can have deleterious consequences for effectiveness and the coverage of the population as a whole (Bolton et al. 1998). Since we do not consider directly the date of vaccination, ignoring the measurement concerns just outlined, these estimates tend to overestimate overall coverage.

While the double-difference average effect estimate of the effect of RPS on vaccination coverage is neither large nor statistically significant, even with the variety of measurement problems described in footnote 30, it is all but impossible to avoid the conclusion that RPS had a large and positive effect on vaccination rates in the program areas.

Child Nutritional Status

In a separate interview module implemented in 2000 and 2002 only, the RPS assessed the nutritional status of all children under age 5 in survey households, measuring their height, weight, and hemoglobin.[31] In this section, we explore whether the improved household diet found in the section on expenditures, and the improved health-care services just described (including the growth and monitoring of children), have been accompanied by improvements in the nutritional status of children under age 5. We first define the indicators we use to measure child nutritional status and how they should be interpreted (height-for-age, weight-for-height, weight-for-age, and hemoglobin) and then describe the findings, placing them in the context of both Nicaragua and Central America, an analysis made possible by the existence of comparable data across countries.

In large part because they are growing so fast, young children have high nutritional requirements. Unfortunately, the diet commonly offered to young children in developing countries to complement breast milk is of low quality (i.e., monotonous and with low energy and nutrient density) and, as a result, multiple nutrient deficiencies are common. At the same time, young children are also very susceptible to infections, because their immature immune systems fail to protect them adequately. In developing countries, foods and liquids are often contaminated and are thus key sources of frequent infections. Infections reduce appetite, increase nutrient loss, and increase metabolic demands. Finally, in many societies, inadequate traditional remedies for childhood

[31] The weight and height for all children ages 6–59 months in the RPS baseline 2000 and follow-up 2002 households were measured according to standard international procedures. The anthropometric indicators were calculated using EpiInfo v6.4c, eliminating all the observations that EpiInfo indicated had an error in measurement.

infections, including withholding of foods and breast milk, are common. Thus infection and malnutrition reinforce each other. Focusing on the nutritional status of children is a powerful way to assess the effects of RPS, in particular because improvements during this vulnerable period persist throughout one's life and therefore have long-lasting positive benefits (Martorell et al. 1995).

Severe protein-energy malnutrition presents itself as kwashiorkor and marasmus, syndromes that are characterized by clinical signs, marked metabolic disturbances, and high fatality rates in the absence of high-quality care (Waterlow 1992). Severe malnutrition is rare, however, even in very poor countries. Most malnutrition in children is best described as mild to moderate and is measured in terms of growth failure against a standard reference population. Weight and height are expressed as age-specific Z-scores[32] (described below) and the criterion of a Z-score of less than –2 for height-for-age, weight-for-height, and weight-for-age is commonly used to identify stunting, wasting, and underweight, respectively. The percentages of stunted and underweight children are standard ways of describing the extent of child malnutrition in societies (UN ACC/SCN 2000). The following are the standard indicators.

Height-for-age reflects linear growth before and after birth. A short stature refers to short height-for-age that can reflect either normal variation in growth or a developmental deficit. This deficit or retardation in growth is the cumulative effect of poor nutrition or inadequate health for an extended period of time. A child is considered to be in deficit, or stunted, if his or her height-for-age Z-score (HAZ) is two or more standard deviations (SD) below the median of the international gender-specific reference population of the National Center of Health Statistics of the United States of America/World Health Organization, known as "NCHS/WHO" (WHO Expert Committee 1995).

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[32] Z-scores are used to normalize measured heights and weights against those found in well-nourished populations. They are age- and sex-specific; for example, a Z-score of height-for-age is defined as measured height minus median height of the reference population all divided by the standard deviation for that age/sex category.

Weight-for-height measures body weight relative to stature and similar to height-for-age, a child is considered to be in deficit, or wasted, if his or her weight-for-height Z-score (WHZ) is more than two standard deviations (SDs) below the median of the international reference population NCHS/WHO. In general, this condition is the product of recent experience such as a severe lack of food or serious illness, which causes substantial weight loss. It is also possible that the deficit originates from chronic nutrition deficiency or chronic illness.

Weight-for-age measures body mass relative to age. It is influenced both by stature and weight of children and, therefore, when there is a deficit, it is an indicator of both retarded growth and of weight loss. For this reason, it is difficult to interpret. When there is no deficit in weight-for-height, weight-for-age also indicates the accumulated effects of poor nutrition or health of an individual (or population), similar to the interpretation for HAZ. Weight-for-age deficit is defined as weight-for-age Z-score (WAZ) more than two SDs below the median of the international reference population NCHS/WHO. The statistical prevalence expected for this deficit, as well as for those for HAZ and WHZ, is 2–3 percent in a healthy population.

Anemia is hemoglobin concentration lower than a reference cutoff value. Anemia can be caused by nutritional deficiencies in iron, folic acid, vitamin B_{12}, or other nutrients. Diseases or hereditary disorders also can cause anemia. Therefore, hemoglobin as an indicator of iron deficiency has a low specificity. However, it is responsive to improvements in iron intakes in deficient populations, has a functional significance, and is suitable for field settings. Low hemoglobin in developing countries is a good indicator of iron deficiency anemia. In this analysis, we use the international suggested cutoff point of 11.0 g/dlL for children 6–59 months (MI 1999).

In 1965–67, Costa Rica had 27 percent stunting (not shown). By 1982, that prevalence had fallen to 8 percent and stabilized (see Table 18, HAZ). The example of Costa Rica shows that it is possible to substantially reduce malnutrition in a Central American country. Guatemala has the highest malnutrition for children under age 5 in the region; in 1998–99, 46 percent of its children were stunted. Nevertheless, since 1987,

Guatemala has been reducing the prevalence slowly, at a rate of about 1 percent per year. Honduras and El Salvador, however, showed little progress over the same period.

Table 18—Malnutrition in Central American countries

Country and year	N	Percent HAZ < -2.0 SD	Percent WAZ < -2.0 SD	Percent WHZ < -2.0 SD
Costa Rica				
1982 (0-71 months)	2,250	7.6	6.3	1.9
1996 (12-83 months)	1,008	6.1	5.1	2.3
Guatemala				
1987 (3-35 months)	2,229	57.7	33.2	1.3
1995	7,768	49.7	26.6	3.3
1998/99	3,591	46.4	24.2	2.5
Honduras				
1987	3,244	37.2	20.6	1.7
1991	5,961	36.3	18.0	1.5
1993/94 (12-59 months)	1,875	39.6	18.3	2.0
1996	1,307	38.9	25.4	1.4
PRAF 2000 (subnational)	5,563	52.8	27.7	1.3
Nicaragua				
1993/94	3,546	22.5	11.0	1.9
1997/98	6,497	24.9	12.2	2.2
RPS 2000	1,199	41.4	15.0	0.7
El Salvador				
1988	>1,539	29.9	15.2	-
1993	3,515	23.1	11.2	1.3
1998	6,523	23.3	11.8	1.1

Source: (*http://www.who.int/nutgrowthdb*), IFPRI (2001c), and RPS 2000 and 2002 surveys.

Notes: Percentages less than –2 SD below the mean of the international reference population NCHS/WHO (WHO expert committee 1995) of children between 0 and 59 months (except where noted) in national surveys and the RPS surveys.

Malnutrition in Nicaragua has been stable, with little sign of improvement since 1987. The prevalence of stunting for 1997–98 is low (25 percent showing retarded growth) in comparison with Guatemala and Honduras (36–40 percent), similar to El Salvador (23–30 percent), and high in comparison to Costa Rica. Table 18 shows that before the program, 41.4 percent of children under age 5 living in the RPS program areas suffered from retarded growth due to malnutrition or illness. This is 1.7 times greater than the national prevalence for the 1997–8 period and nearly 20 times greater than the expected statistical prevalence for a healthy population. The areas where RPS operates

exhibited substantially higher malnutrition than the national average. This is due in part to the fact that, via targeting, poverty levels are high in these areas. In this respect, RPS is similar to PRAF, where the level of stunting is also much higher than the national average for Honduras and where the children do not show recent severe lack of food and/or serious illness (0.7 percent with a WHZ more than 2 SD below the norm for RPS). Lastly, malnutrition increases with level of poverty within the RPS baseline 2000 sample, as well. The poorest deciles (1 and 2) show the highest levels of stunting. Less poor households have lower malnutrition and best-off households in the project areas do not show, on average, marked growth deficits, particularly the wealthiest 20 percent.

Table 19 shows the double-difference estimate calculations for the percentage of children under age 5 who were stunted. Before the program, intervention and control areas showed very similar high rates of stunting. After two years of operation, stunting declined in the intervention areas by 4.7 percentage points, but was essentially stable in the control areas. The average net effect of RPS was to reduce stunting prevalence by 5.3 percentage points.

Table 19—*Red de Protección Social* (RPS) effect on percentage of children under 5 years of age who are stunted (HAZ < -2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	37.1	41.5	-4.3
	[479]	[557]	(4.8)
Baseline 2000	41.9	40.9	1.0
	[614]	[585]	(4.5)
Difference 2002-2000	**-4.7****	0.6	**-5.3***
	(1.8)	(2.6)	(3.1)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets.
*** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

The current nutritional situation, as measured by wasting, is not a major concern in Nicaragua or in the program areas and, unsurprisingly, did not change substantially as

a result of RPS. As shown in Table 20, 0.4 percent of children under age 5 were wasted in the intervention areas in 2002 and 0.2 percent in the control areas.

Table 20—*Red de Protección Social* (RPS) effect on percentage of children under 5 years of age who are wasted (WHZ < -2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	0.4	0.2	0.2
	[479]	[557]	(0.3)
Baseline 2000	1.0	0.3	0.6
	[614]	[585]	(0.5)
Difference 2002-2000	-0.6	-0.2	-0.4
	(0.4)	(0.3)	(0.5)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

With no evidence of wasting, weight-for-age is best interpreted as an alternative indicator of chronic malnutrition. Table 21 shows the effect of RPS on WAZ and, as expected, it is a pattern similar to that seen in Table 19.

Table 21—*Red de Protección Social* (RPS) effect on percentage of children under 5 years of age who are underweight (WAZ < -2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	10.4	15.8	-5.4*
	[479]	[557]	(3.1)
Baseline 2000	15.3	14.7	0.6
	[614]	[585]	(2.5)
Difference 2002-2000	**-4.9****	1.1	**-6.0****
	(1.8)	(1.9)	(2.6)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Before RPS, 15.3 percent of the children under age 5 in the intervention areas were underweight for their age. Two years later, the prevalence of underweight children had declined by 4.9 percentage points; at the same time, the prevalence increased by about 1 percentage point in the control areas. The net effect, then, was that RPS significantly reduced the prevalence of underweight children by 6.0 percentage points.

In addition to examining prevalence rates as in the previous tables, we can also measure program effects by considering the continuous measures of nutritional status upon which the prevalence rates are based. We do this for height-for-age Z-scores in Table 22, finding a statistically significant net average improvement due to RPS of 0.17. Although the sample is not large enough to distinguish the effects among poverty groups, the point estimates suggest that the effects are larger than 0.20 for both the extreme poor and poor groups. In two years of operation, RPS has significantly reduced stunting in children under age 5.[33] Because there are a variety of possible program effects through which child nutrition might improve—some of which we have documented here, such as food expenditures and the well-child health visits—we are careful to claim only that child

Table 22—*Red de Protección Social* (RPS) effect on HAZ for children under 5 years of age

Survey round	Intervention	Control	Difference
Follow-up 2002	-1.65	-1.80	0.14
	(1.15)	(1.18)	(0.13)
Baseline 2000	-1.79	-1.76	-0.02
	(1.14)	(1.15)	(0.11)
Difference 2002-2000	**0.13*****	-0.03	**0.17****
	(0.05)	(0.07)	(0.08)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001). *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

[33] We have also explored differences by sex in all the nutritional status indicators, finding no statistical differences. For that reason (and because of the small sample sizes), we do no present results separated by gender.

nutrition improved as a result of the program; we do not specify the specific channels through which this was achieved.

Results from the two previous national Demographic and Health Surveys (1998 and 2001) show that Nicaragua has reduced stunting by about 1.5 percentage points per year over three years. RPS, in only two years, reduced it by 5.3 percentage points, an annual rate of decline 1.7 times larger than the national trend. Very few programs in the world have been able to rigorously demonstrate such a substantial decrease in stunting in such a short time.

Iron deficiency anemia is a severe problem affecting the entire Central American isthmus, where it is most severe in Guatemala (nearly 50 percent), Honduras, and El Salvador. In 2000, 33 percent of children ages 6–59 months in RPS program areas exhibited iron deficiency anemia. This is substantially higher than the prevalence found in a 1993 national survey in Nicaragua (28 percent), again likely reflecting the RPS targeting of poorer than (national) average areas. One feature of the RPS VPCD was provision of iron supplements to children. In Table 23, we show that the program indeed had a significant effect on the percentage of mother's receiving ferrous sulfate for their children in the past four months, as measured in the anthropometric survey. The double-difference estimated average effect was 36.1 percent in 2002.

Table 23—*Red de Protección Social* (RPS) average effect on percent of children ages 6-to-59 months given iron supplement (ferrous sulfate) in past four months

Survey round	Intervention	Control	Difference
Follow-up 2002	79.8	40.1	**39.7*****
	[440]	[504]	(4.4)
Baseline 2000	23.6	20.0	3.6
	[573]	[541]	(5.4)
Difference 2002-2000	**56.2*****	**20.1*****	**36.1*****
	(4.5)	(3.3)	(5.5)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Despite this apparent success in distributing iron supplements, after two years of operation, RPS had not succeeded in improving the grave situation on anemia—the double-difference estimator is both insignificant and tiny in magnitude (Table 24). Unsurprisingly, the results for hemoglobin mirror those for anemia—there was no average program effect (Table 25).

Table 24—*Red de Protección Social* (RPS) effect on percentage of children 6-to-59 months of age with anemia

Survey round	Intervention	Control	Difference
Follow-up 2002	32.8	30.9	1.9
	[448]	[515]	(5.8)
Baseline 2000	33.7	31.5	2.2
	[579]	[549]	(4.0)
Difference 2002-2000	-0.1	-0.6	-0.2
	(0.5)	(0.5)	(6.8)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001); number of observations are shown in brackets. *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

Table 25—*Red de Protección Social* (RPS) effect on average hemoglobin for children 6-to-59 months of age

Survey round	Intervention	Control	Difference
Follow-up 2002	11.2	11.4	-0.2
	(1.3)	(1.3)	(0.2)
Baseline 2000	11.4	11.5	-0.1
	(1.4)	(1.5)	(0.1)
Difference 2002-2000	-0.1	-0.0	-0.1
	(0.2)	(0.1)	(0.2)

Source: RPS baseline 2000 and RPS 2001 and 2002 follow-up surveys.

Notes: Standard errors correcting for heteroscedasticity and allowing for clustering at the *comarcas* level are shown in parentheses (StataCorp 2001). *** indicates significance at the 1 percent level; ** at the 5 percent level; and * at the 10 percent level.

While Table 23 showed that twice as many children in intervention areas had received iron supplements in the last four months compared to control areas, it is not possible to ascertain from these data whether complete doses were received during *each* VPCD visit, or whether the supplements were actually ingested. Program administration data show that there were severe shortages of vitamins, iron supplements, and antiparasites during 2001, so it is likely that complete supplements were not received at each visit. Unfortunately, this is something that the four-month reference period for the survey question on supplementation delivery would not reflect. The estimated effect on provision does not appear to be telling the whole story. Shortages and inconstant or incomplete delivery to children present one possible reason for the failure to improve hemoglobin in the population, as well as the fact that even though the program effect was massive, fully one-fifth of the children under age 5 in the intervention areas had not received a supplement in the past four months.

Another possibility is that children are deficient in other micronutrients, potentially limiting the hematological response to iron supplementation. Allen et al. (2000) failed to find an improvement in hemoglobin in children ages 18–36 months supplemented with iron over 12 months in a controlled experiment. They concluded that the failure of the treatment could not be attributed to failure to take the supplement, inadequate length of supplementation, or inadequate absorption of the iron provided.

One thing that can be expected from an iron supplementation intervention is an increase in the reserves of iron in the human body. Future evaluations of RPS may benefit from measuring changes in reserves of serum ferritin and other indicators to gain a more complete picture of the effect on the state of iron nutrition. In addition, including iron supplementation as a theme in the health education workshops should be considered.

5. Conclusions

This paper presented the main findings of a quantitative evaluation of a randomized community-based intervention, RPS, against its primary objectives. Where

possible, we erred on the side of assessing effect in conservative manners, for example, in the calculation of standard errors and treatment of possible contamination. In many instances, for example when assessing the effects on expenditures during what turned out to be an economic downturn, the critical importance of a control group for the evaluation was evident—without one it would have been next to impossible to make reliable assessments of the program effect. Even with one, it was at times difficult to make unequivocal assessments of effects for some indicators, since the evaluation did not occur in a vacuum, and a variety of actors not under the control of the program continued to operate in program areas. Working closely with the implementing team proved to be important, so that the IFPRI group was made aware of external influences that could not be gleaned from quantitative surveys.

Overall, we found that RPS had positive and significant double-difference estimated average effects on a broad range of indicators and outcomes. Where it did not, it was often due to similar, though smaller, improvements in the control group. Nearly all estimated effects were larger for the extremely poor, often reflecting their lower starting points (e.g., percentage of children matriculating before the program). Among poorer beneficiaries there was simply more potential for improvement on many of the indicators. As a result, the program has reduced inequality of these outcomes across expenditure classes. The findings presented here played an important role in the decision by the Government of Nicaragua and IADB to continue this effective program.

RPS in its pilot phase supplemented per capita annual total household expenditures by 18 percent, on average. For beneficiary households, this increase compensated for the large income loss experienced by nonbeneficiaries during this period, while producing a small overall increase in expenditures. Most of the increase in expenditures was spent on food; the program resulted in an average increase of C$566 in per capita annual food expenditures and an improvement in the diet of beneficiary households. Expenditures on education also increased significantly, though there was no discernable effect on other types of investment expenditures. Labor market participation was apparently little changed with the program, though there was an indication of slightly

fewer hours worked, on average, in the last week. The economic difficulties experienced by these communities enabled RPS to operate somewhat like a traditional social safety net, aiding households during a downturn.

For schooling, RPS produced a massive average net increase on enrollment of 17.7 percentage points and an even larger effect of 23.0 percentage points on current attendance for the target population. Examining the number of children in Grades 1–4 who advanced two grades between 2000 and 2002, RPS led to an average increase of 6.5 percentage points, despite the fact that advancement past Grade 4 was not a formal requirement of the program. In tandem with the increased schooling, the percentage of children 7–13 years working declined by 4.9 points.

For child health care, RPS induced an average net increase of 11.0 percentage points in the participation of children under age 3 in VPCD. At the same time, the services provided by the program, as measured by process indicators, including whether the child was weighed and whether their health card was updated, improved even more. Participation by children ages 3–5 also increased substantially. While it is not possible to statistically demonstrate that RPS increased vaccination coverage for children ages 12–23 months in the intervention group (relative to the control group), it was demonstrated that vaccination rates climbed over 30 percentage points in the intervention and control areas at a time when they were, on average, decreasing in rural areas nationally. One would be hard pressed not to attribute at least some part of this substantial improvement to RPS.

Finally, the more varied household diet and increased use of preventive health-care services for children have been accompanied by an improvement in the nutritional status of beneficiary children under age 5. The net effect was a 5-percentage point decline in the number of children who were stunted. This decline is more than 1.5 times faster than the rate of annual improvement seen at the national level between 1998 and 2001. Very few programs in the world have shown such a decrease in stunting in such a short time. Despite improvements in the distribution of iron supplements to these same children, however, RPS was unable to improve hemoglobin levels or to lower rates of anemia.

RPS has improved a number of the indicators included in the Nicaraguan national poverty reduction strategy, during a time in which many of them are not on track to achieve the goals set out in that plan (World Bank 2003). The preponderance of evidence from the evaluation suggests that if the program were expanded elsewhere in poor rural areas of Nicaragua, it would be equally successful. As such, it could prove to be an important component of Nicaragua's overall poverty reduction policy. Before expanding to other parts of the country, however, policymakers had to consider the costs—was RPS cost-effective in achieving these outstanding results in comparison to other possible programs or policies?

In a related paper, Caldés, Coady, and Maluccio (2004) compare costs for PROGRESA, PRAF, and RPS. While they fall short of actually assessing cost-effectiveness, which is complicated by the multiple objectives of each program, they find that the pilot phase of RPS was the "most expensive" of the three in delivering a dollar's worth of transfer to a beneficiary household, though it still appears to have been reasonably cost-efficient, given its complicated design (with an involved supply-side component that the other two programs did not have) and its small size, particularly in comparison to PROGRESA. Furthermore, RPS arguably had the most impressive effects. The authors highlight the various costs associated with a number of special program features thought to be crucial, in particular, targeting, monitoring, and conditioning, all of which require resources above and beyond the transfers alone. Removing these costs reduces the administrative costs substantially, but it is not possible to determine by how much it would reduce the effectiveness of the program—indeed, their removal could represent a false savings.

Another crucial question that the current evaluation cannot answer is whether the changes seen here will persist after the program exits, or whether there are longer-term effects that have not been captured in what is only a short-term, two-year evaluation. A companion qualitative study aimed in part at answering those questions, as well as uncovering some of the mechanisms underlying the changes seen or not seen in the quantitative evaluation (such as the lack of an effect on anemia), is underway at this

writing. Furthermore, RPS has recently delivered the final demand-side transfers in the original intervention areas, though it is scheduled to continue offering health-care services and teacher transfers until the end of 2005. We will return to the field in October 2004 to examine the effects of that transition, and begin to understand better the sustainability of the large changes achieved by RPS.

Appendix A: Household Targeting in Geographically Targeted Areas

After implementing a registry census in May 2000 (known as the RPS population census I), RPS excluded a small percentage of households who, even though they were verified to be living in the geographically targeted rural areas, appeared not to be extremely poor. This decision was taken, in part, because the intervention areas had substantially more than 5,000 households—the desired number of beneficiaries in the program during the first stage of the pilot phase. Households satisfying one or both of the following were excluded:

1. Own a vehicle, truck, pickup truck, or jeep;
2. Own more than 20 *manzanas* (14.1 hectares) of land.

Based on these criteria, 169 households (2.9 percent of the households living in the intervention areas as reported in the May 2000 RPS census population) were excluded from the program. In addition to these households, 219 (3.8 percent) households were excluded after the orientation assemblies and program registration for one or more of the following reasons:

1. household comprising a single man or woman who was not disabled,
2. household with significant economic resources or a business,
3. household that omitted or falsified information during the RPS population census.

Finally, 240 (4.2 percent) households did not attend the orientation assembly or chose not to participate. Thus in the first stage of the pilot phase, the program excluded a total of 628 (10.9 percent) of the 5,741 rural households interviewed in the RPS population census of May 2000. An additional 882 households were included as beneficiaries when it was discovered that the May 2000 RPS population census had missed 949 households in the targeted areas (these were integrated into the registry

during the RPS population census II carried out in September 2000 and described in IFPRI (2001b). These households were not included in the original sample frame for the evaluation survey and thus are not included in the evaluation. An examination of their characteristics (collected in the RPS population census) shows that, on average, they tend to have fewer resources than the households included in the evaluation survey. Since most of the program effects were larger for the less well-off, their exclusion from the evaluation is likely to make the average estimated effects smaller (or more conservatively estimated), though probably not by very much, since they represent only about 15 percent of households in the area.

Appendix B: Table

Table 26—Indicators for (RPS) evaluation in Inter-American Development Bank (IADB) loan contract

	Indicator	Goal
1.	Percentage of children under age 3 years of age who participate in the growth and development monitoring program (VPCD)	An increase of more than 10 percentage points in the intervention group over the control group
2.	Percentage of children between 12 and 23 months of age that have received all necessary vaccinations according to Ministry of Health guidelines	An increase of more than 10 percentage points in the intervention group over the control group
3.	Percentage of households that have increased spending on food, as a fraction of the total household expenditures	Observe the tendency of the change
4.	Percentage of children in the first through fourth grades who continue in school	An increase of more than 10 percentage points in the intervention group over the control group
5.	Percentage of children in the first through fourth grades who have matriculated in school.	An increase of more than 8 percentage points in the intervention group over the control group
6.	Percentage households included in the program that are extremely poor	More than 70 percent
7.	Percentage of households included in the program that are *not* extremely poor	Less than 30 percent

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